WHAT SHOULD SOCIOLOGY DO ABOUT DARWIN?: EVALUATING SOME POTENTIAL CONTRIBUTIONS OF SOCIOBIOLOGY AND EVOLUTIONARY PSYCHOLOGY TO SOCIOLOGY

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Submitted to the faculty of the University Graduate School
in partial fulfillment of the requirements
for the degree
Doctor of Philosophy
in the Department of Sociology
Indiana University
December 2000

ACKNOWLEDGMENTS

No need to be mushy; after all, this is only a dissertation. The contributions of others to the graduate career which has culminated in the completion of this document are many and have taken numerous forms, from friendships that will always be cherished to brief conversations that provoked a cascade of enjoyable thought. Instead of being maudlin about it, let me be abecedarian. Forthwith, the support of the following people during my studies at Indiana University is gratefully acknowledged: Sharon Abbott, Art Alderson, Bob Althauser, Julie Artis, Lisa Aucoin, Dave Brady, Buffy Summers, Giovani Burgos, Simon Cheng, Rob Clark, Bill Corsaro, Lara Croft, Doug Freese, Eldon Freese, Ruth Freese+, Peg Freese, Shannon Freese, Rob Fulk, Tom Gieryn, Jan Carolus Gnida, John Gnida, Liahna Gordon, Barb Halpenny, Sara Hare, Kathryn Henderson+, Carol Hostetter, Cher Jamison, Sandy Jones, Amy Kroska, Scott Long, Keri Lubell, Lucille, Karen Lutfey+, Jane MacLeod, Erin Maher+, Doug Maynard, Chad Menning, Brian Powell+, Jude Richter, Jason Schnittker, Aileen Schulte, Karen Segar, Jean Shin, Carla Shirley, Faye Smart, Robert Soto, Sheldon Stryker, Jocelyn Viterna, Terri Winnick, and Mark Zacharias. Within this roster, I have used a plus sign (+) to acknowledge instances of persons going above or beyond the regular "going above or beyond" that so many people have done for me over the years: its use is intentionally rare, to denote the inexplicable and inestimable levels of generosity and support that I have received from some quarters. I am also thankful for the financial support that I have received from the National Science Foundation, the National Institute of Mental Health, and Indiana University.

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Critics both within and outside of sociology have claimed that the discipline is mired in a long-running “crisis” marked by theoretical sterility and diminished public credibility. Some have suggested that one way for sociology to revitalize itself is to adopt a foundation more explicitly and closely based on the insights of evolutionary biology. Taking up these calls to give Darwinian approaches a fresh look, this dissertation examines recent work in these programs and evaluates their potential contributions to sociology. The heart of the dissertation is a series of five “case studies” that examine specific theories that have been offered by evolutionary scholars. The topics of these case studies are: (1) Cosmides and Tooby’s work on social exchange; (2) Thornhill and Palmer’s work on rape; (3) Sulloway’s work on birth order; (4) Fisher’s work on divorce; and (5) the Trivers-Willard hypothesis for parental investment. Included also are extended discussions of evolutionary psychological theories regarding cognitive limitations on group size, female waist-to-hip ratio, parent-offspring conflict, and family structure effects on socioeconomic attainment. In each of the five case studies, both the evidence that has been provided in support of the theory and the strength of its evolutionary reasoning are examined. In several instances, original research projects also examine particular hypotheses. In the concluding chapter, lessons are drawn from these case studies in a discussion of how sociologists can better engage and potentially use Darwinian approaches in their own work.

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1

INTRODUCTION

> *"If I were to give an award for the single best idea that anyone has ever had, I'd give it to Darwin, ahead of Newton and Einstein and everyone else. In a single stroke, the idea of evolution by natural selection unifies the realm of life, meaning, and purpose with the realm of space and time, cause and effect, mechanism and physical law."*
>
> —Daniel Dennett, *Darwin's Dangerous Idea*, 1995

By no means is Daniel Dennett alone in his enthusiasm for the idea of evolution by natural selection as a way to explain how complex structures develop and change. Darwin's idea, with various modifications, has become the dominant and overarching theory by which biologists understand the origins of human beings, the structure and function of human organs, and the relationship between humans and the rest of the animal kingdom. Perhaps the best testament to the power of the theory is that it has attained this authoritative standing despite persistent and highly formidable resistance from religious authorities. Many of the greatest minds of mid-19th century Europe and America set out on the task of proving Darwin wrong and debunking his radical challenge to the idea of a nature built and governed by a Divine will. Today, when evolutionists and creationists engage in scientific debate, evolutionists are able to lay out an comprehensive synthesis from an array of fields (e.g., biology, zoology, paleontology, biochemistry, virology, comparative anatomy, bioanthropology), while creationists are left with little more than some specious claims about fossils and the defense that natural selection will never be more than "just a theory" (see, e.g., Kitcher 1982, Berra 1990). Biblical creationism of course still has its adherents among the broader public, especially in the United States, but, in the world of science, the defeat of creationism by Darwinism has been complete (see Numbers 1992). Some of the more zealous promoters of evolution could be faulted for their smugness, perhaps, but the prevailing view in biology today is that are no good scientific grounds to challenge the decisiveness of Dennett's quote above or his statement that "[A]nyone today who doubts that the variety of life on this planet was produced by a process of evolution is simply ignorant—inexcusably ignorant, in a world where three out of four people have learned to read and write" (1995: 46).

Darwinian enthusiasts have been less successful in their efforts to extend the theory to questions of why human beings behave as they do and why human societies are organized as they are. At first blush, their project seems obvious: If evolutionary theory can explain why human beings walk upright and why they have opposable thumbs, then shouldn't it be able to explain why humans fall in love and why they fight wars? To draw out the question more fully: If human societies are ultimately comprised of individual humans and their behaviors, and if human behaviors are ultimately controlled by human brains, and if the human brain is ultimately just another human organ, and if complex human organs ultimately acquired their particular structure and function through natural selection, then shouldn't we look to natural selection to provide ultimate explanations about the character of human societies? Yet many social scientists have resisted the application of evolutionary theory to their domains, and, despite a recent resurgence of interest, Darwinian approaches have spent the last several decades on the margins of social science.[1] Proponents of these approaches have argued that the resistance of most social scientists is scientifically untenable, and instead results from social scientists being a combination of (a) ignorant of biology, (b) blinded by political prejudices, and (c) protective of their intellectual turf. Take, for

[1] See also the chapter on "Why Have the Social Sciences Failed to Darwinize?" in *A Natural History of Rape* (Thornhill and Palmer 2000).

example, Darwinian sociologist Pierre van den Berghe's (1990:173) broadside against the rest of his discipline:

> The general failure of sociologists to understand, much less accept, an evolutionary perspective on human behavior transcends mere ignorance and ideological bias, although it incorporates a good deal of both. It also includes a general anthropocentric discomfort with evolutionary thinking, a self-interested resistance to self-understanding, and a trained sociological incapacity to accept the fundamental canons of scientific theory construction.

Because they are often skeptical of evolutionary explanations for social phenomena, sociologists and other social scientists are regularly accused of tacitly or unwittingly endorsing a creationist perspective. Michael Shermer (1996: 66) asserts that "historians, sociologists, and many in the humanities" are "cognitive creationists" who believe that "the mind, including intelligence, emotions, beliefs, and attitudes, is immune to laws of nature, be they biological or environmental."[2]

To be sure, some sociologists are strongly resistant to evolutionary explanations of behavior on political grounds, and some may be so adamant in their rejection of any sociobiological theorizing that their resistance may seem tantamount to positing a special creation for humans (see, e.g., discussions by Udry 1995; Ehrenreich and McIntosh 1997; Lopreato and Crippen 1999). The problem with this critique is that most social scientists would not actually deny that human beings and their brains are the product of evolution by natural selection, nor do many social scientists believe that they have taken a theoretical position that contradicts Darwin (Brown 1999). Important premises of sociology have been that social environments are important for understanding why people act as they do and that the structure of social institutions is important for understanding why one aggregate social

[2] Various lines of thought could be cited as developing social conceptions of mind but are not suitably characterized by the *ad hominem* "cognitive creationism"; among these are the work that follows from Mead (1934).

outcome occurs instead of another. For such inquiries, the value of considering the conditions of the present is apparent, while the value of reflection upon the details of our evolutionary past may not be. In other words, the debate is changed from a question of whether the evolution of humans by natural selection happened (answer: it certainly did) to a question of the relative importance of our evolutionary past for understanding social phenomena. One could believe that a focus on aspects of social environments (e.g., institutional forms, normative and cultural systems) as proximate causes is sufficient to provide adequate explanations of social phenomena of interest and that little or no recourse to ultimate causes rooted in our evolutionary past is needed. Proponents of an emphasis on culture, institutions, and norms may point to the immense variation that exists among societies and the rapidity of contemporary social change (orders of magnitude faster than the plodding pace of biological evolution) as evidence suggesting that knowledge of our Darwinian past is of only limited utility for understand human society's present.

Whether implicitly or explicitly, this position has guided much social scientific inquiry and theorizing over the past sixty or more years. How far has it gotten us? Numerous critics have charged that social science is a failed project, a congeries of studies that lack both theoretical coherence and cumulation.[3] Sociology, in particular, is often singled out for the vacuousness or sterility of its theory; simply put, a number of critical observers have claimed that sociology is going nowhere. Sociologists Joseph Lopreato and Timothy Crippen (1999: 20) write:

> "With little exception, what we call sociology today is, on the one hand, an awful extravagance of ideological debates--a forest of words--that go by the name of theory and, on the other, a miscellany of findings uninformed by theory and exuding such quantity of trivia that even sociologists find the whole mortifying. We are in a deep crisis."

[3] See Tooby and Cosmides (1992) for this critique from the standpoint of evolutionary psychology.

Signs of a discipline in trouble are oft-repeated: the number of sociology majors declined over 60% between 1973 and 1991; several colleges and universities have closed their departments; funding for sociological research is more scarce; the public credibility of sociology seems on the wane; and various insiders have been quoted as saying sociology is in a "dismal abyss" or in "a tailspin and no one seems to know what to do" (Kantrowitz 1992; Glenn 1995). Although the past few years have seen some improvement in terms of resources and number of majors, many still feel that the long-term health of sociology is not secure, and the sense of sterility remains. Andrew Abbott (1999: 196) writes that "It has been a long time since we sociologists saw an idea that got us really excited, an idea that could transform our intellectual practice, an idea that could make us want to read the journals."

The proposition that sociology is an ailing discipline has inspired numerous attempts to diagnose the problem and propose its cure. Here, some have blamed the supposed theoretical stagnation in sociology on its failure to adopt an evolutionary stance toward its subject matter, and they contend that a heavy dose of Darwinian reason is the tonic necessary to rejuvenate the field. Sociologist Lee Ellis (1996) argues bluntly that the discipline is doomed if it cannot resolve its "biophobia." After delivering the salvo that I quoted above, Pierre van den Berghe goes on to paint a similarly bleak picture of the discipline's future should it fail to adopt an evolutionary foundation:

> Sociology's claim to scientific status can no longer be upheld primarily through a façade of quantitative methodology. It must be anchored in a theoretical paradigm that satisfies scientific canons. Such a theoretical paradigm sociological signally failed to produce in over a century of self-conscious existence. The main reason for that failure is that sociology turned its back on the on theory that was overwhelmingly successful in explaining

> change and variation in life forms on this planet: evolution by natural selection. If sociology fails to join the scientific mainstream, it might survive for another century as a scholastic discipline, but it will never be taken seriously as a science. (van den Berghe 1990: 183)

Lopreato and Crippen (1999: xiii) contend that evolutionary theory "more than anywhere else, is where the action is today in behavioral science. Sociology will participate in this revolution or it will be cancelled out of the intellectual landscape" (see Ellis 1975 for a similar view expressed two decades earlier).

Such pronouncements have come in tandem with a rise of evolutionary theories of social phenomena in both public and other intellectual arenas. In the past few years, evolutionary explanations of human social behavior have gained adherents within the social sciences, and they have also commanded a large popular following that most sociologists can only envy.[4] Much of the excitement centers on a new program known as "evolutionary psychology." The name is apt because of the program's cognitive emphasis, but it is misleading insofar as evolutionary psychology has drawn scholars from a number of fields and has addressed phenomena beyond the usual purview of psychology. While evolutionary psychology is perhaps still best described as a marginal program in the social sciences, it is one with a growing reputation for having many new and exciting ideas, which is exactly what some critics believe contemporary sociology desperately needs.

What should sociologists do about Darwin? This is a question with which I have preoccupied myself during the writing of this dissertation. I began with a conviction that sociology has done a poor job of articulating its relationship to human biology and that a failure to engage the new Darwinian thinking that is generating considerable excitement

[4] I wish to avoid repeated use of the phrase "human social behavior" in this dissertation. Instead, the phrase "social behavior" should be understood as referring to human behavior unless I specify otherwise. I do this for convenience and ease of presentation; it should not be taken as somehow denying the sociality of many other species.

elsewhere can only make sociology appear close-minded and increasingly outmoded. My larger ambition has been to come to some better understanding of the role that human genetic evolution should play in how sociologists think about social behavior and social organization. The dissertation recounts some beginning efforts toward realizing this ambition through an effort to engage sociobiological and evolutionary psychological claims that have been advanced in areas of interest to sociologists.

I provide a series of five case studies that give close attention to the logic and evidence behind a particular evolutionary explanation that has been suggested for a particular social phenomenon. These case studies arise from my belief that, while there are important insights to be gained from a broad examination of Darwinian programs, a useful critique can proceed at this level only so far. To really engage this literature and draw conclusions about what it has to offer sociology, we must get our hands dirty with actual studies and real data, examining what is being done on the level of specific theories and specific claims. Such an approach is consistent with an orienting belief that there is not any simple answer to the question of what Darwinian programs have to offer to sociology, but instead evaluations must ultimately proceed theory-by-theory and study-by-study. Of course, I cannot consider every Darwinian hypothesis about human behavior that has been offered. Yet, by paying close scrutiny to a few such hypotheses in turn, I hope to illustrate some of the important points that must be considered when evaluating other evolutionary explanations. (In this spirit, in a couple of the case studies that follow, an initial empirical inquiry regarding one theory has been expanded to include a more brief consideration of other work. These are efforts to explore some larger point about the incorporation of Darwinian thinking into sociology.)

As the result of my investigations, these case studies adopt a critical stance toward the proposed contributions that I examine. As such, they illustrate some of the failings of current evolutionary approaches and provide the grounds for my present opinion that, while sociology needs to do more to engage these approaches, incorporating Darwinian ideas into sociology is an endeavor fraught with potential problems. By elaborating my criticisms of Darwinian approaches and drawing lessons from them, I hope ultimately to begin to develop a better answer to the question of what sociologists should do about Darwin than what is provided by any of the current alternatives. Unfortunately, in this dissertation, I can provide only a modest beginning in this regard.

This first chapter will provide an overview of project and take care of a number of preliminaries. First, I will more clearly define what "Darwinian approaches" and "evolutionary explanations" are, and explain how evolutionary explanations are distinct from the explanations of behavior genetics and from social theories that use Darwinian principles metaphorically. I provide a brief history of Darwinian approaches to social behavior, and I highlight some of the political considerations that have long been associated with them. I do this to give the reader some background on how the theoretical debate in this area has come to be so politicized. I also describe some of the core propositions of evolutionary psychology, and I argue that the program has made strides to address many of the criticisms leveled at sociobiology and other predecessors that have given evolutionary explanations such a negative reputation among social scientists. Afterward, I lay out principles of my own critique of evolutionary psychology that summarize my determination to keep my dissertation from being (or seeming to be) another politically motivated or otherwise "unfair" entry into the fray. Finally, I conclude the chapter by briefly describing the works that will comprise the focus of the "case studies" in this dissertation.

WHAT IS AN EVOLUTIONARY EXPLANATION?

Darwinian approaches to social behavior vary considerably in their details, but the unifying idea behind them is that considerable insight can be gained from studying how the process of evolution by natural selection has shaped our brains, our behaviors, our ways of interacting with one another, and our societies. Proponents of such approaches believe that contemporary social phenomena can be better understood through reflection upon processes of natural selection and the conditions of our ancestral past. They propose that many observed patterns of social behavior or organization exists because the human propensities underlying them served some fitness-enhancing function sometime in our evolutionary history.

Let me give some quick examples of evolutionary explanations. Several of these will be revisited in more detail in my dissertation.

- Leda Cosmides and John Tooby (1989, 1992) propose that humans have developed a cognitive adaptation that facilitates the recognition of cheating on social contracts. The ability to detect cheating is proposed to have evolved to protect our ancestors from being exploited in social exchange situations. This adaptation is claimed to affect performance on logic problems, such that people do much better when a problem is framed as a potential instance of cheating than they do when it is not.

- Daly and Wilson (1998: 443-447; see also Wilson 1987) find that children are several times more likely to be abused by step-parents than by biological parents. To explain this pattern, they theorize that natural selection may have favored the development of mechanisms that inhibit angry responses to misbehavior by one's own children but not necessarily to behavior by children who are not kin.

- Frank Sulloway (1996) presents a remarkable array of evidence suggesting that firstborns and laterborns differ consistently in their personalities and other characteristics. He contends that these differences are the enduring consequence of an evolved tendency of children to tailor their ways of acting to maximize the resources they elicit from their parents. Siblings must compete with one another for parental attention, and the behavioral strategy that gets the most investment for a firstborn child differs from the optimal strategy for a laterborn. These behavioral differences between firstborns and laterborns are theorized by Sulloway to arise initially through this competition and persist into adulthood.

- Irwin Silverman and Marion Eals (1992; see also Silverman and Phillips 1998) hypothesize that innate sex differences in spatial skills are the evolutionary result of the division of labor in early human society. They argue that men tend to test better than women on those tasks, such as learning mazes and reading maps, that correspond with skills beneficial for hunting. Meanwhile, women tend to do better than men at remembering the location of objects and being able to recall the contents of a setting after only a brief exposure, which they contend corresponds with skills that would have aided foraging.

- Jay Belsky, Laurence Steinberg, and Patricia Draper (1991; see also Draper and Harpending 1982, 1987; Draper and Belsky 1990) conjecture that, in our evolutionary past, childhood experience may have been correlated with the relative success of different strategies of sexual development and behavior. Accordingly, they propose that the human mind might have evolved mechanisms that determine what strategy one will follow on the basis of the stability of resources and care that one receives during childhood. Instability triggers a developmental path that is marked by precocious sexuality, a higher likelihood of teen pregnancy, and less enduring mating relationships throughout the life course. Meanwhile, a stable family upbringing is said to promote the activation of innate mechanisms that encourage a postponing of first intercourse and the development of more secure sexual relationships later in life.

- Kingsley Moore (2000), Robert Wright (1994) and Geoffrey Miller (1998) argue the men possess a stronger drive for status and achievement than women do as the result of having confronted keener competition for mates throughout evolutionary history, and they conjecture that this sex difference in motivation is a central cause of the drastic overrepresentation of men in the highest positions of government and culture (see also Goldberg 1993).

What evolutionary explanations have in common is the premise that social behavior significantly reflects features of human nature whose understanding requires reflection upon the specific evolutionary history of human beings. Evolutionary explanations are distinct from the explanations of behavior genetics, which account for variation in human traits as the result of individual- or group-level genetic variation. The difference between the two is important and often overlooked, as evolutionary and behavior genetics explanations are commonly lumped together as "biological explanations of behavior." Darwinian social scientists have claimed that the reason humans tend to be more generous toward relatives than strangers is that natural selection has systematically favored individuals whose genes cause them to be disposed to this behavior (e.g., Wilson 1975; Alexander 1987; Reeve 1998). This is an evolutionary explanation. In contrast, a behavior geneticist may look at differences in the magnitude of human generosity—holding constant the situational context—and suggest that some of the individual variation in generosity is the result of genetic factors. In principle, at least, one might be able to isolate particular constellations of genes associated with generosity, and then through DNA testing be able to make better-than-chance predictions about the relative generosity of individuals.[5] Both types of explanations assume that human behavior is to an important extent "in the genes," as the way that natural selection preserves changes in behavioral dispositions or tendencies is through changes in genetic encodings. Even so, we will see later that evolutionary and behavior genetics explanations are often at odds with each other. In a large part, this is because evolutionary explanations tend to focus on how the genes all humans share (as the result of natural selection) make us cognitively similar, while behavior genetics emphasizes how the genes that differ among humans cause us to behave differently.

Evolutionary explanations must also be understood as distinct from explanations that draw on Darwinian principles as *an analogy or metaphor*. The language of fitness, selection and adaptation is sometimes used to talk about why some states or businesses survive and others fail (see Dietz, Burns, and Buttel 1990: 163-66; Aldrich 1999) or why some scientific claims thrive while others flounder (Campbell 1974; Hull 1988; Markovsky 1997), but these are not the same as explanations that posit genetic consequences of natural selection processes over human evolutionary history. Metaphorical applications require no reference to the conditions of our ancestral past, and they make no assertions about the effect of selection processes on the human genome. My own opinion is that both behavior genetics and metaphorical applications of Darwinian principles have been underutilized in sociology and other social sciences, but, even so, they both stand outside the scope of what I seek to evaluate in my dissertation.[6]

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[5] The distinction that I draw between evolutionary explanations and behavior genetics explanations is intended to familiarize the reader with the difference between behavior genetics and Darwinian social science and to make the point that my dissertation focuses on the latter. It is not perfect; for one thing, both behavior geneticists and evolutionary psychologists have tried to formulate evolutionary explanations for instances of genetic variation that exist (see, e.g., Tooby and Cosmides 1990a; Scarr 1995; Hamer and Copeland 1998:81-82).

[6] I do briefly discuss efforts to apply principles of genetic evolution to cultural evolution in Chapter 8.

Instead, I focus on evolutionary explanations and their potential value to sociology. Many previous assessments of the merits and flaws of Darwinian approaches to social behavior have been strongly conditioned by what were perceived to be the political implications of these approaches. I believe that this has greatly harmed the possibility of constructive dialogue between advocates and skeptics of Darwinian approaches, and it may not be an exaggeration to say that things have reached the point where each side considers the other to be either too ideologically prejudiced or too committed to its own position to evaluate the existing theories or research fairly. My contention is that sociology needs to move beyond this if it is to make a genuine assessment of what role Darwinian theory should play in our understanding of the social world. This said, contemporary attempts to provide evolutionary explanations of social behaviors or institutions stand inevitably in the shadow of flawed attempts to apply Darwinian theory to humans in the past. This history is essential for understanding the current character of the debate about evolved biology and social life.

SOCIAL DARWINISM TO SOCIOBIOLOGY TO EVOLUTIONARY PSYCHOLOGY

"If we're all Darwinians, what's the fuss about?"
—title of a paper by Donald Symons, 1987

Social Darwinism. In the years immediately after the publication of *The Origin of Species*, the idea of evolution by natural selection was put to many uses that likely seem unsavory to most contemporary sensibilities. To give just a couple examples, some American scholars used evolutionary theory to justify the seizure of Indian lands or the existence of slavery—the latter on the grounds that slavery protected blacks because they would otherwise be driven to extinction if pitted in free competition against whites (see Faris 1950). For sociologists, no one figures more prominently in the negative political use of evolutionary theory than Herbert Spencer, whose name (along with that of his American disciple, William Graham Sumner) has become practically synonymous with the darker connotations of the phrase "social Darwinism."[7] Spencer used evolutionary theory to support his long-held belief that the state should not intervene in natural processes beyond providing protection for persons and their property. Spencer reserved particular wrath for the Poor Laws, which mandated that British parishes levy taxes to feed their poor. Spencer believed that people were destitute mainly because of character deficiencies that they had inherited from their parents, and that by providing assistance to the poor the government was aiding a proliferation of bad breeding stock that would only ultimately impede society's progress. In these sentiments, Spencer was largely echoed by Darwin himself, who wrote:

> We build asylums for the imbecile, the maimed, and the sick; we institute poor-laws; and our medical men exert their utmost skill to save the life of every one to the last moment. Thus the weak members of civilized societies propagate their kind. No one who has attended to the breeding of domestic animals will doubt that this must be highly injurious to the race of man (see Degler 1991: 12).

Spencer understood evolutionary theory to imply that the social organism possessed its own mechanisms of self-correction and incremental improvement, and that the abject state of the

[7] The association of Herbert Spencer and the phrase "social Darwinism" (a moniker for his work that Spencer would undoubtedly despise, for the reasons below) has led to it being commonly thought that Spencer was *inspired* by Darwin or that Spencerian theory was formulated primarily *as an application* of Darwin's ideas. This is misleading on several counts. First, Spencer drew more heavily on the (erroneous) evolutionary theory of Jean Baptiste de Lamarck than upon anything Darwin wrote (Richards 1987). Second, as Richards (1987) has persuasively argued, Spencer's moral theory preceded and structured his interpretation of evolutionary theory, not the other way around. Finally, Richards notes also that several passages in Spencer's pre-1859 work *anticipate* Darwin's ideas about how selective processes maintain the positive features of a species. Consider the following passage from *Social Statics*, published eight years prior to *The Origin of Species*: "[C]arnivorous enemies not only remove from herbivorous herds individuals past their prime, but also weed out the sickly, the malformed, and the least fleet or powerful. By the aid of which purifying processes, as well as by the fighting, so universal in the pairing season, all vitiation of the race through the multiplication of its inferior samples is prevented; and the maintenance of a constitution completely adapted to surrounding conditions, and therefore most productive of happiness, is insured" (1851: 322). Or, in delineating his political philosophy, this passage from an 1852 essay: "[B]y the ceaseless exercise of the faculties needed to contend with [the complexities of society], and by the death of all men who fail to contend with them successfully, there is ensured a constant progress towards a higher degree of skill, intelligence, and self-regulation—a better co-ordination of actions—a more complete life" (p. 500).

poor was the natural means of weeding out their defective states. To this end, he argued that anyone who supported government assistance to the poor was "blind to the fact that under the natural order of things, society is constantly excreting its unhealthy, slow, vacillating, faithless members" (Spencer 1851: 323-4; for an attempt to recover other aspects of Spencer's thought for our contemporary understanding of social change, see Haines 1992, 1997).

Spencer was far from alone in believing that society could be counted upon to excrete its undesirable elements if only the government would leave it alone. Others took his reasoning a step further, however, and argued that if the state wanted to be truly benevolent, it could act as a laxative to help the evolutionary process along. Proponents of the eugenics movement started (and named) by Darwin's cousin Francis Galton argued that the government could improve the human species by intervening in reproduction. Galton himself had ideas about breeding better people, but in practice the eugenics movement was more concerned with the *defensive* task of preventing those with socially undesirable traits from reproducing. The most common targets were those with family histories of mental illness, mental retardation, or criminal behavior; the prevailing sentiment was perhaps best captured in the Supreme Court's 1927 decision to uphold Virginia's sterilization law, in which Oliver Wendell Holmes Jr.'s wrote for the majority that "three generations of imbeciles are enough" (see Gould 1981: 365). While it is easy to look back now and view the eugenics movement as an ugly episode in social conservatism, it actually had a number of progressive sympathizers, including socialists Beatrice Webb, Margaret Sanger, and Rosa Luxemburg. Prominent progressive sociologist Edward A. Ross—an early chair of Indiana University's Department of Economics and Social Science—testified before the Wisconsin legislature that any objections to a proposed sterilization law "are essentially sentimental, and will not bear inspection" (Degler 1991: 46).[8]

Perhaps predictably, the eugenics movement in the United States was very concerned about the relationship between whites and blacks, just as the movement in Europe was very concerned with the relationship between Europeans and members of "lower" or "savage" societies. Degler (1991:15) writes that "the most prevalent form of social Darwinism at the turn of the [20th] century was actually racism, that is, the idea that one people might be superior to one another because of differences in their biological natures." Officials of the American Eugenics Society openly advocated that use of sterilization laws as a way to "prevent the American people from being replaced by alien or Negro stock" (see Gordon 1976: 283). Meanwhile, in his 1906 State of the Union address, Theodore Roosevelt admonished well-born white women who had put off childbearing for committing "the one sin for which the penalty is national death, race suicide" (cited in Mass 1977: 20; see also Davis 1981). Numerous biologists and social scientists contributed the authority of their reputations and research to asserting the superiority of European racial stock (see Gould 1977a, 1977b for a discussion). More than anything else, the blatant racism of the eugenics movement was what turned its progressive following against it, and it helped form the strong and enduring distaste that has existed in liberal academic circles toward any mixing of biological theory and social policy.

[8] Ross was chair from 1891–1892. Politically, he was far from conservative: Degler (1991: 18) notes that Ross was fired early from Stanford early his career for supporting Populist monetary principles, and he continued to advocate for liberal causes throughout his career. As further trivia about Indiana and eugenics, the state has the distinction of being the first to pass a law permitting involuntary sterilization. It allowed the sterilization of "confirmed criminals, idiots, imbeciles, and rapists" if judged advisable by a committee of experts (see Degler 1991: 45).

The Ascendance of Culture. The intellectual arguments that led to the decoupling of the social sciences from biology were constructed in no small part as a reaction against racism. They came first from anthropology and Franz Boas, who labored long against the idea that the primitiveness of so-called primitive societies was due to its members being savages, mental defectives, or in some other way incompletely evolved. Instead, Boas developed the idea that the divide between more and less advanced societies was not a matter of biology but a matter of history and "culture," the catch-all term he developed to encompass a society's shared knowledge, traditions, and institutional forms (see Degler 1991). Boas and his followers argued that cultures could vary indefinitely while innate human faculties were everywhere more or less the same. Because biology was constant and observed behavior so variable, these early anthropologists concluded that further reflections on biology has little utility for understanding social life, but, instead, attention was better spent exploring the causal properties of culture and recording the details of the world's different cultural systems. Boas disciple Alfred Kroeber was even more adamant than was his mentor on this point, writing that "heredity cannot be allowed to have acted any part in history" (1915: 285).

According to Degler (1991), Boas and Kroeber's line of thinking had an important influence on many sociologists, especially in the United States (Degler 1991). Many of the early American sociologists were keenly interested in social reform, and so, not surprisingly, they were inclined against the social Darwinist position that the harsh conditions faced by the poor were natural, inevitable, or good for the rest of society. Conversely, the concept of "culture" was agreeable with the reformist ethos, as it suggested that the plight of the poor could be ameliorated and their capacity for achievement improved by positive changes to their social environments. As a result, when Talcott Parsons introduced Emile Durkheim to many sociologists with the publication of *The Structure of Social Action* in 1937, his audience was already favorably disposed to Durkheim's (1895: 110, 103) proclamations that "the determining cause of a social fact should be sought among the social facts preceding it" and that "every time that a social phenomenon is directly explained by a psychological phenomenon, we may be sure that the explanation is false."[9]

A thoroughly environmentalist model of the actor quickly came to be dominant within sociology. Social theory endured the occasional criticism for ignoring the role of innate drives and human nature, perhaps most famously in Dennis Wrong's [1961] essay on "The Oversocialized Conception of Man in Modern Sociology."[10] Yet, even as Parsonsian structural-functionalism came to be displaced—in part because of its failure to adequately account for the "agency" of social actors—sociologists still rarely looked to evolutionary biology in its attempts to understand actors' motives. Holton (1978; see Degler 1991: 318; Segerstrale 2000: 374) writes that "sociobiology violates Durkheim's injunction—which is bedrock in the training of social scientists in this country—that social phenomena can only be explained in terms of social variables." Similarly scarce attention to the biology of human action was provided by the sister disciplines of economics, political science, or history. It is

[9] Parsons certainly did provide a role for biological factors within his conceptual scheme and made liberal use of evolutionary metaphors in his theorizing (Parsons 1937; see also Parsons 1964).

[10] Wrong offers an interpretation of Freud as a means for sociologists to better conceive the conflicts that exist within the self. He writes that "when Freud defined psychoanalysis as the study of the 'vicissitudes of the instincts,' he was confirming, not denying, the 'plasticity' of human nature insisted on by social scientists. The drives or 'instincts' of psychoanalysis, far from being fixed dispositions to behave in a particular way, are utterly subject to social channelling and transformation and could not even reveal themselves in behavior wihtout social molding any more than our vocal cords can articulate speech if we have not learned a language" (p. 192). Wrong concludes his essay by writing, "...I do not see how, at the level of theory, sociologists can fail to make assumptions about human nature. If our assumptions are left implicit, we will inevitably presuppose a view of man that is tailor-made to our special needs... we will end up imagining that man is the disembodied, conscience-driven, status-seeking phantom of current theory. We must do better if we really wish to win credit outside our ranks for special understanding of man, that plausible creature whose wagging tongue so often hides the despair and darkness in his heart" (p. 193)

probably not going too far to say that during these years the main appearances of evolutionary theory on the stage of social thought was in the role of a cautionary fable about the perils of social Darwinism.

Sociobiology. Scattered efforts were made in the 1960's and 1970's to apply evolutionary theory to human behavior and institutions. This included attempts by Nobel laureate Konrad Lorenz (1966), novelist and journalist Robert Ardrey (1966), and social scientists Lionel Tiger and Robin Fox (1971), who all sought to account for patriotism, interpersonal aggression, imperialism, and war in terms of evolved instincts for territoriality and dominance-seeking. Trivers's pioneering work on parental behavior (1972, 1974; see Chapter 7) and reciprocal altruism (1971) also made various conjectures as to how these dynamics might apply to humans. But perhaps the real flashpoint for the revival of Darwinian theories of human social phenomena can be traced to the 1975 publication of Edward O. Wilson's *Sociobiology: The New Synthesis* (a description of various interpretations of the history of sociobiology and the relative importance of Wilson's book in it is provided by Segerstrale 2000, especially pp. 316-320). A widely respected biologist, Wilson presented sociobiology as an ongoing project to unify both the work of different disciplines (zoology, population genetics, ethology, behavioral physiology) and studies of a diverse array of different social species. Wilson proposed that the social behavior and organization of different species were a function of various dimensions of their population demographics and evolutionary ecology. Once the function was understood, differences along one or more of these dimensions could explain the social differences that existed both within and between species. Ultimately, as Wilson envisioned it, sociobiology aspired to a framework in which "the same parameters and quantitative theory are used to analyze both termite colonies and troops of rhesus macaques" (p. 4) .

Going even one step further, Wilson proposed that if such a framework was developed, it also could be used to explain many aspects of human social organization. He argued that the social sciences could be placed on a more secure foundation by studying human beings with the same approach that had worked for studying the social behavior of other animals. By treating humans as simply as special case of the same fundamental dynamics that had already been applied to other species, Wilson claimed that sociobiology held the promise of bringing many estranged branches of scholarship underneath the umbrella of the natural sciences:

> It may not be too much to say that sociology and the other social sciences, as well as the humanities, are the last branches of biology waiting to be included in the Modern Synthesis [of evolutionary theory]. One of the functions of sociobiology, then, is to reformulate the foundations of the social sciences in a way that draws these subjects into the Modern Synthesis. Whether the social sciences can be truly biologicized in this fashion remains to be seen (1975: 4).

Additionally, Wilson contended that sociobiology could add a new and needed rigor to efforts to explain the evolutionary roots of human behavior. He criticized the efforts of predecessors such as Ardrey (1966) and Tiger and Fox (1971) for being unscientific "works of advocacy" that depended far more on their author's persuasive skills than on any empirical tests (Wilson 1975: 28-9). He argued that the methods already established for studying other species would allow competing evolutionary explanations for human phenomena to be resolved by crucial and decisive experiments or field studies.

While Wilson's sociobiological program found some enthusiasts within the social sciences (among sociologists, perhaps most notably Joseph Lopreato and Pierre van den

Berghe), the reaction of most sociologists was highly critical. Dozens of critiques of *Sociobiology* and the work influenced by it have been published (see, e.g., Sahlins 1976; Caplan 1978; Bock 1980; Montagu 1980; Gove and Carpenter 1982; Lewontin, Rose, and Kamin 1984; Kitcher 1985; Gove 1987; L. Freese 1994). Of the many objections that have been raised, three stand out to me as most recurrent and important. First, sociobiology has been accused of granting evolved biology a deterministic role in shaping social behavior and organization, while allegedly only paying lip service to the causal influence of sociological standbys like social structure, socialization, or culture. Related contentions are that sociobiology's supposed "biological determinism" has led it to underappreciate the importance of historical contingency and to take an unduly pessimistic view of the possibility of social change (Bock 1980). Second, some critics assert that sociobiology has made naïve and crude simplifications of human social phenomena in its efforts to draw comparisons to other species (for example, failing to attend to the meaning of social phenomena to actors). Two of the more notorious examples are Wilson's (1975, 1978a) juxtaposition of human slavery and ant slavery and Thornhill and Thornhill's (1983) comparison of human rape and forced copulation in birds or insects (see also Chapter 4, on Thornhill and Palmer 2000). Third, sociobiologists have been accused of allowing claims about human behavior and society to be much more freewheeling and speculative—and far more removed from hard empirical evidence—than what biologists would ever permit for studies of other organisms. Philip Kitcher (1985: 35) charges that many sociobiologists write as if "to write about human beings gives one a license not extended to the study of corn"; he claims that sociobiology "descends to wild speculation" in precisely those areas where the public implications at stake would seem to encourage the most scientific care.[11]

[11] On this same point, Lee Freese (no relation to the author; 1994: 367) writes, "When humans are at issue and

While each of these three criticisms have merits, it would be naïve to think that the rejection of sociobiology by social scientists was always based purely on these or any other academic criticisms. Instead, sociobiology provoked a remarkably hostile reaction from many social scientists that was based on the political motives that the program was perceived to serve (see Segerstråle 2000). Wilson's program was accused of being not just politically conservative but racist, classist, and sexist in its conservatism. *Sociobiology: The New Synthesis* was called "a justification for a sexist and class society" (Alper, Beckwith, and Miller 1978: 480), "dangerously racist" (see Wade 1976: 1154); "a deeply conservative politics" (Sociobiology Study Group of Science for the People 1976: 185), and "yet another defense of the status quo" (Allen et al. 1975: 43). Numerous critics portrayed Wilson as dressing up the old Social Darwinism in the language of new science. These reactions were taken to a very personal level, including one episode in which Wilson had to suffer the indignity of being doused with water by protesters at a scientific convention (Segerstråle 2000). Some tried to have him expelled from the American Association for the Advancement of Science, and the American Anthropological Association voted on a resolution condemning sociobiology as a "an attempt to justify genetically the sexist, racist, and elitist status quo in human society" (*Time* Magazine 1976: 93) (The resolution was overwhelmingly defeated after opposing speeches from such luminaries as Margaret Mead, who compared it to the book-burning efforts of fundamentalist Christians: "We are supporting the people who attack everything we believe in! We are getting ourselves into an *insane* position!")

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the characterization of human nature is in view, standards drift lower. Causal attributions, conceptual definitions, hypotheses, measures, acceptable evidence, warranted inferences, and conclusions that would be unacceptably simplistic for the analysis of other species become acceptable for pronouncements about our own—much of it styled in a language guaranteed to provoke emotional reactions."

Wilson reports that he was "surprised—astonished" by the harsh response of many to *Sociobiology: the New Synthesis*. He writes:

> I expected that many social scientists, already convinced of the necessity of a biological foundation of their subject, would be tempted to pick up the tools and try them out. This has occurred to a limited extent, but there has also been stiff resistance. I now understand that I entirely underestimated the Durkheim-Boas tradition of autonomy of the social sciences, as well as the strength and power of the antigenetic bias that has prevailed as virtual dogma since the fall of Social Darwinism (Wilson 1978b: 10).

Today, Darwinian social scientists generally do not encounter the open rancor and public confrontation that Wilson had to face in the late 1970's. Yet many social scientists still regard evolutionary approaches as politically reactionary and are contemptuous of their advocates. Evolutionary psychologist Douglas Kenrick (1995: 59) laments that "[i]n the modern social sciences, …attempts to explain human behavior in evolutionary terms are often greeted as ideological treatises defending the status quo. Without so much as a fair trial, evolutionary theorists are condemned as fellow travelers with the skinheads and the KKK."

As I suggested earlier, the problem is exacerbated by the tendency to lump evolutionary and behavior genetics explanations together as "biological causes." Darwinian social science suffers in many minds from an association with the claims of *The Bell Curve* (Herrnstein and Murray 1994), even though *The Bell Curve* is clearly cut from a much different theoretical cloth and does not attempt to provide any evolutionary explanation for its reported findings.[12] The problem is also exacerbated by the fact that the years spent on the disciplinary margins have led some Darwinians to evince an openly disdainful stance toward the rest of social science (see, e.g., Thornhill and Palmer 2000). Kenrick's statement

[12] Although they do not attempt to provide an evolutionary explanation anywhere in the main text, in an appendix (#5), Herrnstein and Murray do favorably discuss J. Phillipe Rushton's claims about the evolution of race differences (Rushton 1991, 1995).

above was taken from an essay in which he refers to the social scientists who resist evolutionary approaches as a "confederacy of dunces."[13] He goes on to complain:

> Research studies based in an evolutionary framework are more often expected to attain levels of methodological rigor rarely even approached in other areas, are more often required to rule out 'alternative explanations' already found implausible, and are often accused of embodying nefarious political implications. In stating their oppositions to evolutionary psychology, critics are often arrogant, self-righteous, and even insulting. All this can be entertaining, because these arrogant critics are often completely ignorant of the relevant research and theory and, consequently, eloquently wrong. It is all less entertaining when the confederacy of ignorance has resulted in unjustified rejections of journal articles and even public harassment…. In everyday life, it is mainly just mildly annoying, as journal editors ask one to address the same unjustified "concerns" of ill-educated reviewers year after year, as textbook editors ask one to remove actual research findings whose evolutionary implications might "offend potential adopters," or as colleagues make derisive comments based on their credulous misunderstanding of a point originally misrepresented in a magazine column by Stephen Jay Gould. (Kenrick 1995: 57)

An unfortunate consequence of this attitude toward conventional social science is that even valid criticisms of evolutionary approaches by other social scientists can be brushed off as being either politically motivated or based in an ignorance of the details of evolutionary theory (see Chapter 4 for more discussion).

Evolutionary psychology. The enduring antipathy between proponents and opponents of Darwinian approaches to social behavior obscures the fact that these approaches really have changed considerably since the publication of *Sociobiology*. As a result, the hostility that some hold toward evolutionary explanations may well be based on a dramatically outdated understanding of the enterprise. As a label for one's own work, the term "sociobiology" is still used by some who take a Darwinian perspective toward social life, while it was never embraced by some others (see Segerstrale 2000). Today, a growing number of Darwinian

[13] Kenrick is making a reference here to Jonathan Swift's famous statement that "When a true genius appears in the world, you may know him by this sign, that the dunces are all in a confederacy against him." (see also the splendid novel by John Kennedy Toole [1980]).

social scientists eschew the label "sociobiology," and some even refer to certain (putatively outmoded) ways of reasoning about humans and evolution as instances of "the sociobiological fallacy" (Horgan 1995; Buss 1995). Moreover, because "sociobiology" has acquired considerable negative connotations over the past 25 years, there are benefits to being associated with a newly minted name, regardless of a scientist's stance on particular theoretical issues. There are now many Darwinian research programs that owe some debt to Wilson or other earlier work but also reflect efforts to distinguish new ideas from old ones (see Crawford 1998a; Smith 1999 for two recent, comparative reviews).

The most prominent of these programs is "evolutionary psychology." As I suggested earlier, the name is a bit misleading insofar as it has attracted scholars who are not psychologists and has addressed substantive questions beyond the usual purview of psychology. Evolutionary psychology is very much a work-in-progress, and even those who are prominent within the field are not always in agreement about what evolutionary psychology is, how it is different from other contemporary programs of Darwinian social science, and how it is different from sociobiology (Horgan 1999; Segerstråle 2000). Some wish evolutionary psychology to apply *only* to a particular set of theoretical positions that were developed primarily by psychologist Leda Cosmides and anthropologist John Tooby, who coined the term. Others believe Cosmides and Tooby program should be seen as just *one way* of doing evolutionary psychology (see D. S. Wilson 1999). Things are complicated by evolutionary psychology being dubbed by some "a new science" (e.g., Wright 1994a, Buss 1999)—suggesting a clean break from the past—while others may be more apt to describe evolutionary psychology as the manifestation of a series of incremental theoretical improvements, innovations, and clarifications over the past two decades.[14] To the extent evolutionary psychology can be described in terms of a set of core propositions, these tenets diverge from common perceptions about sociobiology in a number of ways that may make evolutionary psychology more congenial to contemporary sociologists.

First, evolutionary psychologists are typically defter than is preceding sociobiological work in talking about the effects of the social environment on behavior (although many maintain that the characterization of sociobiology as a biologically deterministic perspective was never correct [see Segerstråle 2000]). Evolutionary psychologists conceptualize the evolved mind as comprised of a set of specialized cognitive mechanisms whose operation is very often sensitive to selected features of actor's social environments; these mechanisms are hypothesized to trigger different behaviors in response to different social circumstances (Tooby and Cosmides 1992; Buss 1995). Consider Frank Sulloway's theory of birth order effects that I described earlier in this chapter. Sulloway does not propose that there are any genetic differences between firstborn and laterborn children. Instead, he suggests that there are mechanisms for maximizing parental investment that are present in everyone's brains, but these mechanisms trigger the development of divergent personalities and attitudes depending on one's competitive position relative to one's siblings. Consider now Belsky et al.'s theory of sexual development and mating behavior that I also described earlier. Again, the authors do not claim that children with unstable family upbringings are in any way genetically different from those with stable upbringings, but instead they conjecture that the human

[14] Additionally, identifying individuals as evolutionary psychologists, human behavioral ecologists, or sociobiologists (or adherents of any other Darwinian program) is complicated by the fact that many Darwinian researchers do not explicitly embrace any of these labels. Some of the studies I examine in my critique of evolutionary psychology may be by scholars who would not explicitly identify themselves as such, but whose work has been incorporated into the major overviews of evolutionary psychology that have been published (Buss 1995, 1999; Tooby and Cosmides 1992; Pinker 1997; Wright 1994).

mind has evolved in such a way as to respond to cues about the stability of resources in childhood in order to determine which developmental strategy to follow. Both theories are consistent with evolutionary psychology's position that no adequate account of behavior can focus exclusively on "biology" or "the environment," but instead must include a description of both the underlying cognitive mechanism and how its operation is affected by changing environmental conditions.

Second, evolutionary psychologists propose that, with the exception of some important differences between men and women, the evolved cognitive mechanisms that underlie human behavior are more or less the same in everyone (Tooby and Cosmides 1990a). They suggest that the wide differences in behavior that we do observe are due in large part to the interaction between the mental machinery that all humans share and the social environments that we do not share. In other words, most evolutionary psychologists agree with the Boasian stance known as *the psychic unity of humankind*—that racial differences among humans are superficial and that the fundamental properties of human minds are universal (Tooby and Cosmides 1992). This is a decisively different position than that staked out by behavioral genetics, where researchers try to measure how much of the variation in an individual trait (most notoriously, intelligence) can be attributed to individual-level or group- (e.g., race-) level genetic variation. Consequently, as I have suggested, lumping together Darwinian social science and behavior genetics obscures profound and not always friendly differences: one behavior geneticist introduced himself to a reporter at an evolutionary psychology conference by saying "I'm the guy in the black hat here" (Horgan 1995: 180).

Third, the cognitivist foundation of evolutionary psychology leads it away from some of the explanatory devices that drew scorn to sociobiology. Evolutionary psychologists believe that many of the most important cognitive adaptations of the human brain took place after the evolutionary divergence of humans from the ancestral lines shared with other existing species (see Buss 1995, 1999; Pinker 1997). As a result, evolutionary psychologists grant humans a uniqueness in the animal kingdom that their predecessors were often unwilling to acknowledge, and evolutionary psychology places less effort than earlier programs on drawing comparisons between human behavior and that of other species. Evolutionary psychologists also assert that many of our cognitive mechanisms primarily evolved in response to specific problems in humans' ancestral environments (including the social aspects of these environments). Particular emphasis has been placed on the hypothesized environments of the Pleistocene, the era that ranged from 1.8 million to 11,000 years ago. Because the mechanisms developed in an environment vastly different from that of modern, developed societies, evolutionary psychologists argue that it is only to be expected that these mechanisms will not always operate to maximize reproductive success in contemporary environments, but rather could even produce behaviors that strongly reduce such success. This contrasts with a tendency within some sociobiology to assume that humans are evolutionarily hard-wired to always find and follow the best behavioral path to reproductive success; this assumption had led some sociobiologists to engage in all sorts of tortured reasoning to explain how patterns of behavior that would seem obviously to reduce one's reproductive fitness—contraception, suicide, infanticide, volunteering for war—really maximized it under the right conditions (Kitcher 1985; Buss 1995).

Today, proponents of evolutionary psychology contend that their program provides everything that contemporary social and psychological theory is often admonished for lacking: a coherent logical core, broad applicability of its principles to a wide range of problems, and formidable explanatory power. Evolutionary psychology also possesses strong ties to the natural sciences, and here its proponents say it holds the potential to bring sociology the validation as a *science* that many practitioners have long sought.[15] In addition, evolutionary psychology is said to provide these virtues while having discarded some aspects of sociobiology that had made it appear so politically reactionary. When we put all these things together, it might seem as if the time has never been better for sociology to seek a rapprochement with its Darwinian colleagues, as has been urged by a number of sociologists (Macy 1997; Nielsen 1994; Kanazawa 1999, forthcoming a; Lopreato and Crippen 1999).

In sum, evolutionary psychology offers a theoretical alternative that sociologists might see as both more powerful and more palatable than sociobiology, and, regardless of what sociologists ultimately decide to do with evolutionary psychology, it cannot be dismissed by recycling the same criticisms that were used against sociobiology. Instead, we need to make a fresh and full appraisal of this potential source of new insights to sociology. The ultimate question here is what role our evolved biology should play in sociological theory. The more proximate question is what potential contributions the specific work being done in Darwinian social science has to offer to our discipline.

[15] In making this argument, one typically assumes that some extrinsic criteria exists for demarcating "real science" from non-science, but sociologists of science have amply demonstrated that what counts as science is instead a constructed boundary that is regularly contested (Gieryn 1999).

PROSPECTIVES FOR A SOCIOLOGICAL CRITIQUE

I have already suggested that the assessment of the contributions of evolutionary psychology and other contemporary Darwinian approaches for sociology will be in many ways—although certainly not entirely—negative. Let me clarify this immediately by saying that I believe that evolutionary approaches have made many important strides since *Sociobiology*, and I also believe that the knee-jerk reaction that many social scientists still have against evolutionary psychology and related programs is unjustified and perhaps often based on a fairly gross misunderstanding of the tenets of these programs. This said, over the course of the case studies of this dissertation, I will develop what I think are some significant criticisms of problems in some Darwinian social science. These criticisms are wide-ranging and include concerns about the representation of social phenomena, the logic of evolutionary explanation, the fit between theory and evidence, and the demarcation of testable and untestable claims. Taken together, these flaws represent serious limitations to how useful evolutionary explanations can be in pushing social theory forward. I discuss why I am pessimistic about how far evolutionary psychology on its current trajectory can go in correcting these problems. I do not conclude from this that mainstream social science should continue with its tendency to simply ignore the influence of "human nature" on social behavior, but instead I suggest that different ideas about how to incorporate evolutionary ideas into explanations of social phenomena are needed.

I announce my critical intentions here with some trepidation and regret, however, as I know that it can be answered quickly with a "there they go again" retort from proponents of Darwinian approaches. As I noted earlier, the vitriolic reaction to Wilson's *Sociobiology* has left evolutionists convinced that many social scientists reject the Darwinian perspective first and worry about devising rationalizations for doing so only afterward (see Segerstråle 2000). Sociology is portrayed by some of its detractors as having done all it can for almost a century now to ignore or downplay the importance of evolved biology for social life: these arguments attempt to draw boundaries in which sociology is outside science, while its sociobiological alternatives are inside (Gieryn 1999). For example, in the *National Review*, Frank Salter (1996a:48) claims that "Rather than accept biology as a bridge to the natural sciences, sociologists spend a large part of their time rationalizing their failure to cross it." Lopreato and Crippen (1999: 59) contend that "sociologists have long been motivated by ideological agendas, and still interpret any opening to biology as a 'conservative,' and thus 'bad,' attempt to prove that all efforts toward desired changes are destined to fail."

As a result, I worry that the critical thrust of my conclusions may seem all too predictable given my disciplinary background and might assume that my efforts are motivated by a conscious or unconscious desire to defend sociology's politics or its conventional theoretical turf. My desire is to provide an examination of contemporary sociobiology and evolutionary psychology that—while it may ultimately be more critical than not—remains fair and scholarly. In this spirit, nowhere in this dissertation will I suggest that an evolutionary explanation should be rejected because of its apparent political implications. I will also not propose that an explanation should be rejected because of the stated or imputed political motives of the author. This may be what has harmed discussion of the relationship between evolved biology and social life more than anything else: in the place of a thoughtful examination of one another's ideas, some on each side of the debate instead emphasize how members of the other side are too politically biased or too blinded by their theoretical commitments to view the social world objectively. Portraying one's intellectual opponents as ideologically driven is a tack to be avoided for a couple of reasons. There is of course the fact that the motives inspiring a theoretical claim have no necessary implications for the claim's ultimate truth (what philosophers refer to as the "genetic fallacy" [Lewontin, Rose, and Kamin 1984]). Additionally, it is perhaps always a dangerous practice to assume that only those who disagree with you are influenced by political biases.[16]

Another way of saying this is to state that individual theories will be evaluated on the basis of their logical integrity and their consonance with the available empirical evidence (including, in some cases, my own empirical analyses). Adopting such a stance suggests the importance of *details* in developing this critique: criticisms must be clearly and explicitly presented, and every point of my empirical analyses must be clearly documented and replicable. My faith here is that if one is detailed enough in one's exposition, then the untoward influence of any prejudices or biases upon one's reasoning will surface with maximal clarity to readers and will avail themselves best to critique and correction. Additionally, I will try to clearly present my understanding of each of the theories that I will consider, with frequent reference to and quotation from the relevant primary texts, and I hope this will make it easy for any who wish to check up on whether I am reading a particular theory correctly. While I think it is always important to facilitate checks against misreadings, misrepresentations, or oversimplifications, I think it is especially so whenever one embarks on critical work.

In addition, I will not impose a double standard in which the critical stance I assume toward Darwinian approaches is paired with an overly credulous attitude toward the rest of social science. Proponents of evolutionary approaches have accused prior critics of doing just

[16] A different point is that in tackling evolutionary claims on their own ground, the case studies of the dissertation do not discuss the matter of whether the positivistic ethos of evolutionary psychology is a desirable ethos for how sociology should be conducted.

this, complaining that work that invokes an evolutionary explanation is subjected to much more logical and empirical scrutiny than work that invokes a cultural, structural, or other "non-biological" explanation. Kenrick (1995: 57) complains that "research studies based in an evolutionary framework are more often expected to attain levels of methodological rigor rarely even approached in other areas, are more often required to rule out 'alternative explanations' already found implausible, and are often accused of embodying nefarious political implications." John Tooby and Leda Cosmides (1992: 36-7) present this charge even more sharply:

> [T]he [non-Darwinian] social science community lays out implicit and sometimes explicit ground rules in its epistemological hierarchy: The tough-minded and moral stance is to be skeptical of panspecific 'nativist' claims; that is, of accounts that refer in any way to the participation of evolved psychological mechanisms together with environmental variables in producing outcomes, no matter how logically inescapable or empirically well-supported they may be. They are thought to be explanations of last resort and, because the tough-minded and skeptical can generate particularistic alternative accounts for any result at will, this last resort is rarely ever actually arrived at. For the same reason, it is deemed to be the moral stance to be correspondingly credulous of 'environmentalist' accounts, no matter how vague, absurd, incoherent, or empirically contradicted they may be.

I cannot be more emphatic that my aspiration for this project is not to provide social science with a more secure foundation on which to continue to dismiss or ignore the potential contributions of evolutionary theory for understanding social phenomena. Certainly, my dissertation will develop a critique of current attempts to apply evolutionary theory to social behavior. Yet, this will not be done to vindicate the current practices of social science.[17] Many of the methodological and other criticisms I will make about Darwinian social science

[17] Put another way, the dissertation may provide considerable scrutiny of some evolutionary claims, but I believe that this is only a double standard if coupled with a credulous attitude toward research that does not use evolutionary ideas or find support for evolutionary claims. If anything, I think that the weakness of many research designs in the non-evolutionary-minded social sciences is an important factor to understanding why evolutionary claims strike many as a compelling alternative.

could apply equally well to some (or even much) of the rest of social science as well, and when appropriate I will reiterate this point in my dissertation. At the same time, however, my belief is that if we are to entertain the possibility that evolutionary approaches offer an improvement over conventional social science, then it cannot be "let off the hook" for the problems that it and conventional social science share. Instead, the presence of common deficiencies only underscores the need to develop new alternatives.

OVERVIEW OF THE CASE STUDIES

As noted, the bulk of this dissertation consists of five "case studies" that gives close attention to a particular line of research in evolutionary social science. Each case study comprises a separate chapter. The chapters include original empirical research projects that I have conducted, but I also devote attention to the evolutionary logic of each theory and a consideration of some of the empirical evidence that has been raised in support of each theory. Also, for two of the chapters, my initial investigations lead me to consider some other evolutionary social scientific work in order to further develop a more general critical theme. The case studies focus on the following work:

1. Leda Cosmides and John Tooby's theory of cognitive adaptations for detecting cheating on social contracts. While a number of species perform limited interdependent behaviors that could be characterized as "social exchange," none approach the variety and complexity of human social exchange. The ability to engage in fair social exchange has obvious and wide-ranging evolutionary benefits for humans. Cosmides and Tooby (1989, 1992) argue that in order for humans to evolve this general capacity for exchange, a number of more specialized cognitive adaptations had to be in place, and they have given the most attention to a proposed cognitive mechanism that facilitates the detection of cheating. Cosmides (1985, 1989; see also Cosmides and Tooby 1992, 1995; Fiddick, Cosmides, and Tooby 2000) has conducted a series of experiments that are claimed to provide solid evidence of the existence of specialized adaptations for cheating. I examine the fit of their social contract theory to the evidence that they provide and some of the evidence gathered by others. I also report results from my own experimental survey that examines several different conditions of the same reasoning task studied by Cosmides and Tooby, and I propose some additional experiments using this task that may approach some of the potential tensions within their theory.

2. Randy Thornhill and Craig Palmer's (2000) presentation of an evolutionary psychological perspective on human rape. While Thornhill and Palmer do not take any decisive position on whether the male mind contains any adaptations that evolved specifically to facilitate the perpetration of rape, they do contend that an evolutionary psychological perspective can improve our understanding of why rape occurs by focusing attention upon the sexual motivation of rapists. Thornhill and Palmer regard this position as contrary to other social scientific (especially feminist) work on rape, and that suggest that greater attention to the sexual motivation of rapists is needed for the effective prevention of rape. Thornhill and Palmer also propose that specific adaptations do exist that modulate the psychological pain experienced by rape victims. My original goal was to re-examine the data that Thornhill and Palmer claim provides support for their contentions regarding psychological pain; however, I have not been able to obtain these data. My chapter does raise critical points about this evidence, however, as well as about several of their claims regarding the motivation of rapists.

3. Frank Sulloway's theory of the effects of birth order. Sulloway (1995, 1996) reports an array of evidence suggesting that firstborns and laterborns differ in their personality, achievement, social attitudes, and propensity toward radical behavior. He claims that these differences are the enduring effects of thousands of generations of sibling rivalry. More specifically, he proposes that the differences result from an interaction between (a) an evolved disposition of children to tailor their actions to maximize the resources they receive from their parents and (b) the systematically different environments children of different birth orders face as they compete with their siblings. The evidence Sulloway presents for his theory is based primarily on studies of eminent scientists and other historical elites, as well as a "meta-analysis" of the literature on birth order and personality. The evolutionary psychological explanation that Sulloway provides suggests a possible broad generality to his results. I wish to examine whether the systematic differences reported by Sulloway hold in contemporary, representative samples (see Freese, Powell, and Steelman 1999). I also evaluate the meta-analysis and some of the other evidence reported by Sulloway (1996).

4. Helen Fisher's theory of the "planned obsolescence" of human marriage and its effects on the likelihood of divorce. Helen Fisher (1992, 1995) proposes that a variety of factors in our ancestral past led humans to evolve a tendency to enter into monogamous (although not necessarily faithful) pair-bonds with a person of the opposite sex. However, she claims that the tendency is not for lifelong monogamy, but instead human pair-bonds evolved only to last long enough to conceive a single child and rear her or him through infancy. This is analogous to the pair-bonding of species such as foxes, who join with a mate only for a single breeding season and then separate. Because sources indicate that births during our ancestral past were spaced about four-years apart, Fisher proposes that evolution has left humans to pair off for roughly the same period. To support her theory, Fisher presents cross-cultural evidence that divorces in most societies tend to peak around the fourth year of marriage. I re-examine the cross-cultural data used by Fisher and find that it does not provide the support for her theory that she suggests. This failure spurred me to investigate two other lines of research that have been used to advance claims about the importance of specific numerical universals in social life: a "rule of 150" for the size of human groups (associated with Dunbar 1996) and a male preference for females with a waist-to-hip ratio of approximately .70 (associated with Singh 1993).

5. Robert Trivers and Dan Willard's hypothesis about sex bias in parental investment. Trivers and Willard (1973) use evolutionary reasoning to argue that daughters are more valuable to parents in poor "reproductive condition" (i.e., health), while sons are more valuable to parents in above average condition. As a result, they predict that species will have evolved a tendency to give birth disproportionately to daughters when they are in poor condition and to sons when they are in good condition. Moreover, they claim that their hypothesis also implies evolved differences in parental investment: parents in poor condition should treat their daughters better than they treat their sons, while parents in good condition should do the opposite. Trivers and Willard speculate that the theory can be applied to humans if we substitute "position on a socioeconomic scale" for "reproductive condition": parents who are wealthier than average should favor their sons, while those who are less wealthy than average should favor their daughters. While their hypothesis is over 25 years old, it is still often cited by Darwinian social scientists (Gaulin and Robbins 1991; Betzig and Weber 1995; Hrdy 1999; Kanazawa forthcoming a). I examine whether the Trivers-Willard hypothesis holds in contemporary families using the measures of parental investment that are commonly employed by sociologists (see Freese and Powell 1999; response by Kanazawa forthcoming b and rejoinder by Freese and Powell forthcoming). My investigation of the Trivers-Willard hypothesis has led me to a broader consideration of some of the difficulties in applying middle-level evolutionary theories like the Trivers-Willard hypothesis to family life in contemporary, developed societies. In this regard, this chapter also includes a discussion of some of the varied applications of Trivers's concept of parent-offspring conflict, as well as a discussion of an effort by Biblarz and Raftery to use Trivers's theory of parental investment to explain the effects of family structure on socioeconomic attainment in the United States.

Before I present these case studies, the next chapter will provide an introduction to the theory of evolution by natural selection and some of its technical details and a more extensive overview of contemporary Darwinian social science. Following the case studies, a concluding chapter will begin to develop some of my own ideas about how social science should handle the role of our evolved biology in understanding behavior and social organization.

As I wrote earlier, I am not seeking to provide a destructive critique of Darwinian approaches in defense of the social science status quo. Instead, while I believe that there are serious limitations to Darwinian social science as it is currently practiced, I believe even more strongly that the rest of social science needs to do a much better job of engaging the relationship between our evolutionary past and our social lives. What I wish to do in this conclusion is to draw upon some of the themes from my critical examination of current Darwinian approaches to start to flesh out a position on what should be done instead.

2

BACKGROUND ON EVOLUTIONARY THEORY AND EVOLUTIONARY PSYCHOLOGY

Charles Darwin's notebooks reveal that, very soon after he first hit upon the idea of evolution by natural selection, he was convinced the theory explained the origins of human beings just as it did the origins of every other species (e.g., Bowler 1990). Even so, he knew that his theory would meet the most resistance in its application to humans. Perhaps judiciously, then, Darwin does not apply his new theory of natural selection to human beings anywhere in *The Origins of Species* (1859), and only on the next-to-last page is he bold enough to suggest that "Much light will be thrown on the origin of man and his history" through his theory. Full-blown consideration of how the theory explained human origins was postponed until the publication of *The Descent of Man* twelve years later (1871). An analogous strategy has been used by many of those who offer evolutionary theories of human behavior: the relevant principles are established first by examples from other species, and then only afterward are humans brought into the picture. Wilson's *Sociobiology* spends over 500 pages explicating the social behaviors of slime molds, insect colonies, and apes before launching into "Man: From Sociobiology to Sociology" as its final chapter. The trope continues in recent presentations of theories. From the insect world alone, bees have been used to introduce a theory of expropriative crime (Cohen and Machalek 1988), scorpionflies to introduce a theory of rape (Thornhill and Thornhill 1983, 1987; see Chapter 4), and ants to introduce the theoretical underpinnings of kin altruism (e.g., Lopreato and Crippen 1999).

The strategy has its advantages and disadvantages. One disadvantage is that it invites the criticism that the authors are not adequately sensitive to all the complexities that are introduced when making the jump from beetles to humans. When one is presenting evolutionary theories to an audience of traditional social scientists, another disadvantage is that a lot of pages are spent talking about insects to an audience that is waiting impatiently for what one has to say about *people*. This dissertation is unabashedly anthropocentric;[1] there will be no pretense of interest in how well evolutionary explanations can account for the behavior of any species other than human beings. Accordingly, I will try here to keep the focus on humans as much as possible. This chapter begins by introducing neo-Darwinian theory to readers who are unfamiliar with its details, and it does so in a way that maintains a focus on how the principles of the theory have been used in developing evolutionary perspectives on human social behavior. We will move from this to a more specific consideration of some of the tenets of evolutionary psychology. The purpose of this chapter is purely descriptive. My intent is to provide the reader with an adequate background to the arguments that are presented in subsequent chapters.

In the spirit of anthropocentrism, we can begin our introduction to Darwinian theory with a nod to Thomas Malthus, the social theorist who can be said to have served as an important source of inspiration for Darwin's thinking. Darwin recalls in his *Autobiography* that he had begun reading Malthus in 1838 as an "amusement," but he quickly saw that *An Essay on the Principle of Population* provided the necessary grounding for his nascent ideas about natural selection. "Here, then," he wrote, "I had at last got a theory by which to work." While Malthus has the honor of helping Darwin along the way to

[1] "Anthropocentric" here is used to mean an exclusive *interest* in human beings. Elsewhere, the term has been used to connote a belief in the superiority of humans over other animals or a belief that humans are evolutionarily disjoined from their primate relatives. Neither of these alternatives is intended by my usage here.

perhaps the most important idea of the last two centuries, his contribution to contemporary social thought is otherwise limited (although he is remembered as a founder of demography and his central insight still usually receives a passing mention in a course on social theory). In Darwin's time, however, Malthus stood as a very formidable naysayer who contended that biological facts contradicted the unbounded optimism of his more liberal contemporaries.

When Malthus wrote his *Essay*, most believed that a growing population was a sign of a prosperous society and, as a consequence, all the countries of Europe had "an almost fanatical desire to increase population" (Heckscher 1935: 158; see Petersen 1979). Utopian theorists like Condorcet ([1795] 1970) and Godwin ([1793] 1946) saw a bright future ahead in which human society had moved beyond war, inequality, language differences, famine, disease, and perhaps even death. They expected human population to increase as a result, but they maintained that the number of inhabitants the Earth could potentially hold was so large that overpopulation could never be a problem. In contrast, Malthus asserted that human populations had the biological potential to increase in size much more quickly than could their available food supply (Malthus was following Adam Smith ([1776] 1991) and others with this idea). Using the mathematics of geometric progressions, Malthus showed that if human beings reproduced to their full potential in every generation, the size of the world's population would quickly grow well beyond whatever even the most generous utopian might regard as viable. Malthus concluded that some recurrent check on population size was an inevitable part of the human condition: if humans did not impose conscious limits on reproduction to keep populations from growing geometrically, then war, catastrophe, or, ultimately, famine would do the job for them.

Malthus and Darwin both recognized that the strain on resources caused by expanding population size is a problem potentially confronting not just humans but all animal species. If anything makes humans a special case, it is that we have developed technologies that allow us to move beyond having other species' predations serve as a natural check on population growth. What Darwin also saw, however, was that the tension caused by species being able to produce more offspring than their environments could support creates a *de facto* competition among the members of a species. The winners of this competition are those who managed to reproduce successfully in the face of scarce resources, and victory here is not randomly determined. Instead, some of the traits that vary within a species are inevitably correlated in some way with the probability of successful reproduction. If the traits that improve the odds of reproductive success also tend to be inherited by offspring, then the relative frequency of the successful traits will increase from one generation to the next. When such tendencies are sustained over many generations, it can lead to the outcome that Darwin realized upon reading Malthus: "[I]t at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavorable ones to be destroyed. The result of this would be the formation of new species" (p. 120).

That observation concisely describes Darwin's theory, which is (deceptively) simple in its basics. All species are capable of producing more offspring than their environments can handle. As a result, not all members of a species can reproduce to their maximal potential, at least not for very long. This leaves a competition over who survives and who reproduces most successfully. The outcomes of this competition are not random, but instead are at least sometimes correlated with traits that are heritable from parents to offspring. These differences in reproductive success imply a change—most likely very slight—in the distribution of traits from one generation to the next. But over many generations, recurring patterns of selection and inheritance can radically alter the traits typical of a population. If two different populations of the same species are separated from each other for a long period of time, the evolutionary changes that occur in this period may be so great that the populations can no longer interbreed when they are reunited (or are no longer inclined to do so), and therefore they have become effectively separate species.[2] Something like this is presumably what happened 5-6 million years ago, when an ancestral species of ape split off into two distinct lines, one of which evolved into chimpanzees and the other of which evolved into us (Boyd and Silk 1997).

Over time and through differences in rates of reproduction, variation in traits within a species comes to be transformed into variation between species. The constraints on environmental resources and reproduction that made Malthus's portrayal of the social world seem so bleak turn out to be preconditions of the very process that brought humans (and every other complex species) into being. One can say the rest of evolution by natural selection is just details. These details, of course, turn out to be crucial for considering what the legacy of the selection process might be for human behavior, and different beliefs about these details have inspired some of the different positions that have been taken regarding the relationship between natural selection, human minds, and human societies. In this chapter, I will provide a more extensive overview of evolutionary psychology, which is now the most prominent Darwinian approach to human social behavior. Before this, however, I will

[2] Darwin himself was vague about the last link in the chain here: the process by which evolution not only changes the traits of an organism but does it so radically as to create new species. As one geneticist has said, "if there is one thing which *The Origin of Species* is not about, it is the origin of species" (Jones 1993: 20). Even today, speciation is only partially understood. In a curious turn of events, the puzzles of speciation serve prominently in the arguments of some of the more sophisticated of the contemporary Creationists; they accept that evolution may be able to explain small modifications in the traits of species but contend that only the "intelligent design" of a creator could be responsible for the bringing new species into being (Johnson 1991, 1997; see also Behe 1996).

recount some of the details of evolutionary theory that are most essential for understanding evolutionary psychology and other Darwinian approaches to social behavior. We turn first to the matter of what genes are and what they have to do with evolution.

GENES AND EVOLUTION

When Darwin first proposed the idea of evolution by natural selection, everyone knew that some traits tended to run in families, but no one had any idea of the biological mechanism through which this inheritance took place. In fact, it was heavily debated whether offspring only inherited the traits their parents were born with or whether traits parents had acquired during their lifetime were also passed on. The theory that acquired characteristics could be inherited is known as Lamarckism, after its originator Jean Baptiste de Lamarck. It was popular among some prominent early sociologists—including Lester Frank Ward (considered by many to be the father of American sociology)—because it was thought to imply that effort spent educating members of the lower classes would have the additional benefit of causing their subsequent children and grandchildren to be born with higher innate capacities (Degler 1991). Lamarckism was also applied toward more conservative ends, most notably as an integral plank of Herbert Spencer's theory that the natural course of social change was an evolutionary progression towards the ideal society. Indeed, when August Weismann began his attacks on Lamarckism—including his demonstration that one could cut the tails off an indefinite number of generations of rats without affecting one bit the probability that new offspring would be born without tails—Spencer debated vigorously for the Lamarckian side, insisting that "either there has been the inheritance of acquired characteristics or there has been no evolution" (Bowler 1983: 71).

Spencer could not have been more wrong—acquired characteristics are not inherited and yet there has been evolution. We know now that every human being is built from the "instructions" that are sequentially encoded in the famously primordial molecule known as DNA (short for DeoxyriboNucleic Acid). An individual DNA molecule can be thought of as a coded set of instructions that is 3 billion characters long and written in an alphabet that contains only four letters—the nucleotides adenine (A), guanine (G), cytosine (C), and thymine (T). The exact process by which the DNA instructions decode themselves into a human being (or into another organism) is extremely complicated and still only dimly understood. To put things in the absolute briefest of terms: the order of nucleotides in a DNA molecule directs the construction of amino acids, which form protein chains, which construct cells, which combine together in ways determined by their structure to form a fetus and eventually a viable human. The instructions encoded in DNA direct the construction of every cell of the body; in doing so, the same sequence that originated in a fertilized egg comes to be copied with almost perfect fidelity into every cell, which is why DNA identification tests can be based on any sample of an individual's cell matter.[3]

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[3] How do DNA "fingerprints" work? Tests are based on segments of the DNA sequence that are known to have high variability in the human population. While many people may share a particular combination of nucleotides at a single locus, fewer would share the same combination at two separate loci, and still fewer at three or more loci. If the population frequencies for each loci are known (or can be estimated), then calculating the overall probability that an individual would have any given particular combination of these genetic markers is a straightforward exercise in probability theory. Such calculations form the basis of conclusions of the sort that a particular sample of DNA matter has genetic markers characteristic of "one in 7.87 trillion Caucasians," as was the case for the sample of semen taken from Monica Lewinsky's infamous blue dress. Obviously, when such a rare genetic combination is shared by an individual and a DNA sample for which there is already reason to believe may have come from him/her, DNA tests provide (in principle at least) exceptionally decisive evidence. Yet, Kitcher (1996) argues that one problem with these probability estimates is that the intercorrelations among the markers at different loci are not well-established (if having a particular sequence at locus A increases the probability of having a particular sequence at locus B, then this covariance has to be taken into account to calculate the joint probability accurately). Consequently, Kitcher suggests that what DNA tests usually provide is a high-end estimate of the actual probability, which is exactly the opposite of what one would presumably want given the use of these estimates as prosecutorial evidence in criminal proceedings (for a sociology of science perspective on DNA fingerprinting, see 1998 special issue of *Social Studies of Science*, edited by Michael Lynch).

All of the genetic diversity that exists in the organic world (not just among human beings but among everything that has ever lived) is ultimately based upon the different ways in which these four nucleotides can order themselves on a chain of DNA, a fact that may seem remarkable until one considers all of the different computer programs that have been made from a vocabulary of 0's and 1's or all the ideas that have been expressed in books using a 26-letter alphabet. Unless one has an identical twin (or some other sort of clone), the particular DNA sequence responsible for constructing one's body is almost certainly unique. All of the *innate* differences that exist between two human beings are ultimately differences in the order of nucleotides in their DNA sequences.

Traits are inherited because an individual's DNA sequence is derived from the sexual recombination of her or his parents' DNA. In other words, inheritance happens because the set of instructions that built your body and brain was assembled by combining the instructions that built your parents. Your own DNA sequence can be divided into 46 paired chromosomes, 23 of which were copied from your mother's DNA and the corresponding 23 copied from your father's. Any children that you have will receive half of your own DNA sequence, some parts of which will be taken from the chromosomes originally copied from your mother, while the remaining parts will be taken from the corresponding chromosomes originally from your father. This implies that approximately one-fourth of your own DNA sequence was derived from each of your grandparents, and it also implies that parts of your own DNA will account for about a fourth of the DNA of any grandchildren you have. If we keep halving the proportions, this logic can be extended indefinitely either forward or backward in time.

What is a gene in all this? Some define a gene as a segment of DNA at a particular chromosomal location that encodes a specific protein chain (this is also called a *cistron*). Dawkins (1976; following Williams 1966), meanwhile, offers a looser conceptualization of "gene" that has been highly influential among sociobiologists and evolutionary psychologists.[4] He begins with the idea that while the span of existence of every organism and DNA molecule is short from the standpoint of evolutionary time, the potential span of existence for *the information* contained in a segment of DNA sequence is not. A short string of DNA instructions—say, the sequence AGCTAGCT at a particular location on Chromosome #9—can be passed intact from parent to child to grandchild and so on, although at each generational step there is a 50 percent chance that a child will receive the other parent's DNA at this location instead. But if one has multiple offspring and these offspring all have multiple offspring, then a string like AGCTAGCT on Chromosome #9 can potentially be preserved *forever* as it passes through a succession of ephemeral bodies. If a string of DNA has positive consequences for the reproductive success of the organisms that are built with it, then its preservation through the generations becomes more likely.

There is a tradeoff between the length of a DNA segment and the potential immortality of the information it contains. The longer the segment of DNA that one considers, the more likely it is to have functional consequences for the construction of an organism, and, from an evolutionary standpoint, the more likely the presence of the segment in its particular location and sequence will affect the likelihood that an organism that bears it will survive and reproduce. Yet the longer the segment is, the less likely it is to remain intact from generation to generation. Length is directly related to the likelihood that the copying

[4] Another definition emphasizes that genes must be a part of the genome that is expressed in the phenotype, e.g., "a segment of the genotype that produces a recognizable effect on phenotype and segregates as a unit during gamete formation" (Boyd and Silk 1997: A9).

of a segment between generations with be disrupted, either by mutation (an error in copying) or by a crossing-over (random moments in the creation of an egg or sperm cell when the cell ceases copying the DNA of one parent and begins copying the DNA of the other). Seeking a middle ground, Dawkins (1976: 33) invites readers to define a gene as a "little bit of chromosome which potentially lasts for many generations," but which is still long enough to yield functional effects (in practice, he suggests that this would be usually longer than a cistron but much, much shorter than the length of an entire chromosome). By being based in this way on the likelihood of complete replication, Dawkins' definition of the gene proves very convenient for talking about evolution and heredity.

Alleles are alternative genes that different members of a species have at the same chromosomal locus. To keep with our very simplified example, say that some members of a population have the genetic string AGCTAGCT at a given location on Chromosome #9, but that other members have the string AAGGCCTT at this location instead. These two genes would be alleles of each other. Alleles can be thought of as competing with each other. If AGCTAGCT and AAGGCCTT are the only genes that exist at this location in the population, then any increase in the relative frequency of AGCTAGCT in the population can only come through a decrease in the relative frequency of AAGGCCTT. More generally, an increase in the relative frequency of a gene in a population implies a decrease in the relative frequency of at least some of its alleles. Because genes are passed from parent to offspring, the genes that build the members of a species with the highest survival and reproduction rates are those that proliferate in subsequent generations, while alleles that build less successful members decline in frequency. If the organisms that are built with AGCTAGCT on Chromosome #9 systematically outreproduce those built with

AAGGCCTT, for whatever reason, then over evolutionary time AGCTAGCT will come to predominate in the population and AAGGCCTT will eventually be pushed out of existence.

Because DNA sequences provide the instructions for building organisms, genetic differences between individuals are often manifested as differences in their traits. Evolution through natural selection occurs when variation in the genetically influenced traits of individuals is correlated with variation in their reproductive success. When this happens, the relative frequency of the genes that have the greatest positive effect on reproductive success increases, while the frequencies of their alleles declines. If the selective tendency favoring one gene over its alleles is sufficiently persistent over time, the alleles will eventually become extinct—they will not exist in any living member of the population—and the prevailing gene will therefore be universal among members, excepting any mutations that arise. As it turns out, the vast majority of functionally important locations on the genome of any species are dominated by single genes; this is what accounts for the enormous similarities that exist among the fellow members of a species (such as the fact that all normal humans possess two eyes, opposable thumbs, a capacity to learn language and manipulate symbols, and a skeleton conducive to walking upright).

The language of genes also allows us to elaborate upon the distinction between evolutionary and genetic explanations of behavior that was introduced in the last chapter. Proponents of evolutionary explanations typically propose that all human brains are built mostly alike, and that this uniformity—often called the "psychic unity of humankind"—implies a common set of genetic instructions for brain-building that evolved over the course of innumerable generations. They propose that many patterns of human behavior are universal (see Brown 1991), and that we can best understand why these behavior patterns are as they are by trying to reconstruct the selection processes through which they developed. Moreover, as I will describe later, evolutionary psychologists also propose that patterns of behavior that vary among persons still often emanate from underlying mechanisms that are the same in everyone. In short, evolutionary explanations of social behavior usually turn on a proposition of (relative) genetic uniformity among humans, at least in terms of the genes that figure importantly in shaping behavior.[5] In contrast, genetic explanations focus on the effects of human genetic differences. Behavior geneticists attempt to discern how much of the observed variation in a trait (such as intelligence or extraversion) is statistically explained by individual or racial variation in genes. They usually partition the observed variation in a trait into that which is explained by genetic differences and that which is due to differences in developmental environments.

While genetics play an important role in the work of both Darwinian social science and behavior genetics, this should not be taken to imply that researchers in either program work with actual sequences of DNA. A few behavior geneticists do so, typically searching for correlations between alleles and behavioral traits—these are the correlations that are often reported in the media as the finding of a gene "for" anxiety or homosexuality or alcoholism, etc. (for a popularization of the field by a practitioner, see Hamer and Copeland 1998). Beyond this, however, neither program gives much consideration to how the traits they consider are actually inscribed in DNA; as noted, scientists have only the most sketchy understanding of both the biochemical links between the structure of genes and the structure of brains and the neuropsychological links between the structure of brains and patterns of

[5] Evolutionary psychologists have been criticized for not recognizing the potential importance of adaptive genetic variation in the human population (D.S. Wilson 1994). David Buller (2000) writes: "How have Evolutionary Psychologists dealt with Wilson's critique? They have chosen to pretend that it doesn't exist. Tooby and Cosmides have never cited—let alone responded to—the article. Pinker (1997) doesn't cite Wilson at all... Their strategy appears to be: If you can't beat 'em, then act that like they don't exist."

behavior. They usually presume that a trait has a genetic basis if it is more highly correlated among identical twins than among fraternal twins and if it is more highly correlated among biological siblings than among adoptive siblings. Meanwhile, evolutionary psychologists and other Darwinian social scientists typically emphasize how human beings are genetically alike, and their research posits that human behavior is governed by a set of innate mechanisms that everyone possesses.

THE PRIMACY OF THE GENE IN NATURAL SELECTION

> *"Samuel Butler's famous aphorism, that the chicken is only an egg's way of making another egg, has been modernized: the organism is only DNA's way of making more DNA."*
>
> —Edward O. Wilson, *Sociobiology*, 1975 (p. 3)

Evolutionary competition is often popularly described as "survival of the fittest." The phrase originated with Herbert Spencer, and it was embraced by social Darwinism because the word "fittest" seemed to give a moral stamp of approval to the unequal outcomes of natural selection, which they believed also included the unequal distribution of wealth and success in a capitalist society. When John D. Rockefeller, Sr., addressed a Sunday school class, he pronounced that "The growth of a large business is merely a survival of the fittest… This is not an evil tendency in business. It is merely the working out of a law of nature and a law of God" (Hofstadter [1944] 1955: 45). Andrew Carnegie, another multimillionaire devotee of Spencer's writing, wrote that while capitalist competition "may sometimes be hard for the individual, it is best for the race, because it insures the survival of the fittest in every department" (Hofstadter [1944] 1955: 45-46).

Yet, in point of fact, "survival of the fittest" mischaracterizes the process of evolution through natural selection in several ways. First, of course, evolutionary success is not just a matter of *survival*, but the survival and the reproduction of healthy offspring, or rather the survival and reproduction of healthy offspring who survive and reproduce healthy offspring who survive and reproduce healthy offspring. Second, the evolutionary *fitness* of an organism cannot be determined apart from the parameters of the environment in which the population exists (see Sober 1984 for an excellent discussion of fitness). The fittest individual is the one who does the best job of getting her or his genes into the next generation (and vice versa), but reproductive success also strongly depends on a variety of situational factors. While, in a particular environment, one member of a species may have a greater likelihood of evolutionary success than another—and thus be more "fit"—the reverse might have happened had some aspect of this environment differed.

These two things together anticipate the biggest problem with "survival of the fittest": the prepositional phrase is not finished. Survival of the fittest what? Whether or not *individuals* survive is obviously important—and the preservation and development of species is what the theory is ultimately supposed to explain. Yet, we have just seen that what actually survives over the course of evolutionary time is the information passed along in our genes. It turns out that there are a number of different levels of analysis from which one can consider the process of natural selection, and the level one chooses implies different predictions about what sort of traits are likely to emerge in an organism and what traits are highly improbable.[6] Over the past three decades, evolutionary biologists have come increasingly to regard the gene as not just the physical mechanism of inheritance but also as the fundamental unit on which natural selection operates. This means that what is regarded

[6] Selection can be looked at from different levels in ways that are mathematically equivalent (i.e., different ways of expressing the same selection process; see Sober and Wilson 1998). What I am talking about here, however, is the "old" group selection, which proposed traits having evolved for the good of a species without thinking about the restricted conditions under which prosocial behaviors can evolve.

as being ultimately important for whether a genetically-based trait increases or decreases in a population is not the effect of the trait on an individual's rate of reproduction or the reproductive success of the individual's group (or species). Instead, what matters is the effect of a trait on intergenerational replication of the genes that code the traits' presence or absence. When Richard Dawkins proposed the definition of gene described in the last section, he did so while advancing the argument that the best way to think about evolution was to think of genes as being the parties whose "interests" the process of natural selection ultimately served. The idea is captured nicely in the title of his book *The Selfish Gene* (1976).

When Dawkins characterizes genes as "selfish," he is most emphatically not saying that they have wishes, desires, greedy impulses, or any anthropomorphisms of this sort. Genes are ultimately just different configurations of information encoded in segments of nucleotides. The point of the selfish-gene concept is that any gene that causes functional changes in an organism that promote the proliferation of the gene in subsequent generations will increase in relative frequency in the species population, even if the gene sometimes has negative consequences for individual's well-being and even if it is harmful to the general welfare of the species. Put another way, if you want to know which variations in the design of an organism will be favored by natural selection, the selfish-gene perspective says that you need to look strictly at the consequences of each variation for the particular genes that encode for it. In a large number of cases, design modifications that increase the reproductive success of individuals also increase the proliferation of that individual's genes and so will be favored by natural selection. For example, the dramatic increase in cognitive ability that is a hallmark of human evolution presumably happened through a selective process where individual proto-humans who were more intelligent had a greater likelihood of surviving and reproducing healthy offspring than did their species-mates who were less intelligent. Higher intelligence increased the reproductive success of the individuals who had them and thus genes associated with higher intelligence came to predominate in the population in subsequent generations.[7]

Where the reproductive consequences for an individual and for that individual's genes are thought to diverge most importantly is in the case of behavior towards one's relatives. Purely selfish individuals (in the classical sense) would always put their own welfare ahead of that of their relatives. Genes, on the other hand, can be thought of as having a vested interest in the well-being of relatives because the closer the blood relation between two individuals, the greater the number of genes they have in common. So a gene can increase its representation in the next generation not only by enhancing the reproductive success of its bearer, but also by enhancing the reproductive success of its bearer's relatives.

Wilson (1975:3) once called altruistic behavior "the central theoretical problem of sociobiology." An altruistic act can be defined as any behavior which lessens (however slightly) an organisms' chances of survival and reproduction while aiding those of someone else. The classic example is an individual who, having nothing personally to gain by doing so, puts her or his life at risk in order to save someone else's. By definition, a person willing to risk her or his life in this way would reproduce less on average than someone not willing

[7] Since intelligence would always seem to promote reproductive success, one might wonder why every animal did not evolve to be as smart as humans are. Human cognitive evolution required an enormous increase in brain size, which in turn imposed all sorts of formidable costs, including increasing the amount of required dietary protein and increasing the risks of both mother and infant dying in childbirth. In fact, the costs of a brain size increase like ours are so high that many believe that it would only be favored by natural selection under a very unusual set of circumstances, which explains why no other species evolved our big brains (Tooby and Devore 1987; Pinker 1997).

to assume this risk, and consequently any predisposition toward altruistic acts should be strongly selected against over evolutionary time.

In contrast, the selfish gene perspective predicts that evolutionary competition should make blood relatives natural allies of one another, because two related members of a population share more of the same genes than do two unrelated members. When a person helps a blood relative, there is a better-than-chance probability that any genes the altruist possesses that contribute to the helping behavior are also shared by the benefactor. Under the right conditions, any genes that predispose a person to be willing to incur some risk on behalf of relatives would be expected to outreproduce alleles predisposing a person to purely individualistic behavior (the process by which this occurs is known as *kin selection*). The exact amount of risk for which it is adaptive for one individual to assume on behalf of another is partly a function of the closeness of their genetic relationship, as calculated by their coefficient of relatedness (see Table 2.1). The mathematics behind these coefficients were famously presaged by biologist J. B. S. Haldane's off-the-cuff jest that, on purely evolutionary grounds, he would sacrifice his own life if doing so would save more than two brothers, four half-brothers, or eight cousins (see Richards 1987: 541).

Table 2.1. Coefficients of relatedness (*r*)

Relationship to self	*r*	Relationship to self	*r*
Self	1.0	Uncle/Aunt	0.25
Identical twin	1.0	Niece/Nephew	0.25
Sibling	0.5	Grandparent	0.25
Parent	0.5	Grandchild	0.25
Child	0.5	First cousin	0.125
Half-sibling	0.25	Stepparent/Step-sibling/Stepchild	0

The theory of inclusive fitness suggests that if we want to consider the evolutionary success of the genome that built a particular organism, we cannot simply look at that organism's reproductive success, but instead we must also take into account the reproductive success of all the individual's relatives weighted by their coefficients of relatedness (Hamilton 1964). The concepts of inclusive fitness and selfish genes go hand-in-hand, and together they have become the dominant way in which the evolutionary process is presently understood by many proponents of Darwinian approaches to social behavior (Sober and Wilson 1999). Before the development of the gene-centered perspective, the most common way of explaining altruism and other phenomena that seemed to defy individual-level evolutionary explanation was to go in the opposite direction—to propose that traits had evolved for "the good of the species" (Allee 1931; Wynne-Edwards 1962). Called *group selectionism*, the idea was that if a species was separated into a number of discrete local populations, then the presence of group-serving genes in some of these populations could lead them to grow and thrive while populations that lacked such genes in their gene pools died out. Under close scrutiny, the idea of group selectionism falls apart in the vast majority of instances, for a variety of reasons (a particularly lucid description of these is provided by Kitcher 1985: 77-79). It has been shown that almost all of the claims of group selectionism that have been raised can be more satisfyingly re-expressed in terms of gene-level selection, and mathematical models suggest that group selectionism is possible under only fairly unusual evolutionary conditions (Williams 1966, 1975; Maynard Smith 1989; Reeve 1998). Few doubt that natural selection almost always operates most effectively *within* breeding populations, where gene-centered behavior prevails, rather than between them (Reeve 1998). (Recently, however, there has been a rise of so-called "new" theories of group selectionism, especially for application to humans [e.g., Wilson and Sober 1994]. This work suggests that group-level selection may have paid a more important role in human evolution that is commonly credited by sociobiology or evolutionary psychology, and that group-selection is a different way of looking at the same changes in gene frequencies—a different manner of "accounting", one could say—as selfish gene theory.)

The triumph of inclusive fitness and the selfish gene over group selectionism has had two important implications for the development of Darwinian approaches to human social behavior. The first is that evolutionary psychology and almost all related programs assume the stance on the relationship between the individual and society that is known to sociologists as methodological individualism—that "social phenomena are best explained by analyzing the propensities of the individual actors that constitute them" (Levine 1995: 129). As a result, evolutionary psychologists have been able to borrow many ideas from the more individualistic social sciences of psychology and economics—particularly the computational theory of mind from the former and game theory from the latter (Tooby and Cosmides 1989, 1992). Meanwhile, the theorists who are most closely associated with the idea that societies have emergent properties that cannot be reduced to the dispositions of individual actors—Durkheim, Parsons, Boas, and Geertz—are regular targets of criticism from evolutionists (e.g., Tooby and Cosmides 1992). Daly and Wilson (1998: 436) write:

> The individualistic focus of psychology is in general much more compatible with evolutionary analyses than is anthropology's emphasis on group-level entities like 'society' and 'culture'… A corollary is this: If nepotistic Hamiltonian adaptations await discovery, they are likely to be primarily adaptations of the individual social psyche, rather than emergent properties of societies, families, or other collectivities, and the science that will elucidate their structure and functioning will be psychological in its focus.

François Nielsen (1994: 271) has called the displacement of group-selectionism by gene-centered perspectives the "biological equivalent" of the increasing displacement of Durkheimian and Parsonsian functionalism by rational choice theory. (There have been efforts to bring rational choice theory and evolutionary psychology closer together by modifying our understanding of rationality to bring it more in line with the insights of Darwinian social science [e.g., Frank 1988, 1993; Macy and Flache 1995; Macy 1997; Kanazawa forthcoming b].)

The second implication of the triumph of inclusive fitness theory in biology is that evolutionary approaches to human social behavior typically give strong consideration to kinship (see Chapter 7). Early work in sociobiology sought to show that kinship systems were not the fairly arbitrary and highly variable structures that many cultural anthropologists claimed, but instead the real distribution of resources in extant hunter-gatherer or early-agricultural societies matched what would be expected by inclusive fitness theory. For example, Chagnon and Bugos (1979) conduct a detailed analysis of a fight among members of a Yanomamö Indian village. They argue that the alliances that formed during and in the wake of this fight correspond strongly with what would be predicted from members' biological coefficients of relatedness and kin selection theory (their study is criticized compellingly by Kitcher [1985: 307-315]). Evolutionary psychology, meanwhile, focuses on how proposed cognitive mechanisms governing feelings of love, sympathy, the desire to help are fundamentally based on kin selection and are built to favor close genetic relatives over others. Steven Pinker (1997: 401-2) writes:

> Genes are not imprisoned in bodies; the same gene lives in the bodies of many family members at once. The dispersed copies of a gene call to one another by endowing bodies with emotions. Love, compassion, and empathy are invisible fibers that connect genes in different bodies. They are the closest we will ever come to feeling someone else's toothache. When a parent wishes she could take the place of a child about to undergo surgery, it is not the species or the group or her body that wants her to have that most unselfish emotion; it is her selfish genes.

Daly and Wilson (1998: 443-447; see also Wilson 1987) postulate that an adaptation of "child-specific parental love" may inhibit parents from responding angrily to their children's bad behavior. They suggest that the adaptation may be designed only to work for one's own biological children, and they claim that this selectivity might explain why step-children are so much more likely to be victims of abuse than biological children.

Both sociobiologists and evolutionary psychologists regard the family as the primordial form of social organization. Some argue that a natural tendency toward nepotism recurrently and inevitably undermines the efforts of other social institutions to achieve bureaucratically efficient or purely meritocratic operation (Wilson 1978; Pinker 1997). Others note that religious and political movements will often use the language of kinship in an effort to colonize member's natural preferences and get them to think of their comrades as family (Johnson 1986, 1987; Salmon 1998). Ferdinand Mount (1992) claims that many religious have tried to subvert the family; that is, they have tried to convince members to subordinate the interests of the family to the interests of the movement. He contends that these movements understand the destabilized threat that is posed by member's intrinsic affinities for kin. In support of this point, Pinker (1997: 439) recounts the Bible verses Matthew 10:34-37, in which Jesus says that he has "come to set a man at variance against his father, and the daughter against her mother, and the daughter-in-law against her mother-in-law. And a man's foes shall be they of his own household. He that loveth father or mother more than me is not worthy of me, and he that loveth son or daughter more than me is not worthy of me."

ADAPTATION

> *"In Darwinism the element that is both central to the evolutionary world view and yet so powerful that it can destroy Darwinism as a testable theory is that of* adaptation.*"*
>
> —Richard C. Lewontin (1984: 235)

Because belief in creationism—the idea that all species were brought into being by the will of a creator—seems today so much a by-product of religious faith, it is easy to forget how strongly creationism stood *empirically* before Darwin. Evidence that the world's species were the product of intentional design could be seen by all the ways in which organisms seemed to be uniquely and optimally suited for the environments in which they lived. For example, the thirteen species of Galápagos island finches that Darwin studied had enormous anatomical similarities, but each also possessed a different bill that matched perfectly the needs implied by its diet. The finches that ate large seeds had thicker bills; those that ate small seeds had smaller bills; those that ate ground insects had bill shapes ideal for grasping; and the species that ate insects inside cactuses had a longer bill that enabled it to make holes with a stick (Darwin 1859; see also Steadman and Zousmer 1988). What the theory of evolution through natural selection provided was a way of understanding how such exquisitely perfect relationships between an organism's morphology and its surroundings could have arisen as the result of a purely natural, atheistic, and algorithmic process (Dennett 1995). Evolutionary competition is always contingent upon the features of the environment in which the competition is taking place; natural selection very often favors specialized designs that are apt for local environments but are unfortunately ill-suited for anywhere else. (The phrase "fish out of water" provides a nice pun here, as the very respiratory features of fish that allow them to thrive in undersea environments consign them to a quick death whenever they are placed on land).

Evolutionary biologists use the term *adaptation* to characterize any feature of an organism that developed because of the inclusive fitness advantages it conferred in some local environment. Any trait or behavior that enhances the inclusive fitness of an organism in a particular environment at a particular point in time is called *adaptive*, while a trait or behavior that detracts from an organism's inclusive fitness in that environment is called *maladaptive*. The eye is a classic example of adaptation. The various parts of the eye, including the lens, cornea, pupil, and iris (as well as their wiring to the brain), comprise too complex, coordinated, and beneficial of a system to have arisen simply by genetic chance. Instead, we can be confident that the eye must have developed as a series of incremental developments that proliferated and built upon one another over evolutionary time because of the massive fitness advantages that are conferred to the members of a species who possess the genes that enable them to process visual information the best. Vision turns out to be so widely adaptive that it has been maintained in almost all vertebrates, and vision has evolved independently in some invertebrate species (such as octopuses) (see Dawkins 1996). In other words, the eye seems to do well in many environments, with the underground habitats of moles and earthworms serving perhaps as the main exception. Other adaptations are much more environment-specific, as the above examples from finches and fish suggest.

The eye is such an glaring adaptation, however, that it obscures the fact that identifying adaptations is usually a very difficult enterprise. Only occasionally do adaptations serve such obvious and enduring functions as does the eye (for a discussion specifically about the problems of comparing the eye as an adaptation to cognitive mechanisms as adaptations, see Grantham and Nichols 1999). Instead, natural processes can produce adaptations that still exist even though they are no longer adaptive for the organisms that possess them, adaptations that are now adaptive for reasons unrelated to why they originally evolved, or adaptations that evolved their particular form by serving one adaptive function at one point in evolutionary history and another function at another point; one can also observe traits or behaviors that are currently adaptive but originally developed as by-products of other adaptations, as well as features of an organism that never served any adaptive function and are instead simply the result of genetic noise. Determining which of these possibilities is what actually happened is made difficult by the indirect character of evidence about evolutionary history, especially as it pertains to humans (Boyd and Silk 1997).[8] The lack of hard details means that speculations about what *could* have happened are little constrained. Because there are countless way in which the inclusive fitness of an organism may be enhanced—and because evolution favors genes that promote even miniscule improvements in fitness—the number of such speculations about any single phenomenon can grow quickly. Critics complain that competing evolutionary explanations often cannot be adjudicated by any decisive logical or empirical test, and that, as a result, acceptance of particular explanations by particular scientists depends too frequently on unscientific criteria such as disciplinary fads, author reputations, or consonance with political beliefs (e.g., Gould 1997).

Those who defend the value of trying to explicate adaptations can point to successes in the study of other animals and aspects of human physiology. They also cite some of the promising efforts that have been made in recent years to apply adaptationist analyses to human medicine (see Nesse and Williams 1994 for a broad review). Proponents of

[8] Reasons why the study of human beings poses particular problems in this regard include the unusually long length of human generations; the sharp differences between humans and their closest living primate relatives; the unparalleled human capacity for learning and behavior modification; and the inability (on obvious ethical grounds) of scientists to submit humans to the same intrusive experimental methods that can be used to study other species.

evolutionary approaches to human behavior argue that evolutionary analyses can help us understand why humans act as they do because natural selection has shaped the structure of the mental mechanisms that govern human action. They assert that only by taking an adaptationist approach can we get at the functions that the brain evolved to serve and that only by understanding these functions can we develop an adequate theory of how the mind works. In turn, proponents argue that a better model of how the mind works is necessary if we are to develop a truly satisfying model of human behavior or if we are to devise truly effective interventions for social problems. A rallying cry of adaptationist approaches to human psychology is George Williams's (1966: 16) incisive question, "Is it not reasonable to think that our understanding of the human mind would be aided greatly by knowing the purpose for which it was designed?"

THE ADAPTATION OF THE HUMAN MIND

> *"Man, with all his noble qualities, with sympathy that feels for the most debased, with benevolence which extends not only to other men but to the humblest of living creatures, with his god-like intellect which has penetrated into the movements and constitution of the solar system—with all these exalted powers—still bears in his bodily frame the indelible stamp of his lowly origin."*
> —Charles Darwin, *The Descent of Man* (1871)

As I noted in the last chapter, much of the recent attention given to evolutionary approaches to human behavior has focused on evolutionary psychology. Evolutionary psychologists assert that their program represents a marked improvement over some sociobiological predecessors in their conception of how natural selection has adapted the basic structure of the human mind. Some work in sociobiology has tended to operate under the assumption, not always explicit, that natural selection has endowed human beings with a very broad and general tendency to act in ways that maximized their inclusive fitness.

Richard Alexander expresses the idea nicely with his claim that "human beings are inclusive fitness maximizing blobs" (quoted in Buss 1995a: 10). Likewise, Joseph Lopreato and Timothy Crippen (1999), in a recent book very much in line with this older tradition of sociobiology, seek to place sociological reasoning on a Darwinian foundation of which the central "law" is that "organisms tend to behave so as to maximize their inclusive fitness" (p. 118, see also p. 77).

The sociobiological position here is not that maximizing inclusive fitness is always or ever a conscious goal of human activity, but only that our brains have evolved in such a way that we behave (consciously or unconsciously) *as if* this were the case (see Smith 1999 review of human behavioral ecology and Grafen (1991).[9] In this way, the logic of early sociobiology is analogous to that of rational choice theory, which does not claim that humans consciously juggle costs and benefits before every decision but only that they have a tendency to act as if this is what they did. (In fact, in its predictions, early sociobiology can be thought of as structurally equivalent to a rational choice model in which inclusive fitness was the dominant term in the utility function of every actor [see Richerson, Boyd, and Paciotti forthcoming].) Such a conception of the evolved brain also seemed consistent with the selfish-gene perspective, insofar as it appeared to match Dawkins' (1976: 19) characterization of human

[9] On this score, Richard Dawkins has a field day with Marshall Sahlins' (1976) criticism that "[T]he epistemological problems presented by a lack of linguistic support for calculating *r*, coefficients of relationship, amount to a serious defect in the theory of kin selection. Hunters and gatherers generally do not have counting systems beyond one, two, and three. I refrain from comment on the even greater problem of how animals are supposed to figure out that [the coefficient of relatedness for first cousins] = 1/8." Dawkins is able to make the simple point that natural selection has produced all sorts of patterns that can be expressed mathematically, but this in no case entails that the relevant species can consciously do the math these patterns require. As he puts it, "A snail shell is an exquisite logarithmic spiral, but where does the snail keep its log tables; how indeed does it read them, since the lens in its eye lacks 'linguistic support' for calculating *m*, the coefficient of refraction? How do green plants 'figure out' the formula for chlorophyll" (Dawkins 1979). Needless to say, blunders like this one, by a social scientist as eminent as Sahlins, have contributed greatly to the impression that anthropologists and sociologists are too biologically ignorant to offer an appropriate critique of Darwinian social science.

beings as "lumbering robots" programmed to act in accordance with a prime directive of maximizing the proliferation of their genes.[10] In its most extreme formulations, this conceptualization seems to reduce human motivation to an underlying, subconscious, and singular desire to get as many copies of one's genes into the next generation as possible. "Think of it," pens Robert Wright (quoted in Ridley 1996: 36), "zillions and zillions of organisms running around, each under the hypnotic spell of a single truth, all these truths identical, and all logically incompatible with one another: 'My hereditary material is the most important material on earth; its survival justifies your frustration, pain, even death'. And you are one of these organisms, living your life in the thrall of a logical absurdity."

The belief that human possess a general propensity to maximize inclusive fitness (without being consciously aware of it) has inspired a considerable amount of scholarship, especially in anthropology. A series of studies have endeavored to show how an unusual practice of some hunter-gatherer or agrarian society existed because circumstances led it to increase the reproductive success of that society's members. As examples, Crook and Crook (1988; discussed in Symons 1992a) claim to show that peculiar environmental conditions make polyandry adaptive among the groups of Tibetans who commonly practice it; Dickemann (1979) does the same for female infanticide in northern India and China; and Durham (1976) does likewise for headhunting and warfare among the Mundurucú. Other scholars have looked at behaviors in our own society that appeared to be patently maladaptive (e.g., suicide), and they tried to show how it actually did serve functions that increased the success of one's genes. Sociobiology has also provided a number of predictions

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[10] Dawkins' use of the phrase "lumbering robots" was unfortunate, insofar it caught the attention of many proponents and (especially) opponents of his idea, and yet would appear to provide a much more deterministic picture of the influence of genes on behavior than what Dawkins portrays elsewhere in the book (although he spiritedly defends his use of "lumbering robots" in the 1989 edition of The Selfish Gene [p. 270-1], blaming others' interpretations on erroneous popular conceptions of what it means to be a "robot").

about behavior. Many of these have been criticized for trading on (sometimes outdated) cultural stereotypes, such that some readers might have found them convincing even though they had never been tested (Kitcher 1985). An example here might be Alexander's (1987: 218) claim that an evolutionary perspective on human behavior implied the prediction that human males should be "reluctant" to use contraception and "exceedingly reluctant" to have vasectomies.

The idea that humans have a general orientation to maximizing their inclusive fitness has been compellingly criticized by evolutionary psychologists (Symons 1992a; Buss 1995) and others (Kitcher 1985). In the critics' view, the most glaring problem with the idea is its incompatibility with a modern world where contraception is widely practiced, where those with the most abundant resources tend to have the fewest children, and where agencies make a thriving business out of pairing orphaned or unwanted children with completely unrelated adults who are eager to care for them. As Donald Symons (1992a: 155) puts it:

> [If] people actually wanted to maximize inclusive fitness, opportunities to make deposits in sperm banks would be immensely competitive, a subject of endless public scrutiny and debate, with the possibility of reverse embezzlement by male sperm bank officers an ever-present problem.

In addition, evolutionary psychological critics contend that the idea doesn't jibe with evolutionary theory: natural selection does not endow organisms with a general capacity for doing the right thing—even when they are placed in novel environments—but instead produces specific adaptations that evolve in response to specific pressures in past environments (Symons 1987, 1989, 1992a; Pinker 1997). The Tibetan tendency to practice polyandry could really only be a specific adaptation if (a) the essential environmental conditions recurred in our evolutionary past, (b) under these conditions, those who married polyandrously outreproduced their contemporaries who married monogamously or polygynously, and (c) the decision of whether or not to marry polyandrously was somehow influenced by genetic factors (Symons 1992a). More generally, when human environments differ from those that were dominant during most of our evolution, we should not expect the mind to "figure out" what optimizes inclusive fitness in the new environment and adjust behavior accordingly, but instead we might predict that the behavioral predispositions that were successful in the past would continue to predominate even if they were to be now maladaptive.[11]

The alternative conception of mind advanced by evolutionary psychology proposes that the mind has no global tendencies but instead is a collection of specific and discrete cognitive mechanisms. Each mechanism is considered a separate mental adaptation that evolved as a solution to a different evolutionary problem. What appears to be the action of a unified mind is actually determined by the selective activation and operation of an array of mechanisms that are independent of one another in their origins and largely independent in their execution. A metaphor for the mind popular among evolutionary psychologists is that of a Swiss army knife. (Cosmides 1994) The knife "contains separate tools—each designed to perform a particular task effectively. The human brain also appears to come equipped with cognitive tools designed to carry out specific functions" (Buss and Kenrick 1998: 991). In other words, while some earlier sociobiologists had largely conceptualized the mind as a general fitness-maximizing device, evolutionary psychology sees the mind as a collection of an indefinite number of specialized mental tools that are brought into play at different times in response to different situations.

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[11] Advocates of traditional sociobiology have responded to this criticism by arguing that evolutionary psychology has exaggerated the differences between it and earlier approaches and have exaggerated the flaws of sociobiology (Alexander 1990; Lopreato and Crippen 1999; Horgan 1999).

This view of mind was presaged by the Chomskyian revolution in linguistics. Before Chomsky, languages were thought to be able to vary infinitely, and children were thought to learn language by observation and by trial and error, just like they (seemingly) learned anything else. Chomsky's first important publication was a critique of the behaviorist theory that children acquired language through a training process in which they were reinforced for syntactically correct utterances and punished for incorrect ones (Chomsky 1959; see Skinner 1957). Chomsky compellingly argued that the behaviorist position could not possibly be correct because children learned language too quickly and they learned it even though the linguistic information they were presented with was selective and incomplete. Moreover, Chomsky maintained that linguistic fluency could not be gained simply by being told which utterances are structured correctly and which are incorrect, as fluent speakers are constantly producing or understanding sentences that have never been constructed before (such as the sentence you are reading right now).

Chomsky believed that the speed and success with which children everywhere master their native language can only happen if they have a head start. Learning language is not like learning anything else. Instead, Chomsky proposed that humans are born with the basic structure of language already built into their brains, as a system independent from the rest of cognition that he claimed could even be thought of as a separate "mental organ." He theorized that the variation that exists among the world's languages is actually superficial, the result of a series of transformations away from a deep structure that all languages share. The transformations used to produce grammatical utterances in any particular language are, in turn, thought to be a function of a smaller set of parameters that are sufficient to define the syntax of the language. Language acquisition, in Chomsky's view, is not so much a matter of learning as it is of bringing the language system on-line and by setting the necessary parameters on the basis of the linguistic input children hear in the world around them. The process happens naturally and effortlessly as long as some linguistic input is provided; if a child is not exposed to language until later (for example, as the result of childhood deafness or isolation), then language may never be acquired perfectly—many believe this is why people usually never gain native-like proficiency in a second language unless they pick it up when they are young. While Chomsky believes that language is an autonomous and innate system, he has shied away from the claim that the system is an evolutionary adaptation. Others have not: the idea of language as an adaptation is promoted eloquently in Steven Pinker's widely-read book *The Language Instinct* (1994; see also Pinker and Bloom 1990).

What evolutionary psychology has done is extend Chomsky's idea to argue that there are all sorts of different systems inside the mind.[12] We are not simply hard-wired to acquire language, but we are hard-wired for all sorts of skills, feelings, and behavioral tendencies. A commonly held belief about human evolution is that it has involved an erasure of many of the "instincts" that are supposed to drive the behavior of other species. Some evolutionary psychologists turn this idea on its head, arguing that various innate mechanisms can be thought of as "instincts" and that the cognitive complexity of human life suggests that humans have *more* instincts than other species, not less (Tooby and Cosmides 1992; Cosmides and Tooby 1997). This position is a strong version of a theory sometimes called the *modular theory of mind*, to draw out the analogy between it and the modular technique of computer programming (Fodor 1983). Programmers everywhere know that the best way to build a large and complicated computer program (like a spreadsheet or word processing

[12] Chomsky, it must be pointed out, is no fan of evolutionary psychology; he has asserted that it is not a real science but "a philosophy of mind with a little bit of science thrown in" (quoted in Horgan 1997: 44).

package) is to break the job down into many small subprograms—called modules—each of which is designed for a single task. Evolutionary psychologists propose that natural selection has developed a human mind that is similarly organized. The different modules of the brain are activated only by the problems for which they evolved as specialized solutions. As Charles Crawford (1998:20) describes it, "When activated by an appropriate problem content, [evolved cognitive mechanisms] focus attention, organize perception, and memory, and call up specialized procedural knowledge that leads to domain-appropriate inferences, judgments, and choices."

Leda Cosmides and John Tooby, pioneers in developing evolutionary psychology's perspective on the mind, have proposed that our capacity to engage in social exchange is predicated on the evolution of a series of specialized cognitive adaptations (Cosmides 1985, 1989; Cosmides and Tooby 1989, 1992). Social exchange is practiced in all known human societies, and the ability to engage in social exchange (without being taken advantage of) affords obvious evolutionary benefits. Cosmides and Tooby contend that, in order for humans to have developed the ability to engage in social exchange, they must have developed specialized algorithms that allow them to identify costs and benefits and to recognize situations in which the benefits of a potential exchange exceed the costs. Additionally, the mind must have developed procedures that enable it to detect instances of cheating on social exchange or social contracts. They argue that humans who continually engaged in disadvantageous social exchanges but who could not detect cheaters would be systematically selected against over evolutionary time. As I will consider in Chapter 3, Cosmides (1985, 1989) has conducted experiments that are proposed to suggest the presence of cognitive mechanisms specialized to detect cheating.

David Buss (1995a, 2000a) cites jealousy as another example of an example of an evolved psychological mechanism. He claims that jealousy is a "cognitive-emotional-motivational" complex that developed as an adaptation because of its value for preserving relationships that are evolutionarily valuable to an individual—especially mating relationships. Jealousy is said to be activated by cues which indicate the possibility that another person may usurp or attempt to usurp the valued resources provided by the relationship partner. When the jealousy mechanism is activated, Buss (1995a:14) writes, it "channels attention, calls up relevant memories, and channels thought in particular directions. Ultimately, it motivates actions designed to reduce or eliminate the threat and retain the valuable relationship and the resources it provides." Over evolutionary time, presumably, those who exhibited jealous responses tended to have greater reproductive success than those who do not become jealous, and the jealous mechanism consequently because a species-typical feature of human mind (even while the degree of jealousy exhibited by individuals may still vary). When Daly and Wilson (1988) examined the ethnographic record, they found evidence for the universality of sexual jealousy, including its presence in societies that some had claimed had no restrictions or proprietary attitudes toward sexual conduct (Daly, Wilson, and Weghorst 1982).[13]

An advantage of the modular theory of mind over its sociobiological predecessor is that evolutionary psychologists are not put in the awkward position of trying to explain how human activity that clearly decreases reproductive success is somehow actually adaptive.[14] Instead, evolutionary psychologists argue that since the mind's cognitive adaptations evolved

[13] An interesting exercise for sociology of science would be a look at the interpretive work through which claims of cross-cultural universals such as this one come to be made and substantiated.
[14] For an indication of how the desire to presume that human behaviors are mostly adaptive lives on, especially in the work of human behavioral ecology, see Betzig (1998) or Smith (2000).

largely in the environments of our hunter-gatherer past, some oddities in the function of these mechanisms is to be expected given all of the novelties of our present environment (Buss 1999). For example, Barkow (1992: 629-30) suggests that the modern fascination with celebrity gossip may be the result of our cognitive adaptations for keeping tabs on fellow band members being confused by the advent of mass media:

> Mass media may activate the psychological mechanisms that evolved in response to selection for the acquisition of social information. The media may mimic the psychological cues that would have triggered these same mechanisms under Pleistocene conditions. As a result, strangers present only on cathode ray tubes in our living rooms, or magnified many times life-size on the screens of motion picture theatres, are mistaken for important band members by the algorithms of the evolved mechanisms of our brains. We see them in our bedrooms, we hear their voices when we dine. If this hypothesis is correct, how are we not to perceive them as our kin, our friends, perhaps even our rivals? As a result, we automatically seek information about their physical health, about changes in their relative standing, and, above all, about their sexual relationships.

In some cases, mismatches between present and past environments can have maladaptive consequences. A classic example is our dietary preference for sugar, fat, and salt, which may have been adaptive in these Pleistocene era when these were indicators of scarce and valuable nutrients but which is a maladaptive bane to human health now that junk food and fast food are plentiful (Barkow 1989; Wright 1994a; Gould 1997 criticizes this example). In the realm of behavior, some evolutionists have speculated that adaptations of judging the likelihood or appropriateness of aggressive behavior in a society might be confounded by the large amount of violence shown on television, causing some to react more fearfully or more aggressively than modern social situations actually warrant (Crawford 1998b).

The modular theory of mind has made evolutionary explanations more difficult to test (as a grounds for criticizing evolutionary psychology, see Betzig 1998). All evolutionary explanations are predicated on the idea that some trait or behavior increases inclusive fitness, and this proposition can only be tested by tracking rates of reproduction over time. How do we know that jealous behavior increases inclusive fitness, especially since we can all point to examples where relationships were harmed or terminated because of jealousy?[15] For those sociobiologists who believed that humans maximized their inclusive fitness in the present, evolutionary hypotheses could be tested using contemporary fertility data. Because evolutionary psychologists believe that mechanisms could have had adaptive consequences in the past but maladaptive consequences in the present, studies of reproductive success in an evolutionarily novel environment such as ours has little value. Moreover, because they believe that many of the most important cognitive adaptations took place during humans' great brain size increase, which happened after humans' ancestral line broke off from that of other primates. Consequently, evolutionary psychologists do not make as much use as sociobiologists of comparisons with primate species (sociobiologists Lopreato and Crippen [1999: 130] accuse evolutionary psychology of "belittling" the value of cross-species comparisons for understanding humans). Instead, evolutionary psychologists have given considerable attention to hunter-gatherer societies, as these are seen as living in much the same way as humans did in the Pleistocene. The problem here is that hunter-gatherer societies vary immensely, and it is unclear which society resembles our actual adaptational environment most closely (Betzig 1998). For traits like higher intelligence or greater skills at social exchange, claims of adaptive values are less controversial; matters are more ambiguous for something like jealousy.

[15] With the eye or with language, the existence of a system comprised of so many complexly organized and intricately structured parts would seem to suggest that they must serve some adaptive purpose, even if we don't know what it is. It is much less clear that this criterion applies to a phenomenon as amorphous as "jealousy."

THE ENVIRONMENTAL SENSITIVITY OF EVOLVED MECHANISMS

The computational metaphor implied by the modular theory of mind has given evolutionary psychology an elegant language for talking about the interaction of biological and environmental factors. Scholars of a wide range of orientations have long recognized that human behavior is determined by such an interaction, but, in the past, acknowledging this has often served as a starting point for arguing which side of the interaction is more important (Pinker 1997). Evolutionary psychologists ask of us to conceptualize our evolved cognitive mechanisms as our mind's (innate) "software," while all environmental influences can be thought of as different forms of "input." Consider real computers and real software for a moment, and think of all of the computer's output—to the screen, printer, or disk—as being its "behavior." Is a computer's behavior determined more the software it runs or by the user's keystrokes and mouse clicks? Most of the time, the question is meaningless: Once an application is launched, everything it does is simultaneously and irreducibly a function of both the program's structure and user input. Similarly, for humans, Tooby and Cosmides (1992: 83-84) maintain that

> [E]very feature of every phenotype is fully and equally codetermined by the interaction of the organism's genes (embedded in its initial package of zygotic cellular machinery) and its ontogenetic endowments—meaning everything else that impinges on it. By changing either the genes or the environment any outcome can be changed, so the interaction of the two is always part of every complete explanation of any human phenomenon. As with all interactions, the product simply cannot be sensibly analyzed into separate genetically determined and environmentally determined components or degrees of influence. For this reason, everything, from the most delicate nuance of Richard Strauss's last performance of Beethoven's Fifth Symphony to the presence of calcium salts in his bones at birth is totally and to exactly the same extent genetically and environmentally codetermined. "Biology" cannot be segregated off into some traits and not others.

By conceiving the mind as a series of evolved mechanisms whose operation is affected by environmental inputs, evolutionary psychology claims to move past the debate over whether biology or environment is more important by proposing that behavioral explanations requires a description of both the evolved modules and how the operation of these mechanisms is affected by the environment.

Evolutionary psychology proposes that the mind contains "a dazzling array of psychological mechanisms specifically designed to respond to the environment" (Buss and Kenrick 1998: 1018). From the standpoint of natural selection, it is not difficult to see how a sensitivity to environmental conditions could develop; as Michael Bailey (1998: 226) puts it, "The evolution of a contingent human nature simply requires that, across generations, humans are regularly exposed to different environments in which different strategies would work best." We can assume that over the millions of years of the Pleistocene era, when most of our unique human cognitive adaptations are presumed to have evolved, human beings experienced a good deal of environmental variation both in terms of the raw conditions that they faced and in terms of their position (on various dimensions) relative to other members of their hunter-gatherer bands. Consequently, ample opportunity seems to have existed for human cognitive adaptations to have developed varying responses to varying conditions. Even so, evolutionary psychologists propose that at least some aspects of many cognitive mechanisms are relatively inflexible to environmental conditions and may operate in a stable and predictable manner across a wide range of extant environments. Buss (1987, 1993, 1994, 1998; see also Singh 1993; Symons 1992b; Cunningham et al. 1995), for example, has presented evidence that he says shows that some of the qualities that men and women find attractive in prospective mates are relatively constant across societies. This is taken to suggest that the selection pressures that shaped the development of the mechanism underlying the evaluate of prospective mates tended to favor the same characteristics over a wide range of environments.

Evolutionary psychologists propose that the sensitivity of cognitive adaptations to environmental conditions is partly responsible for both (a) the enormous situational variability in behavior that all individuals exhibit and (b) the stable personality differences between persons that are evinced over time (Buss 1999). Regarding the former, environmental cues may be what activates an otherwise dormant mechanism, such as when Buss suggests that we have cognitive adaptations of jealousy that are activated by cues indicative of a threat to a valued relationship. Additionally, once they are activated, mechanisms can trigger different responses depending on characteristics of one's situation. A mechanism activated in response to a physical threat might trigger different behaviors depending on the magnitude of the threat, such as how we might respond differently to the threat of a barking dog depending on whether it is a poodle or a Doberman. Mechanisms may also trigger different responses depending on characteristics of those with whom we are interacting. This can be seen in Daly and Wilson's proposal that evolved mechanisms may lead parents to treat biological children differently than stepchildren, as well as in the proposal that persons in social exchange situations may respond differently depending on whether the potential exchange partner is someone who has cheated them before (Mealey, Daood, and Krage 1996).

Evolutionary psychologists contend that innate mechanisms that are the same in everyone can still result in stable personality differences by maintaining "settings" based on environmental conditions that vary from person to person. For example, mechanisms can take into account information about how its bearer compares to other members of society on a trait. Tooby and Cosmides (1990) suggest that mechanisms regulating aggressive behavior may have evolved a sensitivity to individual characteristics that were correlated with the relative success of aggressiveness as a behavioral strategy in our ancestral past. They suggest that this may lead those who are "large and strong" to tend to be more aggressive than those who are "small and weak," and it may explain why young males with muscular body types are more likely to be juvenile delinquents than males with less imposing body types. Evolutionary psychologists have also theorized that some mechanisms can be permanently calibrated by conditions or events in early childhood in ways that influence individual's behaviors for their rest of their lives. I introduced two such theories in the last chapter: Belsky et al.'s (1991) proposal that paths of sexual development may be set according to the stability of care and resources received during childhood and Sulloway's (1996) theory that siblings may develop lifelong differences in personality and attitudes as the result of their competition with one another for resources. In addition to these, Tooby and Cosmides (1990: 54) propose that the mechanisms regulating aggression may be calibrated by the level of violent treatment in childhood, because harsh childhood treatment may have served as an indicator that one has been "born into a social environment where violence is an important avenue of social instrumentality." They speculate that this may lead those who were treated violently as children to exhibit more aggressive behavior as adults than do those who did not experience violence.[16]

[16] For their conjecture regarding body type and juvenile delinquency, Tooby and Cosmides draw empirical support from Glueck and Glueck (1956). For their conjecture about violent treatment in childhood and subsequent aggression, they cite various studies that have found that abused children tend to be more aggressive as adults, including McCord (1979, 1983) and Tarter, Hegedus, Winsten, and Alterman (1984).

The idea that a person's developmental experiences can permanently affect the operation of her or his innate cognitive machinery marks an interesting split between evolutionary psychology and behavior genetics. The findings of behavior genetics has influenced a number of books in recent years—with titles like *The Nurture Assumption* (Harris 1998), *The Myth of the First Three Years* (Bruer 1999), and *The Limits of Family Influence* (Rowe 1994)—that essentially argue that developmental psychology has greatly overestimated the effects of early childhood experience in shaping the adult self. The scholars cited in the last paragraph, however, accept the premise that early childhood can have profound and lasting effects, but they seek to recast these effects in terms of the calibration of mechanisms. Tooby and Cosmides (1990) also suggest that if cognitive mechanisms underlying behavior are sensitive to enduring individual characteristics (such as one's relative physical size or relative physical attractiveness), this could lead behavior genetics to overestimate the extent to which personality is directly heritable, as any estimates of the heritability of personality traits may be inflated by a confounding relationship with physical traits that are undisputedly heritable (see Harris 1998: 30-32 and Piliavin and Lepore 1994 for related discussions).

BIOLOGY AND INEVITABILITY

> *"Biology is rhetorically yoked to "determinism," a concept that threatens to clip our wings and lay waste to our utopian visions, while culture is viewed as a domain where power relations with other humans are the only obstacles to freedom."*
>
> —Barbara Ehrenreich and Janet McIntosh (1997)

In 1982, sociobiologist Melvin Konner published his book *The Tangled Wing: Biological Constraints on the Human Spirit*. The image that this title evokes, of a human spirit that wants to fly high and free but is instead inexorably bound to the Earth, wonderfully captures a belief about the relationship between biology and society that has been taken for granted by many proponents and opponents of sociobiology alike. The belief is that where biology comes into social theory is as a way of setting constraints on the possibilities of human beings and human societies, while cultural and environmental forces allow us to evince the different ways in which humanity is free to vary.[17] Biology is "in control" of some domains or some percentage of human behaviors, while culture is "in control" of the rest. Human nature is seen as a set of innate tendencies that are either impervious to environmental intervention or at least strongly resistant to it.

For the Darwinian social scientists who have promoted this view, various historical events may be taken as cautionary tales of what happens when social systems are designed which run afoul of human nature: second-generation kibbutz women demand to spend more time with their children because women's innate nurturing instincts cannot be suppressed (Tiger and Shepher 1975; van den Berghe 1979); plantation slave systems founder because the human spirit naturally resists servitude (Wilson 1978, drawing on empirical data from Patterson 1967); the Soviet Union and other communist societies fail because human's intrinsic greed makes it an unworkable economic system (Simon 1990; Wright 1994a; Ridley 1996). Meanwhile, some opponents of Darwinian social science have asserted implicitly or explicitly that biological explanations should be accepted only as an epistemological last resort because, unlike cultural or structural explanations, they are thought to circumscribe the possibility of meaningful social change. As we have seen, this is an antinomy that goes back at least to Malthus, and, as much as anything else, it sustains the

[17] For another examples of a title that suggest this same conception of biology as imposing limits, see Udry's (2000) "Biological Limits of Gender Construction."

widespread impression that biological explanations are inherently conservative while cultural explanations of social phenomena are inherently reformist or liberal.

Tooby and Cosmides (1992: 36) express weariness with this entire debate, which they call "a morality play, seemingly bound forever to the wheel of intellectual life." By insisting that any adequate account of behavior requires both a description of the underlying innate mechanism and a description of how its operation is affected by environmental conditions, evolutionary psychology suggests the futility of trying to divvy up behavior into domains controlled by biology and domains controlled by culture. On the more basic conception of biology as constraining human behavior, evolutionary psychologists ask us to consider again the computational metaphor: how useful is it to talk about software programs as constraining a computer's behavior? Certainly, in any given program, there are all sorts of conceivable outputs that cannot be produced, but these can only be rightly considered as constraints if we first recognize that it is only by executing lines of a program's code that a computer can do anything. A mediating program is required in order for user input to yield any output whatsoever. In the same way, evolutionary psychologists maintain that whatever influence culture or other aspects of social environments have on human behavior must be through an interaction with mechanisms in our brains. As Tooby and Cosmides (1992: 38-39) put it:

> The notion that inherited psychological structure constrains is the notion that without it we would be even more flexible or malleable or environmentally responsive than we are. This is not only false but absurd. Without this evolved structure, we would have no competences or contingent environmental responsiveness whatsoever. Evolved mechanisms do not prevent, constrain, or limit the system from doing things it otherwise would do in their absence. The system could not respond to 'the environment' (that is, to selected parts of the environment in an organized way) without the presence of mechanisms designed to create that connection.... [A]ny time the mind generates any behavior at all, it does so by virtue of specific

> generative programs in the head, in conjunction with the environmental inputs with which they are presented. Evolved structure does not constrain; it creates or enables.

Their point is that it is misleading to portray biology as constraining when everything that human beings do is ultimately made possible by our (evolved) biology. Yet, of course, even while evolved structure "creates or enables," it does not create or enable everything. Our evolved physiology allows us to walk and pick things up, but not to levitate or move objects using only our minds. Likewise, when considering our evolved brains and our behaviors and societies, we are left with the question of what is within human capacity and what is not.

For evolutionary psychologists, this question is the same as asking what outcomes can and cannot be obtained by any conceivable set of environmental inputs. In practice, cross-cultural universals are often cited by evolutionary psychologists as evidence of a uniform human nature and a relative inflexibility of some mechanisms to different environmental inputs (see Chapter 6). At the same time, a common position among evolutionary psychologists is to maintain that our understanding of the cognitive workings of the mind is currently much too limited for anyone to be able to make many legitimate assertions about inevitable patterns of social life (Tooby and Cosmides 1992; Buss and Kenrick 1998). That a particular pattern is universally observed is far short of implying its inevitability, as the range of social environments that have existed is much narrower than the range that will or could exist—especially as technological advances push humanity into ever more novel environmental situations.

Just as we should not equate biology and inevitability, so too do evolutionary psychologists argue that we should not equate variability across societies with the need for a cultural explanation. Many scholars believe that if a phenomenon varies across societies, this constitutes *prima facie* evidence for a cultural explanation and against an evolutionary or other biological explanation. Because societies vary so widely on so many dimensions, some have used this reasoning to infer that the role of evolutionary explanations in understanding social life must be highly circumscribed. Within evolutionary psychology's framework, stable differences across societies can be explained as the result of stable environmental differences acting on universally-shared cognitive mechanisms. For example, some evolutionary psychologists have predicted that a man's mating strategy sexual behavior will be sensitive to variation in his society's sex ratio: men will be more likely to pursue multiple sexual relationships when there is a surplus of women relative to men, and they will be more willing to commit resources to a single relationship when there is a shortage (Pedersen 1991; Buss 1994; see also Guttentag and Secord 1983). Depending on their sex ratio, then, societies or eras within a society may appear to be relatively "promiscuous" or "monogamous." In addition to sex ratio, Buss and Kenrick (1998) also suggest the prevalence of parasites, the scarcity or reliability of food resources, and the frequency of warfare and other external threats as variables that could potentially interact with evolved mechanisms to produce cross-societal differences in behavior (see also Symons 1979; Gangestad and Buss 1993; Hill and Hurtado 1996).

RACE, GENDER, AND SOCIAL CLASS

The founding theorists Emile Durkheim, Karl Marx, and Max Weber often have been referred to as the "holy trinity" of sociology. Some within the discipline have joked, however, that the emphasis of much contemporary work in sociology suggests the rise of a new holy trinity: race, gender, and social class (see Griffin 1995: 1248). Interest in these three topics follows naturally from sociology's longstanding preoccupation with social cleavages and the unequal distribution of life conditions and rewards across them. Sociologists' academic interest in the dynamics of race, class, and gender has also been usually accompanied by a strong desire to reduce levels of prejudice and inequality across all three of these dimensions. As I have suggested earlier, these sentiments may have played (and continue to play) a substantial role in many sociologists' resistance to Darwinian approaches. Critics have not only accused Wilson's sociobiology of being politically conservative, but some have leveled more specific charges of racism, classism, and sexism against the program (see Segerstråle 2000). These accusations have been fervently denied by proponents of Darwinian approaches. Part of the response has been to assert that detractors need to be reminded of the "naturalistic fallacy": Darwinian social scientists may propose that there are unpleasant realities about human nature, but this does not mean that they like these realities or believe that we should not try to moderate them however much we can (e.g., Thornhill and Palmer 2000).

A ground rule of this dissertation is that Darwinian approaches or theories will not be judged on the basis of their perceived political implications. While keeping this in mind, it is worth looking at how evolutionary psychology approaches the topics of race, class, and sex differences, both because these are central concerns to many sociologists and because evolutionary psychology is moving in a very different theoretical direction in its thinking on these topics than are some areas of sociology. Within sociology, a number of theorists have criticized the treatment of race, class, and gender as separate areas of research interest, and they have argued that a much more accurate picture of social life can be obtained by considering the three together. Part of the rationale is that race, class, and gender are thought to play such a central and complexly interacting role in both day-to-day life and in the long-term determination of life outcomes that one can only get an accurate picture of the effects of any one of these variables by simultaneously looking at its interaction with the other two. Additionally, some theorists propose that, despite their many differences, race, class, and gender share some fundamental properties and deserve treatment within a unified theoretical framework. Take for example, the following statements by Patricia Hill Collins and Sandra Harding:

> "Instead of starting with gender and then adding in other variables such as age, sexual orientation, race, social class, and religion, Black feminist thought sees these distinctive systems of oppression as being part of one overarching structure of domination." (Collins 1990: 222)

> "If feminists wish to integrate race, class, and gender issues, they must transform our largely separate theories of the origins and natures of gender, race, and class hierarchy into a single theory." (Harding 1991: 214)

While a number of sociologists advocate studying race, class, and gender together, evolutionary psychologists have come increasingly to regard differentiation across each of these three dimensions as fundamentally separate processes that do little to illuminate one another.

Turning first to race, evolutionary psychologists typically propose that the innate cognitive differences between members of different races are minimal or nonexistent. Tooby and Cosmides (1992: 25) state that the only major tenet on which evolutionary psychology and the Standard Social Science Model fully agree is a belief in the "psychic unity of humankind"—the idea that "infants everywhere are born the same and have the same developmental potential, evolved psychology, or biological endowment." Several different lines of evidence have been brought together in support of the thesis that the basic network of mechanisms comprising the mind do not vary substantially across members of different races: the existence of rapid historical change; the ability of humans from any parts of the world to interbreed without substantial ill effects; the logic of sexual recombination and the complex genetic structure that is thought to encode our underlying cognitive machinery; and studies indicating that the amount of within-population variation in DNA structure is much greater than the variation between populations (Tooby and Cosmides 1990). Not all evolutionary psychologists share this view: some have claimed that racial differences in behavior are innate and have an evolutionary basis. The most prominent work here is that of Canadian psychologist J. Phillipe Rushton (1995), whose theory claims that the same underlying evolutionary dynamic can account for purportedly intrinsic differences among Blacks, Whites, and Asians in phenomena as disparate as brain size, IQ, life span, marital stability, aggressiveness, impulsiveness, sociability, and rates of sexually transmitted diseases. Advertisements for Rushton's book call it "an incendiary thesis," and his work has been criticized by evolutionary psychologists and behavior geneticists (Bailey 1998), as well as by other social scientists (Horowitz 1995). More importantly for our purposes here, Rushton's work has not garnered much support from other evolutionary psychologists, and, merits or flaws aside, it is not representative of the work now being done in the field. None of the major overviews of evolutionary psychology gives much, if any, attention to the possibility of innate racial differences in the basic structure of cognitive mechanisms.

Evolutionary psychologists also emphasize the psychic unity of humankind with respect to possible innate differences among members of different social classes. This is not to say that evolutionary psychologists deny that people vary in their genetic endowments on variables like IQ, or that they necessarily deny that such variation is correlated with individuals' race or social class. Instead, they claim that such differences are superficial when compared to the subject matter evolutionary psychologists are after. Steven Pinker (1997: 34) says that his interest is in "how the mind works, not about why some people's minds might work a bit better in certain ways than other people's minds. The evidence suggests that humans everywhere on the planet see, talk, and think about the world in the same basic way. The difference between Einstein and a high school dropout is trivial compared to the difference between the high school dropout and the best robot in existence, or between the high school dropout and a chimpanzee." He continues with a jab that dissociates evolutionary psychology from the race-and-class-and-IQ studies by behavior geneticists: "Nothing could be farther from my subject matter than a comparison between the means of overlapping bell curves for some crude consumer index like IQ" (Pinker 1997: 34).

While evolutionary psychologists propose that members of upper- and lower- classes have the same underlying mechanisms, predictable class differences in behavior are thought to emerge from the interaction of these shared cognitive mechanisms with the different environmental cues associated with different positions in the class hierarchy. Mechanisms are thought to be able to evolve a sensitivity to its bearers relative wealth if the most adaptive course of action in a particular situation was for some reason dependent on social standing in our ancestral past. A longstanding example of this sort of reasoning is the Trivers-Willard hypothesis about parental investment, which I examine in Chapter 7. Trivers and Willard (1973) use Darwinian logic to predict that the amount parents invest in children of each sex will vary according to their socioeconomic status: they predict that parents of below-average socioeconomic status should invest more in their daughters than their sons, while parents of above-average status should invest more in their sons than their daughters. Trivers and Willard do not argue that there are genetic differences between rich parents and poor parents, but rather they contend that the same underlying mechanism has evolved (for adaptive reasons) to bias parents' investments differently depending on their relative wealth. Similarly, evolutionary psychologists have suggested that the sensitivity of mechanisms to relative wealth may explain differences between rich and poor in fertility rates, willingness to delay gratification, and criminal behavior (Geronimus 1987, 1991; Rogers 1994; Buss 1995; Pinker 1997). Insofar as race and class are correlated (for whatever reason), status-contingent mechanisms are thought to potentially also explain aggregate race differences in behavior.

Although most evolutionary psychologists do not believe that there are any important differences in the innate psychologies of members of different races or classes, they do believe that intrinsic differences exist in the minds of men and women. Describing possibly innate sex differences and providing evolutionary explanations for them has been a central preoccupation of evolutionary psychology. In David Buss's undergraduate textbook *Evolutionary Psychology*, there are 297 pages in the chapters that explicate evolutionary psychological explanations on particular substantive topics (Chapters 3-12). By my count, some discussion of or reference to a putatively innate difference between men and women is made on 202 of these pages (69.6%). Stephen Jay Gould (1997: 50) writes that "the most-publicized work in evolutionary psychology has centered on the universality in all human societies of a particular kind of *difference*: the putative evolutionary reasons for supposedly universal behavioral differences between males and females." Consequently, one could argue that evolutionary psychologists are overstepping their bounds when they claim to endorse the "psychic unity of humankind"; a more accurate characterization would be that they endorse the "psychic unity of mankind" and the "psychic unity of womankind" but not the unity of the two.

Evolutionary psychologists typically presume that observed sex differences in behavior emanate from innate psychological differences, and, in turn, these innate psychological differences are thought to result from men and women having faced inherently different adaptational problems and conditions over the course of evolutionary history. Why, for example, do mothers in every known human society invest more in parental care than do fathers? Evolutionary psychologists propose that this difference is due to underlying mechanisms that (perhaps by mediating underlying emotions such as “love” or “attachment”) dispose women to provide greater parental care than men (Trivers 1972; Alcock 1993; Buss 1999; see discussion of Biblarz and Raftery [1999] in Chapter 7). Two factors are thought to have driven the evolution of this difference. First, over evolutionary history, men have recurrently confronted the problem of paternity uncertainty. The value of each unit of parental investment provided by men historically has been diminished by the risk that the investment is being provided for a child who does not bear his genes. Second, parental investment is thought to have imposed higher opportunity costs for men than for women. Women’s reproductive capacity is limited by their own physiology, while men’s reproductive capacity is primarily limited by the number of females to whom they can successfully gain sexual access. A consequence of this is that the evolutionary returns of parental investment for men has historically been offset partially by the cost of not using the time or resources in the pursuit of additional chances to mate.

Human males may not tend to invest as much in children than do human females, but they do tend to provide more investment than do males of most other animal species (Buss 1999). The relatively high level of parental investment by human males is thought to be the result of selection processes in which women have disproportionately preferred as mating partners men who were able to amass resources and willing to expend these resources on offspring. This tendency is thought to influence contemporary women's mating preferences: studies find that women are more attracted to men who are socially dominant, men who are wealthy, and men who display a fondness for children (Buss and Schmitt 1993; La Cerra 1994). Men, meanwhile, have evolutionary incentives to be less concerned about the status of (especially short-term) female partners and to be more concerned about their potential fertility. Studies indicate that men place more emphasis than women on the physical attractiveness of partners, and evolutionary psychologists argue that the physical features that men find more attractive in women—including youth, body shape, various facial features, and clear skin—are all correlates of health and high reproductive potential (Singh 1993; Buss 1994; Etcoff 1999). (In *long-term* mating contexts, the potential costs of being cuckolded are thought to have led to the evolution of a tendency for men to favor also indications of spousal faithfulness [Buss 1999: 148-152]).

The difference between male and female reproductive capacity also implies that the variance in reproductive success is greater among males than among females. This is also suggested by the permissibility of polygyny (but not polyandry) in most of the world's societies: every man with multiple wives entails at least one other man with none. Higher reproductive variance, in turn, is thought to imply a more intense competition for mates. Evolutionary psychologists have proposed that this may be a prominent reason why men tend to be more physically aggressive than women. They have also theorized that men may have evolved a stronger innate drive for status than women. Geoffrey Miller (1998, 2000) claims that differences in the drive for status may explain why men have tended to dominate various domains of the artistic world, citing Bach, Balzac, and Jimi Hendrix as examples of men who converted cultural displays into social status and social status into numerous mating opportunities (see also Diamond 1992: 172-9). Robert Wright (1994b) proposes that men's higher drive for status may also explain their tendency to overwhelmingly occupy the positions of economic and political power in a society (see also Browne 1999; Buss 1999: 349-356). He contends that this may have important implications for how we think about sex discrimination and gender-based affirmative action: concerning the latter, he argues that such programs are "sometimes (not always) justified on [these] grounds: that, in the absence of discrimination, men and women would be equally represented at the higher levels of corporate and government life. But if men on average work harder at self-advancement, this rationale won't work" (Wright 1994b).[18]

Given theories such as this, it is perhaps not surprising the considerable antagonism remains between evolutionary psychologists and many feminists, including feminists who are otherwise quite sympathetic to evolutionary explanations of human behavior. Barbara Ehrenreich (1999: 57) writes:

> The sociobiologists of the '60s and '70s, followed by the evolutionary psychologists of the '90s, promoted what amounts to prostitution theory of human evolution: Since males have always been free to roam around, following their bliss, the big challenge for the prehistoric female was to land a male hunter and keep him around in a kind of meat-for-sex arrangement. Museum dioramas of the Paleolithic past still tend to feature the guys heading out after the mastodons, spears in hand, while the gals crouch slack-jawed around the campfire, busily lactating. The chivalrous conclusion is that today's woman can do whatever she likes—start a company, pilot a plane—but only by trampling on her inner female.

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[18] Wright's argument here can be contrasted with this statement by Steven Pinker (1997: 492): "The Darwinian approach to sex is often attacked as being antifeminist, but that is just wrong. Indeed, the accusation is baffling on the face of it, especially to the many feminist women who have developed and tested the theory. The core of feminism is surely the goal of ending sexual discrimination and exploitation, an ethical and political position that is in no danger of being refuted by any foreseeable scientific theory or discovery. Even the spirit of the research poses no threat to feminist ideals. The sex differences that have been documented are in the psychology of reproduction, not in economic or political worth, and they are invidious with regard to men, not women."

More recently, other (mostly female) evolutionary scholars have risen to challenge the orthodox view of Darwinian social science on both women's role in evolutionary history and its implications for innate differences between the sexes (Angier 1999; Fisher 1999; Hales 1999). Ehrenreich (1999) dubs this the "femaleist" movement. Putting things in the simplest of terms, these scholars believe that women contributed more to the welfare of early human groups than previously credited by evolutionary theorists, and they also believe that women are inherently lustier, more competitive, and more (non-physically) aggressive than Darwinian social science has previously acknowledged. Helen Fisher (1999) also proposes that the cultural and technological changes afoot in advanced societies favors the innate talents of women over those of men. Fisher's work has problems of its own, however, as we will see in Chapter 6.

CONCLUSION

This chapter was intended to provide the (possibly unacquainted) reader with background information on evolutionary theory and evolutionary psychology. The next five chapters will provide case studies of specific works of evolutionary social science. Before embarking on this, however, let us take stock of the considerable ground we have covered here. We began with some of the basics of evolutionary theory. First, we looked at the process of natural selection itself, with its key ingredients of variation, inheritance, and differential reproduction. Then, we moved to a discussion of what genes are, how Darwin's theory of evolution can be re-expressed in terms of the language of genes, and why genes are considered to be the fundamental ("selfish") units of natural selection. Two of the most important points to remember from this discussion are that genes are what endure over the course of evolutionary time and that all of the "design improvements" that natural selection makes in an organism—every aspect of physiology and every behavioral predisposition—are carried through the generations as information encoded in DNA. We continued on by considering evolutionary adaptations, which included a brief mention of some of the difficulties of identifying adaptations and specifying the original function that led them to be incrementally favored by natural selection.

In the second half of this chapter, we turned our attention from the basics of evolutionary theory to a more specific consideration of evolutionary psychology's perspective on the human mind. We saw that evolutionary psychology conceives the mind as a series of specialized mechanisms that each developed as adaptations to a specific evolutionary pressures. The operation of these mechanisms is thought to be contingent upon environmental "input", and thus can consist either of immediate situational factors or of calibrating events or circumstances that occur in early childhood. The sensitivity of mechanisms to environmental conditions is one reason why we should resist the tendency to associate the innateness or universality of a behavioral pattern with its inevitability. Likewise, variation across cultures cannot be automatically attributed to culture, as it could be the result of stable environmental differences between two societies. Consequently, evolutionary psychology gives less importance to the role of culture in determining social behavior. We concluded our survey by considering evolutionary psychology's position on the innateness of differences between members of different races, classes, and sexes. As we have seen, evolutionary psychology's emphasis on the "psychic unity of humankind" leads most of its adherents to posit that race and class differences are largely or entirely due to the interaction of different environmental conditions with common psychological mechanisms. In contrast, men and women are thought to have fundamentally different psychologies that underlie the observable sex differences in everything from mating behavior to socioeconomic attainment. This emphasis on the innateness of sex differences has led to considerable antagonism between evolutionary psychologists and feminists, some of which we will see in Chapter 4.

3

DOES THE EMPEROR HAVE NO CHEATER-DETECTOR?

> *"If, like a key in a lock, the properties of the hypothesized adaptation are sufficiently better than random at solving the adaptive problem (in a way that can be computed in some fashion, given a consensually agreed on statistical criterion) then one is justified in concluding it is an adaptation."*
>
> —John Tooby, essay, 1999

Evolutionary psychology is regularly presented as a "new science" of human behavior, a Great Leap Forward from the sociobiological inquiries that preceded it. Many of the intellectual foundations for this new enterprise were provided by Leda Cosmides and John Tooby, who together coined the term "evolutionary psychology." Their most important essay in this regard, "Evolutionary Psychology and the Generation of Culture," was originally published in two parts.[1] The first part laid out the principles of their fledgling program, including its conception of the mind as composed of many, highly specialized cognitive adaptations (Tooby and Cosmides 1989). These "Darwinian algorithms" are dedicated to specific content domains, and they were shaped in response to particular adaptive problems. The second part put these principles into practice with an evolutionary "task analysis" of social exchange (Cosmides and Tooby 1989).

The task analysis sought to specify some of the prerequisite abilities that humans must have developed in order to for us to have our observed capacity to engage in beneficial social exchange. They draw on Robert Trivers's (1971) theory of reciprocal altruism and his discussion of how the evolution of reciprocal altruism required the need to recognize failures to reciprocate, which can be thought of as cheating, in the sense of accepting a benefit without returning the favor. Based on their analysis, Cosmides and Tooby (1989) predict

[1] A reprise of this essay (with considerable revision) is provided in Tooby and Cosmides (1992).

that the mind contains "specialized procedures that are efficient at detecting potential cheaters." Importantly, consistent with the precepts of their program, the prediction is not just that humans had to be able to detect cheaters, but that this detection is accomplished through cognitive mechanisms that are *specialized* and *efficient* for this task.

In support of their idea that such a "'look for cheaters' procedure" exists, the paper presents findings of a series of experiments that Cosmides had conducted as her dissertation research (1985). She published these in a paper that received a prestigious award from the American Association for the Advancement of Science (1989).[2] As I will describe shortly, these experiments are based on a reasoning problem called the Wason Selection Task. The centrality of this instrument to this work is hard to overstate: At the 2000 Human Behavior and Evolution Society (HBES) meetings, Cosmides and Tooby gave a talk on the "convergent evidence" for the existence of the "'look for cheaters' procedure" as a specialized adaptation; this convergent evidence was virtually all based on different sorts of implementations of the Wason Selection Task.

Cosmides and Tooby (1989, 1992, 1995, 2000) assert that these experiments have provided evidence that our ability to detect cheaters reflects the "special design" indicative of adaptation. The cheater-detector experiments are arguably the most important line of empirical research in evolutionary psychology *qua* evolutionary psychology—that is, in evolutionary psychology as a new program for understanding evolution and human behavior. Part of this importance stems from its connection to one of the keystone statements of the principles of the field (not to mention restatements—see, e.g., Cosmides and Tooby 1997). The paper self-consciously provides an "exemplar" of theoretical deduction and hypothesis

[2] The paper was awarded the Behavioral Science Research Prize "because of its substantial and surprising increase in understanding of the rules of thought." (Science, 2/3/89, p. 672).

testing for other evolutionary psychologists to follow. In addition, Cosmides and Tooby were strategic in applying their programmatic ideas to the topic of reasoning, which they regarded as a "citadel" of the "domain-general" view of mind that evolutionary psychology opposes. By trying to show how selectionist thinking was essential to understanding how humans reasoned, they sought to convince psychologists of the basic value of evolutionary analyses for their discipline, as a tool for discovering and explicating the "domain-specific" procedures comprising the mind. Demonstrating the importance of an evolutionary perspective for research also opened up the domain of potential inquiry well beyond the studies of sex, sex differences, and kinship with which Darwinian perspectives are often associated.

Also, in emphasizing the importance of innate, specialized, and evolved mechanisms for reasoning, the research attacks the assumptions of "The Standard Social Science Model" that they saw as dominating the fields of anthropology and sociology. They write, "If even human reasoning, the doctrinal 'citadel' of the advocates of content-free, general-purpose processes, turns out to include a large number of content-dependent cognitive adaptations, then the presumption that psychological mechanisms are characteristically domain-general and originally content-free can no longer be accorded privileged status. Such results would jeopardize the assumption that whenever content-dependent psychological phenomena are found, they necessarily imply the prior action of cultural or environmental shaping" (Cosmides and Tooby 1992: 165).

Cosmides and Tooby's "social contract theory" and the cheater-detection research that supports it are enthusiastically discussed in virtually every book-length treatment of evolutionary psychology, some of which I list in the next section, as well as in many books and articles by evolutionary psychologists on topics that are far removed from the study of reasoning. Indeed, the existence of the cheater-detection mechanism seems to be taken by some discussants of evolutionary psychology as established fact:

- RANDY THORNHILL AND CRAIG PALMER: "Leda Cosmides and John Tooby have found that the human brain contains a mechanism designed specifically to detect cheating in social exchanges" (2000: 13).

- MATT RIDLEY: "We do not know for sure where the social-exchange organ is, or how it works, but we can tell it is there as surely as we can tell anything else about our brains" (1996: 131).

- WILLIAM ALLMAN: "Evidence that evolution has crafted our minds to be specialized in certain tasks comes from Cosmides' remarkable discovery of a mental mechanism that is designed to detect 'cheaters'." (1994: 37)

At the same HBES meetings I just mentioned, two prominent evolutionary psychologists separately cautioned their audiences that the program needed to be more judicious in drawing the conclusion of adaptation, but both were quick to identify Cosmides and Tooby's studies of cheater-detection as an exemplar of careful inference about adaptation.[3] Even some who are critical of Cosmides and Tooby's larger program of evolutionary psychology regard the cheater-detector studies as exemplary empirical work (Shapiro and Epstein 1998; de Jong and van der Steen 1998).

Contrary to these convictions, however, my own consideration of the literature has caused me to be more skeptical about whether we should believe that a specialized "'look for cheaters' procedure" exists as an evolved feature of human minds. This chapter outlines some causes of this skepticism. First, I contend that there is much less evidence now that reasoning about cheating represents a "special ability" than it may have seemed in the initial presentations of the theory (Cosmides 1985, 1989; Cosmides and Tooby 1992). Second, I

[3] The evolutionary psychologists are Douglas Kenrick and Steven Gangestad. The other exemplar was the studies of jealousy associated primarily with David Buss.

argue that the experiments that have been marshaled in support of the cheater-detector fail to provide persuasive indications of the "special design" that evolutionary psychologists hold as the standard for adaptation. Third, although the cheater-detector mechanism is predicated on its operation only in a specific domain, I identify problems with Cosmides and Tooby's efforts to define this domain and to reconcile this definition with their evolutionary analyses. Discussion of each of these reservations is presented below. Some of the criticisms I raise in this chapter are amplifications of criticisms that cognitive psychologists, philosophers, and others have already raised (e.g., Cheng and Holyoak 1989; Pollard 1990; Manktelow and Over 1990; Sperber et al. 1995; Liberman and Klar 1996; Lloyd 1999), although certainly my own statements and refinements are added to these. Also, no previous treatment has considered all of these critical sources together, with Cosmides and Tooby's various articles, to consider how persuasive is the evidence that a "look for cheaters" procedure exists. Indeed, as we will see, virtually none of these criticisms is ever considered in the enthusiastic portrayals of the "cheater-detector" as an exemplar of the emerging "paradigm" of evolutionary psychology.[4]

In examining one of evolutionary psychology's exemplars, our discussion of the cheater-detector research calls attention to what might prove to be one of the central flaws of evolutionary psychology—that its concept of mind implies a larger and more highly specialized set of genetically-encoded mechanisms in the mind than what actually exists (Shapiro and Epstein 1998; Samuels 1998; Buller 2000). We will see that the logic that Cosmides and Tooby set forth in their original papers has implied that new findings about the Wason Selection Task be handled by proposing further specialized cognitive adaptations,

[4] Buss, particularly, is associated with statements announcing evolutionary psychology as a "new paradigm for psychological science" (see title of Buss 1995a).

with seemingly little constraint on their number. Some critics have questioned whether there is enough information on the genome to code for the proliferating specialized procedures that are proposed to be developed by the unfolding of a genetic program (Deacon 1997; Buller 2000; Erlich 2000). Others doubt evolutionary psychology's "massively modular" concept of mind is compatible with what is known about developmental neurobiology (Buller and Hardcastle 2000). In addition to these concerns, however, I believe that one can question how convincing are the program's experiments for demonstrating that the observed patterns imply the operation of an innate, specialized set of procedures. Beyond this, even should such procedures exist, one may question whether the evolutionary explanation offered by Cosmides and Tooby is the correct evolutionary historical account of why.

Over the course of this chapter, I also recount the results of an exploratory experimental study that I have conducted. The experiment compares subject performance on different versions on the Wason Selection Task. I conducted these exploratory experiments to see for myself how the Wason Selection Task worked when administered to students. The subjects in the experiments were all students drawn from introductory sociology courses at a large Midwestern university. Twelve different versions of the task were used, and each student was randomly given one version of the task (i.e., the experiment employed a between-subjects design). Because the ABSTRACT and DRINKING AGE problems (see below) were likely to provide the most useful and conventional standards of comparison, these tasks were given to twice as many subjects as the others, to increase statistical power of tests against these two conditions. The text of all of the Wason Selection Tasks that I administered to students is provided in an appendix, along with a summary of subject performance on each of the conditions.

This exploratory study plays only a modest role in explicating the points that I make in my criticism below. The studies have, however, provided a useful background to my understanding of subject performance on the Wason Selection Task. Additionally, conducting these experiments alongside a deeper reading on the literature on the selection task has suggested further, more focused experiments that could be brought to bear on particular points of contention regarding Cosmides and Tooby's social contract theory. I describe several of these in the text or in footnotes, as potentially lucrative directions for future research.

THE WASON SELECTION TASK: A CHEATER -DETECTOR DETECTOR?

The Wason Selection Task was originally designed to test if people could apply the logical principles implied by scientific hypothesis testing—specifically, if people could reason about what is required for a hypothesis to be falsified (Wason 1966). In discussions of the cheater-detector research, the Wason Selection Task is commonly introduced through the example of the ABSTRACT problem presented below:

Abstract Problem

Part of your new clerical job at the local high school is to make sure that student documents have been processed correctly. Your job is to make sure the documents conform to the following alphanumeric rule:

"If a person has a 'D' rating, then her or his documents must be marked code '3'."

You suspect the secretary you replaced did not categorize the students' documents correctly. The cards below have information about the documents of four people who are enrolled at this high school. Each card represents one person. On side of a card tells a person's letter rating and the other side of the card tells that person's number code.

Indicate only those card(s) you definitely need to turn over to see if the documents of any of these people violate this rule:

D	F	3	7

According to the rules of the propositional calculus, the correct answer to this problem is 'D' and '7.' The 'D' card must be turned over because the proposition would be falsified if a '3' was not on the back, while the '7' card must be turned over because the proposition would be falsified if there was 'D' on the other side. Meanwhile, the cards with 'F' and '3' on the visible sides cannot falsify the proposition no matter what is on the other side.

Formally, we can express the above proposition as "if P then Q" ($P \rightarrow Q$), and the cards above correspond to P, *not*-P, Q, and *not*-Q, respectively. In formal logic, the conditional rule $P \rightarrow Q$ is false only in the case of P & *not*-Q, and successful performance on the WST requires the subject recognizing that the P and *not*-Q cards (i.e., 'D' and '7') are the only cards that need to be turned over to see if the rule is false.

Performance on the ABSTRACT version of the selection task is typically poor (Cosmides and Tooby 1992). In my experiment, 8.5% (12 of 142) of respondents gave the correct answer, a percentage which is consistent with other research. (This is barely better than what we would expect if subjects just randomly decided whether or not to circle each of the four cards [$(.5)^4$=6.25%]) Failing to recognize that the *not-Q* card needs to be turned over is the main reason for poor performance on many versions of the task, and subjects also often believe that the *Q* card must be turned over. Additionally, some subjects seem erroneously to read the rule as a biconditional—importantly, in formal logic, the rule above does not imply that all cards with a '3' on the back have a 'D' on the front.

In the 1970's and early 1980's, a variety of studies indicated that performance on the WST sometimes improved dramatically when the problem was framed in certain contexts. The ABSTRACT problem is often contrasted with the DRINKING AGE problem (Griggs and Cox 1983):

Drinking Age Problem

In its crackdown against drunk drivers, state law enforcement officials are revoking liquor licenses left and right. You are a bouncer in a local bar, and you'll lose your job unless you enforce the following law:

"If a person is drinking beer, then he or she must be over 21 years old."

The cards below have information about four people sitting at a table in your bar. Each card represents one person. One side of a card tells what a person is drinking and the other side of the cards tells that person's age.

Indicate only those card(s) you definitely need to turn over to see if any of these people are breaking this law:

drinking beer	drinking coke	25 years old	16 years old

The DRINKING AGE problem uses a rule with an "if *P* then *Q*" structure, just like the ABSTRACT problem, and so ostensibly both are variations of the same reasoning task. Yet the reader very likely finds it much easier to see the correct answer to the DRINKING AGE problem. To determine whether the drinking age law is being violated, one needs to find out if the person "drinking beer" is under 21 and whether the "16-year old" is drinking beer. Neither the person who is "25 years old" nor the person who is "drinking coke" can possibly be breaking the law. Among the pitfalls avoided are that there is no confusion that the rule might somehow imply that in order to be 21 years old one must be drinking beer. To many, the correct answer to the problem seems to "pop out", without any conscious deliberation at all (Cosmides and Tooby 2000). In my experiment, 64.5% (91 of 141) answered a version of the DRINKING AGE problem correctly, and other studies have observed around 75% correct answers for the DRINKING AGE problem.[5]

There are various differences between the ABSTRACT and DRINKING AGE problem that one might think could be responsible for this dramatic "content effect" on performance. Early experiments demonstrated that merely putting the task in a concrete context does *not* make subjects any more likely to answer it correctly (Evans 1972). Familiarity with the rule may make the problem a little easier, but by no means can it account for the massive improvement in subject performance (Cosmides 1989). For example, a TRANSPORTATION problem with the rule "If I go to Boston, then I take the subway" (revised to use places and means of transportation local to place where the experiment is done) has only sporadically produced content effects (Wason and Shapiro 1971).

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[5] This observed result in my study is significantly lower than .75 ($p < .01$, one-tailed). This reduction could be due to differences in the abilities of different subjects (notably, many of Cosmides's experiments used Harvard and Stanford undergraduates). Alternatively, my problem was different than the one in the example in that subjects were instructed to identify potential instances of "violating the rule" rather than "breaking the law." I did this to make the problem more directly comparable to the ABSTRACT problem. If this is a source of decreased performance on the task (which further experiments could investigate), it would provide additional demonstration on the sensitivity of performance on the WST to small changes in the wording of the problem.

As Table 4.1 indicates, the comparison of the ABSTRACT and DRINKING AGE problems is a common trope of many secondary discussions of Cosmides and Tooby's theory. Cosmides and Tooby propose that the primary reason that subjects answer the DRINKING AGE problem right and the ABSTRACT problem wrong is that performance on the DRINKING AGE problem is facilitated by the activation of an innate "look for cheaters" procedure that enhances reasoning. The "look for cheaters" procedure is activated whenever anyone is faced with the task of detecting cheaters on a some kind of social contract, where a social contract "expresses an exchange in which an individual is required to pay a cost (or meet a requirement) to an individual (or group) in order to be eligible to receive a benefit from that individual (or group)" (Cosmides 1989: 197). In reviewing the literature on the WST, Cosmides (1985, 1989) found that the only rules that had produced "robust and replicable" effects were those that had involved detecting violations of a social contract. On the WST, subjects appear to reason much better on social contract problems than problems in which the rule was purely descriptive, which Cosmides and Tooby interpret as indicative of the "special design" of an adaptation for detecting cheating on social contracts. They write:

> To show that people who ordinarily cannot detect violations of conditional rules can do so when that violation represents cheating on a social contract would constitute evidence that people have reasoning procedures that are specially designed for detecting cheaters in situations of social exchange (Cosmides and Tooby 1992: 181).

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Table 4.1 Details on some secondary presentations of social contract theory.

	Approx. length (words)	Presents abstract vs. drinking age problem?	Indicates that only social contracts yield content effects?	Mentions other evidence than SC vs. descriptive rules?	Any indication of dispute or contradictory findings?
Allman, *The Stone Age Present*, 1994	975	No	May yield impression	No	No
Boyd and Silk, *How Humans Evolved*, 1997	1150	Yes	No	Yes	Yes
Brown, *The Darwin Wars*, 1999	525	No	Yes	No	No
Buss, *Evolutionary Psychology* 1999	1100	Yes	May yield impression	Yes	No
Cronin, *The Ant and the Peacock*, 1991	2500	Yes	"almost always to do with social exchanges"	Yes	Yes
Dennett, *Darwin's Dangerous Idea*, 1995	875	Yes	Yes	Yes	No
Dugatkin, *Cheating Monkeys and Citizen Bees*, 1999	550	Yes	"virtually all cases"	No	No
Dunbar, *Grooming, Gossip, and the Evolution of Language*, 1996	450	Yes	No	No	No
Evans and Zarate, *Introducing Evolutionary Psychology*, 1999	500	Yes	Yes	No	No
Petrinovich, *Human Evolution, Reproduction, and Morality*, 1995	375	No	May yield impression	Yes	No
Pinker, *How the Mind Works*, 1997	950	Yes	No	Yes	Two cites in endnotes
Ridley, *Origins of Virtue*, 1993	1750	No	Yes	Yes	No
Ridley, *The Red Queen*, 1996	575	Yes	Yes	Yes	No

As already described, Cosmides and Tooby develop a lengthy "task analysis" that predicts the existence of an improved capacity to reason about social contracts from a Darwinian perspective, but the basic argument regarding the cheater-detector is not hard to grasp. Human beings have the ability to engage in social exchange. In order for the capacity for social exchange to evolve, it must have conferred adaptive benefits to those who engaged in it. Social exchange is only beneficial when the benefits received outweigh the costs paid, and social exchange is only mutually beneficial when the benefits for both parties outweigh the perceived costs for each. Social exchange is not beneficial for those who get cheated by giving something to the other party without receiving a benefit in return. Those early humans who could not grasp this logic of cheating would be at a severe and persistent selective disadvantage to those humans that could. As a result, Cosmides and Tooby assert that, among the numerous "Darwinian algorithms" responsible for our ability to engage in social exchange, there "must include procedures that allow us to quickly and effectively infer whether someone has cheated, or intends to cheat, on a social contract." (1989: 84).

Of course, the comparison of the abstract problem with social contract problems is not the only empirical evidence that Cosmides and Tooby have provided to support the idea that our brains contain a specialized cheater-detector mechanism. Cosmides and Tooby, as well as others (most notably Gigerenzer and Hug [1992]) have conducted various additional experiments using the WST that are interpreted as lending support to their theory. In what follows, Steven Pinker, a prominent evolutionary psychologist, summarizes the case for what other empirical evidence is persuasive about the theory:[6]

[6] I would perhaps rather have used a summary by Cosmides and Tooby here, but they have not provided one as succinct and eloquent (very few academics are as gifted with prose as Pinker is). Pinker wrote *How the Mind Works*, the book from which the passage is taken, while spending a year at Santa Barbara with Cosmides and Tooby.

> Cosmides showed that people do the logical thing whenever they construe the P's and Q's as benefits and costs, even when the events are exotic, like eating duiker meat and finding ostrich eggshells. It's not that a logic module is being switched on, but that people are using a different set of rules. These rules, appropriate to detecting cheaters, sometimes coincide with logical rules and sometimes don't. When the cost and benefit terms are flipped, as in "If a person pays $20, he receives a watch," people still choose the cheater card (he receives the watch, doesn't pay the $20)—a choice that is neither logically correct nor the typical error made with meaningless cards. In fact, the very same story can draw out logical or nonlogical choices depending on the reader's interpretation of who, if anyone, is a cheater. "If an employee gets a pension, he has worked for ten years. Who is violating the rule?" If people take the employee's point of view, they seek the twelve-year workers without pensions; if they take the employer's point of view, they seek the eight-year workers who hold them. The basic findings have been replicated among the Shiwiar, a foraging people of Ecuador. (Pinker 1997: 337).

To paraphrase briefly what Pinker cites: (1) people reason correctly about social contract rules even when they are about things with which they are completely unfamiliar; (2) when the costs and benefits are switched around and the answer that corresponds to cheater-detection no longer corresponds with the answer of formal logic, people answer in the way that identifies potential cheaters; (3) when the subject is cued to a perspective, the subject answers according to what constitutes being cheated from that perspective; (4) these studies have been replicated among the Shiwiar (as well as other subjects in other parts of the world). Also, Cosmides (1989) presents experiments that indicate that improved performance does not extend to rules of permission that are not social contracts, and Cosmides and Tooby (1992) report that detecting altruists on the WST does not yield comparable effects to detecting cheaters. As a clarification of a matter that I will consider in more detail below, the activation of the "look for cheater" procedure is not confined only to social exchanges, but also to instances where one is entitled to a benefit only when some requirement is met (this is why the theory is called "social contract theory" rather than "social exchange theory", a point I return to later).

Pinker's account otherwise aptly summarizes the main evidence that is cited by discussants of evolutionary psychology in advancing the argument that the cheater-detector mechanism exists. We will consider other lines of evidence and what they contribute toward inferring specialized adaptation, but first I consider the comparison between social contract problems and other types of problems, as this has been the evidence that others have found most convincing (judging by the secondary presentations I have reviewed, see Figure 4.1).

MASTERS OF OUR EVOLUTIONARILY SIGNIFICANT DOMAINS

Social contract theory is said to predict that "people should show a special ability to detect cheaters in social exchange" (Fiddick, Cosmides, and Tooby 2000: 14). A first question one can ask: "special" relative to what? As we have seen, the usual comparison has been between WSTs that involve social contract and those that use purely descriptive rules, such as the frequently-mentioned comparison of the ABSTRACT and DRINKING AGE problems. Several of the experiments conducted by Cosmides (1992) and by Gigerenzer and Hug (1992) compare subject performance on WSTs that use exactly the same rule but place the rule in either a social contract context or a purely descriptive one. These studies find consistently that subjects do better on the social contract versions of the problems, which is taken as evidence strongly supporting social contract theory.

Does a special ability to solve social contract problems mean that we should expect that only social contract problems will yield strong content effects? As noted, Cosmides (1989) observes "robust and replicable content effects are found only for rules that relate terms that are recognizable as benefits and costs in the format of a standard social contract." Some proponents of evolutionary psychology have read this and found very compelling the idea that the WST is easy for subjects to solve only when it is framed in terms of a social contract. Daniel Dennett writes, "Cosmides and Tooby [1992] came up with an evolutionary hypothesis, and it is hard to imagine this particular idea occurring to anyone who wasn't acutely aware of the possibilities of Darwinian thinking: the easy cases are all cases that are readily interpreted as tasks of patrolling a social contract." Matt Ridley writes, "If the law to be enforced is not a social contract, the problem is difficult—however, simple its logic; but if it is a social contract, like the beer-drinking example, then it is easy" (1993: 335). Given such statements, would confidence in the existence of a "special ability" to detect cheaters be dampened by the finding of strong "content effects" on problems that have nothing to do with social contracts?

Critics of the theory have thought so (Cheng and Holyoak 1989; Manktelow and Over 1990: 158; Pollard 1991). Even in the pre-1989 literature on the WST, there is reason to question whether the "robust and replicable" content effects were really confined to social contract problems (although "robust" and "replicable" are words that can vary in their inclusivity or exclusivity, which makes the appropriateness of this determination harder to judge).[7] In any event, subsequent studies have provided ample evidence of faciliation effects in non-social contract contexts. A prominent example is rules regarding *precautions* (Cheng and Holyoak 1989). Manktelow and Over (1990) proposed a WST with the rule "If you clean up spilled blood, you must wear rubber gloves":

[7] See especially, however, the SEARS problem discussed in Manktelow and Over (1990), and Cosmides's (to my mind, unsatisfactory) attempt to explain it as a social contract in her dissertation (1985).

Spilled Blood Problem (see Manktelow and Over 1990)

You work as a government inspector of hospitals and other health care facilities. Because of the dangers of diseases that can be transmitted through blood, all hospital employees in the United States are supposed to adhere strictly to the following rule:

"If you clean up spilled blood, you must wear rubber gloves."

You are inspecting a hospital and need to know whether employees follow this rule. The following cards represent four incidents where a hospital employee was called upon to clean up a spilled substance. One side of the card tells whether blood was among the spilled material, and the other side tells whether the employee wore gloves when cleaning up the spill.

For each incident, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the rule had been violated in that instance:

blood was spilled	blood was not spilled	employee wore gloves	employee did not wear gloves

Consistent with others' results, in my study, subjects correctly answered this problem 82.4% of the time (61 of 74), which was significantly better than their performance on the DRINKING AGE problem (61/74 versus 91/141, χ^2=7.50, *df*=1, *p*=.006). In other words, people seem to be able to do at least as well on precaution problems as on social contract problems, even though this does not involve rationing a benefit.

While critics have seen such results as evidence against social contract theory (Cheng and Holyoak 1989; Manktelow and Over 1990; Pollard 1990; Lloyd 1999), Tooby and Cosmides have not. Indeed, not only do they deny that facilitation effects on non-social contract problems constitute evidence against social contract theory, but they say that they had anticipated that such effects would be found before others reported them:

> "Although social contract theory provided a parsimonious explanation for a series of newly predicted content effects, and indeed for all of the Wason content effects that had been detected at the time of the theory's introduction in 1983, *we nevertheless fully expected that other content effects would be*

subsequently discovered or intentionally generated for other evolutionarily significant domains... We ourselves, together with our colleagues, have been working on additional evolutionarily significant domains, such as hazards and threats, and on adaptationist grounds expect there to be many others." (Fiddick, Cosmides, and Tooby 2000: 19, emphases added)[8]

Three clarifications should be made regarding this passage. First, although "hazard management theory" has been offered as an evolutionary explanation for facilitation effects on precaution rules, it should be clear that this theory entered the literature only after non-evolutionary psychologists had demonstrated that content effects for precaution rules existed. Cosmides and Tooby (1992) acknowledge this more clearly than does the Fiddick, Cosmides, and Tooby (2000) paper. Second, to my reading, Cosmides and Tooby's early formulations (1989 and before) provide little, if any, explicit indication of any expectation that effects in other "evolutionarily significant" domains would be found. In her dissertation, Cosmides does look at a problem that uses a threat as a conditional rule, but it is not clear how this problem was viewed in the context of evolutionarily significant domains. Third, one might at least sometimes read these early formulations as suggesting the opposite. For example, Cosmides writes "Non-social contract rules, either descriptive or prescriptive, should not show this particular pattern of variation [i.e., evidence of a specialized procedure[9]], regardless of their familiarity. In general, *they can be expected to elicit the same low levels of P & not-Q and very low levels of not-P & Q typically found in the literature for non-social contract problems*" (1989: 199, emphases added).

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[8] Also: "Obviously, the discovery of some content effects outside of the domain of social exchange does not falsify social contract theory, since such discoveries are what those who hold to an adaptationist approach to reasoning expected in the first place." (Fiddick, Cosmides, and Tooby 2000: 19)

[9] More completely, the reference to "this pattern of variation" is a reference to earlier contention that "by comparing performance on standard and switched contract rules, one can tell if reasoning is governed by a logical procedure or a 'look for cheaters' procedure.

So critics may dispute whether Cosmides and Tooby really expected such effects all along, at least as indicated by their publications from 1989 or before. This aside, however, Cosmides and Tooby's argument that adaptationist grounds would lead us to expect effects in other, "evolutionarily significant" domains seems sensible. If success in early human environments required us to be very good at reasoning about social contracts, there is no reason to think that it would not also help to be very good about reasoning about precautionary rules as well (or reasoning about aggressive threats, etc.). Consequently, where critics have taken content effects for precautionary rules to be evidence against social contract theory, proposing instead that these effects reflect the operation of separate, innate "hazard management algorithms" has served as a launching pad for research by Cosmides and Tooby's graduate students (Fiddick 1998, 2000; Pereyra 2000).[10]

One hurdle for extension of the logic of Cosmides and Tooby's computational theory to other "evolutionarily significant" domains is the need to convincingly explain why the effects hold for evolutionarily novel hazards and threats, rather than being confined only to facilitating reasoning for items that are clear analogues of items encountered in the Pleistocene. For social exchange, their reasoning is as follows :

> That tools, information about tool making, and participation in opportunistically created, coordinated behavioral routine were important items for exchange has implications for the structure of human cognitive algorithms regulating social exchange. The more limited the range of items exchanged, the more specific the algorithms regulating exchange can be.... Whereas the exchange algorithms of other organisms can be specific to the relatively few items they exchange, human algorithms regulating social

[10] We can also question how clear a line can be drawn between precaution rules and social contract rules, which would not seem a trivial matter since they are supposed to be handled by discrete, domain-specific mechanisms. Cummins (1996, 1997) looks at performance by 3- and 4-year old children with the rule "If you play outside, you must wear a coat." The children are shown four pictures, including one of a girl playing outside without a coat, and asked in which picture the girl is "being naughty and not doing what he or she is supposed to do." Is this a precaution rule, because we understand that wearing a coat is a precaution against a cold? Or is it a social contract, because the children are cued to its violation by reference to someone "being naughty" and not doing what they are supposed to do?

> exchange should be able to take a wide variety of input items, as long as these items are *perceived* as costs and benefits to the individuals involved in the exchange... The prediction, then, is that the algorithms regulating social exchange in humans will be item-independent (Cosmides and Tooby 1989: 71-72).

Justifications will need to be provided for each new domain for which "item-independent" context-specific effects are asserted to reflect adaptations. Given evolutionary psychology's arguments about the benefits of increased cognitive specialization (Cosmides and Tooby 1992), it might seem incongruous if results from the WST led to the postulation of various specialized reasoning procedures for *types* of rules that were otherwise minimally domain-specific in terms of their *substantive content*.

In addition, content effects on the WST do not seem to be confined in any way to "evolutionarily significant" domains. To demonstrate this point, I developed the following problem that also has the same "if *P* then *Q*" form:[11]

Class Scheduling Problem

You are choosing what courses you are going to take next semester. You have a morning part-time job that puts constraints on what classes you can take. Specifically, your schedule has to conform to the following rule:

**"If a class meets on Monday or Wednesday,
then it must be in the afternoon."**

The cards below represent four different classes that you might be interested in taking. Side A tells the day of the week that the course meets, while Side B tells whether the course meets in the morning or the afternoon. For each class, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the class conflicted with the rule above.

Monday and Wednesday	Tuesday and Thursday	Afternoon	Morning

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[11] The reference to a "morning" part-time job is a cue to the *not-Q* card, although it is by no means as heavy as the cueing that is provided in some experiments by others. Still, a future experiment I would like to perform is to test this condition with the "morning" reference removed.

This problem requires one to understand that the scheduling rule is violated by classes that meet on Monday and Wednesday mornings. In my experiment, 80.9% of respondents answered the CLASS SCHEDULING problem correctly, which was also significantly better than performance on the DRINKING AGE problem (61/74 versus 91/141, χ^2=7.50, *df*=1, *p*=.006). Scheduling is a task involving preconditions that must be met, but scheduling problems are not social contracts. While "hazard management" is a task that we can easily imagine being as useful to our Pleistocene ancestors as it is to use today, "time management" is not. Very possibly one can come up with way of contorting a scheduling task into some seemingly evolutionarily significant domain, but the *post hoc* character of such explanations should be noted. If further studies indicate that content effects for scheduling problems are robust and replicable, this might provide a challenge for the idea that enhanced performance in a particular content domain is indicative of evolutionary "special design."[12]

THE CURIOUS CASE OF THE ALTRUIST DETECTOR

If "cheater-detection" is an evolutionarily significant domain, what about "altruist detection"? Wouldn't it be good for early humans to be able to spot altruists so they could exploit them, or at least choose them as ideal partners for exchange? For a WST involving

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[12] In my exploratory study, I found significantly better, but still quite weak, performance than the abstract task for a number of other, different rules that also had nothing to do with social contracts or other evolutionarily significant domains: "If a country's name begins with the letter Z, then it must be in Africa" (20/70, 28.6%), "If a person is severely overweight, then he or she must eat vegetarian lunches," (16/56, 28.6%), "If you buy a computer from Dell, then it comes with a modem installed" (20/71, 28.2%) (all p<.001 when compared to the 8.5% correct on the abstract rule). Other descriptive rules did not do as well as this: "If a movie contains any nudity, then it must receive at least an 'R' rating" (12/69, 17.4%, p=.06, two-tailed) and "If someone spells their name 'Robyn', they must be female" (11/71, 15.5%, p=.12, two-tailed). In none of these cases, however, did I apply the "recipe" of cues to constructing a successful Wason problem provided by Sperber et al. (1995), which I discuss below. Instead, these tasks explored performance on problems whose context surrounding the rule was kept concise (more brief than in the experiments used by Cosmides [1989]). These other tasks are described in the Appendix.

social contracts, one can frame the problem so that the subject is asked to look for cheaters—those who take the benefit but do not pay the cost—or one can frame the problem so that the subject is asked to look for altruists—those who give the benefit regardless of whether or not the cost was paid. Cosmides and Tooby regard the altruist problem as informative because they regard it as unlikely to be an evolutionarily significant domain. They contend that while a "look for cheaters" procedure follows deductively from evolutionary analyses, the same kind of evolutionary analysis make the existence of a "look for altruists" procedure implausible:

> "The game-theoretic models for the evolution of cooperation... require the existence of some mechanism for detecting cheaters or otherwise excluding them from the benefits of cooperation. This is because the capacity to engage in social exchange could not have evolved in the first place unless the individuals involved could avoid being continually exploited by cheaters. But most models do not require the existence of a mechanism for detecting "altruists"...Indeed, because individuals who were consistently altruistic would incur costs but receive no compensating benefits, under most plausible scenarios they would be selected out. Because they would not be a long-enduring feature of the adaptive landscape, there would be no selection pressure from 'altruist detection' mechanisms." (Cosmides and Tooby 1992: 194)

The prediction would seem an important "design feature" for social contract theory, because it would help establish that the observed successful performance on social contract problems was not the result of something that improved reasoning about social contract problems generally, but instead suggested the specialized operation of a "look for cheaters" procedure.[13]

Cosmides and Tooby conducted the experiment by modifying one of their cheating WSTs. In the CASSAVA ROOT problem, a chieftain named Big Kiku makes his subjects get tattoos on their faces, which, because of Big Kiku's many enemies, effectively mark these

[13] The perspective change studies of Gigerenzer and Hug (1992) are also cited as evidence that the social contract effects really are specialized to looking for cheaters; I discuss this research later in the chapter.

subjects for death should they ever leave his domain.[14] Four men who have been kicked out of their own villages end up independently at Big Kiku's feet. Big Kiku offers each of them a deal: "If you get a tattoo on your face, then I'll give you cassava root." Big Kiku tells the men that they must get their tattoos tonight, but he won't give them the cassava root until the next morning. The cards in the WST represent the "fates" of the four men, where one side tells whether the men got a tattoo and the other tells whether Big Kiku gave them cassava root. The cards read: "got the tattoo" (*P*), "no tattoo" (*not-P*), "Big Kiku gave him cassava root" (*Q*), and "Big Kiku gave him nothing" (*not-Q*). In the "look for cheaters" version of the problem, subjects were asked what cards they would have to turn over to see if Big Kiku had cheated any of the four men, while in the "look for altruists" version, subjects are asked what cards they would have to turn over to see if Big Kiku had behaved altruistically.

Cosmides and Tooby assert that an altruist is someone who provides a benefit when the cost is not paid, so the correct answer to the problem is *not-P* and *Q*.[15] Subjects were substantially more likely to give this answer when the word "selflessly" was used instead of "altruistically" (38% vs. 18%), suggesting that the some subjects—alas, even though they were Stanford undergraduates—might not have known the meaning of the latter word.[16]

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[14] This is a description of the task, not the CASSAVA ROOT problem itself, whose text is longer (see Cosmides and Tooby 1992: 197)

[15] This may actually be incorrect. That night, the men either get the tattoo or they do not, and if they get the tattoo it is permanent. The next morning, Big Kiku must decide whether to give the men cassava root (presumably costly to him) or not. He has already received the benefit, the men have no retaliatory capacity if he cheats, and so what does he gain by giving the men who got the tattoo any cassava root? (Especially since subjects are told that "Big Kiku hates some of the men for betraying him to his enemies".) In other words, given the way Cosmides and Tooby set up the problem, Big Kiku is behaving altruistically whenever he gives the men cassava root, regardless of whether they got the tattoo, and so the correct answer would seem to actually be *P*, *not-P*, and *Q*. Then again, one could counter that if I am right and Cosmides and Tooby did reason incorrectly about their own problem, then the vast majority of their experimental subjects did so as well, and it just goes to show that we do not have a special ability to spot altruists built into our brains.

[16] These results are combined across another experimental manipulation where Big Kiku is either presented as completely ruthless or ruthless with an occasionally generous streak.

(This is a harbinger of things to come, as it suggests that what might seem like minor changes in the WST can make an important difference in performance.) Performance for the "selflessly" condition is clearly higher than the results for the ABSTRACT problem that we saw earlier. Performance on the "look for altruists" task was substantially lower than the roughly 75% that has been observed for social contract problems that involve detecting cheating.[17] Cosmides and Tooby (1989: 195) use the difference in performance between the altruist- and cheater-detection problems to conclude that "people do not have inference procedures specialized for detecting altruists on social contracts, which is just what social contract theory predicts."

On the other hand, William Brown and Chris Moore (2000) propose that we *should* expect the mind to contain a "look for altruists" procedure. They posit that such a procedure could serve an important role in the evolution of cooperative behavior, because "altruistic-detection could be a possible mechanism to solve the adaptive problem of subtle cheating" (Brown and Moore 2000: 26). Subtle cheating is the occurs when party reciprocates but not in an equal amount to what they were given (Trivers 1971). Briefly, according to this argument, an altruist-detector would be adaptive because it would allow us to separate genuine cooperators from those who appear to cooperate but really cheat us whenever they can get away with it. Brown and Moore give subjects WSTs in which they are asked to identify potential altruists, where the task is designed around the concept of avoiding subtle cheating. In their study, performance on the "look for altruists" problem was only slightly worse than performance on a cheater-detection problem. Their experimental evidence for the existence of a "look for altruists" procedure also included the same

[17] Unfortunately, there is no hard-and-fast rule for how large a content effect needs to be in order to constitute evidence for the existence of a specialized procedure as a universal feature of the human psyche.

comparisons of "standard" and "switched" social contract problems that Cosmides and Tooby had used as evidence that people have innate cheater-detectors (I discuss "switched social contract" problems below).

These results would seem to contradict the earlier findings of Cosmides and Tooby, which may seem especially important since the absence of a "look for altruists" procedure was a key piece of evidence for "special design" and against one alternative to social contract theory. This said, Brown and Moore did not see their results as conflicting with Cosmides and Tooby's, but instead, they noted that the "altruist detection" problem that Cosmides and Tooby used had not been "designed with reference to any theory that predicts the existence of altruist-detection" (33). Even so, that Brown and Moore develop similar evidence for a separate "look for altruists" procedure that Cosmides and Tooby develop for their "look for cheaters" procedure illustrates the potential proliferation of specialized mechanisms to explain new significant results on the WST.

To be sure, there are differences between the altruist problem that Cosmides and Tooby administered to subjects and the ones that Brown and Moore gave to subjects, but, instead of their suggested explanation, the crucial difference might be in the amount of cues to the falsifying *P* and *not-Q* condition that each condition provides. Cosmides and Tooby only changed the CASSAVA ROOT problem to ask whether Big Kiku "behaved selflessly" instead of "got away with cheating," and the context of the problem remained otherwise focused on cheating, e.g., "You suspect [Big Kiku] will cheat and betray some of [the men]." Meanwhile, Brown and Moore's problems seem to provide numerous cues that "help" cue respondents to the not-Q card as being relevant to the response. Indeed, for at least one of their problems, making the correct inference would seem not to depend on actually reading the rule at all (the BLOOD DONOR problem, Brown and Moore 2000: 35) to make the correct inference.[18] Recall that the WST is supposed to be about how well subjects reason using conditional rules. If a problem contains so many cues that one can take the rule out, can one be justified in interpreting performance as being based on the rule? Brown and Moore's results therefore also point to the sensitivity of the WST to cues in the context surrounding the problem, which I discuss more in the next section.

TO CUE OR NOT TO CUE (TO NOT-Q)

The effects of small changes in wording on the WST have long been known: simply giving subjects a rule with the form "If *P* then <u>not</u> *Q*" achieves 60% correct answers (Evans 1972). For other purely descriptive rules, Sperber, Cara, and Girotto provide a "recipe" that one can use to create WSTs that produce high levels of subject performance (Sperber, Cara, and Girotto 1995; Liberman and Klar 1996). In an experiment that makes the possibility of strong effects for descriptive rules especially plain, Sperber et al. present subjects with a new rendition of the ABSTRACT problem:

[18] Comparing performance with the rule included and omitted is a potentially interesting experiment for future research. I think this would also be interesting to try for some of Cosmides's switched social contract problems. Fiddick, Cosmides, and Tooby (2000: 26) write about one of Sperber et al.'s problems: "If performance turned out to be the same when the conditional rule is absent, then it would be difficult to maintain that the... problem contributes much to our understanding of how people reason about conditional rules *per se*..."

Machine (Revised Abstract) Problem (Sperber, Cara, and Girotto 1995: 75)

A machine manufactures cards. It is programmed to print at random, on the front of each card, a 4 or a 6. On the back of each card, it prints a letter.

- When there is a 4, it prints either an A or an E at random.
- When there is a 6, it prints an E.

One day, Mr. Bianchi, the person in charge, realizes that the machine has produced some cards it should not have printed. On the back of the cards with a 6, the machine has not always printed an E:

sometimes it has printed an A instead of an E.

Mr. Bianchi fixes the machine, examines the newly printed cards and says: don't worry, the machine works fine,

"If a card has a 6 on the front, it has an E on the back."

Your task is to indicate which cards need to be turned over in order to establish whether what Mr. Bianchi said is true or false, at least as far as these four cards are concerned. Indicate only the cards that it is absolutely necessary to turn over.

6	7	A	E

Notice that while Sperber et al. have put the abstract rule in a different context, it is not one in which respondents are being asked to "look for cheaters," hazards, threats, or anything else with an easy connection to some specific task that would have been straightforwardly useful in the Pleistocene. In their study, 12 of 21 (57%) respondents got the problem right, as opposed to only 1 out of 21 (5%) for a version in which most of the cueing contextual information had been removed.

One key to producing these effects is setting up a context that in some way cues that *P* & *not*-Q is relevant as a violation. Perhaps one might look at the above problem and think that, like the BLOOD DONOR problem, the cueing is excessive and somehow not thus a fair comparison to other WSTs, especially when we compare it to the relative terseness of the

ABSTRACT and DRINKING AGE problem. But how much greater is this cueing extraneous to the rule than it is in the Cosmides (1989) experiments? If people have some mechanism in their heads that make them efficient at detecting cheaters, why should they need a lot of prompting to catch on that the task is to detect cheaters? The text introducing some of the Cosmides social contract rules is longer than this and seems to contain substantial cueing in their own right (see, e.g., the ostrich eggshell problem described in Cosmides 1989: 266).

Sperber et al. followed their proposed recipe in three other examples of descriptive rules and observed facilitation effects for all: 78% (21/27) correct for the rule "If a woman has had a child, she has had sex."; 65% (11/17) for the rule "If a volunteer is male, then he is married", and 70% (14/20) for the rule "if a person is of working age, then this person is married." Sperber et al. also show that performance on the WST was significantly diminishes as the cues they predicted to facilitate performance are incrementally removed.

Consider again the comparison between the "classic" ABSTRACT problem and the DRINKING AGE problem. The difference in performance is striking. But put the ABSTRACT PROBLEM in a different context—one that maintains its status as a descriptive rule—and performance improves to almost 60%, which doesn't make the performance on the drinking age problem seem nearly as remarkable. Which abstract problem should we use for comparison? Especially when we consider the very lengthy "set-ups" of Cosmides's social contract problems, there seems no defensible reason to prefer the "classic" problem over a version that elicits stronger performance (other than convention). This said, in very concise versions of the task, people do better with social contract rules than purely descriptive rules (e.g., Cosmides and Tooby 1992). Yet if it is the case that social contract problems require less cueing than descriptive problems, this would still seem a different claim than what

Cosmides and Tooby (1989, 1992) took as the implication of their "task analysis" of social exchange.

As part of their response to Sperber et al., Cosmides and Tooby (writing with Laurence Fiddick, their student) suggest that facilitation effects for descriptive rules are irrelevant to how we should view social contract theory:

> "Social contract theory is a content-specific theory: it was developed to *predict and explain reasoning about social contracts, and only social contracts*. Because social contract algorithms are not designed to be activated outside of the domain of social exchange, social contract theory was not intended as an explanation of content effects or of apparently logical performance in other domains." (Fiddick, Cosmides, and Tooby 2000: 18-19, emphases added).

Social contract theory is a theory about social contract problems, but it is also a theory about "special ability" that must be defined relative to something else. Comparisons of social contract problems and descriptive rule problems have provided a large part of the experimental evidence cited in support of social contract theory, and it has been arguably the most prominent part of the case for why we should believe that humans an evolved cheater-detector in our brains. Cosmides (1989: 207) writes, "[F]or social contract theory, the major determinant of responses is whether a rule is a social contract or descriptive...." Also: "Critical test 2: Are there more social contract responses to an unfamiliar standard social contract than falsifying responses to a familiar descriptive problem?" (215). In other words, before it was demonstrated that effects could readily be produced for descriptive rules, the comparison of social contract rules and descriptive rules was considered evidence *for* the theory (Cosmides 1989; Gigerenzer and Hug 1992), but now that it appears that such effects can be produced for descriptive rules, this evidence is argued to be *irrelevant* for the theory. When the predictions are made that humans "should show a special ability to detect cheaters in social exchange" or that "the human mind must include inferential procedures that make one very good at detecting cheating on social contracts," the terms "special" and "very good" are only meaningful if they are relative to something else.

Sperber's et al's research demonstrates that cueing can make a large and systematic difference on performance on the Wason Selection Task. The implications for cheater-detection research is addressed more directly by Liberman and Klar, who re-examine earlier experiments by Gigerenzer and Hug (1992).[19] Like Cosmides (1989), Gigerenzer and Hug (1992) conduct several experiments in which exactly the same rule was presented to subjects either in a context that implied cheating or one that did not (that is, the same rule was cast in, e.g., a cheating and a purely descriptive [or just permission] context). Liberman and Klar conjectured that Gigerenzer and Hug's results were confounded by differences in the cues provided in the different contexts. Roughly, they conjectured that subjects received much more cueing to the correct answer in the cheating conditions of the Gigerenzer and Hug experiments than in the no cheating conditions, even though, again, this kind of extra cueing is not supposed to be necessary to activate our cheater-detectors. They created alternative views of both conditions, removing the facilitating cues from the cheating condition and inserting them in the no cheating condition.[20]

Social contract theory predicts that facilitation effects should be observed for the cheating condition even when the confounding cues are removed. Gigerenzer and Hug and the earlier work of Cosmides also would seem to predict low performance on either rendition of the no cheating condition. Instead, Liberman and Klar's results appear to support their

[19] The Liberman and Klar experiments were performed on Israeli subjects, in Hebrew. Trying to replicate their findings with English-language versions of the task would be an interesting task for future research.

[20] In one instance (the CHOLERA problem), Liberman and Klar's alterations effectively changed the "no cheating" condition of a social contract problem into a hazard management problem, which, since evolutionary psychologists have proposed that a separate innate algorithm is invoked for hazard problems, confounds their results (pointed out by Fiddick, Cosmides, and Tooby 2000). Liberman and Klar looked at four other problems from Gigerenzer and Hug, and, in none of these other problems did they change the "no-cheating" condition to some evolutionarily relevant context.

suspicion that differences in cueing, not type of context *per se*, were responsible for the differences that Gigerenzer and Hug had observed for these problems. By manipulating the contextual cues—but not the underlying structure of the problem being about detecting cheaters or not—Liberman and Klar showed that you could greatly diminish the effects for "look for cheaters" problems and greatly increase the effects for problems that did not involve detecting cheaters. In sum, Liberman and Klar's findings therefore complement Sperber et al.'s conclusions that facilitation effects on the WST can be generated for a wide range of descriptive rules that have nothing to do with "evolutionarily significant" domains. This might then seem to cast additional doubt on the idea that humans have any "special ability" for reasoning specifically about cheating. In the next section, I examine a different line of evidence that is cited in support of the existence of this "special ability": performance on "switched" social contracts.

SWITCHED SOCIAL CONTRACTS

> *"It is one of the most pernicious aspects of the present climate of discussion, that the situation is often set up as a forced choice between accepting the particular theories and oversimplified principles of evolutionary psychology, or retreating to a pre-Darwinian denial of the fact that we are evolved animals."*
> —Elisabeth Lloyd, essay, 1999 (p. 213)

In the CASSAVA ROOT problem, Big Kiku pronounces to four starving men that "If you get a tattoo on your face, then I'll give you cassava root." When asked which cards they would need to turn over to see if Big Kiku had cheated any of these men, subjects typically recognize that Big Kiku could only possibly have cheated those men who got a tattoo and those men who did not get cassava root. That is, they select the *P* and *not-Q* cards on the task. In the "switched social contract" version, subjects are given exactly the same problem, except that the rule is turned around to read “If I give you cassava root, then you must get a tattoo on your face.” Subjects are asked what cards they would need to turn over to know if Big Kiku cheated.

As before, subjects appear to recognize that Big Kiku could only have cheated the men who got a tattoo and those that did not get any cassava root. But, for the switched problem, doing so means that they select the *not-P* and *Q* cards instead. Since the rule is still of the form $P \rightarrow Q$, if we interpret the rule according to propositional logic, then P and not-Q is still the “logically correct” answer. Switching the rule around does not confuse subjects into thinking that Big Kiku has cheated men who get cassava root even though they didn’t get a tattoo on their face, which would be implied if they still chose the *P* and *not-Q* cards. Evidence for the “special design” indicative of adaptation is claimed from the “switched social contract” and similar experiments, because subjects are supposedly answering in a way that is “non-logical (but adaptively sound)” (Fiddick, Cosmides, and Tooby 2000: 69).

Especially given the elision of the “must” in the first problem, that subjects understand the sentences “If you get a tattoo on your face, then I’ll give you cassava root” and “If I give you cassava root, then you must get a tattoo on your face” as meaning the same thing might seem more the province of a theory of language/discourse comprehension than for reference to a specialized “‘look for cheaters’ procedure.” Gigerenzer and Hug (1992: 158) write: “The deontic ‘must’ in the original rule disappeared. Cosmides stated that this deletion was unavoidable, since a switched rule that keeps the ‘must’ would have made little sense (‘If a man must have a tattoo on his face, then he eats cassava root.’). It seems evidence that the switched rule is not quite the converse of the original one; it is less clear what precisely follows from this for evaluating her results.”

Except for switching the antecedent and consequent of the rule, everything is kept the same in the standard and switched social contract versions of the same problem. With the BLOOD DONOR problem, we have already found reasons to wonder how essential the rule actually is for some versions of the WST. For the CASSAVA ROOT problem, the following is part of the contextual information that is presented before the rule:

> You are a very sensual people, even without the aphrodisiacal properties of cassava root, but you have very strict sexual mores. The elders strongly disapprove of sexual relations between unmarried people, and particularly distrust the motives and intentions of bachelors. Therefore, the elders have made laws governing rationing privileges.

After being presented with the rule, subjects are told:

> Cassava root is such a powerful aphrodisiac, that many men are tempted to cheat on this law whenever the elders are not looking. The cards below are about four young men sitting in a temporary camp; there are no elders around... Your job is to catch men whose sexual desires might tempt them to break the law—if any get past you, you and your family will be disgraced. Indicate only those card(s) you definitely need to turn over to see if any of these Kaluame men are breaking the law.

Given all this cueing, subjects have no trouble reading the rule in the switched version as meaning the same thing as the rule in the original version. Again, we might even wonder how important the actual statement of the rule is for subjects' comprehension of the problem.[21]

To understand part of the reason why this result is taken by some to be compelling evidence of an specialized cognitive adaptation, one must understand what is seen as one preeminent alternative. Recall that Tooby and Cosmides (1992) coin the phrase "the Standard Social Science Model" to describe what they interpret to be the dominant

[21] Like the BLOOD DONOR problem, it would be interesting to try an experiment where the rule was omitted from the CASSAVA ROOT problem.

assumptions of non-evolution-minded social science. For the Wason Selection Task, Cosmides and Tooby (1992: 166) contend that

> "the Standard Social Science Model predicts that the reasoning procedures applied to situations of social exchange should be the same reasoning procedures that are applied to other kinds of content. On this view, reasoning is viewed as the operation of content-independent procedures, such as formal logic, applied impartially and informally to every problem, regardless of the nature of the content involved."

If this is the case, then the Standard Social Science Model would predict that switching the rule around should confuse subjects into thinking that Big Kiku is cheating those men who get the benefit even though they did not pay the cost. Subjects would not able to understand that the word "cheating" implies a different set of inferences than does formal logic. Despite the fact that elsewhere Cosmides and Tooby criticize the SSSM for its undisciplined invocation of amorphous concepts like "learning" and "socialization," the SSSM would not predict that the distinction between formal logic and cheating is something that people could acquire or that would follow from an understanding of the logic of intentionality. Cosmides and Tooby's finding does reveal the misguided character of an exaltation of propositional reasoning by some earlier cognitive scientists (Cosmides 1989; Cosmides and Tooby 1992). They err, in my opinion, in trying to tie this work to the claims about the Standard Social Science Model. On the specific point of whether switched social contract imply an evolved, specialized "look for cheaters" procedure, Paul Pollard (1990) writes "We surely do not need a million years of evolution to be able to take for granted that, say, going into a restaurant, paying for a meal, and then sneaking out without eating it is not really a case of cheating the restaurant" (1991: 198).

Something else might seem amiss here. Subjects are given a Wason Selection Task that asks them to detect potential cheaters; they correctly identify the potential cheaters; but then they are said to have not reasoned logically in doing so. Why is propositional logic given such a privileged position here, such that people who follow propositional logic are said to be "logically correct" in their response even though they fail to perform the explicit purpose of the task, which is identifying potential instances of cheating? Cosmides and Tooby (1992: 187) assert that "The propositional calculus is content-independent: The combination of *P* and *not-Q* violates any conditional rule of the form *If P then Q*, no matter what '*P* 'and '*Q*' stand for." But this use of "content-independent" is debatable, as the *propositional* calculus would seem only appropriate for contents that are *propositions*.

Normative rules about permissions and obligations, called *deontic* rules, are not propositions and do not necessarily follow propositional logic (Davies, Fetzer, and Foster 1995). Some argue that Wason Selection Tasks that use deontic rules are not "really" Wason Selection Tasks; for example, Griggs and Cox (1993: 650) write that "to understand performance on the original selection task, it appears that we need to study it and not the numerous "deontic" versions of the task that have emerged." In any event, the fundamental logical difference between descriptive rules and deontic rules is yet another reason why we should not put much stock in the comparison of performance on descriptive rules and social contract rules as evidence of evolutionary "special design."[22] Moreover, it is not clear what is gained by showing that subjects do not engage in propositional reasoning when presented with a deontic rule. In addition, we should not think there is anything special about asking about cheating here: recall that Cosmides and Tooby's answer to their "look for altruists" problem also required subjects to turn over the *not-P* and *Q*, which was also not a solution

[22] The original Wason abstract problem was more purely a propositional logic problem. The secretary context of the abstract problem in the text makes it a rule that people reason from rather than about, but it is does not have the permission/obligation structure of a deontic rule. Sperber et al. (1995) discuss the status of the "classic" abstract problem (with the possible erroneous secretary) in terms of deontic vs. descriptive rules.

based on “formal logic.” Although almost no subjects answer *not-P* and *Q* to a descriptive rule, 38% of subjects answered *not-P* and *Q* in the “look for altruists” problem discussed above, but this was not taken as evidence of a specialized adaptation. In sum, that subjects answer cheating problems correctly regardless of whether they are presented in a standard or switched form would not seem to entail the existence of cognitive mechanisms specialized to detect cheating.

PERSPECTIVE CHANGE STUDIES

Another line of evidence for social contract theory, based initially on experiments by Gigerenzer and Hug (1992), are studies that manipulate the perspective of the subject. Consider the rule “If an employee works on the weekend, then that person gets a day off during the week.”

In the first condition, subjects are told to pretend that they are employees and that this is one of the rules of the company they work for. They are told that there are rumors that this rule has been violated, and then they are asked—as employees—to identify possible violations of the rule. In the second condition, subjects are told to pretend that they are employers, that this is a rule of their company, and that there are rumors that the rule is being violated. Subjects are then asked—as employers—to identify possible instances in which the rule is violated. Subjects cued to being employees treat as possible violations instances in which an employee works on the weekend but then does not get a day off. Subjects cued as employers treat as possible violations instances in which the employee does not work on the weekend but gets a day off anyway. In other words, the rule is such that either party can be cheated, and when asked what cards need to be turned over to see if the rule has been violated, they orient to the task in terms of what would constitute cheating from the perspective from which they are cued.

Like the switched social contract experiments, the perspective change experiments show that, given tasks that involve detecting cheating, subjects pick the cards that are consistent with detecting cheating instead of applying propositional or some other singular logic to a deontic problem. More than this, the perspective change studies could be taken as suggestive of a specialized mechanism that is activated in defense of oneself being cheated. David Buss (1999: 266) writes that "Perspective, in short, appears to govern the sorts of cheaters that one looks for—if you are an employee you are sensitive to being cheated by your employer; if you are an employer you are sensitive to being cheated by your employees." Based on their results, Gigerenzer and Hug (1992: 165) offer the following proposal about the character of the "look for cheaters" procedure: "If a person represents one party in a social contract, and the other party has a cheating option, then a cheating-detection algorithm is activated that searches for information of the kind 'benefit taken & costs not paid (requirement not met).'"

The perspective change studies show that respondents can be cued to attend only to one side's taking of the benefit without meeting the condition, but experiments by Liberman and Klar (1996) indicate that this neither requires adopting the perspective of the cheated party nor does adopting the perspective of the cheated party preclude the subject from being able to reason about what constitutes cheating from the other's perspective. Liberman and Klar also looked at the employer/employee rule, but added two conditions in which the respondent is asked to consider the problem from a third-party perspective (not someone with any stake in the social contract described). In one condition, subjects were cued to being a third party interested in seeing if workers got what they deserved, and, in the other condition, subjects were cued to being a third party interested in seeing if workers got more than they deserved. These conditions produced responses similar to what was obtained by asking subjects to adopt the perspectives of employee or employer, respectively, indicating that the perspective change was not necessary. Liberman and Klar also tested conditions where subjects were asked to take the perspective of an employer or an employee, but told that they were concerned about the possibility of either employees or the company being cheated. When these cues were used, the effect attributed to the "look for cheaters" procedure disappeared. Complementing these findings, Staller, Sloman, and Ben-Zeev (2000) show that perspective change results can be obtained for non-social contract problems, which suggests that there is nothing necessarily special about the effects of perspective change when respondents are asked to look for cheaters.

Reflection also suggests that there does not seem anything privileged about the perspective of the cheating party for recognizing instances of cheating. When we see someone sneak something out of a store without paying, we would seem to have no more trouble recognizing this as cheating-from-the-standpoint-of-the-storeowner as if we were the storeowner (which is not to say that we necessarily have the same inclination to stop the person). Moreover, deciding to fulfill or to try to cheat on a social exchange requires an understanding of what constitutes cheating. In an environment where people understand that someone who does not pay them back is cheating them, someone who could not reason about what they needed to do to fulfill a social exchange (i.e., the uncomprehending cheater) would appear to be unable to form extended reciprocal relationships and so would seem to be at a significant selective disadvantage to someone with a facility for recognizing cheating from any perspective. Consequently, one could argue that a perspective-dependent mechanism for reasoning about social exchanges does not necessarily make good evolutionary sense.[23, 24]

ARE SOCIAL CONTRACTS SOCIAL EXCHANGES?

> *"[T]he properties of an adaptation can be used to identify the class of problems, at the correct level of specificity or generality, that the adaptation was designed to solve. The eye allows humans to see hyenas, but that does not mean that it is an adaptation that evolved particularly for hyena detection: There are no features that render it better designed for seeing hyenas than for seeing any of a far larger class of comparable objects."*
>
> —Leda Cosmides and John Tooby, 1992

"Social contract theory" is the name that Cosmides and Tooby have given to the theory that, among other things, predicts the existence of the "look for cheaters" procedure. But the major papers on the theory have in their titles "The logic of social exchange," "A computational theory of social exchange," and "Cognitive adaptations for social exchange." Their whole "task analysis" is a lengthy explication of the prerequisites required for the observed human capacity for social exchange: "The ability to engage in a possible strategy of social exchange presupposes the ability to solve a number of information processing problems that are highly specialized. The elucidation of these information processing problems constitutes a computational theory of social exchange." Given the consistency of focus on social exchange, why introduce the term "social contract" at all, instead of just

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[23] This is not to say that there couldn't be biases, such as a tendency to remember favors we are owed better than favors that we owe. But this is memory, not reasoning, much less the reasoning about gross cheating (Trivers 1971) that the WST evidence takes as evidence of a specialized mechanism.

[24] In one of the conditions of the exploratory experiments that I conducted, subjects were given a problem in which they were cued to the perspective of a professor worried that he might have cheated students out of a grade (see GRADING problem [TEACHER PERSPECTIVE] below). Even though subjects were not cued into the perspective of the cheated party, 64.3% (45 of 70) answered the problem correctly. A useful follow-up experiment, of course, would be to compare subjects cued into the role of professor with those cued into the role of student.

calling it “social exchange theory”? (Actually, “social exchange theory” is what the theory is called in the abstract of the Cosmides (1989) paper, but “social contract theory” is used in the remainder of the text.)

Consider again the drinking age problem. Recall that the strong content effects for this problem were known prior to the formulation of the theory and that Cosmides and Tooby presented the theory as an explanation of all “robust and replicable” content effects that were known at the time. Does the rule “If a person is drinking beer, then he or she must be over 21 years old” express a social exchange? Although the right to drink beer could be construed as a benefit granted by the government, it is not something that is actually exchanged between the government and the drinker. Likewise, being 21 years old is not something that one exchanges in order to drink beer, but instead it is a requirement that must be met. On the one hand, we have a theory developed using the evolutionary concept of reciprocal altruism and a game-theoretical specification of the logic of social exchange. On the other, we have content effects for social laws that do not fit what is normally thought of as social exchange. Consequently, Cheng and Holyoak (1989) contend that if a theory of social exchange was to account for known effects on the Wason Selection Task, Cosmides and Tooby needed to “broaden [the] definition of an exchange to include situations in which no cost is paid in order to account for the many non-exchange contexts (e.g., the drinking age rule) that have yielded facilitation.”

The notion of social contracts might seem to solve this problem, by broadening the usual definition of social exchanges to encompass benefits provided by society (i.e., the right to drink) and circumstances in which one had to meet a requirement rather than pay any cost. In the initial expositions of the theory and research, the language of exchange dominates. Indeed, the addition of group- provided-benefits and requirements-instead-of-costs might seem perfunctory, or even *ad hoc* given the findings existing in the literature at the time:

> A social contract relates perceived benefits to perceived costs, expressing an exchange in which an individual is required to pay a cost (or meet a requirement) to an individual (or group) in order to be eligible to receive a benefit from that individual (or group). Cheating is the failure to pay a cost to which one has obligated oneself by accepting a benefit, and without which the other person would not have agreed to provide the benefit [citing Cosmides 1985]... In a social exchange situation..., a "look for cheaters" procedure would draw attention to any person who has *not* paid the required cost (has he illicitly absconded with the benefit?) and to any person who has accepted the benefit (has he paid the required cost?). (Cosmides 1989)

Notice how "(or meet a requirement)" and "(or group)" are parenthetically inserted into the definition of social contract, but then subsequent definitions of "cheating" and the description of the "'look for cheaters' procedure" are presented more in terms of a one-to-one exchange.[25] The word *or*, however, cannot be idly added to a central theoretical concept, especially when the concept is being used to define an "evolutionarily significant" domain. The idea of a "social contract" as a singular form on which something like a highly specialized "'look for cheaters' procedure" may operate would seem to depend on there being some way in which "paying a cost" and "meeting a requirement" can be thought of as different shades of the same thing. Perhaps the most obvious way of conjoining paying a cost and meeting a requirement is to conceive of the former as being a special case of the

[25] The accommodation of social laws into the theory has still other problems. For an underage drinker to show up on the radar of a "look for cheaters" procedure, an underage drinker must be a cheater. But is it right to say that one has obligated oneself to be 21 by accepting a beer (that is, can one be obligated to do something that is not an activity?). Also, who is the "other person who would not have agreed to provide the benefit" in the drinking age problem? The bouncer is not the state (the provider of the privilege), or an agent of the government, but an agent of the bar, which would have incentive to provide the benefit if it was not for the potential punishment of the state. Put another way, the bar is not the party who is cheated. The state (or, if you want, "society") is the cheated party, but the state is not the party who agrees whether or not to provide the benefit. (The bar does run the risk of losing its liquor license by providing the beer to a minor, but this is a red herring, because the liquor license is not the benefit with which the underage drinker is illicitly taking.)

latter (that is, to think of paying a cost as being one type of precondition that can be required to receive a benefit).

Then again, if social exchanges are only a subset of social contracts, then the value of a "task analysis" of social exchange for understanding how we reason about violations of this type of rule is not apparent. If we have highly specialized mechanisms in our brain fashioned specifically for spotting cheaters on social exchanges, then why should this mechanism also be activated by rules about underage drinking? To draw on the quote from Cosmides and Tooby that begins this section, if we are going to say that the mechanism is an adaptation that evolved particularly for detecting cheaters on social exchanges, then it would seem like there should not be some larger class of tasks (i.e., detecting violations of social laws rationing benefits) that people can do just as well as detecting cheating on an exchange.

Importantly, Cosmides and Tooby do not consider the move from social exchange to social contracts to be broadening the definition of their key concept. Instead, they regard the social laws like the drinking age law *as being a form of social exchange*. Paying a cost and meeting a requirement are theoretically joined together as conditions that are required to fulfill one's end of an exchange. They contend that, like an individual who proposes a trade

> "the people who proposed the drinking age law also wanted a good: safer streets. People restrict access to benefits to create a situation that benefits them. That situation can be that which obtains when access to a good is restricted. This is why *Cosmides and Tooby have always considered social laws of the form, "If you take the benefit, then you must satisfy the requirement" to be instances of social exchange.* This is the only theory we know of that accounts for why not all social laws elicit high performance, but ones that have the form of social contracts." (Fiddick, Cosmides, and Tooby 2000: 64, emphasis added).

For the moment, we will accept their account of the motivation behind the drinking age law.[26] If the "people who proposed the drinking age law" get safer streets as part of an exchange, what is it that they give up to obtain it? The answer: nothing. Imposing the law still counts as an exchange because Cosmides and Tooby do not consider incurring a cost to be a necessary part of a social contract. Indeed, in the passage below, they protest that critics who read them as suggesting otherwise (e.g., Cheng and Holyoak 1989) are relying on "folk notions":

> The presence of a cost is not, and never has been, a defining feature of a social contract. The computational theory of social exchange... derives nonarbitrarily from evolutionary analyses, not folk notions. Accordingly, we defined social exchange as cooperation for mutual *benefit*—not the imposition of mutual costs. For you to offer or agree to a social contract, the situation created must benefit you—this is a necessary condition—but it need not impose a cost on you or anyone else. (Fiddick, Cosmides, and Tooby 2000: 73)

The confusion on this point may be partly due to Cosmides and Tooby basing their evolutionary analyses on Trivers's (1971) theory of reciprocal altruism, which they take as being synonymous with social exchange (discussed below), and the first sentence of the Trivers paper defines altruism "behavior that benefits another organism, not closely related, *while being apparently detrimental to the organism performing the behavior* [in inclusive fitness terms]" (35). Reciprocal altruism seems to imply that the altruist incur some kind of apparent or short-run cost (that is made up when the favor is returned), even if fulfilling a social contract/exchange as Cosmides and Tooby define it does not.[27]

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[26] Do they say safer streets because the drinking age problem mentions a crackdown against drunk drivers? This is the stated motivation for the crackdown, but not the motivation for the existence of the law in the first place.
[27] Although not explored in this chapter, a potential way in which exchange theory in sociology may contribute to social contract theory is in its explication of the variety of exchange relations that may exist among actors (Emerson 1972; see Molm and Cook 1995 for a review of exchange theory). One question for future research might be whether variation in the type of exchange relation affects subject performance on a social contract WST.

Putting that aside, if the social laws like the drinking age law are social exchanges and social exchanges are cooperation for mutual benefit, what is the benefit to those persons whose actions are restricted by the law? In the classic sense of "social contract," people relinquish some of their naked self-interest for the greater benefit of social order, and those who pursue their naked self-interest might be characterized as cheaters in this sense. The citizens of a traffic-congested city could pass a law for its freeways like "If you drive in the far left lane during rush hour, you must have at least two passengers in the car." Although citizens recognize that they might be tempted to cheat, they can also recognize that by having the state enforce the law they provide an disincentive to cheating for everyone, and, over the long haul, everyone benefits more than they would if a law did not exist or if it was not enforced.

But such a benign worldview does not cover for all social rules of the form "if they take the benefit, then they must meet the requirement." Faced with the same traffic congestion, the leaders of an oppressive state could declare a rule like "If you drive in the far left lane, you must be a member of the Party." The subject is cued to the perspective of a police officer charged with enforcing this rule. Who are potential violators: a car in the left hand lane, a car in the right hand lane, a member of the Elite, a non-Party member?[28] The rule has the form of a social contract, and it can be seen as providing a perceived benefit for those who had the power to declare it. But in no way does it represent a mutual benefit for all of those who fall under its jurisdiction (unless one believes Party propaganda that might characterize it as such). Police officers enforcing the law might not even be party members themselves: like the bouncer in the bar, they may enforce the rule only because they have

[28] This is an example of a rule that fits the form that Cosmides and Tooby give for social contracts, and, at least to my reading, the answer seems as apparent as with other social contract problems. Even so, of course, I would need to run the experiment to assert that this rule actually does yield a content effect.

been threatened with punishment if they do not. We can imagine instances in which the rule could be based on a purely inborn characteristic, like "If you drive in the left-hand lane, you must be a member of the upper caste," or where the benefits accrue to only one person, "If you drive in the left-hand lane, you must be the King." These instances stretch the interpretation of such rules as instances of exchange for mutual benefit, either from the standpoint of the party whose actions are constrained by the rule or its potential enforcer.

Cosmides and Tooby's task analysis contains a long consideration of the cost-benefit relations of "sincere social contracts", based on the discussions of the payoffs required for one party to make a "sincere offer" and another to make a "sincere acceptance" (see, e.g., Cosmides and Tooby 1992: 172). While this makes sense for exchanges between two parties, it cannot be applied generally to social laws that have the form of a social contract. A law against underage drinking is not something that is offered to minors that they can accept or reject. A law restricting access to the left lane is not something offered to non-Party members. Instead, the law is *imposed* on them. The nonconsensual character of the law does not in any obvious way affect our ability to spot when people are breaking it.[29] More politicized examples of course exist in which social privileges (voting, being outdoors after curfew, entering certain areas, etc.) are restricted to statuses that some who are expected to follow the rule are restricted from permanently (e.g., on the basis of sex, race, caste).

Few social scientists will be surprised by my assertion that many people are compelled to live under laws that are not of their choosing and not to their benefit, but yet this reality contradicts the key contention of social contract theory that social laws of the form "if you take the benefit, then you must meet the requirement" are always instances of

[29] I have developed a set of alternative versions of the task that explore the extent to which a social law restricting a benefit is an example of "cooperation for mutual benefit."

cooperation for mutual gain. If social contracts cannot be defined either in terms of mutual benefit or mutual cost, then they are not in any sense social exchanges. Consequently, the cognitive processes responsible for our understanding of the violations of these rules might not be easily explain by a "task analysis" of the prerequisites for the capacity to engage in social exchange.[30]

CONCLUSION

The subtitle of the paper reporting Cosmides's experiments was "Has Natural Selection Shaped How Humans Reason?" That the answer to this question is affirmative, however, is not in doubt. Whatever reasoning capacities humans possess, we have them in the first place because of our big and complexly ordered brains, whose big-ness and complex ordered-ness is the result of Darwinian selection. Research on the cognitive capabilities and biases of humans may help evolutionarily-oriented scientists reconstruct a plausible account of the selection processes that resulted in our brains having the structure that it has. A more expansive vision of evolutionary psychology, however, aspires to go the other way: from reflection on the adaptive problems our ancestors faced to the discovery of new features of the mind. This gives the program a predictive project that would seem to make it much more potentially important to the rest of psychology than an explanatory program that focuses only on providing adaptive accounts of the proximate mechanisms that experimental psychologists uncover (for a critique of the prospects of this predictive project, see Grantham and Nichols 1999). In the program of evolutionary psychology, the predictive project is

[30] Perhaps Sperber's (1996) distinction between actual and proper domains might provide one answer here: the actual domain is the information processed by a mechanism in its current environment, while its proper domain is the information "that it is the module's biological function to possess" (p. 136). Social exchange could be the proper domain, but these other rules could be part of the actual domain given the novelties of contemporary environments.

strengthened by a view of mind in which cognitive mechanisms are so specifically shaped in response to discrete adaptive problems that detailed investigation reveals evidence of "special design" for solving those problems.

In the case of social exchange, many people's behavior evinces an understanding that in order for exchange to be profitable, we must get something to our benefit that is greater than that which we give up and that someone who says they will give us something in return but does not has cheated us. Reflection on evolutionary dynamics indicates that people had to be able to comprehend these things in order for successful social exchange to yield important selective advantages, or else they would be vulnerable to relentless exploitation by others. The evolutionary psychological leap would seem to be the assertion that this implies that a *specialized* adaptation for making these sorts of inferences exists, in the form of an algorithm that is activated only in social contract contexts and that yields correct inferences we would otherwise not make (Cosmides and Tooby 1989).

The evidence for the existence of this algorithm comes almost exclusively from subject performance on the WST, which subjects are much more likely to answer correctly when given a "social contract problem" instead of the classic "abstract" problem. But, as has been discussed in this chapter, there are many routes to improved performance on the WST. Presenting subjects with a negated rule dramatically improves performance, as does the addition of cues that do nothing to change the substantive domain of the rule. Equivalent performance has been achieved for problems in which the rule is a precaution, which are explained as facilitated by a separate algorithm (for a separate evolutionarily significant domain), although we have seen subjects also perform better on a project that deals with class scheduling, which seems less easily to describe as evolutionarily significant. The domain "social contract" is supposed to be based on operations of "cost-benefit" operations, but this requires us to consider meeting a requirement to be fulfilling a social exchange and to regard social rules that ration benefits on the basis of innate characteristics to also be social exchanges.

In Cosmides and Tooby's (1992) own experiments, a social contract problem that asked subjects to look for "altruists" rather than "cheaters" yielded 38% correct answers, while one that that took the benefit out of the social contract yielded around 50% correct answers. Because these percentages are significantly less than what are achieved for social contract problems, they are not considered evidence against social contract theory, although they would seem to indicate that a majority of the initial "content effect" is not due to subjects being given the task of detecting cheaters. Where social contract theory once appeared to explain a difference in performance between very few respondents getting the problem right and a vast majority of respondents answering right, the look-for-cheaters context now appears to make the difference for no more than one-third to one-fourth of respondents. Consequently, it is hard to take this as good evidence for the operation of a *universal*, highly specialized mechanism that is activated by the social contract context and that enables us to make these inferences. Putting all this together, I believe that there is little persuasive evidence of any species-typical trait that underlies improved performance on look-for-cheaters problems, and little reason to believe that the results for these problems reflects a key-in-a-lock-like "special design" for the domain of social exchange.

If humans do not have a "'look for cheaters' procedure" in our brains, does it mean that humans apply some single, content-independent logic to every type of content they confront? Does it imply that everything about social exchange must be either induced from observing social exchanges or taught to us by others? Does it lead us to conclude that natural selection has not shaped how humans reason? No, none of these. That people seem to make more sense out of some versions of the Wason Selection Task than others perhaps may reflect aspects of human discourse comprehension that are likely to be partly innate (e.g, Pinker 1994; Sperber and Wilson 1986). Moreover, that the task seems more sensible when the restricted action is a benefit may well reflect innate human understanding and reasoning about intentionality (Leslie 1987; Baron-Cohen 1995). Considering these possibilities is much different than asserting that every aspect of reasoning is acquired from the environment, and their plausibility is only strengthened by finding that members of other societies exhibit similar responses on the selection task as do American undergraduates (Sugiyama 1996).

But, what must be recognized is that exploring these possibilities is also much different than attributing the content effects to the activation of a specialized cheater-detection mechanism. Indeed, we could even propose that people have innate knowledge of certain aspects of deontic reasoning, and this would not force the conclusion that people have separate, discrete mechanisms that evolved especially for cheater detection (Samuels 1998). The problem with Cosmides and Tooby's (1994: 92) task analysis approach is its operating assumption that tasks with different solutions must be implemented by different mechanisms: "Because what counts as the wrong thing to do differs from domain to domain, there must be as many domain-specific cognitive mechanisms as there are domains in which the definitions of successful behavioral outcomes are incommensurate." Shapiro and Epstein (1998: 175) compare this to saying that because tightening screws requires turning them in one direction and loosening screws require turning them in the other, what counts as the wrong way differs for the two tasks and thus we need separate screwdrivers for tightening and loosening. We should not confuse the diversity of tasks the mind is able to accomplish with the diversity of mechanisms that are needed to accomplish them, or else we have no restraint on invoking a new mechanism for every reasoning distinction that humans are able to draw, as well as no incentive to try to explain more with less. Writes Elisabeth Lloyd (1999: 224) in her critique of Cosmides and Tooby: "It is *not scientifically acceptable* within evolutionary biology to conclude that, because a given pattern of responses contributes to evolutionary success, then there is some 'organ' (or part of the brain) producing such a pattern, that is therefore an adaptation. This is because the 'organ' or 'module' may not actually exist as a biologically real trait, and even if it does, its current function may or may not be the same as the past function(s)."

The idea that mind contains something so specialized as a cognitive algorithm that somehow only comes into play when reasoning about gross cheating is a conclusion that we should come to reluctantly, not one that should serve as the starting point for a program of experiments. Regarding cheating, each significant difference in content that is teased out becomes potential grounds for asserting the existence of a new algorithm that exists as a discrete product of natural selection. For example, Shackelford and Buss find that people understand betrayal differently depending on whether one is talking about a romantic relationship, a friendship, or a strategic coalition, leading Shackelford (1997) to conclude from that "Cosmides and Tooby's cheater-detector mechanism may be too domain general" and that even more specialized mechanisms are needed—"three relationship contexts, three cheater-detector algorithms." Cummins's (1999) finding that one can find different content effects on the WST depending on the social rank of the parties in the social contract scenario could also be taken to imply separate adaptations for differences in reasoning that are required by different configurations of "social rank" among parties. And we also still have separate algorithms for reasoning in different domains like precautions, as well as whatever algorithms are required to support content differences in inferences on other reasoning tasks that have yet to be explored.

There are some real, material limitations here that may prove an insuperable barrier for the paradigm of evolutionary psychology. If all of the cognitive complexity responsible for making these distinctions really is supposed to suggest specific and separate adaptations, then the instructions for building these adaptations must in some way be encoded genetically. Cosmides and Tooby (1992: 39) have proposed that "This rich array of cognitive specialization can be liked to a computer program with millions of lines of code and hundreds or thousands of functionally specialized subroutines." Yet, if the genes contain all the specialized instructions for building an enormous number of adaptations for human sociality, then we might expect that cognitive complexity would be positively correlated with genome size.[31] Not only is this not the case, but humans have the same number of genes as the common house mouse (Deacon 1997; Buller 2000). Biologist Paul Ehrlich (2000: 126) writes, "The exact degree to which a genome consisting of some 100,000 genes can effectively be modified to fine-tune human behavior remains an open question, but the possibilities must be very highly constrained in relation to the number of possible behaviors." As a result, parsimony in proposing the innate structure of mind is not simply a philosophical desideratum. Accordingly, we perhaps should not be surprised if, sooner or

[31] Tom Gieryn (personal communication) points out that one criticism of phrenology was that the brain was not large enough to hold all the localizable functions that were attributed to it; despite the other problems with phrenology, this particular criticism proved ill-founded. Perhaps the limitations of genome size will prove an equally ill-founded criticism of evolutionary psychology.

later, the extreme domain-specificity of this program of evolutionary psychology collapses under the weight of its own compendium of proposed adaptations.

For sociologists, the capacity to engage in social exchange has long been considered one of the most fundamental components of human sociality. Psychological research into the heuristics and biases of human reasoning has provided insights into some robust features of exchange behavior that deviate from the maximizing predictions of a rational actor theory (see, e.g., Kahneman and Tversky 2000; Gigerenzer 1999). Although evolutionary accounts of these findings can be devised, the generative value of the evolutionary approach for explicating the psychological machinery of exchange has been less apparent. Cosmides and Tooby's "task analysis" makes strong claims about the indispensability of the evolutionary approach for understanding the organization of mind, and their work on social exchange has served as the pioneering empirical example for evolutionary psychology as a new theoretical program. While works advocating the evolutionary approach typically make no mention of any counterevidence or counterarguments to Cosmides and Tooby's social contract theory, this chapter brings together and elaborates some of the critical literature that exists. In calling into question the existence of a special "'look for cheaters' procedure," this chapter adopts a more skeptical stance toward the potential contributions of their program of evolutionary psychology for understanding the psychological underpinnings of social exchange, and it suggests that a more cautious approach to findings portrayed as established in some of the secondary literature may also be warranted.

Appendix. Results and Text of Wason Selection Tasks of Exploratory Study

	# correct	# incorrect	% correct	*p* vs. abstract	*p*. vs drinking age
Abstract	12	130	8.5	--	< .001
Africa	20	50	28.6	< .001	< .001
Bo	38	33	53.5	< .001	.12
Cassava Root	33	36	47.8	< .001	.02
Class Schedule	55	13	80.9	< .001	.02
Drinking Age	91	50	64.5	< .001	--
Diet	11	62	15.1	.13	< .001
Diet (2)	16	40	28.6	< .001	< .001
Dell	20	51	28.2	< .001	< .001
Grading	45	25	64.3	< .001	.97
Movie Rating	12	57	17.4	.06	< .001
Robyn	11	60	15.5	.12	< .001
Spilled Blood	61	13	82.5	< .001	< .001

Abstract Problem

Part of your new clerical job at the local high school is to make sure that student documents have been processed correctly. Your job is to make sure the documents conform to the following alphanumeric rule:

"If a person has a 'D' rating, then her or his documents must be marked code '3'."

You suspect the secretary you replaced did not categorize the students' documents correctly. The cards below have information about the documents of four people who are enrolled at this high school. Each card represents one person. Side A tells a person's letter rating and the Side B tells that person's numerical code.

For each document, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the rule was violated for that document.

D	F	3	7

Africa Problem

You are taking a geography course. Your instructor is very pompous and convinced that he is always right, so much so that at the beginning of the semester he offered $20 to anyone who could prove that he made a mistake in any of the material he presents to the class. In one class, in passing, he said that all of the countries in the world whose names began with the letter Z had something in common. He said the following rule was always true:

"If a country's name begins with the letter Z, then it is located in Africa."

You suspect that your geography professor might be wrong about this, and you would love to get the $20 for proving it. The cards below represent four countries. Side A gives the name of the country, while Side B gives the continent on which this country is located.

For each country, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if it contradicts your geography teacher's rule.

Zirconia	Polkastan	Africa	South America

Bo Problem

You are an anthropologist study the Namka, a hunter-gatherer people who live in southwest Africa. Bo is an old Namka man in the village that you are studying. Bo is always accidentally breaking his ostrich eggshell and would like to "stockpile" some—the Namka use ostrich eggshells as canteens because they are light and hold lots of water.

Four strangers stumble into Bo's village one day. They are hungry and want to bring meat back to their families. Bo approaches each man privately and offers him the following deal:

"If you give me your ostrich eggshell, then I'll give you dui ker meat."

Bo explains that he will need the eggshell tonight to give to his son, who is going on a long hunting expedition, but Bo will not be able to deliver the duiker meat until the next day. You suspect that Bo might try to cheat on these deals. The cards below have information about the four deals Bo made with these four men. What happened in one deal had no effect on the outcome of any other deal. Each card represents one man. Side A tells whether or not the man gave his ostrich eggshell to Bo, and Side B tells whether Bo gave the man duiker meat the next day.

For each man, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the rule had been violated in that instance.

Gave eggshell	Did not give eggshell	Received meat	Did not receive meat

Cassava Root Problem

You are an anthropologist studying the Kaluame, a Polynesian people who live on Maku Island in the Pacific. "Big Kiku" is a Kaluame village chief known for his ruthlessness. As a sign of loyalty, he makes his "subjects" put a tattoo on their face. Big Kiku has made so many enemies in other Kaluame villages that being caught in another village with a facial tattoo is, quite literally, the kiss of death.

Four men from different villages stumble into Big Kiku's village. They have been kicked out of their own villages for various misdeeds, and they are now starving. Big Kiku offers them all the following deal:

"If you get a tattoo on your face tonight, then I'll give you cassava root tomorrow."

Cassava root is a very sustaining food that Big Kiku's people cultivate. You suspect that Big Kiku may try to cheat some of these men. The cards below have information on the fates of the four men. Each card represents one man. Side A tells whether or not the man went through with getting the facial tattoo that evening, and Side B tells whether or not Big Kiku gave the man cassava root the next day.

For each man, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the rule had been violated in that instance.

Gets tattoo	Does not get tattoo	Receives cassava root	Does not receive cassava root

Class Scheduling Problem

You are choosing what courses you are going to take next semester. You have a morning part-time job that puts constraints on what classes you can take. Specifically, your schedule has to conform to the following rule:

"If a class meets on Monday or Wednesday, then it must be in the afternoon."

The cards below represent four different classes that you might be interested in taking. Side A tells the day of the week that the course meets, while Side B tells whether the course meets in the morning or the afternoon.

For each class, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the class conflicted with the rule above.

Monday & Wednesday	Tuesday & Thursday	Morning	Afternoon

Dell Problem

You recently ordered a computer from Dell, and it will arrive next week. After ordering it, you begin to worry that it might not come with a modem, which you need in order to do e-mail. You know some computers ship with modems installed and others do not. Your friend George tells you that if you bought a Dell, you don't have to worry, because every computer Dell ships conform to the following rule:

> "If you buy a comp uter from Dell, then it comes with a modem installed."

The problem is that George is often spouting off about things he knows nothing about. You know his rule is sometimes true, but is it always true? The cards below represent four different computers that people you know recently bought. Side A tells the company that they bought the computer from, while Side B tells whether or not the computer came with a modem installed.

For each computer, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if it contradicts what George said.

Dell	Gateway	Came with a modem installed	Did not come with a modem

Diet Problem #1

Medical research suggests that dietary restraint is particularly important for persons with weight problems. Consequently, doctors are told to emphasize the following rule to their patients:

> "If a person i s severely overweight, then he or she must always eat healthy meals."

You are a health journalist and want to interview people who break this rule. The cards below have information on four men, all of whom are about six feet tall. Side A tells what the man weighs, while Side B tells what they usually eat for lunch.

For each man, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the man's typical lunch breaks the doctor's rule above:

375 pounds	135 pounds	eats healthy salad for lunch	eats a dozen donuts for lunch

Diet Problem #2

Medical research suggests that dietary restraint is particularly important for persons with weight problems. The American Medical Association has now told doctors to emphasize the following new rule to their patients:

> "If a person is severely overweight, then he or she must eat veg etarian lunches."

You are a health journalist and want to interview people who routinely break this rule. The cards below have information on four men, all of whom are about six feet tall. Side A tells what the man weighs, while Side B tells what they usually eat for lunch.

For each man, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the man's typical lunch breaks the doctor's rule above:

375 pounds	135 pounds	eats lettuce salad for lunch	eats a hamburger for lunch

Drinking Age Problem

In its crackdown against drunk drivers, state officials are revoking liquor licenses left and right. You are a bouncer in a local bar, and you'll lose your job unless you enforce the following rule:

> **"If a person is drinking beer, then he or she must be over 21 years old."**

Below are cards representing four different customers sitting at a table in your bar. Each card represents one person. Side A of the card tells what the customer is drinking, and Side B tells that customer's age.

For each customer, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the customer was violating the rule.

drinking beer	drinking coke	25 years old	16 years old

Grading Problem

You are a professor. For one of your courses, you decide you want to reward those students who attend class every day. You put the following on your syllabus.

"If a student has no absences, then he or she will receive at least a C for their final grade."

At the end of the semester, you worry that you might have forgotten this rule when assigning some students' final grades. The cards below have information about four students in your class. Each card represents one student. Side A tells the number of times the student was absent, and Side B tells the final grade you assigned to them.

For each student, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if you had violated your rule for that student.

0 absences	3 absences	Grade: B	Grade: F

Movie Rating Problem

You and some friends are talking about how it arbitrary it sometimes seems about whether a movie is rated PG, PG-13, R, or NC-17 and how it isn't clear how the ratings board takes violence, sex, and profanity into account when deciding what rating to give a movie. Your friend George claims that the ratings board does have a rule that it always follows:

"If a movie contains any nudity, then it must receive at least an 'R' rating."

The problem is that George is often spouting off about things he knows nothing about. Is his rule always true? The cards below represent four different movies that have been released in the last year. Side A tells whether the movie contains any nudity, and Side B tells the rating the movie received from the ratings board.

For each class, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the movie contradicts George's rule.

Contains nudity	Does not contain any nudity	R rating	PG rating

Robyn Problem

You work for a telemarketing company that gives you a long list of people to call. Before you call someone, you would like to know what sex they are, so you can ask for them as "Mr." or "Ms." This is no problem for persons with first names like John or Mary. However, some first names, like Robin, are frustrating because they are commonly given to both males and females (as in actor Robin Williams and actress Robin Wright Penn). On your list, you see that there are a few people who spell their name "Robyn" instead. A co-worker tells you the following rule:

> "If someone spells their name 'Robyn', then they are definitely female."

In the past, your co-worker has said a number of things that you later found out weren't true, and you suspect she might also be wrong about this. The cards below have information on four people, all of whom are named either Robin or Robyn. Side A tells how they spell their name, while Side B tells whether they are male or female.

For each person, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the person contradicts your co-worker's rule.

Robyn	Robin	Female	Male

Spilled Blood Problem

You work as a government inspector of hospitals and other health care facilities. Because of the dangers of diseases that can be transmitted through blood, all hospital employees in the United States are supposed to adhere strictly to the following rule:

"If you clean up spilled blood, you must wear rubber gloves."

You are inspecting a hospital and need to know whether employees follow this rule. The following cards represent four incidents where a hospital employee was called upon to clean up a spilled substance. Side A of the card tells whether blood was among the spilled material, and Side B tells whether the employee wore gloves when cleaning up the spill.

For each incident, you are shown one side of the card but not the other. On the basis of this information, indicate whether you would *definitely* need to see the other side of the card to know if the rule had been violated in that instance.

blood was spilled	blood was not spilled	employee wore gloves	employee did not wear gloves

4

EVOLUTIONARY PSYCHOLOGY AND RAPE

> *"Armed with logic and copious data,* A Natural History of Rape *will force many intellectuals to decide which they value more: established dogma and ideology, or the welfare of real women in the real world."*
>
> —Steven Pinker, blurb for *A Natural History of Rape*, 2000

A common complaint about social scientific debates is that too often they are just exhibitions of competitive verbiage with no implications beyond some isolated quarter of the Ivory Tower. As such, there are no real negative consequences if a majority of scholars in an area choose to hold fast to some erroneous idea. Although evolutionary psychologists have sometimes complained that the academy's continued resistance to their approach is "going to increase human suffering" (John Tooby, quoted in Horgan 1999: 173), such remarks may seem melodramatic. For example, it is not clear what great societal harm would ensue if psychology got the wrong answer on what causes content effects on the Wason Selection Task (although, of course, one hopes for true understanding). On the other hand, there are some topics of social science research that have obvious gravity and potential implications for a pressing social problem. What causes men to commit rape is surely one of them, and so we would hope that neither academic politics nor political ideologies would impede whatever contributions any scientific research can make toward preventing rape.

The opening quote from Pinker is on behalf of a book that presents an evolutionary psychological perspective on rape. By making plain that "the welfare of real women in the real world" is at stake, Pinker provides a pre-emptive strike against any who would dismiss *A Natural History of Rape* (*NHR*) out of some kneejerk or partisan reaction against evolutionary psychology. Feminists would seem the most obvious targets of this comment The authors of *NHR*, Randy Thornhill and Craig Palmer, make plain their stance that feminist scholars and activists have stood in the way of the development of a genuinely scientific (i.e., evolutionary psychological) approach to rape. From the beginning of their book, they criticize feminists for, in their view, promoting the theory that rapists are never in any way motivated by sexual desire, a theory they say "is based on empirically erroneous, even mythological, ideas about human development, behavior, and psychology" (p. xiii). They charge that feminist scholars have, by sticking to an incorrect theory and resisting the insights that evolutionary psychology can offer, stymied the creation of effective rape prevention programs and may, consequently, even be responsible for increasing the occurrence of rape (p. xii).

These are strong charges, and they are stridently advanced in the book. Thornhill and Palmer anticipated that their book would draw strong criticism, especially from those who might be taken as representing feminist perspectives, and they were right. Susan Brownmiller (2000) calls the book "baloney"; Barbara Ehrenreich (2000: 88) calls it "daffy"; Natalie Angier (2000: 81) says it is "frothily free of real scientific data"; and Margaret Wertheim (2000) says that she's had "bowls of spaghetti that were more tightly structured than this argument." The book also drew a highly negative review from primatologist Frans de Waal (in *The New York Times* [2000]) and two negative reviews from evolutionary biologist Jerry Coyne (in *Nature* [Coyne and Berry 2000] and *The New Republic* [2000]). Against such criticisms, Thornhill and Palmer have provided a pre-emptive rebuttal when they contend that many feminists, social scientists, and other opponents of evolutionary psychology do not read work in the area fairly or competently. In an earlier *New York Times* article on the book (1/15/2000: A21), Thornhill anticipates that the book would provoke criticism: “The problem is basically a very limited understanding of how evolutionary biology applies to people. It takes people a long time to pull back from what is politically correct and do an analysis on it.” The harsh reception might thus be explained away as reflecting ignorance of principles of evolutionary psychology and the pervasiveness of “established dogma and ideology” about rape among academics and public intellectuals. Criticism of the book might even be seen as *predictable* given public hostility to Darwinian explanations. The authors’ pressing ahead with publication despite the prospects of such strong fallout might thus be regarded as an act of intellectual determination. Thornhill and Palmer (2000b) write, “Why have we chosen to make such an argument knowing full well the controversy that challenging such a widely held position would cause? The answer is that inaccurate knowledge about the causes of such behavior hinder attempts to change it, and we want very badly to eradicate rape.”

The book had already generated so much controversy prior to its publication that MIT Press pushed up the publication date of *NHR* to take advantage of it. Few academic presses get to advertise a book with a slogan like “You’ve seen the coverage. Now read the book”, as MIT Press did in some of its ads for NHR. NHR received considerable exposure in newspapers and magazines, as well as a segment on *Nightline*.

The book also helped land evolutionary psychology a large feature on the front of the *New York Times* science section. The animating angle of the *Times* story was to clarify that *NHR* "was not, as some assumed, a fringe theory developed by a pair of renegade researchers.”

> Rather, the arguments made by Dr. Thornhill and Dr. Palmer fit into a larger theoretical framework, the work of a group of scientists who have ushered Darwin into new and provocative areas...."

> And while some of these researchers, who call their approach 'evolutionary psychology', ...would quibble with some of the methods used and conclusions drawn by the authors of the rape book, most would endorse the larger principles that underlie the work. (Goode 2000)

Thornhill and Palmer's work was thus presented as an example of "normal science" within the new paradigm of evolutionary psychology. The presentation of NHR as paradigmatic evolutionary psychology is strengthened on various grounds: Thornhill is one of the most cited authors by his fellow evolutionary psychologists (see Table 5.1); Pinker leads a series of prominent sociobiologists or evolutionary psychologists in providing blurbs for the book jacket; Margo Wilson wrote the book's foreword; and John Tooby and Leda Cosmides wrote an angry letter to the *New Republic* in response to evolutionary biologist Jerry Coyne's negative review.

Table 5.1 Individuals most cited in two general treatments of evolutionary psychology

Index entries in the *Handbook of Evolutionary Psychology* (1998)			Works cited in David Buss's *Evolutionary Psychology* textbook (1999)		
1	John Tooby	94	1	David Buss	37
2	Leda Cosmides	92	2	Martin Daly	21
3	Martin Daly	78	3	Margo Wilson	19
4	Margo Wilson	74	4	John Tooby	13
5	David Buss	61	5	Leda Cosmides	11
6	Donald Symons	59		Ted Shackelford	11
7	Robert L. Trivers	53	7	Douglas Kenrick	8
8	Richard Alexander	47		Randy Thornhill	8
9	Charles Crawford	42	9	Donald Symons	7
	George C. Williams	42	10	Richard Dawkins	6
	Edward O. Wilson	42		W.D. Hamilton	6
12	W. D. Hamilton	37		David Sloan Wilson	6
13	Charles Darwin	36			
14	Randy Thornhill	34			

The Tooby and Cosmides letter accused Coyne of attributing to Thornhill and Palmer claims that the authors did not make. "Coyne congratulates himself for (supposedly unlike others) evaluating [NHR] as a 'scientist,'" they write, but:

> If so, he needs to reacquaint himself with such scientific basics as a committment to be factually accurate rather than to originate falsehoods, to

> learn at least a bare minimum about the fields he pronounces on, to ground generalizations on a representative set of cases rather than typologizing tendentiously from imaginary or idiosyncratic examples, and, when engaged in critical evaluation, to fairly and accurately characterize the views of those criticized rather than to propagate academic urban legends.

Coyne does claim that much of NHR is devoted to "proving the truth" of the theory that rape is an evolutionary adaptation, but the book is actually quite ambivalent about this proposal, reflecting the authors' disagreement on the issue—Thornhill is much more inclined toward the adaptation hypothesis than is Palmer. Coyne also contends that most evolutionary psychologists believe "that virtually every human action or feeling... was put directly into our brains by natural selection." This is incorrect, as I have already discussed (Chapter 2). Thornhill and Palmer repeat George Williams's (1966: 4) famous dictum that "adaptation is a special and onerous concept that should be used only where it is really necessary." Such apparent errors, especially within what is presented as a self-consciously scientific critique of the book, only increase the plausibility of the counter-argument that the harsh reaction to *NHR* is a product of unscientific hostilities toward evolutionary explanation that would have been present regardless of the book's merits.

Critics have also given much attention to some of the policy suggestions in *NHR*, such as the authors' criticisms of rape reform laws and their ideas for how women can decrease their likelihood of being raped (including dressing more demurely and dating with chaperones). These are easy targets: Thornhill and Palmer's proposals have been criticized as naive and unlikely to work even by evolution-minded scholars who are otherwise supportive of the book.[1] These policy suggestions also make it easy for critics to decry the book as politically reactionary, rather than being objective science, since the policy recommendations

[1] Lionel Tiger (interviewed in Washington Post 1/28/2000, p. C01; Tooby and Cosmides 2000).

are consistent with what could be regarded as a politically conservative stance. Thornhill and Palmer do not help themselves in this regard with the passion of some of their criticisms of feminism and mainstream social science. Then again, dubious policy recommendations could just be bad derivations from a sound scientific and theoretical foundation, and, in fact, a useful role for an evolution-minded sociology could be translating the insights of evolutionary psychology into more feasible and effective social interventions.

Open for examination, then, is the question of how well NHR brings an evolutionary perspective to the social problem of rape, which this chapter intends to address. Keeping with the spirit of the rest of the dissertation, I will confine my attention to the book's substantive arguments and the science and scholarship marshaled on their behalf. Unlike some of the work examined in other chapters, *NHR* is not dedicated to the elaboration of a single theory. Instead, it offers a diverse set of arguments on different issues, joined by the theme of arguing for why an evolutionary perspective is valuable for understanding rape and why existing social science perspectives have not been valuable. Accordingly, my discussion of it will engage the book on various fronts.

I begin by considering Thornhill and Palmer's representation of the "social science explanation of rape." Next, I consider their treatment of what I see as being two false dichotomies: that human rape is motivated either by sex or violence, and that human rape is either an evolutionary adaptation or an evolved by-product of other adaptations. I argue that their perspective places too strong of an emphasis on the role of female mate choice in determining the expression of male sexuality, and I contend that they overestimate the value of evolutionary argumentation for making judgments about the environmental causes of behavior. After this, I take up Thornhill and Palmer's theoretical treatment of the psychological pain experienced by rape victims, and I argue that *NHR* overstates the evidence in favor of the theory.[2] Taken together, these various discussions raise important questions about not just the arguments of NHR about rape, but fundamental aspects of its approach, which may then also be reasons to be careful about similar "strong form" works of evolutionary psychology applied to social problems.

A STRAW FEMINIST?

As we have already noted, Thornhill and Palmer ultimately take no position on what would seem to be a critical question for an evolutionary psychology account of rape: are there specialized adaptations in the brain that developed specifically for the perpetration of rape (just like there are supposedly specializations for detecting cheaters), or does rape occur as a by-product of psychological mechanisms that were not in any way evolved to promote rape? Thornhill and Palmer contend that an evolutionary perspective still provides valuable insights even though this question has not been answered. With the seemingly central question unresolved, the main contribution that they see an evolutionary perspective offering is by emphasizing the importance of sexual motivation to rape. The evolutionary view is opposed to what Thornhill and Palmer call "the social science explanation of rape," where they explain that "social science" in this sense is used interchangably with "feminist psychosocial analysis."[3] The opposing view is that rape is a behavior that individual rapists

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[2] Unfortunately, as I describe below, I had begun this chapter with the hope of its including as an empirical component a re-examination of the data that are used to support the theory of psychological trauma that is recounted in *A Natural History of Rape*. I have not as yet been able to obtain these data. At the same time, the consideration I had given to the book in preparation of this expected analysis had suggested enough, other theoretical points of consequence for evaluating the book that the chapter is included even though the potential empirical component is absent.

[3] Zuleyma Tang-Martinez's phrase "a feminist psychosocial analysis" [1997: 122] accurately describes what has become the dominant explanation of rape in the social sciences over the last 25 years... Because the phrase "feminist psychosocial analysis" is a bit awkward, we will refer to it as "the social

"learn" from their experiences with society, and more specifically that what rapists learn is the desire to dominate women and the belief that rape is a particularly effective means of achieving this domination:

> To the general framework of learning theory many feminist social scientists added the assertion that 'sexual coercion is motivated by power, not lust'... In combination, learning theory and the feminist assertion that rape is motivated by a desire for control and dominance produced the view that rape is caused by supposedly patriarchal cultures where males are taught to dominate, and hence rape, women. (p. 124-5)[4]

Consequently, what Thornhill and Palmer confront as "the social science explanation of rape" is largely captured by the familiar slogan that "rape is violence, not sex," if that slogan is read as a theory of rapist motivation (instead of, e.g., a statement of how rape should be regarded by society and treated in the legal system).

Thornhill and Palmer provide many reasons why the proposition that a desire for sex is never in any way part of the motivation underlying rape is difficult to defend. When admitted rapists are asked why they did it, they frequently report sexual desire as one of their motives, and so denying this motive requires that an explanation for why rapists are so often lying or self-deceived about their own motivation (see *NHR* pp. 135; Groth 1979; Scully 1990; for discussion of evidence that sexually aggressive men cite power and anger as motives more than sex, see Crowell and Burgess 1996: 59). Thornhill and Palmer marshal this and other evidence in building their case against the "violence, not sex" theory. Women of the ages most strongly associated with sexual desirability are targets of rape more often than are, for example, more vulnerable elderly women and more powerful high-status middle-aged

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science explanation" (p. 123) The interchangeable use of "feminist" and "social science" is maintained throughout the book.

[4] The embedded quotation here is from Stock (1991: 61). A few pages later, Thornhill and Palmer the following elaboration of the social science position: "[V[iolent behavior is something that exists only when it is taught, and individuals will find sexually attractive only those beings and other objects in the environment that they are told to find sexually attractive" (p. 129)

women (Kilpatrick et al. 1992).[5] Evidence also suggests that even many rapes by strangers often do not involve more physical violence than is needed to overcome the victim's resistance (see *NHR* pp. 136-8).

An equally relevant question, however, is how accurately the "not sex" position characterizes the prevailing thought of feminist/social scientists on rape? Just because this is a slogan used by some feminist activists does not mean that this is the consensus position of those feminists who conduct rape research, which would seem a minimal requirement for declaring it "the social science explanation of rape." If the idea that rape is not "not sex" is to be credited as an important contribution of the evolutionary perspective to our understanding of why men rape and how they can be prevented from doing so, then Thornhill and Palmer must be accurate in their claim that the "not sex" theory of rapists' motives represents a consensus position among those who study rape from a feminist/social scientific perspective.

Thornhill and Palmer point to statements made by feminists (or statements made characterizing the feminist position) that seem clearly to adopt the stance that rape is never motivated by a desire for sexual gratification. Yet, despite the impression given by the material Thornhill and Palmer quote, feminist responses to *NHR* have been virtually uniform in asserting that these evolutionary psychologists are attacking a position that few current feminist social scientists actually hold (Brownmiller 2000; Koss 2000; Ehrenreich 2000). In evaluating such responses, however, we must be cognizant of the possible revisionist biases of hindsight—supporters of a position can always try to respond to its refutation by saying "well, we never really believed that anyway." So a key question that I

[5] Although the large number of child rape victims found in the Kilpatrick et al. study—29% were 11 years of age or younger—is hard to reconcile with a motivation for sex with fertile young women.

sought to examine is whether Thornhill and Palmer have represented the existing feminist literature on rape fairly and accurately.

In this regard, Thornhill and Palmer themselves acknowledge more than once that the position they are ascribing to feminists is one that feminists have regularly disavowed. In their introduction to the chapter on "The Social Science Explanation of Rape," they write:

> We have been told that some of the positions we are about to criticize have been abandoned by social scientists studying rape. We are not convinced. Not only does the recent literature on rape repeat these positions; assertions reflecting these positions often continue to be made by the same people who claim that the arguments have been abandoned [citing Palmer et al. 1999]. Hence, we feel that our use of the label "the social science explanation" is quite justified. (p. 123)

If we grant that individual feminist scholars do contradict themselves on this score, we can either assert that they are being (perhaps unconsciously) *disingenuous* when they assert one part of the contradiction or that they are *ambivalent* or *inconsistent* in their views. The above passage is one of several in which Thornhill and Palmer seem to suggest the position that "rape is violence, not sex" reflects the *real* feminist/social science position on the motivation of rapists and statements disavowing this are instances of scholars *mischaracterizing* their own true beliefs.

Yet, arguments about what other authors *really* believe, in the face of statements by these authors to the contrary, skate on thin scientific ice.[6] One way to evaluate the fairness of Thornhill and Palmer's claims about the positions of feminist scholars is to go back and check the original feminist sources from which they quote. When I did so, I found indications that Thornhill and Palmer may have quoted selectively in substantiating the "not

[6] We might expect sociobiologists and evolutionary psychologists to be particularly sensitive to the potential misuse of this tactic, as early days of sociobiology yielded numerous such unfair critiques in which Wilson, Dawkins, and others were subjected to "moral readings" in which isolated sentences were held up as true indications of what the authors really-truely-actually believed [Segerstråle 2000].

sex" position as the feminist consensus on the motivation of rapists. The issue is worth examining at length, since the idea that evolutionary psychology reveals the error of the "not sex" explanation is central to their claims that an evolutionary perspective can be useful for helping to stop rape.

EXAMPLE #1—PEGGY SANDAY

What Thornhill and Palmer write: "[Peggy] Sanday (1990:10) states that during rape 'the sexual act is not concerned with sexual gratification but with the deployment of the penis as a concrete symbol of masculine social power.'" (Thornhill and Palmer 2000, p. 125)

Although this quote is presented as if it were a *general* statement about rape, it is actually from a book titled *Fraternity Gang Rape* and concerns this specific type of rape only. Indeed, the word "rape" does not appear on the page in which the quoted text appears, but instead Sanday there refers to her subject matter exclusively as "pulling train." The quote in its context: "The male sexual bonding evident in 'pulling train' is a sexual expression and display of the power of the brotherhood to control and dominate women" (Sanday 1990: 10).

Given that gang rape is a form of sexual assault that males commit in front of other males—and for which there is evidence of males sometimes pressuring reluctant males to participate (Sanday 1990)—we should perhaps not be surprised that a book on the topic would emphasize the themes of male bonding, dominance, and masculinity, but this need not be intended as comprising a general and exclusive motivational theory about all rapes.

EXAMPLE #2—SUSAN BROWNMILLER

What Thornhill and Palmer write: "Susan Brownmiller herself endorsed the 'feminist' view she had popularized in the 1970's: 'The central insight of the feminist theory of rape identifies the act as a crime of violence committed against women as a demonstration of male domination and power.' (Brownmiller and Mehrlof 1992: 382)." (Thornhill and Palmer 2000, p. 126)

One might first wonder why is it news that Brownmiller "endorsed" a position that she "had popularized" two decades earlier. Here is the quote in its fuller context:

> The central insight of the feminist theory of rape identifies the act as a crime of violence committed against women as a demonstration of male domination and power. The sexual motivation, orgasmic release, is a secondary component. [Randy] Thornhill and [Nancy Wilmsen] Thornhill [1992] misread feminist theory. Brownmiller [1975] did not suggest that rape is 'primarily or solely' caused by arbitrary differences in the way men and women are socialized about heterosexual conduct. (p. 382)

The sentence immediately following what Thornhill and Palmer quote emphasizes that the feminist position is *not* that rape is "not sex" but that sexual motivation is *secondary* to nonsexual motivation (which, as we will see, is a real difference from the position Thornhill and Palmer take, but this is much more reasonable point of debate than the alleged rejection of a sexual motivation to rape altogether). Moreover, as one can see above, *the passage from which the quoted text is taken is actually an explicit disputation of how Thornhill has read feminist theory*. Over Brownmiller's objection, *NHR* repeats Thornhill's contention that feminist theory on rape focuses almost entirely on socialization (or "learning theory" as Thornhill and Palmer consider it, see pp. 123-4).

EXAMPLE #3—KIMBERLY DAVIES

What Thornhill and Palmer write: "Davies (1997: 133) stated that it is a contention of feminists and 'that rape is an act of power, not sex.'" (p. 126).

The cited article is based on the dissertation research of a graduate student and appears to be the only paper that she has ever published. The article is not a work *of* feminist theory or even a work *about* feminist theory, but instead it reports results from a survey on men's exposure to pornography and their attitudes about feminism and rape. The quote in its context:

> Garcia found that those exposed to coercive sexual material held more traditional attitudes toward women and believed rapists should be severely punished. However, those exposed to mostly nonviolent pornography believed that rape is an act of power, not sex, which is a contention of feminists, findings that are more in agreement with the work of Linz et al. (1988). [end of paragraph]

At least to me, this quote does not provide evidence of a consensus feminist position, as it is an in-passing statement in an obscure article by an obscure author who does not claim this "contention" as her own and provides no citations for it.

EXAMPLE #4—ELEANOR SMEAL

What Thornhill and Palmer write: "In 1994, after hearing Eleanor Smeal of the Fund for a Feminist Majority testify that 'rape is never an act of lust', (quoted in Shalit 1993: p. 7B) the US Congress passed the Violence Against Women Act...". (p. 161)

This sentence might be read as suggesting that Eleanor Smeal's testimony was in some way linked to the passage of the Violence Against Women Act, but Smeal actually never testified before Congress regarding VAWA (Julie Bernstein, Feminist Majority Foundation, personal communication 8/3/2000). Instead, her comment was apparently delivered to a reporter as a response to a question about the motivation of rapists that was provoked by an argument by Senator Orrin Hatch suggesting that the moral/legal culpability of a rapist should be somewhat mitigated if the rape was motivated by a "lust factor" (see Muehlenhard et al.

1996). Smeal's remark is thus a sound-bite response by a political activist to a political argument by a powerful Senator.

EXAMPLE #5—CHARLENE MUEHLENHARD

What Thornhill and Palmer write: "That the "not sex" explanation remains popular among feminists, and that it has dominated feminist writings on rape, is even admitted by some individuals who point out that not all feminists have supported it. For example, Muehlenhard et al. (1996: 129) admit that "in general, ... feminist theorists have emphasized the goals of dominating and controlling rape victims and women in general." (p. xx)

Even as they present it, we can question whether feminist theorists *emphasizing* the role of goals other than sex in rape should be read as equivalent to saying that sexual desire is *not also* part of the motivation of rapists. Logically, the statement would seem not even to imply a belief that a desire to dominate or control women is a stronger motivating factor in rape than the desire for sex: scholars who look at pay inequality from a feminist perspective might emphasize inequalities by gender, but this does not entail that they believe that gender is the single most important determinant of how much a worker is paid.

Setting this issue aside, are the authors in this quoted passage "admitting" that the "not sex" explanation "dominates feminist writings on rape" while "pointing out that not all feminists" endorse it? The quote in its context:

> In general, nonfeminist theorists have emphasized that the goal of sex, and feminist theorists have emphasized the goals of dominating and controlling rape victims and women in general. *It would be inaccurate, however, to say that feminists claim that sex is never the goal of rape* (Muehlenhard et al. 1996: 129; emphasis added).

The *very next* sentence from what Thornhill and Palmer quote explicitly disavows the position that Thornhill and Palmer seek to attribute to feminist perspectives. Far from saying just that the "not sex" explanation is something that "not all" feminists endorse, Muehlenhard et al. deny altogether that the idea that rape is categorically not sex is a feminist claim.

In all five cases, Thornhill and Palmer's quotes from these sources seems to present the authors as evincing the 'not sex' explanation of etiology more strongly than they appear to when we consult the original source. Indeed, in two cases, the sentence *immediately following* what Thornhill and Palmer quote is a qualification about sexual motivation that seems to warn against the very interpretation that Thornhill and Palmer present. The five quotations considered above are not the worst cases from some larger number that I have re-examined; instead, *these were the first five quotes that I looked up.* This exercise has led me to conclude that Thornhill and Palmer's representations of the writings of scholars with opposing views are not always reliable. This gives merit to the critical response that the "not sex" explanation that Thornhill and Palmer attack is a straw actor. In the next section, I question Thornhill and Palmer's opposition of sex and alternative motives for rape, as well as their opposition of "adaptation" and "by-product" as possible ultimate causes of rape.

TWO INADEQUATE DICHOTOMIES

1. Is rape motivated by sex or violence?

In articulating their own position on the question of what motivates rapists, Muehlenhard et al. (1996: 123) note the complexities of making categorical assertions about motives in the first place and conclude that "to say that rape is motivated by sex or violence or by any other single factor is likely to be overly simplistic" (see also Koss 2000). This may seem far removed from the extreme position that the Thornhill and Palmer present as "the social science explanation of rape." Indeed, more generally, we can perhaps imagine the terrain of the debate over the causes of rape as being a planet whose poles are easily depicted. The North Pole is that all rapists are motivated exclusively by a desire to hurt or dominate women, and that the rapist gets sex in the process is just incidental. The South Pole is that all rapists are motivated exclusively by a desire to get sex, and that the victim gets physically or psychological hurt in the process is not part of the rapists' intention. As is the case with most metaphorically polarized debates in academia, relatively few scholars seem to actually live at either pole, and those that do tend to be at some remove from where the bulk of Northerners or Southerners live. Positions that cite the complexities of motivation and the potential diversity of different motivations in different cases would seem to reside squarely within such temperate zones, and such would seem to be the best characterization of most "feminist psychosocial analysts" and "social scientists" on the matter. This is not to say that all middle-ground positions are the same or that the differences among them are trivial: some (or even many) feminists may well not give enough attention to sexual motive as a cause of rape, and they might overemphasize nonsexual motives (examples of this from my own reading that I would cite are Gilmartin-Zena 1988 and Stock 1991). But the opportunity for this potentially constructive critique is lost when Thornhill and Palmer try to attribute the position that rape is never sexually motivated to feminists/social scientists.

Meanwhile, Thornhill and Palmer seem to interpret the debate about the motivation of rapists as being much more of an either/or question than do many of those scholars that they criticize. Thornhill and Palmer's critique of feminists sometimes reads as a cataloguing of the reasons why the explanation that rape is not at all sexually motivated in order to convince the reader that rape is almost entirely sexually motivated instead. That is, they take great pains to debunk one extreme position, but seemingly toward the end of replacing it with one that seems virtually as extreme—a new etiological slogan: rape as sex, not violence. This argumentation seems to skip over a vast middle ground of positions that acknowledge the potential multiplicity of motives for rape behavior. In this way, Thornhill and Palmer's analysis inadequately appreciates the possible complexity of motives involved in any instance of rape, not to mention the diversity of types of events that are all collected under the single, umbrella term of "rape."[7]

Perhaps where Thornhill and Palmer's attempt to discount any non-sexual motive for rape seems most excessive in their discussion of rape in wartime. As is well-known, the rampant rape of the women of a conquered group by the conquering male soldiers has been a recurrent feature of war throughout history (Brownmiller 1975). Thornhill and Palmer seek to account for the elevated frequency of rape in war as the result of men having the opportunity to rape without the threat of retaliation:

> Rape in war also shows that men pay attention to benefits and costs during rape. The war context provides the proximate benefit of mating with young and thus attractive women. The proximate costs of rape are low, since there is little or no protection of female targets by family and mates and since punishment for rape is relatively unlikely. (p. 66)
>
> Rape by conquering soldiers is common because the benefits are high (many young women are available) and because the costs are low (the women are vulnerable; the rapists are anonymous and relatively free from sanctions against rape). (p. 194)

[7] Because their evolutionary perspective identifies the erotic so strongly with what maximized reproductive success in the Pleistocene, their analysis also does not appreciate the possibility of the eroticization of violence as one way that sexual and nonsexual motives can combine in the perpetration of sexual assault. As Beneke (1982: 16) writes, "For a man, rape has little to do with sex, we may as well add that sex itself often has little to do with sex, or, if you like, that rape has plenty to do with sex as it is often understood and spoken about by men" (quoted in Stock 1991).

If the incidence of rape in war can be reduced when leaders of the conquering party make plain that they will punish soldiers who are found to have raped, then no doubt this is *part* of the explanation, as this is saying only that rapists respond to disincentives. But is this changing balance of costs and benefits sufficient to explain the higher frequency of rape in war? Alternatively, we might consider that the frequency is also increased by a loosening of the inherent moral constraints against rape because the women are seen as being members of the hated, just-defeated outgroup. We take for granted that soldier's moral compunctions about killing the enemy are reduced when they hold a "demonized" or "dehumanized" view of that enemy; might not this demonization of the enemy also lower inhibitions against raping the enemy's women? Put another way, do we want to rule out entirely the possibility that a soldier's desire to rape is increased by the knowledge of the pain that the rape will cause to the woman, her mate (if still alive), and her family, given that the soldier is in the midst of or just finishing a fight against this outgroup? Thomas and Ralph (1998: 211) write:

> [A]n overemphasis on gender alone... can obscure other characteristics of a woman's identity that determine which women are raped. In Bosnia, a woman's religion or nationality as well as her gender makes her a target for rape. In Burma, government soldiers rape Rohingya women, thus identifying their victims by their sex *and* their ethnic affiliation. Rape by the security forces in Peru is strongly determined by race and class: rape victims are overwhelmingly poor and brown-skinned. And Somali women refugees report that they are asked by their rapists to which clan they belong. Women who are in the same class as their attackers may still be robbed, but often are spared rape.

I am not proposing that perceived impunity is not also part of the explanation. Instead, I am proposing that conquered women being members of a hated outgroup could contribute to the higher frequency of rape in war, *in addition to* the cost-benefit alteration that Thornhill and Palmer describe. The focus on wartime rape as opportunistic sex also misses the opportunity to see the potential strategic functions that rape might serve for an army in war (demoralizing the enemy, increasing the desire for the enemy to flee), and how raping enemy women may be encouraged by army officers for these reasons (Thomas and Ralph 1998). Regrettably, Thornhill and Palmer do not consider any such pluralistic explanation in any discussion of rape in war in their book, but instead give the impression that we might expect rape to be similarly rampant against ingroup members when men are given the chance to commit rape with impunity.

2. Is rape an adaptation or by -product?

With the question of whether rape is motivated by sex or by something else, Thornhill and Palmer could be criticized for seeing things as a binary choice that are almost certainly more complex. Perhaps a similar inclination is evinced in their consideration of the question of whether any rape-specific adaptations exist. As noted, extensive but inconclusive consideration is given to a question posed as a chapter subheading: "Human Rape: Adaptation or By-Product?" (p. 59) As Thornhill and Palmer put it, "there are currently only two likely candidates for ultimate causes of human rape:" (p. 59)

> 1. "It may be an adaptation that was directly favored by selection by way of increasing mate number. That is, there may be psychological mechanisms designed specifically to influence males to rape in ways that would have produced a net reproductive benefit in the past..." (p. 59)
>
> 2. "It may be only a by-product of other psychological adaptations, especially those that function to produce the sexual desires of males for multiple partners without committment."

As noted, Thornhill and Palmer pose the question "Adaptation or By-Product?" but do not provide any conclusive answer, but instead they disagree on what they believe the likely answer is, with Thornhill more strongly inclined than Palmer toward the "adaptation" possibility. To understand what they mean by adaptation, Thornhill and Palmer more than once cite the male scorpionfly as their exemplar of rape-specific adaptation (see pp. 63-64, 79-80). The male scorpionfly has a clamp on the top of his abdomen that is used for holding a resistant female in place while he inseminates her. Evidence for the clamp as a rape-specific adaptation is that the structure of the clamp exhibits functional design for carrying out this specific task effectively; the clamp has no other known use; and use of the clamp increases the ability of the scorpionfly to inseminate a resistant female and thereby increases the fly's reproductive success. Thornhill and Palmer write that "what is needed to support the human rape-adaptation hypothesis is evidence of a phenotypic feature in the human male analogous" to this clamp (p. 63). Since obviously human males do not possess a morphological analogue to the clamp, Thornhill and Palmer's search for rape-specific adaptations focuses mostly on possible psychological adaptations, which are obviously much more difficult to positively identify (or to rule out) as adaptations than the scorpionfly's rape clamp.

A different way of looking the "adaptation or by-product" question, however, is to wonder whether it represents an oversimplification of the domain of possibilities. Thornhill and Palmer are left with the adaptation vs. by-product opposition after they systematically eliminate what they consider to be the other alternatives. These other alternatives are eliminated if they cannot stand as a *general explanation* of why rape occurs (that is, if it cannot account for all rapes). For example, they rule out the alternative "human rape is the result of an evolutionarily novel environment" on the grounds that rape occurs in all known cultures and that "the fact that rape is seen is many non-human species is further evidence that evolutionary novelty *is not a useful general explanation*" (p. 58, emphasis added). If an alternative cannot explain all types of rape, then it is eliminated from consideration as a worthwhile explanation of any. But this logic of elimination never considers another possibility: that no useful general explanation exists.

Instead, at least in my reading, an operating assumption of *NHR* is that a relatively unitary causal explanation of the psychology of rape exists and that it is only a matter of figuring out if it is an adaptation or a by-product. Notice the pronoun "It" that begins both of the enumerated alternatives above, and then consider the diversity of activities that serve as the referent for this pronoun: the soldier who rapes during war; the stranger who rapes a woman walking alone at night; the man who rapes a woman he is dating; the older male relative who rapes young girl placed in his care; the man who breaks into women's homes and rapes them; the husband who rapes a defiant wife; the man who slips roofies into a woman's drink and rapes her when she falls unconscious; and the fraternity males who commit gang rape. To what extent should we expect this diversity of behavioral phenomena to have a useful, general psychological explanation?

In any case, are the adaptation and by-product "hypotheses" really mutually exclusive? Thornhill and Palmer seems to invite confusion when they ask whether a behavior is an adaptation but adopt the evolutionary psychological position that the key issue is whether any of the psychological mechanisms implicated in the act were selected for specifically because they facilitated the successful perpetration of rape. A separate logical possibility is that a rape-specific adaptation could exist, but that it could only operate in the perpetration of *some* instances of rape. It would take a complete refutation of the by-product hypothesis to imply the conclusion that rape only occurs because of the existence of rape-specific adaptations. Put another way, if we had the power to intrude into the genome and eliminate the genetic basis of any rape-specific adaptations, we would still have no reason to presume that the fruit of this intervention would be the nonoccurrence of human rape. Instead, thinking about things from a purely evolutionary perspective, we would seem to be talking about a difference in *rates*: what we might call the by-product baseline rate, or the frequency of rapes that would occur in the absence of any rape-specific adaptation, and the adaptation-modified rate that includes all the by-product rapes and the others that only occurred because of the presence of the adaptation(s).[8] What this would imply is that the crucial issue for evaluating selection pressures might not be the well-worn question of whether committing rape was adaptive for Pleistocene man, but whether exhibiting a different sort of rape behavior than what would be produced by by-products alone was adaptive. This said, how such a question might be answered is not easily discerned, and its answer would still not imply that such adaptations actually existed, as part of the human male psychology.

FEMALE CHOICE AND MALE SEXUALITY

> *"I think one of the reasons straight guys are so violent and crazy is that sex is hard to find when you're a straight guy. It really is."*
> —Dan Savage (gay sex advice columnist), interview, *Ms. Magazine*, 2000[9]

[8] An interesting possibility that Thornhill and Palmer do not consider is that the by-product baseline rate could be higher than the adaptation-modified rate; that is, that what specialized psychological adaptations actually do is prevent some rapes that would otherwise occur. This would be not unlike Daly and Wilson's proposition regarding child abuse that the relevant adaptations do not cause the abuse of stepchildren by stepparents but prevent the abuse of biological children by their biological parents.

[9] April/May issue, p. 79.

Earlier, I noted that when Thornhill and Palmer seemed to give credit to evolutionary psychology for displacing the "not sex" view of rapist motivation that supposedly dominated the social sciences, reviewers challenged the contention that the "not sex" explanation was an accurate representation of the prevailing view of feminist/social science scholars about why men rape. In addition, Thornhill and Palmer sometimes write as if the idea of rape as sexually motivated did not already have a long history in the public consciousness. Indeed, the feminist emphasis on non-sexual motivation could be seen historically as a reaction against a perceived, longstanding public tendency to overemphasize the sexual motivation of rapists, especially with regard to date rape (e.g., Estrich 1987; Warshaw 1988; Bourque 1989; Spohn and Horney 1992; for a discussion of cross-cultural differences in public attitudes about rape, see Ward 1995: 55). Consequently, even if we were to conclude that rape is motivated exclusively by a desire for sexual gratification, evolutionary psychology could not claim credit for delivering to the world this insight about proximate cause.

To talk about rape as a crime of sexual desire is to point to a proximate asymmetry in the sexual desires of men and women: namely, that (especially young) males typically covet sexual access to a much larger number of females than what are willing to give such access to them. Another way of saying this is that women tend to be much *choosier* than are men regarding the targets of their sexual desire and especially their willingness to engage in sexual behavior. That we have observed pronounced changes in female sexual openness and behavior within a couple generations in contemporary American society would seem to indicate that the extent and expression of this asymmetry is quite malleable. At the same time, I believe that we are almost certainly harboring an illusion if we maintain that innate dispositions are irrelevant to the matter: as far as we know, *every* human culture conceives of sex as more a matter of male pursuit and female choice than the other way around (Symons 1979).

An evolutionary perspective can help us understand why this innate sex difference in desire seems to exist (Trivers 1972; Symons 1979). The minimal "start-up cost" required to bring a child into being is much larger for women than for men: ejaculation for men vs. an egg, nine months of gestation, and a risky childbirth for women. Moreover, this higher initial cost can be used to derive the prediction that women will be willing to invest more of themselves in raising a child—since mothers have more of their reproductive potential bound up in any one child than fathers do—and breast-feeding alone constitutes a significant tax on a mother's caloric resources and delays the ability to give birth to or care for other children. The implication of all this is that, from an evolutionary standpoint, men have little reason to be discriminating in their mating behavior.[10] Meanwhile, for women, it makes sense to be highly discriminating, as becoming pregnant by a man implies a heavy subsequent investment as well as the opportunity cost of not being able to bear another males' child during this period. All else being equal, selection would then be expected to favor men who followed less discriminating mating strategies (at least in the short-term) and women who followed more discriminating strategies.

This difference in level of discrimination alone would be sufficient to produce conditions in which a male desire for sex was very often thwarted (Symons 1979). But also, since females essentially provide the limiting resource for the production of offspring, we can also predict keen competition among men for females, with the end result that some men

[10] At least in the absence of any primitive equivalent of the "shotgun wedding," in which some mechanism existed for requiring further investment in the event of a pregnancy.

end up fathering children by multiple mates and other men end up never getting to mate at all (Symons 1979; see also Miller 2000).[11] In short, the males who are the least desirable to females are in a double bind: male mating opportunities generally are circumscribed by female choice, but their own opportunities are circumscribed even more by the fact that other men are far more likely to be the beneficiaries of female choice than they are.

To see rape as sexually motivated is to see physical coercion as a tactic for obtaining sex from unconsenting females—an extreme tactic, to be sure, and a risky one, given the possibility of subsequent retaliation and punishment (Thornhill and Palmer 2000). Consequently, one might expect it to be a tactic more commonly employed by those who cannot attain mates through other means. This hypothesis has been attractive to some evolutionary psychologists (Thornhill and Thornhill 1987; Lalumiére, Chalmers, Quinsey, and Seto 1996). It has been called the *mate deprivation hypothesis*, although I prefer another evolutionary psychologist's term for it—the *rapist-as-loser hypothesis* (Malamuth and Heilman 1998). Evidence for the rapist-as-loser hypothesis does not imply anything about whether the male mind contains rape-specific adaptations—that is, you could derive the prediction even if you believed there were no such adaptations—so it is not really relevant to the "adaptation or byproduct" debate that I have already mentioned. In conjecturing possible rape-specific cognitive adaptations, however, Thornhill and Palmer suggest that a possible psychological mechanism could operate by means of a threshold, below which the male became willing to rape, and that this threshold could be raised or lowered according to the availability of alternative mating opportunities (p. 67).

[11] In some species, all the males in a group gather together and try to make themselves look as mighty and alluring as possible while the females comparison shop and then choose which male to mate with. As you can imagine, the best males are chosen repeatedly and the worst males are never chosen at all.

Because evolutionary psychologists focus so strongly on the correlation between resources and mating opportunities, it is not surprising that their consideration of the rapist-as-loser hypothesis has focused on the question of whether poor men are more likely to commit rape. In support of this conjecture, Thornhill and Palmer cite evidence that men of lower SES are overrepresented among rapists in the penal system and among adolescent sexual offenders. They also note that women of lower SES are overrepresented among rape victims (the relevance of which to the rapist-as-loser hypothesis requires a probably uncontroversial assumption about offender-victim propinquity). At the same time, connecting rape rates to low SES is hard to see as evidence for a dynamic specific to rape (much less any hypothesis about a rape-specific *adaptation*), given that low SES is associated with the commission of such a large and diverse number of crimes. Meanwhile, low SES does not imply low sexual opportunity, especially for the younger male age groups that appear to comprise the bulk of rapists (Day 1992; Brewster 1994).

An even greater challenge to the rapist-as-loser hypothesis as any kind of general account of rape is that "in comparison to their less aggressive counterparts, sexually aggressive men often begin to have sexual intercourse earlier in life, have more sexual partners throughout the life course, and may consider themselves no less successful in their ability to attract mates" (Malamuth and Heilman 1998: 524). In other words, rapists on the whole may actually be more successful than non-rapists in obtaining sex through non-coercive means.

Thornhill and Palmer respond by suggesting that rape by wealthy males can be explained by "a combination of impunity and the hypothetical adaptation pertaining to evaluation of a victim's vulnerability" (p. 78). What about rape by men who are not necessarily wealthy but are physically attractive enough to be able to obtain consensual, casual sex? Thornhill and Palmer offer a saving conjecture that we might call the *rapist-as-deceived-winner* hypothesis:

> Although women sexually desire physically attractive men, they may receive few material benefits from them. The female's strategy might, therefore, include displaying to physically attractive males an unwillingness to mate. This display may function as a signal to the male that the female is discriminating about mates, which may increase the man's perception of her value in terms of paternity reliability, and thus may result in her eventually getting more material benefits from the male than she would get in the absence of the display. If a woman's display of reluctance is truly effective, a man who achieves copulation with her will perceive that he achieved it by force. (p. 70)

The argument is that because women have a short-term desire for sex with attractive men but also know that seeming "too easy" can harm their chances for winning long-term interest, a woman might feign resistance and trick sexually attractive men into believing that they have raped her (i.e., "perceive that he achieved it by force.") These deceived men then go on to report in surveys that they have committed sexually aggressive behavior, causing confusion for evolution-minded researchers since the same men also report having numerous opportunities for consensual mating with other women.

Given that the authors do not provide empirical evidence for the rapist-as-deceived-winner hypothesis, the hypothesis as it stands might seem little more than an *ad hoc* speculation to save the rapist-as-loser hypothesis as an account of those rapes in which the woman's resistance is genuine. This also seems a rather dubious line of speculation, as we are moving into the *victim's* motives in order to understand why *men* rape. Proposing that the woman's resistance sometimes might not be genuine tells us nothing about why some men force sex over this (possibly feigned) resistance and other men do not. Also, the hypothesis would seem incompatible with their "threshold" proposal about a rape-specific adaptation (described earlier in this section), as physically attractive men would be presumed to have more alternative mating opportunities and so a higher threshold in willingness to commit rape. Moreover, even if subsequent work was to find evidence for the predictions that Thornhill and Palmer offer (which I doubt that it would), this would do nothing to show that the woman's behavior was a ploy to gain material benefits. Evidence outside of any legal or other punitive context also strongly suggests that if there is a disagreement between a man and a woman over whether sex was or was not consensual, the disagreement is far more likely that the man believes he did not rape a woman who believes that he did (e.g., Warshaw 1988: 85).[12]

Alternatively, instead of offering *ad hoc* speculations to save the rapist-as-loser hypothesis, one could take the stance that mate deprivation may well serve as a *partial* explanation of why *some* men rape. For example, Malamuth's (1996) "confluence model" of sexual aggression, which draws on both evolutionary and feminist sources, proposes that rejection by women can be one factor that increases hostility toward women and that hostility toward women is a contributing cause to rape. At the same time, one can still expect that the *overall* explanatory value of mate deprivation is limited, and *general* predictions about mate deprivation and rapist behavior do not jibe with observed data.

Additionally, even if we grant that males have a stronger innate desire for sexual gratification than females and that the evolutionary explanation for why this is so is correct, this does not entail the position that invoking female choosiness will be useful for understanding how the asymmetry in desire is actually manifested in coercive sexual behavior. Indeed, one could instead read evolutionary theory as suggesting that male

[12] See also Scully and Marolla's [1984] finding that even some convicted rapists who had used a weapon to force a woman to have sex with them did not regard this behavior as rape.

response to new sexual opportunities should be little affected by the availability of existing alternatives, we might think that particular male manifestations of sexuality may be only partially related to the breadth of other sexual opportunities (see, e.g., discussions of the so-called "Coolidge effect" [Buss 1994: 79-81]). In other words, we once again run into the problem where, if a particular pattern holds, it does not provide evidence for an evolutionary explanation over easily devised alternative explanations, and where the predicted pattern of behavior does not seem unequivocally implied by evolutionary theory in the first place.

In contrast, Thornhill and Palmer believe that female choosiness explains far more about male sexual behavior than rape, as evinced in the following passage:

> "[M]any human behaviors other than rape clearly are by-products of the intense sexual desires of human males and the sexual choosiness of human females: Sexual abuse of children can be seen as an example of males attempting to gain sexual access to individuals who, because of their age, are relatively unable to control sexual access. Bestiality is a means of experiencing sexual stimulation somewhat like that experienced in intercourse with a human female without having to be chosen by one. Masturbation—far more common among males than among females—is the most widespread male behavior that can be seen as a means of obtaining sexual stimulation without being chosen by a human female as a sexual partner. ...[A]ll these are examples of males' attempting to gain sexual gratification without meeting the criteria of an adult human female's mate choice... " (p. 60)

How "clearly" are these behaviors connected to the choosiness of human females? Another way of putting this question is how confident should we be that if men were able to get all sex they wanted from consenting adult women, the rates of sexual abuse of children, bestiality, and masturbation would drop to zero?

Regarding bestiality, Martin Weinberg (personal communication) is in the midst of a research project interviewing people who regularly have sex with animals. The subjects of this study have been found via "snowball" sampling of Internet-based and other social networks, and there is no pretense that those interviewed comprise a representative sample of all those who engage in this behavior. Even so, the sex composition of the sample is what Thornhill and Palmer would expect: at the time of our discussion, 121 men and 4 women. Weinberg says that the men that have been interviewed tend to fall into one of two broad types. The first are those who do fit the zoophile-as-loser hypothesis, i.e., men who report having had difficulty finding affection or sexual gratification from women in their youth and who, for whatever reason, came then to find such gratification through pets or livestock. The second are pansexual swingers who are into sexual experimentation of many kinds, of which bestiality is only one. If anything, members of this second group may have more sexual contact with women in the course of their pansexual pursuits. So the idea that bestiality can be explained by the greater choosiness of females only goes so far. And even for those in the first group, the common pattern is that while an inability to get sex from women might explain the initial breach of the interspecies sexual boundary, the continuation of the behavior is harder to explain this way. A taste for bestiality may persist even when the man has obtained a human sexual partner, or it could lead to the efforts to obtain such a partner being greatly circumscribed.

Weinberg suggests that much the same could be said for men who molest children: no doubt that the inability to obtain sexual access to females of suitable age explains some of the initial instances of the behavior, but once the boundary has been crossed, the behavior can persist on a trajectory that cannot be explained by the unavailability of age-appropriate mates. As for masturbation, ample reason exists to believe that men often masturbate when they could be having sex with a female; the decrease in the frequency of masturbation after marriage is small (Kelley and Byrne 1992: 231).

Taking these various behaviors together, we can see that chalking things up to the consequences of choosiness is often invalid and, when valid, may still yield a crude oversimplification of what are often complex careers of deviant behavior. Moreover, reference either to the strength of male desires or female choosiness, when valid, may still be uninformative for understanding which of various routes a particular males takes to satisfy desires unfulfilled because of female choice. Instead, evolutionary psychology may prove to be useful only for painting the broadest brushstrokes of these phenomena, while filling in their complicated detail might rely on proximate inquiries that benefit not at all from reasoning about ultimate causes.

CAN EVOLUTIONARY PSYCHOLOGY OFFER PLAUSIBILITY JUDGMENTS ABOUT ENVIRONMENTAL CAUSES?

By giving detailed consideration to how evolved mechanisms are sensitive to specific environmental cues, evolutionary psychology is consonant with the traditional social science premise that social environments importantly affect behavior (Tooby and Cosmides 1992). More than this, by relating specific environmental cues to specific behaviors as evolved designs to solve specific adaptive problems, evolutionary psychologists have claimed that their approach offers rigor to the analysis of environmental effects unmatched by the "incoherent environmentalism" of the Standard Social Science Model (Tooby and Cosmides 1992). If evolutionary psychology can make good on this promise, then a practical way that it might contribute to social science—even for those social scientists otherwise not interested in ultimate causes—is by providing assessments of the relative plausibility of various conjectured environmental influences on behavior. Evolutionary analyses could perhaps tell us that, given our ancestral conditions, natural selection was very unlikely to produce a mind that was sensitive to one environmental variable in a particular way but was much more likely to produce a mind that was sensitive to another variable.

NHR offers exactly these sorts of plausibility judgments about environmental effects. Since social programs to prevent rape are environmental interventions of some kind, such judgments could be an important contribution of the evolutionary perspective to preventing rape if the judgments were based on sound theoretical criteria that could be validated by subsequent empirical testing. Unfortunately, although Thornhill and Palmer make strong arguments about the relatively plausibility of different environmental cues, these judgments do not seem to be rigorously grounded in evolutionary analyses. Instead, they might even stand as examples of why "arguments from incredulity" about the implausibility of certain environmental effects are difficult to establish with consistency and fairness from Darwinian principles.

Let us first consider Thornhill and Palmer's rejection, on evolutionary grounds, of the likelihood that exposure to violent pornography affects rape behavior. Feminist scholars, of course, have long hypothesized that exposure to such pornography can shape men's attitudes and beliefs about sexuality in a way that diminishes moral compunctions against committing rape (e.g., Brownmiller 1975, Dworkin and MacKinnon 1988; Dworkin 1991). But one can argue that it proves little to show that men who view violent pornography commit more rapes since the men who expose themselves to violent pornography may well have a higher proclivity to commit rape in the first place. Laboratory experiments can provide random assignment but are not a realistic counterpart to the usage of pornography over extended periods of time and in its usual contexts (for laboratory evidence that pornography increases aggression toward women see Linz, Donnerstein, and Penrod 1988; Linz, Wilson, and Donnerstein 1992).

Thornhill and Palmer regard the idea that violent pornography might affect rape behavior as exemplary of the unsound and arbitrary reasoning of feminist/social science scholars and why our understanding of rape would be on firmer footing if this reasoning was replaced with the logic of evolutionary psychology: They write:

> [A]n understanding of the ultimate evolutionary reasons why humans have facultative adaptations that respond to variables in the social environment greatly enhances our ability to specify what social variables influence development in what ways. This also gives the evolutionary approach an advantage over approaches that use arbitrarily chosen environmental factors to explain rape. For example, many proponents of the social science theory of rape (e.g., Denmark and Friedman 1985; Stock 1991) hold that one specific way in which males in some cultures are taught to rape is through the viewing of violent pornography, which inspires imitative behavior. The social scientists pushing this notion, however, cannot explain why the human brain is purportedly structured so as to respond in this specific way to the specific environmental stimulus of violent pornography. Why, for example, should males seek out and imitate violent pornography but not other human activities depicted in videos? There is no consideration of the ultimate basis for the asserted proximate explanation, no sound theoretical foundation for it. (p. 170)

Yet, for feminists or others to propose that violent pornography affects proclivity to rape is not so facile as just asserting that men are simply imitating something that they see. For example, the notion that repeated exposure to a strong stimulus can "desensitize" moral reactions against it cannot be reduced to imitation (e.g., Linz, Donnerstein, and Penrod 1984). Nor can we reduce to imitation the idea that violent pornography could provide an erroneous source of beliefs about rape ("rape myths") that contribute to its perpetration (e.g., Malamuth and Check 1981, 1985). While such explanations might not make reference to ultimate causes, they are far from "handwaving" accounts of how media sources are supposed to affect male psychology.

Thornhill and Palmer's argument here can be juxtaposed with proposals by other evolutionary psychologists that media exposure can affect psychological mechanisms in ways that produce socially undesirable behavior. Crawford (1998), for one, raises the possibility that our evolved psychologies could be designed to calibrate their operation in response to cues about the appropriateness of physical aggressiveness as a behavioral response (especially regarding same-sex aggression among males), and chronic media depictions of violence can trick the brain into believing that aggression rates are higher in our society than they are and biasing behavior toward aggressive responses accordingly. What is important for our purposes here is not to judge the adequacy of this proposal about aggression but to note how the logic of the explanation has been considered by other evolutionary psychologists to be perfectly consistent with evolutionary principles. An analogous argument about sexual aggression would seem to invoke the same logic. In other words, while exposure to violent pornography is a variable associated with the feminist perspectives that Thornhill and Palmer clearly oppose, its possible influence is not inconsistent with environmental interactions proposed by other evolutionary psychologists.

Thornhill and Palmer continue:

> Aside from the obvious fact that violent pornography cannot account for the historical and cross-cultural (indeed cross-species) occurrence of rape, such an arbitrary environmental explanation is refuted by everything we know about biases in human development, perception, cognition, emotions, and motivation. It also has a logical flaw: An environmental factor is identified as a cause of human behavior without any attempt to explain why other kinds of environmental variables that could conceivably also influence the same category of behavior do not do so (p. 170-1).

Against this argument, one can question whether, in order to be causally relevant, a variable has to be able to account for the cross-cultural and cross-species distribution of a phenomenon. To me, the answer seems to be obviously not: unless one is trying to argue that violent pornography is at the root of all rape, it seems possible that one could propose merely that violent pornography has some influence on the rate of rape in a society in which it is available. The availability of handguns in a society might affects its homicide rate, even though this cannot account for all cross-cultural and cross-species instances of homicide. When Thornhill and Palmer conclude that "[A]lthough the removal of violent pornography may be desirable in its own right, it is very unlikely to solve the problem of rape," it is not obvious whom they are arguing against. While some feminists have argued for the laws against violent pornography partly on the supposition that it would reduce rates of rape (notably Dworkin and MacKinnon), is there really a credible opposition arguing that removing violent pornography would eliminate the problem entirely?

Also, saying that violent pornography represents an "arbitrary" environmental explanation implies that feminists have invoked some environmental feature that bears no obvious connection to rape and have trumped it up into a causal argument. But how arbitrary is it to propose that rape behavior might be influenced by experiences viewing depictions of rapes? That this possibility is supposedly refuted by "everything we know" about psychology seems an impotent assertion without further explanation of how it is so refuted. As before, then, the possibility looms that Thornhill and Palmer are devoting their energies to attacking a straw actor.

From here, Thornhill and Palmer continue by asserting that:

> [A]lthough the viewing of violent pornography may figure in the proximate causation of the raping behavior of some men, this view is severely limited in its ability to predict anything useful about rape or related behaviors. It cannot explain the data on who is raped, or the data on when or where rape occurs. (p. 171)

But this list of what it would be useful to be to predict about rape would seem to omit one of important possibilities: *who commits rape*. That exposure to violent pornography contributes to rape behavior seems to imply a intrinsic prediction: that those exposed to large amounts of such pornography will commit more rapes than those who are not. (As I have noted, untangling causality here is difficult, but this is beside the point).

In sum, if we look back now over Thornhill and Palmer's arguments about the evolutionary implausibility of the idea that exposure to violent pornography affects proclivity to rape, we see that even though their conclusions are strongly worded, their actual evolutionary justification seems much more weak. As such, their judgment smacks more of advocacy for evolutionary psychology against feminist perspectives than it does a rigorous assessment based firmly on Darwinian principles.

We can contrast their treatment of exposure to violent pornography as an important environmental variable with that of father absence in childhood, as they repeatedly suggest that father absence may affect men's likelihood of committing rape (p. 69, 77, 80, 81, 154, 175, 176-7). Some evidence does suggest that men raised without a father present in the home are more likely to be sexually aggressive than other men (see Malamuth and Heilmann 1988), but, as far as proving causality goes, father absence runs into the same problems as exposure to violent pornography. The absence of a father from a family is neither a random event nor one that can be experimentally manipulated. Consequently, even when father absence can be correlated with all sorts of outcomes, whether the correlation reflects true causality or a spurious relationship resulting from a common underlying cause is difficult to determine.

At the same time, father absence is a variable frequently invoked by evolutionary psychologists. Recall that evolutionary psychological discussions of female sexuality have given much focus to a preference for status or resources that are presumed to be valuable for females over evolutionary time partly because of males' parental investment for the child's well-being and subsequent reproductive success. To assert a strong evolved female preference for men with resources, then, might then also be to assert a strong disadvantage for those children who, for whatever reason, did not have an investing father. An evolutionary psychologist may thus expect father absence to lead to adaptation for behavioral strategies that make the best out of a bad situation for children with this disadvantage. For example, Darwinian psychologists have proposed that father absence provides an important environmental cue influencing whether a child subsequently adopts a "short-term" mating strategy, marked by strategy marked by earlier first intercourse, more partners, and shorter pair-bonds over the life-course, or more a "long-term" strategy (Draper and Harpending 1982; Belsky, Steinberg, and Draper 1991).

I do believe that some evolutionary psychologists might overstate the importance of father absence, and that part of the variable's allure may involve reading some of the perceived social problems of the present onto our evolutionary past. My point, however, is not to argue that father absence is not causally important for why some men have a greater proclivity to rape. Instead, the important question for our purposes here is whether evolutionary theory alone provides good justification for surmising that father absence is an environmental factor that influences proclivity to rape, while exposure to violent pornography is not. Recall Thornhill and Palmer's accusations about arbitrariness: is the idea that rape proclivity might be influenced by one's consumption of depictions of rapes really substantially more arbitrary than the idea that this proclivity is affected by whether one had an enduring relationship with one's father in childhood? Consider also that father absence is not of any obvious use for predicting who is raped, or when or where rape occurs, although, like exposure to violent pornography, it may well be useful for telling us which men are more likely to rape. In the end, we must ask whether evolutionary grounds are sufficient to warrant the conclusion that Thornhill and Palmer draw: that the variable associated with the feminist perspective does not make Darwinian sense as a contributing factor to rape, while the variable associated with an evolutionary psychological perspective does. If we suspect the answer is no, this would seem to indicate the prospects of an evolutionary perspective for offering reliability plausibility judgments about environmental effects is compromised by at least the appearance of vulnerability to subjective biases. Consequently, even if such a winnowing function were possible, evolutionary reasoning might have too many "degrees of freedom" for scholars from different viewpoints to converge upon the same conclusion.

THE PSYCHOLOGICAL PAIN OF RAPE VICTIMS

The experience of being raped surely is psychologically devastating to a large portion of its victims, who often exhibit evidence of psychological trauma years after the event (e.g., Gidycz and Koss 1991; Allison and Wrightsman 1993; Shapiro and Schwarz 1997). Even if it is inaccurate as an account of the rapist's motives, a black-or-white slogan like "rape is violence, not sex" might indeed provide an apt account of rape as it is experienced by its victims, as well as a maxim for how rape should be treated by the criminal justice system. *NHR* contends that the reason that rape is so psychologically painful is that this pain serves an adaptive purpose. Indeed, while Thornhill and Palmer are uncertain about whether men possess rape-specific cognitive adaptations, they are more confident in the evidence that women's traumatic reactions to sexual assault stem from special-purpose adaptation. They write, ""Women's psychological pain after rape—a fairly well-studied trait—appears to be an adaptation that defends against events that, during human evolutionary history, resulted in reduced reproductive success of rape victims" (p. 103). As opposed to their lengthy discussions of each side of the "adaptation-vs.-byproduct" question regarding rapist behavior, Thornhill and Palmer do not entertain any possible by-product explanations for observed patterns of pain among rape victims. Given their own discussion of how the conclusion of adaptation should be only reluctantly drawn, the confidence of such a presentation would seem to require strong evidence that observed patterns of psychological pain among rape victims exhibit evidence of the "special design" of domain-specific adaptation.

How, according to Thornhill and Palmer, is the psychological trauma of rape adaptive? The authors' account of the evolutionary functions of rape trauma derives from a more general hypothesis about psychological pain developed by Randy Thornhill and Nancy Wilmsen Thornhill (former collaborator and ex-wife). The hypothesis attempts to extend to the psychological realm an evolutionary explanation of the adaptive benefits of physical pain:

> Physical pain serves to draw an individual's attention to some aspect of anatomy that needs tending and *can be fixed* by the individual's attention. Mental pain seems to focus an individual's attention on the significant social events surrounding the pain and promotes correction of the events causing the pain and avoidance of these events in the future (Thornhill and Thornhill 1990a: 158; emphasis in original).

With regard to rape trauma, the event causing the pain has already happened, and so the adaptationist argument instead emphasizes the "avoidance of these events in the future." According to the theory, the victim's pain focuses her attention more closely on the circumstances surrounding the rape, with the consequence that she will engage in preventative behavior that will reduce her likelihood of being raped again in the future. Also, Thornhill and Thornhill suggest that rape trauma may also function to help the victim convince potentially suspicious parties close to her—in particular, her husband—that she really was raped and that she resisted the rape wholeheartedly. In potentially serving these functions, the psychological trauma of rape victims is supposedly part of women's "evolved defense against rape." Accordingly, Thornhill and Palmer urge readers to recognize the adaptive value of this pain, and they caution those therapists who would do too much to try to alleviate it. In describing their ideas for an "evolutionary informed post-rape counseling and treatment program for victims," they recommend that psychotropic medications should be used only "selectively and cautiously, so as not to eliminate the defense that psychological pain provides" (p. 188).

Although I believe that neither of these proposed explanations actually follows properly from an evolutionary perspective, I want to focus on the evidence that Thornhill and Palmer present on behalf of the theory of psychological pain. *NHR* reports that a series of secondary data analyses conducted by Thornhill and Thornhill (1990a, 1990b, 1990c, 1991) provide support for the theory. The data were a survey of 790 rape victims that was conducted through the Joseph J. Peters Institute in Philadelphia between 1973 and 1975. The sample comprised females who had reported a sexual assault to authorities and who had been examined at a hospital; they were interviewed by a social worker sometime within five days of the rape. Thornhill and Palmer report that these analyses found that "the psychological mechanisms that regulate psychological pain in response to rape are affected by a woman's age (and thus her fertility), by her marital status, by her treatment by the rapist [e.g., use of force], and by whether there was penile penetration of the vagina" (p. 94).

Thornhill and Palmer report that the predictions implied by the theory were supported by empirical tests, although reading their account and the additional studies suggested that the original studies could have been conducted using better and more thorough statistical analyses than what had been done. Moreover, there were other testable predictions that one could derive from the theory but that Thornhill and Thornhill had not tested. Accordingly, I have sought to obtain the data that had been used in these studies, for the purposes of replication and testing new hypotheses. I began my efforts by contacting Randy Thornhill. Unfortunately, he replied that all the computer data files were lost in a hard drive crash, and the printed copy of the data that he had was lost when he changed offices in the early 1990's. At Thornhill's suggestion, I contacted the Peters Institute in Philadelphia, and I also independently tracked down Linda Williams (formerly Linda Meyer), who was one of the investigators on the original data collection project (see McCahill, Meyer, and Fishman 1979). To date, I have not yet obtained these data, and it is unclear to what extent these data will be available for researchers.

Important also to note is that the original results reported in the Thornhill and Thornhill papers do not always match the results reported by Thornhill and Palmer. Regarding the relationship between marital status and psychological pain, Thornhill and Palmer report:

> "Data on victims' self-reports of force and violence and independent evidence of violence taken by medical examiners make it possible to test whether reproductive-age victims who had not experienced violence exhibited more psychological pain than victims of the same age who had been attacked violently. In fact, this has been shown (Thornhill and Thornhill 1990c). As also predicted, Thornhill and Thornhill found that the married woman whose rapes had been marked by violence exhibited less psychological pain. Thus, reproductive-age married woman appear to be less psychologically traumatized when the rape includes violence, thus providing clear evidence to their husbands that copulation was not consensual." (p. 92-93)

In the original study, Thornhill and Thornhill (1990c: 314) do test the hypothesis that reproductive-age women who had experienced violence would exhibit more psychological pain than women who had not experienced violence, but they actually report that "*The data do not support the prediction*" (Brownmiller 2000; Coyne 2000). Only two of the thirteen psychological trauma variables that they tested were statistically significant, and both significant effects were opposite the predicted direction. That is to say, in these two cases, increased violence was associated with *increased* psychological pain, the reverse of what Thornhill and Thornhill predicted. These data clearly do not provide any evidence for the evolutionary prediction, Thornhill and Palmer's statements to the contrary.

As the second half of the quoted paragraph above reports, Thornhill and Thornhill also tested the hypothesis only among those reproductive-aged women who were married. Here, two effects in thirteen tests were significant in the direction predicted by the hypothesis, and five other effects were in the predicted direction but not significant. Do these findings warrant Thornhill and Palmer's conclusions? Taken together, we have twenty-six statistical tests for the hypothesis about violence and psychological pain. All of Thornhill and Thornhill's (1990c: 314-5) statistical tests were one-tailed at the .05 level, meaning that if degree of violence had no effect whatsoever on psychological pain, we would still expect to observe significant effects by chance alone 10% of the time. They observed four significant effects, which is not much more than the 2.6 would be expected by chance. More importantly, only two of these significant effects were in the predicted direction, and two were opposite the predicted direction. If we ignore statistical significance, fourteen effects were in the direction predicted by the hypothesis, compared to twelve effects in the other direction. In short, these results are striking in the extent to which they match the sort of results we would expect to observe in data that were completely random. They can only be taken as evidence of adaptation if one focuses on only the two significant effects that support the theory and ignore the rest, which is what Thornhill and Palmer apparently do in claiming support for the hypothesis.

Do reproductive-aged women experience more psychological trauma after being raped than non-reproductive aged women? Thornhill and Palmer say the earlier studies "showed that reproductive aged victims suffered significantly more psychological trauma than non-reproductive aged rape victims" (p. 90). Actually, the significant difference is only between reproductive-aged women and young girls; the difference between women aged 12-45 and women over 45 is not large enough to be statistically distinguished from chance (Thornhill and Thornhill 1990a: 164-66). As other critics have pointed out, the data on child rape victims were often gathered from caretakers rather than the victims themselves (Coyne 2000; Brownmiller 2000). So the only statistically significant comparison is between two groups for which data were collected mostly using different methods. We thus do not have solid evidence that the result that Thornhill and Palmer regard as providing important evidence of evolutionary "special design" is not just an artifact of the difference in how data was collected. Thornhill and Palmer must also explain the seeming contradiction between the predictions of their theory and the high levels of fear of rape reported among elderly women and the severe, long-lasting psychological trauma that is frequently associated with the sexual abuse of children (Warr 1985; Saunders, Kilpatrick, Hanson, Resnick, and Walker 1999; Walker, Unutzer, Rutter, Gelfand, Saunders, VonKorff, Koss, and Katon 1999; see Koss 2000).

Thornhill and Palmer also wish to take the greater evidence of physical resistance to rape among reproductive aged women as indicating their stronger desire not to be raped ("because of the greater evolutionary historical cost to their reproductive success of bring raped" [p. 91]), and they do not consider the seemingly obvious alternative that women in their 20's and 30's are more capable of substantial physical resistance than are elderly women or a girls aged 11 or under (Coyne 2000; Brownmiller 2000).

In sum, Thornhill and Palmer propose that the psychological pain experienced by rape victims is an adaptation, but their evidence would seem to fall well short of this conclusion, especially if the conclusion of adaptation is only supposed to be drawn reluctantly (Williams 1966). The results of Thornhill's own tests of the theory are not as supportive as they report, and some of the supportive results that actually were observed have obvious alternative explanations. Further, some of the other supportive findings have been contradicted by later studies with other data (namely, other studies have found that the psychological pain of rape increases with the amount of physical violence used, which contradicts the Thornhills' theory [see Koss 2000]). Finally, the reader is cautioned that the data used in the studies by Thornhill and Thornhill may also be unavailable for any further examination, which precludes any close scientific evaluation or replication of the data analysis they performed.

CONCLUSION

> *"The ability of ideology to blind people to the utter implausibility of their positions is perhaps the greatest threat to accumulating the knowledge necessary to solve social problems."*
> —Randy Thornhill and Craig Palmer, *A Natural History of Rape*, 2000 (p. 152)

The dashing Thomas Henry Huxley became known as "Darwin's bulldog" for his vigorous advocacy of the theory of evolution by natural selection and his fierce counterattacks against its proponents. (Bowler 1990).[13] Huxley's zeal on behalf of the theory has been vindicated by time and indeed seems heroic in retrospect. With NHR, Thornhill and Palmer may have cast themselves in the role of being two of evolutionary psychology's bulldogs: venturing into the politically charged area of rape research, declaring the superiority of evolutionary psychology over other alternatives, and asserting that evolutionary thinking provides a key to finding better ways to prevent rape. Given the boldness of their presentation and the seriousness of the subject matter, that their book drew considerable fire is not surprising. As I said in the introduction, depending on one's perspective, criticism of NHR can be seen as a justified reaction to flawed scholarship or as a politically kneejerk reaction that would have occurred regardless of the book's merits.

Indeed, the book is likely to be a "polarizing" piece of work in the sense that readers who have divergent views when they begin the book will probably be even further apart in their thinking when they finish. The book likely appeals most among those who begin with a set of unfavorable ideas about feminism (e.g., in its baldest form, that feminist is a monolithic theoretical perspective; that it dominates the social sciences; that it exerts a massive and negative influence on popular consciousness and political life; and that feminist scholars are very often blinded by their ideological commitments). Readers who begin the book with these beliefs will likely have them reinforced and strengthened by the representation of feminist scholarship in the book. Even if such readers find the evidence for some of the authors' specific evolutionary claims wanting, they might still come away

[13] In Huxley's case "research has suggested that Huxley's commitment to selection theory was at best only lukewarm... He had substantial reservations about the theory that Darwin was never able to overcome" (Bowler 1990: 142).

believing that Thornhill and Palmer have made a valuable contribution in debunking the "not sex" explanation.

On the other hand, negative reactions to the book can be expected from those who begin with a strong skepticism toward evolutionary psychology and a more favorable set of beliefs about feminism. These readers might not know much about natural selection selection, but this does not preclude them from perceiving the book's attacks on feminism—which start in the book's preface and continue all the way to the back of the cover jacket—as being gratuitous, as well as perhaps perceiving them as an inaccurate portrayal of actual feminist thought. When these readers encounter Thornhill and Palmer's speculations about how physically attractive men might be duped into thinking they have raped a woman who actually wanted sex or about how rape reform laws have met resistance because they contradict an evolved disposition to see a woman's past sexual history as relevant to her claim of having been raped, they may also find themselves skeptical of the authors' contentions that they are the objective scientists and that only their opponents stances reflect ideology. For these readers, negative impressions of evolutionary psychology will almost certainly be strengthened. Indeed, those who find the treatment of feminism particularly troublesome might take the book as an argument for why social scientists *do not need to know* anything more about evolved biology, reinforcing a sentiment described by Lee Freese (1994: 367): "Many social scientists are suspicious of biology, not because they understand the subject well (they do not) but because they understand some of its interpreters all too well." We can see an informal indication of this polarization in the rating the readers have given the book at the Amazon.com website (Table 5.2). Of the 52 ratings that had been provided, 44 (85%) gave the book either the highest possible or lowest possible rating.

Table 5.2. Sign of a polarized public reaction? Ratings of Amazon.com patrons of *A Natural History of Rape* (June 27, 2000)

★★★★★ (best)	★★★★	★★★	★★	★ (worst)
17	1	5	2	27

In the introduction, we saw Tooby and Cosmides (2000) take an evolutionary biologist's hostile review of NHR to task for mischaracterizing both some arguments of Thornhill and Palmer and some orienting principles of evolutionary psychology. Among other things, Tooby and Cosmides asserted that if the reviewer wished to present himself as a scientist he has an obligation "to fairly and accurately characterize the views of those criticized rather than to propagate academic urban legends." In my reading, at least, the reviewer did make mistakes in his presentation of evolutionary psychology, and the hostile tone of Tooby and Cosmides's review is no great escalation over the nasty tone of the review itself. But, at the same time, when we looked at some of Thornhill and Palmer's quotations of the work of feminist scholars, we also saw evidence of an inaccurate characterization of an opposing view. Thornhill and Palmer freely acknowledge that many feminists have explicitly disavowed the position that their book attributes to them, but they maintain that disregarding these disavowals is justified. Consequently, one might regard the contentions that all evolutionary psychologists think that everything is an adaptation and that all feminists think that rape is never in any way sexually motivated would seem equally deserving of the phrase "academic urban legends."

So a critic of Thornhill and Palmer is sharply attacked for committing a sin that Thornhill and Palmer themselves appear to commit; supporters of evolutionary psychology can focus on one sin and skeptics upon the other. One does not have to witness all that many cycles of academic vituperation and counter-vituperation before getting the cynical sense that an unfortunately large amount of intellectual debate may be little more than alternating rounds of each side caricaturing the other's position and then screaming bloody murder when their own position is caricatured. This is especially for debates on politically and emotionally charged issues and debates that are carried out in places like *The New York Review of Books*. In discussing some of the public debates over sociobiology (e.g., between Dawkins and Gould), sociologist Ullica Segerstråle (2000) suggests that we might be being naive when we wonder why these debates seem to carry on interminably and why they seem to be couched in a language that seems to offer little prospect of finding common ground. Segerstråle suggests that instead of there being incentive to compromise, vocal opponents may need extreme characterizations of each other to gain attention for one's own points and counterpoints. In this way, one might see evolutionary psychologists and their loudest critics keeping each other in the spotlight with their back-and-forth invective.

These debates and the polarization evoked by NHR might seem more worthwhile—or, at least, necessary—if the research really was exemplary and if the reactions of critics were just ideological posturing. The presumed credibility of NHR is increased by the support it has received from other evolutionary psychologists in its efforts to be seen as "normal science" with that paradigm—"just another brick in the wall" supporting evolutionary psychology, to borrow a phrase from the of one of Buss's essays (1997). In this chapter, however, I have raised concerns about several different aspects of Thornhill and Palmer's argument, including criticisms of the theoretical approach to the question both of why men rape and why women are so traumatized by rape. Add to this the question of whether social scientists should approach the topic in terms of a search for some general explanation of rape, as opposed to presuming from the outset that the diversity of events collected by the term "rape" have an array of causes whose prominence varies greatly across instances.

Taken together, these criticisms have convinced me that regardless of the extent to which other criticisms of *NHR* can be blamed on widespread ideological aversion against applying Darwinian thinking to sensitive areas, strong criticisms of the book are warranted on grounds completely independent from any politics. Unlike Huxley, the advocacy bark of Thornhill and Palmer may not be supported by academic bite. (Here again I should note my regret that I have not yet been able to obtain the data on the psychological trauma of rape victims that provides the entire empirical basis for one of the book's chapters.) The stridency of the book's rejection of alternative approaches thus seems only unfortunate, as any polarization that it provokes might serve to inhibit productive dialogue between scholars of different viewpoints about what causes rape. This chapter has sought to provide an example of an extended substantive engagement of an evolutionary psychological offering on a politically controversial topic.

Importantly, while this chapter sides with the critical verdict on Thornhill and Palmer, I do not wish to assert that evolutionary approaches have nothing to offer toward understanding rape. Indeed, the possible contributions have been recognized as part of mainstream rape research (see, e.g., the section on evolutionary causes in Crowell and Burgess [1996]). Malamuth's (1996; Malamuth and Heilman 1998) "confluence model" sees the likelihood of male sexual aggression as increased through the interaction of hostility toward women and an impersonal view of sex, both of which he has attempted to elaborate in light of both evolutionary and feminist considerations. Malamuth emphasizes the need to develop a comprehensive model to rape that considers universal and gender-specific mechanisms, but that also considers the importance of "cultural and subcultural differences" in ideology and social climate, as well as various sources of individual differences among males (citing Barkow 1989). Of course, these efforts have not received anywhere close to the publicity of *A Natural History of Rape*, but this is one of the banes of reading evolutionary psychology—the lack of positive correspondence between the care and reflectiveness evinced by a work and the public attention it receives (as well as perhaps the enthusiasm with which it is received by many fellow evolutionary psychologists). For sociologists, however, greater attention to work like Malamuth's may help to provide a more complete understanding of the motivation underlying some sexual aggression. At the same time, the specific contribution of the evolutionary component of the model may be less compelling than the concomitant rejection of a cultural-determinist approach to motivation.

What this work does suggest, however, is that evolutionary and feminist scholars might be able to engage in a productive dialogue that improves our understanding of sexual aggression. Yet these prospects are not enhanced by a polemic like Thornhill and Palmer's that denigrates the explanatory importance of culture and that can be read as presenting evolutionary psychology as the only valid way from which social behavior can be considered (see their treatment of culture, pp. 24-29, and their chapter on "Why Have the Social Sciences Failed to Darwinize?", pp. 105-122).[14] The potential contributions of evolutionary psychology deserve greater attention from sociologists, anthropologists, and others, but this does not mean these disciplines are infirm until they have been "Darwinized." Indeed, one of the most frustrating things about NHR is how the authors' Darwinized worldview

[14] The chapter on why the social sciences have failed to Darwinize does provide an overview of some of the unjustified reasons why many social scientists summarily reject the potential contributions of evolutionary psychology, with misunderstanding and political biases being foremost among them. The alternative to this is a more sincere effort to consider what contributions Darwinian thinking may have to offer the social sciences, not a wholesale "Darwinization" of these disciplines.

prevents them from acknowledging very simple alternative explanations for phenomena that they interpret as evidence of evolved dispositions. We saw a prime example of this earlier in their failure to consider that differences in physical strength and agility might have explain why victims of reproductive rage provide greater physical resistance to rape than elderly victims.

There is some irony in the thought that, if Darwinian reasoning is to gain more acceptance in the academy, it may require the dissociation of new lines of evolution-minded scholarship generally from at least the bulldog and expansive efforts of some of those who are evolutionary psychologists. One place where moderation by some practitioners is needed is in the attitude toward the rest of social science (just as much of the rest of social science would benefit from a more temperate view of evolutionary approaches). Another is in the degree of emphasis placed on ultimate causes: as a central question of their endeavor, Thornhill and Palmer deliberate over whether rape-specific adaptations exist or whether they are a by-product of other adaptations, but they do not make a good case for why social scientists who are not otherwise interested in evolutionary origins should find this debate compelling.

As I have already suggested, another place where greater moderation may be warranted is in the willingness to attribute adaptation. I have already indicated that no evolutionary psychologist would claim that virtually every behavior is an adaptation (Cosmides and Tooby 1997), and Thornhill and Palmer repeat Williams's famous dictum that the concept of adaptation should be used only where it is really necessary. But this often does not carry forth into practice. Elisabeth Lloyd (1999: 225-6) accuses evolutionary psychology of often providing "ritual recitations" of caution and restraint regarding adaptation that have a "peculiar disconnect" from how evidence is subsequently considered. In NHR, once Thornhill and Palmer get beyond the question of why men rape, and consider such things as why women are so traumatized by rape and why others are sometimes skeptical about reports of rape, their conclusions about adaptation spring forth much more freely and do not exhibit the same careful consideration of flaws in their own conjectures or possible explanations other than adaptation. Contrary to this, the evolutionary approaches that will integrate best with other social sciences are those that not only state Williams's dictum but are mindful of it throughout.

5

REBEL WITHOUT A CAUSE OR EFFECT

"Never Mind Your Horoscope: Birth Order Rules All"
--title of *Psychology Today* article by Walter Toman, 1970

Birth order has had a long, cyclical, and somewhat disreputable history as an explanatory variable in the social sciences. Many hundreds of studies of the effect of birth order have been done, looking at its effects on virtually every conceivable dependent variable. Its enduring popularity as a topic of study is likely explained by a combination of factors, including the ease with which it can be measured, the presence of various folk wisdoms about its effects (e.g., the "responsible" firstborn, the "spoiled" lastborn), and the curiosity of many researchers as to how they and their siblings could have come from the same womb and yet turn out so different. Its popularity cannot be explained by the strength of its demonstrable consequences, as birth order studies have produced maddeningly inconsistent results and have been recurrently blasted for their poor methodology (perhaps most notably Adams 1972; Schooler 1972; Ernst and Angst 1983). For many years, the opinion of many family researchers has been that birth order effects in most domains are either small or non-existent.

But in late 1996, birth order began another comeback, this time dressed in Darwinian garb. The occasion was the publication of the book *Born to Rebel: Birth Order, Family Dynamics, and Creative Lives*, by Frank Sulloway (hereafter *BTR*). The fruition of over twenty years' effort, *BTR* presents a series of dramatic claims for the importance of birth order in shaping individuals' personalities, attitudes, and behaviors, and these claims are supported by quantitative studies of data on historical personages, as well as by a statistical re-examination (meta-analysis) of the inconsistent results of earlier birth order research. One could jest that *BTR* depicts birth order as sort of the *Zelig* or *Forrest Gump* of social-scientific variables, popping up to play a previously unrecognized role in the unfolding of numerous historical events: the Protestant Reformation, the French Reformation, various scientific controversies, and even the marriages of King Henry VIII. Within these dramas, those who had been laterborn children were typically the rebels, the innovators, the embracers of new and radical ideas, while firstborns were the conservatives, the traditionalists, the resisters to change.

BTR was meant to make a splash: the book was written for a general audience and published by a major house that paid Sulloway a $500,000 advance. Its publication was hailed by long articles in *Newsweek* and *The New Yorker*. *BTR* received an even more enthusiastic response from *Skeptic*, a general-interest science magazine that is a favorite among many evolutionists. The magazine invited Sulloway to serve on its editorial board; its Skeptics Society gave money to help fund a follow-up mail survey that I discuss later; the magazine even sold "Rebel With A Cause" T-Shirts celebrating Sulloway's work (the shirt features Charles Darwin's head superimposed on the leather-jacketed body of Marlon Brando from *The Wild Ones*.) Its editor, Michael Shermer, wrote an article declaring that *BTR* was "simply put, the most rigorously scientific work of history ever written" (p. 63). He holds out hope that "Sulloway's book... marks the beginning of the end of history as it has been practiced for the past two and a half millennia... a transition comparable to the Kepler-Galileo-Newton impact in changing astrology into astronomy" (p. 66).

Importantly, however, the initial decision that Sulloway's book represented an important social scientific breakthrough was not made by journalists or by some other lay judges of "pop science." Rather, *Born to Rebel* was backed by enormous advance praise by some weighty figures in the academy. Granted, book blurbs generally report the words of scholars at their most generous, but, even by these standards, *Born to Rebel*'s cover reported a series of endorsements that were downright hagiographic:

EDWARD O. WILSON: "Frank Sulloway has delivered one of the most authoritative and important treatises in the history of the social sciences."

ERNST MAYR: "Every once in a long while a book is published which changes a whole field of scholarship, perhaps even everybody's thinking. Such a book is *Born to Rebel*."

ROBERT K. MERTON: "A quarter century in the making, this brilliant, searching, provocative and readable treatise promises to remain definitive for twice as long."

Trumping even these heady kudos, anthropologist Sarah Blaffer Hrdy predicts that *Born to Rebel* "will have the same kind of long-term impact as Freud's and Darwin's."

Sulloway offers an evolutionary psychological explanation for why birth order exerts the substantial effects that he finds.[1] As we shall see, Sulloway's theory exemplifies evolutionary psychology's emphasis on the environmental contingency of mental adaptations: in this case, the same underlying mechanism yields systematic differences in the behavior of firstborns and laterborns because it responds to systematic differences in firstborns' and laterborns' childhood environments. The book thus also provides an effective antidote to the view that evolutionary psychology implies genetic determinism. As John Tooby put it in *Newsweek*, "[Sulloway] attacks questions that could not be more contingent—why some countries ended up Protestant, why France resisted Darwinism, who ended up in which faction in the French National Assembly—and shows that they fit into larger patterns." Even so, not all evolution-minded scholars were enthusiastic about

[1] Indeed, Sulloway (1995) first published his meta-analysis of the existing birth order literature as a supportive response to a review article on evolutionary psychology by David Buss.

Sulloway's theory: the journal *Evolution and Human Behavior* published two negative reviews of the book, by philosopher Michael Ruse (1997) and behavior geneticist David Rowe (1997). Unlike much of abovementioned praise, these reviews emphasized the importance of reserving judgment about the book's claims until more independent scrutiny of Sulloway's analysis was done and more independent testing could be conducted.

In this chapter, I will argue that this urging of caution was well-warranted. After providing more background about the book and its findings, I will first present results of a study of my own indicating that Sulloway's hypotheses about birth order and social attitudes do not hold among a representative sample of American adults. Then, I discuss Sulloway's meta-analysis of the scientific literature on birth order and personality, including criticisms of other authors, as well as my own, which suggest that this meta-analysis may be seriously flawed. Following this, I offer some possible explanations of the discrepant results between Sulloway's historical sample and results from a contemporary, representative sample, and I discuss the potential problems of using Darwinian theories to make assertions about motives in individuals' biographies. In conclusion, I look again at the public attention given to *BTR*, and I suggest that such publicity could compromise the health of the theoretical program of evolutionary psychology and may contribute to some of the divisiveness that still surrounds evolutionary explanations of behavior.

BORN TO REBEL

As noted, evolutionary psychologists propose that behavior is generated by psychological mechanisms that developed in response to specific, recurrent problems in humans' evolutionary past. Commonly invoked are conflicting evolutionary interests between human counterparts, such as the competition between members of the same sex for the best mates, the conflict between mothers and fathers over how much each will invest in the care of children, and the conflict between parents and offspring over how much investment they will receive and how long the period of intensive parental investment will last (see Chapter 7). To this list, Sulloway adds the competition between siblings over the resources they receive from their parents. He theorizes that the sibling competition over resources causes children to develop divergent attitudes, personalities, and behavioral tendencies in order to maximize the resources that they receive from their parents, and that these differences persist into adulthood. More specifically, Sulloway's thesis is that the competition for resources among siblings affords a systematic advantage to those children who are able to tailor their activities in a way that stakes out a "family niche," and that the persistence of this advantage over many generations has yielded the adaptation of environmentally-contingent psychological mechanisms that underlie many sibling differences.

Although sibling rivalries play out uniquely within each family, Sulloway contends that the optimal strategies for the conflicts are highly predictable because firstborns have a consistent edge when competing with their younger siblings. In the early years, at least, firstborns tend to be larger, stronger, and more intellectually developed than their siblings. As a result of all these advantages, firstborns systematically occupy the dominant ("alpha") position among their siblings, and they develop the attitudes and personalities that are optimally suited for protecting this position. Consequently, firstborns are proposed to develop attitudes that are more conservative and tough-minded and personalities that are more jealous, conscientious, and dominant. Meanwhile, laterborn children are chronic underdogs who have to work to differentiate themselves enough from the privileged firstborn (as well as perhaps other siblings) to increase their share of parental resources. Sulloway argues that laterborns, in their efforts to secure a family niche, develop attitudes that are more liberal and compassionate and personalities that are more rebellious and open to new experiences.

Sulloway's theory exemplifies the notion of natural selection shaping psychological mechanisms to respond to varying environmental conditions. Let me italicize this: *Sulloway does not assert that there are any genetic differences between firstborn and laterborn children.* Instead, he argues that all children have adaptations for maximizing the investment they receive from their parents, but that the optimal strategy for doing so depends on one's birth order, leading to numerous differences that persist into adulthood. To use the language of Tooby and Cosmides (1990a), we can say that the mechanisms are "calibrated" in childhood to produce the behavioral tendencies that maximized parental investment for children in analogous positions in the environments of our evolutionary past. The same underlying adaptation leads persons of different birth orders to exhibit different attitudes and personalities.

The real persuasive power of *BTR* lies in the voluminous empirical evidence that Sulloway presents in support of his theory. Sulloway begins by describing a pattern that he observed among 18th and 19th century scientists. On average, firstborn scientists were more accomplished than laterborns, but laterborns were disproportionately responsible for many of the most radical and important innovations of the period (for example, Charles Darwin and Michael Faraday were laterborns.) Using biographies of scientists and expert ratings by historians, Sulloway assembled data on over 3000 scientists who participated in 28 different scientific controversies. These data indicated that laterborns were twice as likely as firstborns to adopt a scientific innovation early (pp. 36-41). After the publication of the *Origin of Species* in 1859, laterborn scientists tended to be quick to adopt Darwin's theory, while firstborns clung longer to Creationism (pp. 34-36). Laterborns also adopted Einstein's theory of relativity first, while firstborns held on longer to Newtonian mechanics. Firstborn scientists did adopt these innovations eventually, of course; they just did so later. An important exception in these results was that firstborns were more likely than laterborns to endorse theories that had *politically conservative implications*, such as the eugenics movement (p. 38).

Sulloway moves from this analysis of scientists to a broader examination of the role of birth order in history. Again, using biographies and ratings from historians, he gathered data on a variety of different historical figures and historical events. His analyses of these data support the hypothesis that laterborns tend to drive liberal social movements while firstborns tend to be conservative, tough-minded reactionaries. In the Protestant Reformation, he finds that birth order is the single best predictor of whether prominent individuals adopted Protestant ideas or remained loyal to the Catholic faith (p. 262). In the French Revolution, he finds that firstborn deputies were more likely to be staunch Royalists, while laterborn deputies were overrepresented among the early leaders of the Revolution (pp. 309-321). Sulloway looked at a sample of female American conservatives and reformers, and he found that while laterborns were overrepresented among suffragettes (among them Elizabeth Cady Stanton and Susan B. Anthony), firstborns have been overrepresented among those who cling to traditional gender roles and definitions of femininity (including Phyllis Schlafly and Anita Bryant) (pp. 154-58). He also studies Supreme Court appointments in this century and finds that while firstborn justices have tended to be Republican appointees and so more conservative, laterborn justices have tended to be Democratic appointees and so more liberal (pp. 294-296). The exception that proves the rule is liberal justice Earl Warren, who was a *Republican* appointee (by Eisenhower) but was a *laterborn*.

These examples are by no means all of the historical evidence Sulloway cites in support of his theory. At the same time, historical and scientific elites comprise an unusual group to study in order to making general claims about psychological mechanisms. Moreover, as Sulloway recognizes, existing birth order studies have produced maddeningly contradictory findings, and psychologists Cecile Ernst and Jules Angst (1983:241) concluded their massive review of the birth order literature by predicting that "if any consistent personality differences between sibs [of different birth orders] are found with better methods, they will represent only a small part of the variance." For his claims about birth order and personality, however, Sulloway conducts a meta-analysis in which he pools the results of all of the studies that used at least the minimum necessary controls (p. 72-75). Sulloway finds evidence for all of the effects expected by his theory, and he reports that the odds of these results being due to chance are "less than a billion billion" (p. 73).

These various lines of evidence lead Sulloway to conclude not only that birth order has important effects on personality, attitudes, and behavior, but also that "the effects of birth order transcend gender, social class, race, nationality, and—for the last five centuries—time" (p. 356) *BTR* includes an appendix in which readers can calculate their own "propensity to rebel" based on the logistic regression estimates from Sulloway's sample of scientists (pp. 440-444).[2] The evolutionary psychological framework provides theoretical grounds for suggesting the potential universality of these patterns. If the theory is correct, we

[2] My own propensity to rebel came out to 96 percent.

would expect the same systematic birth order differences to be evinced anywhere siblings are raised together. *BTR* emerged in print as the product of more than twenty years of effort by a deservedly respected scholar.[3] The obvious next question was how well the work would stand up to others' scrutiny and to independent efforts to test its hypotheses.

BIRTH ORDER AND SOCIAL ATTITUDES

In Sulloway's procedure for calculating one's own propensity to rebel, the variable that makes the biggest difference is whether one identifies oneself as a "social conservative," "social moderate," or "social liberal." Not surprisingly, persons with liberal social attitudes are more likely to engage in socially radical behavior. But Sulloway finds that birth order is an important determinant of social attitudes: the privileged childhood of firstborns leads them to develop into social conservatives, while the underdog position of laterborns leads them to become liberals. Indeed, Sulloway reports that in his data birth order is 14 times more important for predicting social attitudes than is social class.[4] These same dynamics also lead firstborns to hold attitudes that are more supportive of existing authorities and to be more "tough-minded" than laterborns. Again, while Sulloway bases these conclusions mainly on samples of historical elites, he suggests—and his evolutionary psychological explanation implies—the applicability of the conclusions to average members of contemporary societies.

But are firstborns in the United States today really more conservative than laterborns? Are they really more supportive of authority and more tough-minded? With collaborators Brian Powell and Lala Carr Steelman, I sought to answer these questions using

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[3] See, for example, the very positive reception to Sulloway's earlier book on Freud (1979).
[4] Based on a comparison of standardized coefficients.

the 1994 General Social Survey (GSS), a nationally representative survey of almost 3000 American adults that measures attitudes on a wide variety of social and political issues (Freese, Powell, and Steelman 1999). This research is described in detail as Appendix A, and I only summarize it here.

We looked at 24 different items that could be considered modern-day versions of the issues that Sulloway's theory suggests have divided firstborns and laterborns ever since Cain and Abel. We sought to address six broad questions:

1. Are firstborns more politically conservative than laterborns?
2. Are firstborns less likely to support liberal social causes than laterborns?
3. Are firstborns more likely to hold traditional attitudes about gender roles than laterborns?
4. Are firstborns more likely to resist initiatives of racial reform/equality than laterborns?
5. Are firstborns more supportive of existing authorities than laterborns?
6. Are firstborns more tough-minded than laterborns?

Sulloway's theory predicts *yes* for all of these questions. We tested the hypotheses using models that included increasingly stringent controls for other variables that might confound the effects of birth order. Our full model included controls for a respondent's race, sex, age, number of siblings, education, income, childhood religion, the loss of a parent in childhood, and the region of the country where they were raised, as well as their parents' education and occupational prestige.

Across the board, we found no evidence supporting Sulloway's hypothesis about birth order and social attitudes (see Table 6.1). Firstborns were not more likely than laterborns to identify themselves as conservatives; they were not more likely to be Republicans; they were not more likely to have voted in 1992 for George Bush over Bill Clinton (in our data, firstborns were neither over- nor underrepresented among those who voted for Perot). Birth order did not differentiate respondents in terms of their beliefs about abortion, environmentalism, marijuana legalization, euthanasia, animal rights, free speech or social welfare programs. Although Sulloway cites evidence that firstborns may be particularly inclined toward racism (p. 286; also see Lieberman and Reynolds 1978; Sherwood and Naputsky 1968), we found that white firstborns and laterborns did not differ in their opinions about social programs for blacks, residential segregation laws, immigation policies, or English-only laws. Despite Sulloway's finding that laterborns have more liberal attitudes about gender than firstborns, we found in the GSS sample that birth order did not systematically affect respondent's beliefs about women's roles in politics, the workforce, or the home.

Table 6.1 Results of tests of the effect of birth order on social attitudes

	Is the effect in the expected direction?	Is the effect significant ($p < .05$)?
Political identification		
Self-identification as conservative	No	No
Self-identification as Republican	Yes	No
Voted for Bush, 1992	Yes	No
Opposition to liberal movements/causes		
Opposes abortion rights	Yes	No
Opposes assisted suicide laws	Yes	No
Opposes legalization of marijuana	No	No
Opposes animal rights	No	No
Opposes environmental movement	Yes	No
Opposes free speech	Yes	No
Opposes social welfare programs	Yes	No
Belief in traditional gender roles		
Against mothers working	No	No
Supports traditional div. of labor between spouses	No	No
Against women in politics	Yes	No
Resistance to racial reforms		
Thinks government is too generous to blacks	Yes	No
Excluding blacks is OK	Yes	No
Believes blacks lack ability or willpower	No	No
Against benefits to immigrants	No	No
Supports English-only laws	No	No
Support for existing authority		
Important to teach children to obey	Yes	No
Trusts social institutions	Yes	No
Patriotism	No	Yes
"Tough-mindedness"		
Tough on crime	No	Yes
Supports capital punishment	No	Yes
Supports corporal punishment	Yes	No

Note: Results are for regression models with full controls, although similar results were observed across all models. Binary and ordered regression models were used for those dependent variables that were categorical (see Freese, Powell, and Steelman 2000).

In fact, the only statistically significant results that we found among the 24 variables were all *opposite* the direction expected by Sulloway's theory. Sulloway supports his contention that firstborns are more "tough-minded" than laterborns with data from the French Revolution, where firstborn deputies to the National Convention were more likely than laterborns to vote to execute Louis XVI (p. 321-325). But the GSS data indicates that contemporary firstborns may actually be *less* likely to support capital punishment than laterborns, and firstborns may be *less* likely than laterborns to believe that the judicial system needs to be tougher with criminals.[5] *BTR* presents laterborns as enthusiastically embracing revolutions while firstborns staunchly defend the current order. In seeming contrast, our analyses suggest that, if anything, laterborns in the GSS were more patriotic than were firstborns.

To guard against the possibility that our results were biased by the decisions that we made about what variables to examine, we expanded our inquiry to look at all 202 items on the GSS that could be seen as measures of conservatism, support for existing authority, or "tough-mindedness." The effect of birth order was statistically significant for only 16 of these items, not much more than would be expected by pure chance (7.9% vs. 5%). Moreover, of these 16 significant effects, only 8 were in the direction predicted by Sulloway's theory. Consequently, even if there are some isolated birth order effects on social attitudes within the GSS sample, the theory provides no better insight into predicting their direction than does a coin-flip.

[5] Of course, reported attitudes about the death penalty is different from the actual act of voting to execute someone, even if they may both be interpreted as indices of "tough-mindedness." As I discuss later, the distinction between attitudes and behavior is one explanation of the divergence between our findings and the expectations provided by Sulloway's analyses.

Many of Sulloway's findings and discussions discount the influence of other variables in comparison to birth order (as in the example I mentioned earlier, in which Sulloway reports that in his data birth order is 14 times more important for predicting social attitudes than is social class) (p. 507). Accordingly, we decided to see how birth order stacked up against age, sex, race, parents' education (a rough proxy for social class of origin), and number of siblings for predicting social attitudes on the GSS. For these variables, we offered a "predicted direction" for effects based on others' previous research, but nothing more fine-grained than the same general "conservative" or "liberal" prediction that, following Sulloway, we used for birth order.

Table 6.2 presents the results. All of the other variables exert more significant effects on social attitude items than does birth order, and the direction of the effects are more predictable. Birth order does not even do as well as number of siblings, suggesting that not only might birth order be fairly unimportant compared to social science mainstays like gender or social class, but *it may not even be the most important family configuration variable for predicting attitudes*.[6]

Table 6.2 Effects of different independent variables on social attitudes

	Predicted direction of attitudes	% of GSS items in predicted direction	% of items significant	% of significant items in predicted direction
Firstborn	Conservative	57.9	7.9	4.0
Age	Conservative	76.2	59.4	52.0
Parents' education	Liberal	74.3	52.5	44.1
Female	Liberal	68.8	44.1	31.7
Black	Liberal	56.8	36.5	24.3
Number of siblings	Conservative	56.9	18.9	14.9

[6] Although my suspicion is that the reason number of siblings affects attitudes may be that it serves as another proxy for socioeconomic status.

Sulloway's analyses also suggest a number of interaction effects between birth order and other variables. As one example, Sulloway reports a significant relationship between birth order and age spacing between siblings: the most pronounced attitudinal differences occur when there is a moderate gap (2 to 5 years) between siblings (pp. 133-36). Drawing upon Hamilton's (1964) theory of inclusive fitness, Sulloway argues that siblings who are more closely or distantly spaced are less (evolutionarily) costly than those who are moderately spaced, and so consequently firstborns should feel the greatest rivalry when the next child is moderately spaced. Sulloway also suggests that laterborn children born after a long gap are much like firstborns and therefore tend toward conservatism. As another example, Sulloway suggests that social class and birth order mediate the effect of parental loss through a three-way interaction effect: lower-class firstborns, thrust into the role of surrogate parents, become even more doggedly conservative, while upper-class firstborns, who often do not have to bear this burden, become somewhat more liberal (pp. 136-145). Using the GSS data, we were able to test these and some of the other interaction effects that Sulloway proposes. We found no evidence for any of these.

Putting all of our results together, we find no indication that birth order influences social attitudes among the 1994 GSS respondents in the way that Sulloway's theory would predict. As a final note, the foregoing results are like Sulloway's and most other birth order studies in that they compare persons from different families. Birth order studies are generally considered stronger when they are based on comparisons of *actual pairs of siblings*. To my good fortune, the 1994 GSS asked respondents to provide contact information for a randomly selected sibling, and over 1100 of these siblings were later interviewed as part of the Study of American Families (SAF), a companion survey to the GSS. This companion survey asked siblings a subset of the social attitude items used on the GSS, including items comprising 9 of our 24 dependent variables. Combining the GSS and SAF data allowed us to compare firstborn and laterborn siblings. When we did these comparisons, we found that none of the nine mean differences were statistically significant (at the $p < .05$ level, two-tailed). Indeed, for only three out of the nine dependent variables was the birth order difference in the predicted direction.

BIRTH ORDER AND PERSONALITY

> *"[Born to Rebel] definitively settles the question of birth order's importance in the development of personality. I'd be surprised if there are serious scholars who can mount significant arguments against it."*
>
> —Edward O. Wilson, quoted in *The New Yorker*, 1996

Does *BTR* definitively settle the question of birth order and personality, even though it draws conclusions contrary to prevailing wisdom (e.g., Ernst and Angst 1983)? Again, the original data collection recounted in *BTR* concerns mostly eminent historical figures, who do not provide an adequate sample for testing whether birth order effects hold across non-elite populations. As I have already noted, however, Sulloway's hypotheses about birth order and personality are supported not only by his study of scientists but also by a large "meta-analysis" of the existing literature on birth order and personality. It is through this meta-analysis that Sulloway is able to contradict the assessment of researchers Ernst and Angst, who conclude from their massive review of the birth order literature that its effects on personality (and IQ) were "widely overrated." Sulloway (1996: 472; see also Sulloway 1995: 77) writes:

> "In the preface to their 1983 book, Ernst and Angst remarked that meta-analytic methods, which were just coming into usage at this time, would have provided their analysis with 'a much firmer footing' [xi]. When applied to

Ernst and Angst's own data, these methods do just this, revealing surprisingly consistent trends in birth order research."

Because these varying assessments are based on readings of the same literature—scores of studies conducted independently of either Sulloway or Ernst and Angst—examining the methodology of Sulloway's meta-analysis provides a crucial first step to judging whether Wilson's faith in the decisiveness of *BTR*'s findings about birth order and personality is justified.[7]

As the preceding quotation from Sulloway would suggest, the meta-analysis draws on "Ernst and Angst's own data"—their large review of the birth order literature. From the studies listed in this review, Sulloway first discarded any that failed to control for either social class or number of siblings, which Ernst and Angst described as the two most pervasive confounding variables in birth order research. He reports that after one does this, "196 controlled studies remain in Ernst and Angst's survey, involving 120,800 subjects." Next, Sulloway classified all of these studies into one of the personality dimensions that are collectively known to psychologists as the "Big Five": extraversion/introversion, agreeableness/antagonism, conscientiousness, neuroticism, openness to experience. Sulloway had already formulated hypotheses about the effect of birth order on each of these dimensions. For each dimension, Sulloway counted the number of studies that reported statistically significant results that supported his hypothesis (which he calls "confirmations"), and he calculated the probability of observing as many confirmations as were observed if birth order had no effect on any personality dimension and every confirmation in the literature was simply a random fluke (i.e., because Sulloway used the standard $p < .05$ level

[7] A paper by Frederic Townsend critical of Sulloway's meta-analysis is forthcoming in *Politics and the Life Sciences* (with responses from more than ten commentators and Sulloway).

for counting a finding as a confirmation, one would expect 1 out of every 20 studies to yield a "fluke" confirmation even if birth order had no effect on personality).

The results of the meta-analysis are reproduced in Table 4.3. For four of the five dimensions, Sulloway tallied far more significant findings in the direction he predicted than significant findings contradicting his hypotheses. The probability is extremely low that one would observe so many confirming results simply by random chance. These results are therefore taken to support Sulloway's hypotheses that firstborns are less open to experience, more conscientious, less agreeable, and more neurotic than laterborns. With regard to extraversion, Sulloway notes that both confirming and disconfirming results occur more often than chance would lead us to expect. His explanation is that research on extraversion "tends to lump 'sociability', a laterborn trait, with 'assertiveness,' a firstborn tendency," and he contends that the findings on birth order and extraversion are consistent with his theory when extraversion is broken down along these lines (p. 74).

Table 4.3 Results of Meta-Analysis Reported in *Born to Rebel* (p. 73)

Personality Dimension	total number of studies	findings in predicted direction	*findings in opposite direction*	null findings	"Likelihood of Outcome by Chance"
Openness to Experience	43	21	2	20	"Less than 1 in a billion"
Conscientiousness	45	20	0	25	"Less than 1 in a billion"
Agreeableness	31	12	1	18	"Less than 1 in a billion"
Neuroticism	48	14	5	29	"Less than 1 in a billion"
Extraversion	29	5	6	18	"Less than 1 in a million (but studies conflict)"
Total	196	72	14	110	"Less than 1 in a billion billion"

A reader might object that the probabilities reported in Table 4.3 are biased because studies that find significant effects would seem to be more likely to be submitted and accepted for publication than studies that find non-significant effects. Sulloway (1995) uses a formula by Rosenthal (1987) to take this problem into account, and he finds that the rate of confirmation is still well above what chance would predict. Sulloway (1995: 79) concludes from this "file-drawer test" that "by the most stringent criteria of meta-analytic excellence, birth-order effects are consistently present." By my calculations, however, the p-values that one obtains using the test that Sulloway describes are *not* consistently significant when one considers the five dimensions separately: openness (21 of 225, p=.002), conscientiousness (20 of 235, p=.008), agreeableness (12 of 165, p=.08), neuroticism (14 of 250, p=.19) , extraversion (11 of 155, p=.15). (Consistent with Sulloway's analytic decisions, all but the last of these significance tests are one-tailed.)

A reader might also complain that Sulloway should have provided a list of which studies were included in the meta-analysis and which were counted as confirmations, so that readers could replicate his findings. In reporting that "Data are tabulated from Ernst and Angst (1983)" [p. x], however, Sulloway seems to be providing the next best thing: a review source that readers can use to reconstruct his results. Moreover, by "tabulating" his results from this source, Sulloway allays the possible criticism that he was somehow biased in his reading of the studies.

Unfortunately, when a few scholars (including Judith Rich Harris [1998], John Modell [1997], and Frederic Townsend [forthcoming]) went back to Ernst and Angst to try to reconstruct Sulloway's results, they reported that they were not able to do so. Part of the confusion was that although Sulloway reports that his meta-analysis comprises "196 controlled studies... involving 120,800 subjects" (p. 72), one learns only from the table footnotes that the words "studies" and "subjects" are being used idiosyncratically, because "each reported finding constitutes a 'study'". In other words, if a study reported three findings about birth order, then the study was counted three times in Sulloway's meta-analysis, and each of the subjects was counted three times. Harris (1998) speculates that the real number of studies examined by Sulloway is no more than 115, and the real number of subjects is about 75,000 (about 40% less). Sulloway's statistical tests also should have been adjusted accordingly; it is not clear that these pooled results would have been significance once a correction for unpublished null findings was made.

But even taking into account the confusion of "studies" and "findings" does not solve the problem. Instead, Sulloway interpreted the results of many studies differently than Ernst and Angst did in their review, reporting no fewer than 45 "errors and inconsistencies" in their work (Sulloway 1998). In her critique of Sulloway's meta-analysis in *The Nurture Assumption*, Harris complains about Sulloway's failing to note that he had not strictly followed Ernst and Angst's interpretations; he replied that "[i]t is customary for researchers performing a meta-analysis of a specific literature to actually read the original literature" (Sulloway 1998). Harris contends that Sulloway's "reassessments almost always resulted in an increase in the number of outcomes favorable to his theory and/or a decrease in the number of no-difference outcomes" (Harris 1998: 369).

Sulloway, meanwhile, has charged that he had given Harris a list of corrections to Ernst and Angst after reading a draft of her critique and before her book went to press, but that she did not modify her critique accordingly.[8] This has led to a protracted disagreement between Sulloway and others. While it is perfectly possible that Ernst and Angst's review may have been rife with error, it also seems that much confusion could have been avoided had Sulloway from the outset provided a complete list of studies/findings that were counted as confirmations or null results for each dimension. To his credit, Sulloway added a note to the paperback edition of *BTR* explains that his meta-analysis was not tabulated directly from Ernst and Angst but included corrections that he had made, which has helped clear up some of the confusion.

Sulloway and Harris have also disputed how Sulloway counted interaction effects in his meta-analysis (e.g., when birth order effects were found for men in a sample but not for women) (see Harris 1998: 370). Harris contends that Sulloway's method of counting interactions substantially overestimates the true ratio of confirmations to null findings; I have

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[8] Writes Sulloway (1998): "... Harris made no attempt to verify these inaccuracies or to implement the necessary corrections in her own tallies. Instead, she has published her original tallies in unaltered form as part of her critique of my own meta-analysis. She has also withheld from her readers the extent of these errors, as

not been able to discern exactly how Sulloway handled interactions from the information that has been made available in print.

A different criticism by Harris seems to me to be much more incisive, as it could be taken to undermine the whole justification for taking seriously the results of a "vote-counting" meta-analysis in the first place. Vote-counting meta-analysis makes sense when inconsistent findings are the result of the studies having too small of a sample size for real effects in the data to be reliably revealed (at least, this is Sulloway's own justification, see p. 72). Many birth order studies do indeed have small samples (in many cases fewer than 100 subjects), and it is not surprising that such studies would only sometimes find significant birth order effects on personality even if they existed. But, if this was the principal reason why so many studies have failed to find significant birth order effects, then we would expect larger studies to be more likely to report birth order effects than are small studies. Instead, *Harris reports that exactly the opposite is the case*. Of the well-controlled studies in Ernst and Angst, 40% of the small studies (31-140 subjects) found effects supporting Sulloway's theory, while only 19% of the large studies (384-7274 subjects) did so. [9]

Indeed, disturbed by the lack of rigor of birth order studies, Ernst and Angst (1983: 245-284) conducted their own study, using a larger sample than any previous birth order study and avoiding the methodical pitfalls they outline in their review. They found no evidence of personality differences between firstborns and laterborns on any of the personality dimensions discussed by Sulloway. Although this study appears in the same book

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well as her own prior knowledge of this information. In science, the knowing publication of erroneous data is considered serious misconduct."

[9] Harris (1998) contends that the vulnerability of meta-analysis to these types of problems is one reason why many social scientists and medical researchers feel that a single, large, well-controlled study often provides better results than a "vote-counting" conglomeration of many smaller but inferior studies. This would seem especially apropos for a literature as notorious for its methodological shortcomings as birth order research has been.

that provides the basic resource that Sulloway uses for his meta-analysis, Sulloway makes no mention of these disconfirming results in *BTR*, nor does he include these results in his meta-analysis.

But even if one does not have the resources to conduct a single large study of one's own, there are more sophisticated ways of combining the results of multiple studies than the "vote-counting" method that Sulloway uses. (See Hedges and Olkin 1985 for an introduction to meta-analysis). Although the *term* "meta-analysis" was fairly new in 1983, Sulloway's method is just a straightforward application of calculating probabilities using a binomial probability distribution, which dates back to the early days of statistics. More sophisticated techniques than what Sulloway uses to statistically judge the results of multiple studies have been around since the early 1930's (Fisher 1932: 99; see Olkin 1990). As it turns out, when one goes back to Ernst and Angst, what they actually wrote was this:

> The excellent meta-analytic method with which [Smith, Glass, and Miller 1981] conducted their survey only came to our notice when this manuscript was finished. Had we been able to use this method our own survey would have stood on a much firmer footing. [Ernst and Angst 1983: xi]

So what Ernst and Angst were referring to was not meta-analysis generally—and certainly not "vote-counting" meta-analysis, as Sulloway suggests—but a specific *way* of doing meta-analysis. The study that Ernst and Angst reference is the pioneering *The Benefits of Psychotherapy* (see especially pp. 55-84), whose meta-analysis takes into detailed account differences in effect sizes, samples sizes, and differences in study design and quality, all within a multivariate regression analysis framework.[10] In fact, Smith et al. are *explicitly critical* of

[10] See Hunt (1997) for some background details of *The Benefits of Psychotherapy* and the development of meta-analysis. His discusses the "vote-counting" method in a three-page section entitled "Vote-Counting—A Plausible But Unreliable Way to Sum Up Research."

aspects of the sort of methodology for combining results that Sulloway uses.[11, 12] Seeing this sort of apparent misunderstanding of source material may be even more distressing than usual in the case of *Born to Rebel*, since the empirical research depends so much on the author's objective reading of biographies and conducting of interviews with historical experts.[13]

BIRTH ORDER AND ACHIEVEMENT

First Son
—title of a biography of George W. Bush, 1999

In addition to personality and social attitudes, Sulloway also discusses the relationship between birth order and achievement. In his theory, dominant firstborns are usually the ambitious achievers, while laterborns on average do worse but occasionally rise up

[11] For example, they write, "Nearly all previous reviewers of psychotherapy-outcome research have relied on the statistical significance found on the outcome measure to indicate whether that study supported or failed to support therapy effectiveness. However, using statistical significance confounds the magnitude of the effect produced by a treatment with the size of the sample and other technical features of the experiment, independent of the treatment effect." (Smith, Glass, and Miller 1981: 68))

[12] My own opinion of vote-counting meta-analysis was diminished further when I ran across a report of a comprehensive review of the literature on biorhythms (Hines 1998; reported in Frazier 1998). Biorhythms are based on the idea that your acuity on various dimensions waxes and wanes according to cycles whose periodicity is the same for everyone and never deviates after birth, sort of like being able to chart all the way to menopause the dates when any given woman will begin menstruating based only knowing her date of menarche. This review reported that of 134 available studies, 35 reported supportive results. If the probability of a spurious confirmation is the standard 5%, then the probability of observing this many supportive findings by chance alone is 4.37×10^{-16}. Nonetheless, the study's author carefully reviewed the individual studies involved and concluded that more than enough flaws existed in the "confirmations" to conclude that biorhythm theory is not supported by the available evidence.

[13] Research on birth order and personality published since the publication of *BTR* has been, as before, mixed (see Paulhus, Trapnell, and Chen 1999 for supporting results; Beer and Horn ms. for very weak results; and Hauser, Cartmill, and Kuo 1997 for null results). The person who has been most active in testing Sulloway's theories has been Sulloway himself. Sulloway (personal communication) has argued that the usual method of scaling personality tests does not work as well for capturing birth order effects as a scale in which people rate themselves relative to a sibling. However, these studies have not yet been published, and it is difficult to know what to make of them until they are. Based on a forthcoming paper, I have prepared commentary on this follow-up study, but I do not wish to present it here in advance of the actual publication of the study. Consequently, my comments on birth order and personality in this chapter are limited to a discussion of what was presented in *BTR* and reactions to it, although I look forward to the possibility of expanding my consideration of this topic in the future.

to become the true intellectual revolutionaries. For this reason, Sulloway speculates in *Forbes* magazine that firstborns usually make better business executives (Koselka and Shook 1997).[14]

At various places in *BTR*, Sulloway cites research claiming that firstborns have higher IQ's, higher academic achievement, and a greater probability of achieving eminence than laterborns (although he notes that the correlation with IQ is weak compared to what he reports for birth order and personality). While the institutional privileges accorded to firstborns in past eras certainly gave them educational advantages, the claim that firstborns achieve more than laterborns in contemporary, developed societies is more difficult to advance.[15]

Regarding birth order and eminence, Somit, Arwine, and Patterson (1996) look at the achievement of various leadership positions in society and conclude that there is no systematic correlation between birth order and eminence (measured by the attainment of high-ranking positions within society). Indeed, they liken the whole enterprise of birth order theorizing to a cyclically recurrent "vampire" that cannot be extinguished by either contravening evidence or rational argument. I do not believe that their work is as particularly strong methodologically, although they do appear to undermine any notion of more than a weak relationship between birth order and political eminence. In the footnotes, Sulloway offers several valid and incisive criticisms of their studies (pp. 426, 472, 520), but acknowledges the upshot of their results: "Somit et al. rightly question the relevance of birth

[14] An interviewer asked Sulloway whether graduate departments should take birth order information into account because "firstborns would be good puzzle solvers and win Nobel Prizes, but laterborns would go off to the Galapagos and lead revolutions" (*Skeptic* 4:4, p. 71). (One pauses at the thought of how, in an era of substantial electorate antipathy toward race-based and gender-based admissions criteria, evolutionary psychology could lead some to rally against birth-order-based admissions decisions.)

[15] While Sulloway claims that to have statistically documented some of these effects using meta-analysis (p. 473), I have already presented some possible problems with his use of meta-analytic techniques.

order to many aspects of political leadership, noting its meager and inconclusive influence in a variety of political contexts" (p. 473).[16]

In his *Newsweek* interview, Sulloway dismisses Somit et al.'s work as irrelevant for his theory, "The question isn't whether firstborns are more eminent than laterborns. Eminence isn't even a personality trait. It's an outcome. What's interesting is that firstborns and laterborns become eminent in different ways" (Cowley 1996: 74). Yet, in *BTR*, when Sulloway is marshalling evidence for his claim that firstborns are more "assertive" and "dominant" than laterborns, he seems to suggest that a clear relationship *does exist* between birth order and political eminence—"firstborns are overrepresented among political leaders, including American presidents and British Prime Ministers" (p. 69)—and he presents this relationship as supporting his theoretical claims about birth order and personality.

Regarding the effects of birth order on IQ and academic achievement, Sulloway cites the work of psychologist Robert Zajonc (pp. 471, 496, 510, 520). Zajonc's well-known "confluence model" hypothesizes that children are affected by the intellectual milieu of their home environment, which is turn influenced by the ages of the other persons in the home (the older, the better; see Zajonc and Markus 1975). As a result, his model predicts for most societies that firstborns will outperform laterborns, and evidence for this has been found. But, the predictions of the confluence model vary sharply from society to society; in some cases, laterborns are predicted to outperform firstborns, and support for this has also been found (Zajonc and Bargh 1980). Sulloway's evolutionary theory, meanwhile, predicts that the effects of birth order will be relatively consistent across societies (since, as he notes, his

[16] Sulloway's discussion of Somit et al. in Appendix 9 (p. 426) also shows his willingness to switch back and forth between relative birth rank and the firstborn-laterborn dichotomy as measures of birth order; he claims the difference between the two measures is merely one of "statistical power," when actually they imply very different models of birth order effects.

theory draws upon an evolutionary dynamic as supposedly timeless as Cain and Abel). Consequently, the research of Zajonc and his associates cannot be said to support Sulloway's theory.

At the same time, as I noted before, the best birth order studies use data comparing siblings from the same family (Ernst and Angst 1983). In his review of *BTR*, behavior geneticist David Rowe (1997) presents sibling data in which the correlations of IQ among siblings adjacent and removed in birth order did not vary and in which IQ also did not significantly differ by birth order. Using sibling data from the Wisconsin Longitudinal Study that I described earlier, Robert Retherford and William Sewell (1991) also found no effect of birth order on IQ or academic achievement (see also response by Zajonc et al. 1991).[17]

WHY MIGHT RESULTS DIFFER?

So far, we have seen that there may be various reasons to doubt that the effects of birth order on attitudes, personality, or achievement are nearly as strong as one might believe after reading *BTR*. If this depiction of birth order as a "bit player" in the drama of human life is correct, then how can it be reconciled with the starring role that birth order occupies in Sulloway's historical analyses, especially his quantitative findings? Perhaps birth order plays a much more important role in the determination of behaviors such as joining radical movements than in the determination of any constructs that are measured by self-report questionnaires. Divisive historical events may bring out real birth order effects that are not

[17] I have done my own preliminary examination of the effects of birth order and achievement, using data from a large survey of adolescents in the United States (the National Educational Longitudinal Survey [NELS]). Looking at achievement scores and grades for about twenty thousand students, I did find that firstborns on average tended to do better than laterborns. Yet, once you divided the sample by race/ethnicity, important differences emerged. White firstborns still performed better than white laterborns. But the differences in performance between firstborns and laterborns were not significant for either Latinos or Asians. Among Blacks,

visible from subject responses to standard survey items. Unfortunately, the systematic sampling and painstaking quantitative study of biographies necessary to carry out such studies is something that researchers other than Sulloway have not been eager to undertake.

A different possibility is that substantial birth order effects on social attitudes may have existed in the historical eras that Sulloway studies but do not exist today. Primogeniture may have encouraged conservatism among firstborns by binding them to their ancestral properties, while encouraging liberalism among laterborns by allowing them greater freedom to travel and have a broader range of experiences.[18] Another possibility is that birth order effects may be confined to elites, whether historical or contemporary.[19] Sulloway's sample includes members of lower social strata only when they rose from their conditions to make a place for themselves in history. Primogeniture certainly affected wealthy families more than peasant families who had no lands to pass on, and birth order differences in education and the opportunity to enter science or politics may also manifest themselves most strongly among elites. Of course, there is no way to compare the relative birth order effects of representative groups of elites and peasants in 18th and 19th century society.[20]

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there was even a small tendency for laterborns to outperform firstborns. These are between-family comparisons, and I caution one making too much of them.

[18] The privileged position of firstborns has been long in decline, as evidenced, especially over this century, by the steady replacement of inheritance practices biased toward firstborns with practices that divide wealth equally among children (a process which has itself been the topic of evolution-minded explanation [Hrdy and Judge 1993]). In contemporary society, Steelman and Powell (1991) find that laterborn children may even sometimes have the upper hand in receiving parental economic investments because they are born at a time when their families are more likely to be economically secure.

[19] Sulloway (1996:416-418) discounts the possibility that his results are confined either to past eras or to elites. He finds that the birth order effects in his data do not significantly vary over time, and that the effects exist among 115 scientists and historical figures in his sample that were born after 1900. Concerning elites, Sulloway (1997:382) also argues that birth order effects turn up in analyses of the "most obscure" scientists and historical figures in his data; here, however, it seems likely to us that the most obscure members of his sample are still more aptly characterized as "elite" than "typical" members of their respective societies.

[20] In our study of birth order and social attitudes, we did test whether birth order differences exist among contemporary economic or intellectual elites by restricting our sample first to respondents with upper-class parents and then to respondents who scored in the top 15% on the cognitive tests included on the 1994 GSS. In neither instance did we find any evidence of firstborns being more conservative than laterborns.

If either of these latter two possibilities are the case, it would preserve the validity of Sulloway's data analysis, but it would still undermine his evolutionary psychological theory of birth order effects, which predicts that these effects should be reasonably universal. Failures to replicate Sulloway's findings could also signal problems with his historical data or analyses. While reminiscent of some early work in sociology (e.g., Sorokin 1928), the general strategy of using biographies and historians' ratings for testing general propositions about behavior is unusual, and the lack of established procedures for this method leaves open several potential problems. One problem is that the ratings from historians were obtained through in-person interviews conducted by Sulloway well after he began constructing his arguments about birth order. And, although he used a standardized rating instrument, the interview procedures themselves are not well-documented. As a result, the possibility of substantial interviewer effects seems hard to rule out. Medical researchers prefer double-blind experiments because they have learned that physicians are vulnerable to (likely unconscious) biases when they know the experimental group to which a patient has been assigned (Meeker and Leik 1995). Likewise, we should not presume that historians' expertise somehow puts them beyond providing (likely unconscious) biased responses when interviewed by a researcher looking to test his theory.

Sulloway's reliance on biographies also results in more missing data than many social scientists would accept. For the eight variables in one of his key models, Sulloway has complete records on *fewer than 200* of the 3,890 scientists in his sample. Although Sulloway deploys sophisticated statistical methods for handling missing data (outlined by Rubin 1987), it is not clear that their use necessarily solves such a pronounced problem. It is also unclear from the text when these methods were applied, as Sulloway says only that he uses them “occasionally” (p. 453), even though his data would seem to require some accommodation for missing data in every model. One can also question Sulloway’s use of stepwise regression-like procedures to derive some of his main quantitative models.[21] Stepwise regression is frowned upon for theory testing because it often increases the apparent fit of a model by capitalizing on chance patterns in the data.[22] Perhaps because the inclusion of independent variables is based on empirical fit rather than on substantive justification, Sulloway’s theoretical arguments often have the flavor of *post hoc* attempts to explain the observed results (e.g., Wolfe 1996; Rowe 1997; Spitzer and Lewis-Beck 1999 raise similar criticisms). This is especially the case for some of the interaction effects Sulloway reports regarding birth order.[23]

In addition, Sulloway’s evolutionary explanation of birth-order effects depends crucially on his distinction between “functional” and “biological” birth order. Louis Agassiz was the fifth child out of his mother’s womb, but he is coded as a firstborn because the older four children all died in infancy (p. 23). The finding that biological birth order is unimportant once functional birth order is controlled is based on a test of only 29 biologically laterborn scientists who were raised as “functional” firstborns (note [2]81, p. 465). Not only is this a small number to be drawing crucial inferences from, but also, given the high rates of infant mortality and other sources of childhood instability in the eras

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[21] Specifically, Sulloway uses the All Possible Subsets Regression algorithm in BMDP, which finds the set of variables that yields the best Mallows’ C_p.

[22] A good discussion of the problems with stepwise regression is available at http://www.stata.com/support/faqs/stat/stepwise.html.

[23] In a footnote, Sulloway (1996:507) describes a model of social attitudes that includes an interaction between birth *rank* and sibship size. He provides no theoretical justification for why such an interaction should be included other than that the result is significant using stepwise methods in his sample. Because birth rank and sibship size are highly correlated (r = .64 in the GSS data), using the product of birth rank and sibship size is very much like using the square of sibship size, which makes more theoretical sense because it raises the possibility that the marginal effect of siblings on attitudes changes with each additional sibling (cf. Downey

predominantly represented in Sulloway's data, it is almost certain that more than 29 cases of laterborns raised as "functional" firstborns exist among the almost 4000 scientists in the sample, implying that there is error in Sulloway's measure of functional birth order.

What if the "functional" status of some sample members was investigated a bit more thoroughly than others, *precisely because they otherwise would have been exceptions to the study's general findings*? Such a bias need not be at all conscious on Sulloway's part, and even if it was, scientists are supposed to think more carefully about the observations that do not seem to fit their theory. If analyses suggest that a case is an outlier, it is reasonable to examine that observation in more detail to make sure that the data is correct.

Consider that at least 3 of the 29 functional firstborn/biological laterborn scientists—Louis Agassiz, Georges Cuvier, and Tycho Brahe—are likely among the most eminent 1 percent of all the scientists Sulloway examined. Imagine randomly drawing balls out of a giant urn, in which 1% of the balls have been colored red (to represent the most eminent scientists) and the rest are white (to represent all the other scientists). If one draws 29 balls at random, the probability that one will draw three or more red balls (i.e., one of the top 1% of scientists) is only .003. Thus, it appears that the probability of being a biological-laterborn-recoded-as-a-functional-firstborn is correlated with how prominent of an exception the scientist would have been to Sulloway's theory had the scientist remained coded as a laterborn. Consequently, we should perhaps not be surprised that Sulloway found strongly significant differences between these 29 scientists and other biological laterborns.[24]

1995). In our study of birth order and attitudes, we found no evidence of an interaction of birth rank and sibsize in GSS data once the square of sibship size was controlled.

[24] A study of adopted children and their biological siblings by Beer and Horn (ms.) reports only very weak effects of rearing order on personality.

An entirely different question is whether Sulloway's theory makes sense from an evolutionary standpoint anyway. When we tally up the evolutionary benefits of tailoring one's behavioral template (e.g., personality and attitudes) to maximize the resources received during one's childhood, we must subtract out the evolutionary costs of having this behavioral template dominate one's interactions in subsequent environments. Just because being a rebel paid off when you were a kid, why should evolution have built your brain so that you also tended toward rebellion as an adult? Wouldn't a better brain allow for strategic flexibility throughout the life course? In addition, we have no evidence that all the various behaviors, personality traits, and attitudes that Sulloway associates with firstborns and laterborns really do maximize parental investment when they are performed?

What we may have is a sort of environmental sensitivity by fiat, in which a cognitive adaptation is proposed to be highly sensitive to environmental cues when it fits what the theorist is trying to explain, but is not sensitive whenever environmental sensitivity would cause problems for the theory. Because natural selection does not necessarily converge upon the best outcomes that we can imagine, there is no reason that mechanisms must evolve to respond to environmental cues in the best possible way. But what must be recognized in these instance is that a sensitivity to these conditions but not other conditions is not something that we could have *deduced* in advance from evolutionary theory. There is no *a priori* reason to suppose that natural selection is going to build mechanisms that "stick" after being set to what maximizes the resources acquired in childhood. Consequently, in no sense does the theory of evolution by natural selection *imply* that strong and enduring birth order effects exist, even though an evolution-minded explanation can surely be *devised* for whatever birth order effects can be shown to exist.

HAZARDS OF DARWINIAN BIOGRAPHY

Compare these passages from a Forbes Magazine interview with Frank Sulloway after *BTR* was published (Koselka and Shook 1997: 150):

> [Quoting Sulloway:] "I knew Bill Gates wasn't a firstborn as soon as I heard that he dropped out of Harvard after his sophomore year over the objections of his parents. A firstborn would never do that. He would have stuck it out to get a degree to please his parents."
>
> "Steve Jobs was adopted at birth, but grew up in the role of the firstborn. Sulloway thinks it was predictable that Jobs would insist on maintaining a proprietary system rather than licensing Apple's systems. Sulloway says firstborns don't like sharing. They are conscientious but territorial and inflexible."

Then again, Jobs dropped out of college after one semester at Reed, and Gates business reputation is long on territoriality and short on sharing. The *Newsweek* cover story on Sulloway also uses Bill Gates as one of its celebrity exemplars, but it offers a different way that Gates fits the theory: "Since later-borns are more open to radical new ideas, it's a no-brainer that the techno-revolutionary behind Microsoft has an older sibling" (Cowley 1996: 74). But which executive should get more credit for shepherding radical ideas: Bill Gates or Steve Jobs?

Both proponents and critics of evolutionary psychology have noted that once one starts to view life though the evolutionary psychological lens, seeming applications of the theory can appear everywhere: in one's own life, in the foibles of one's friends, and in the tabloid dramas of celebrities (Lewontin, Rose, and Kamin 1984; D.S. Wilson 1999; Burnham and Phelan 2000). For many, biographical applications make the perspective as a whole, or particular theories within the perspective, seem much more compelling. When writing about evolutionary psychology for a wider audience, there is an obvious incentive to portraying the Darwinian shadow as looming large over the course of people's lives. An appealing rhetorical strategy then is to show how an evolutionary psychological theory can be used to make sense of the behaviors or decisions of someone readers know. *Forbes* and *Newsweek* used celebrity examples to show how their biographies "fit" Sulloway's theory. But our reading of these examples suggests a problem with Darwinian biography: a person's life is sufficiently complex that one can pick and choose episodes that conform to the expectations of the theory (e.g., Wolfe 1996; Modell 1997; Spitzer and Lewis-Beck 1999 raise similar criticisms).[25] A related problem that we will also see in this section is that there also exists considerable leeway in how a given episode in an individual's life can be interpreted, and some interpretations can be favored over others not on the basis of biographical evidence but because they conform best to the theoretical point one is trying to make.

Sulloway invokes dozens of biographical examples in support of his birth-order theory in *BTR*: Henry VIII and Carlos the Jackal are among those who get extended treatment. Yet, as the epigraph to this section would suggest, the most prominent example that Sulloway cites is that of Charles Darwin himself. Indeed, Sulloway writes that "In the spirit of Darwin's own empirical tenacity, I have ended up studying more than 6,000 lives in the hopes of understanding just one—that of Darwin—well" (p. 456). Perhaps not surprisingly, the life of Darwin has also provided confirmatory fodder for other books about evolutionary psychology. In his widely read book *The Moral Animal*, Robert Wright (1994: 14) makes extensive use

[25] Responses to survey items, like that of the General Social Survey, could also be seen as a kind of episodic behavior, whose potential for making statements about *the* attitudes held by a person rests on assumptions of the durability of these responses over time.

of Darwin's life for illustrative purposes, and he offers the following defense of this tactic:

> I like to think that if Darwin were looking back today, with the penetrating insight afforded by the new Darwinism, he would see his life somewhat as I'll be depicting it.
>
> Darwin's life will serve as more than illustration. It will be a miniature test of the explanatory power of the modern, refined version of his theory of natural selection... If we're right, the life of any human being, selected at random, should assume new clarity when looked at from this viewpoint. Well, Darwin hasn't exactly been selected at random, but he'll do as a guinea pig.

A useful exercise in the reliability of Darwinian biography—in both senses of the term—is to read Sulloway's and Wright's accounts of the same events in Darwin's life against one another. Both authors discuss Darwin's relationship with the "co-discoverer" of the theory of evolution by natural selection, Alfred Russel Wallace. Although Darwin's notebooks show that he had the basic idea of the theory since at least the late 1830s, he did not publish it, even as he spent the next two decades privately amassing evidence in its support. In June 1858, Darwin received in the mail a manuscript from Wallace, a young naturalist collecting specimens in Malaysia, that outlined Darwin's incipient theory in striking detail: "Even his terms now stand as heads of my chapters," Darwin complained (Wright 1994: 302).

Wallace did not explicitly ask Darwin to submit the manuscript for publication, but he did ask Darwin to forward the manuscript to Charles Lyell, an eminent geologist and close friend of Darwin's. Darwin dutifully did so, along with a letter asking Lyell if he thought it would be acceptable to publish Wallace's paper along with a brief sketch of the theory that Darwin had earlier sent to Lyell, or if doing so would rob Wallace of rightful credit. Lyell consulted with fellow scientist Joseph Hooker and decided that Darwin's and

Wallace's theories should be treated as equals. Their papers were presented together at the next meeting of the elite Linnean society. The next year, Darwin published *The Origin of Species* and immediately became the figure everyone associated with the theory. Even Wallace later published a book on natural selection under the title *Darwinism* (1889).

Sulloway (1996: 103-4) presents this episode within a broader discussion of "magnanimity in science." In his view, Darwin was disappointed yet clearly willing to step aside and let Wallace take full credit, until he was persuaded by Lyell that it would be acceptable to present the two papers together and share priority. Darwin wrote in the letter to Lyell that "I would far rather burn my whole book than that [Wallace] or any man sh^{d} think that I had behaved in a paltry spirit."[26] After learning of the copublication of the papers, Wallace replied that "it would have caused me much pain and regret had Mr. Darwin's excess of generosity led him to make public my paper unaccompanied by his own much earlier & I doubt not much more complete views on the same subject." Some years later, Darwin told Wallace that "most persons would in your position have felt some envy or jealousy. How nobly free you seem to be of this common failing of mankind." Sulloway quotes Robert Merton (1973: 289) as saying that the relationship between Darwin and Wallace represents a extraordinary case of two scientists trying "to outdo one another in giving credit to the other for what each had separately worked out."

Because Sulloway's theory posits that firstborns tend to be more jealous, assertive, antagonistic, and ambitious than laterborns, he predicts that firstborns will be more likely than laterborns to engage in acrimonious priority disputes (Sulloway 1996: 68-75). The nasty dispute between firstborns Newton and Leibniz over the invention of the calculus is

[26] All of the quoted text in this paragraph is presented in Sulloway (1996: 104).

cited as an example of theory in action (p. 101). So is the amicability of Darwin and Wallace, as both were laterborns. Writes Sulloway, "it is hard to imagine two firstborns reacting so magnanimously under the same circumstances." (p. 104)

Wright, on the other hand, takes a much less charitable view of Darwin's motives in this affair. He considers the affair to be Darwin's "biggest single crime," and he believes that Wallace was "taken to the cleaners" (Wright 1994: 307, 304). Wright regards the piousness and magnanimity of Darwin's letter to Lyell to be a (perhaps unconscious) effort to steer Lyell toward the alternative of copublication.[27] Wright contends that Darwin must have known that even if their names were given equal billing in front of the Linnean Society, his stature would lead the theory to be soon attached to his name rather than that of the young and unknown Wallace, far away in Malaysia. As it turned out, Wallace did not get much by way of an equal billing, as Hooker and Lyell's introduction to the Society announced that "while the scientific world is waiting for the appearance of Mr. Darwin's complete work, some of the leading results of his labours, as well as those of his able correspondent, should together be laid before the public" (Wright 1994: 304)[28]

Wright sees the episode as exemplifying a number of ways in which evolutionary psychology has shaped human minds. He points to the concern for priority as reflecting the

[27] Wright presents part of this letter with his own interpretation of its true subtext in brackets: "I should be extremely glad now to publish a sketch of my general views in about a dozen pages or so; but I cannot persuade myself that I can do so honourably. [maybe you can persuade me.] Wallace says nothing about publication, and I enclose his letter. But as I had not intended to publish any sketch, can I do so honourably, because Wallace has sent me an outline of his doctrine [Say yes. Say yes.] … Do you not think his having sent me this sketch ties my hands [Say no. Say no.] … I would send Wallace a copy of my letter to Asa Gray, to show him that I had not stolen his doctrine. But I cannot tell whether to publish now not be base and paltry [Say nonbase and nonpaltry]." (Wright 1994: 303)

[28] Wright presents Wallace as essentially too young, naïve, and far away to recognize that he was being possibly be being duped and that he should be anything other than thrilled that his work was being read with Darwin's in front of a prestigious society. Wright (1994: 304) produces a beautifully poignant quote from a letter by Wallace to his mother: "I sent Mr. Darwin an essay on a subject on which he is now writing a great work. He showed it to Dr. Hooker and Sir Charles Lyell, who thought so highly of it that they immediately read it before

innate drive for status that we all (and especially men) have as the result of its payoffs for reproductive success in our evolutionary past. The event is also supposed to show how moral appeals are often used to serve (evolutionary selfish) ends, and how pliable values are in response to threats to status. Because Wright suggests that Darwin's manipulation of Lyell may have been unconscious, he also takes the event as possible evidence of *self-deception*—how evolution could have shaped us to behave in ways that serve evolutionarily selfish ends even when we consciously believe that we are benignly or altruistically motivated (see Trivers 1985).

In summarizing his arguments, Wright contends that when viewed through the lens of evolutionary psychology, Darwin's entire career "assumes a certain coherence." He writes:

> It looks less like an erratic quest, often stymied by self-doubt and undue deference, and more like a relentless ascent, deftly cloaked in scruples and humility. Beneath Darwin's pangs of conscience lay moral positioning. Beneath his reverence for men of accomplishment lay social climbing. Beneath his painfully recurring self-doubts lay a fevered defense against social assault. Beneath his sympathy toward friends lay savvy political alliance. What an animal! (Wright 1994: 310)

But both Wright and Sulloway have given Darwin's life coherence.[29] So have Darwin's Marxist biographers (e.g., Desmond and Moore [1991]). But how much of this coherence is in Darwin's life and how much is in the eye of the biographer? What the juxtaposition of Sulloway and Wright here shows is that two people can having strikingly different interpretations of the motivations underlying an individual's behavior and that both interpretations can be presented with earnest coherence from the perspective of evolutionary psychology.

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the Linnean Society. This assures me of the acquaintance and assistance of these eminent men on my return home."

[29] In saying this, I should make plain that Sulloway has a much longer and more respected history as a student of Darwin's life, and that Sulloway's quantitative data analysis draws on ratings from other expert historians.

One thing to keep in mind is that, just as is the case with other behavioral science perspectives, evolutionary psychology's evidence is generally statistical evidence, a demonstration of tendencies rather than certainties. Many tendencies are strong enough as to be distinguished from chance but are quite weak in terms of how much they augment our power to predict outcomes in specific instances. When dealing with a simple bifurcation of the population into two groups, the variation within each group is likely to overwhelm the differences between groups, even when these differences are statistically significant. Even if birth order effects are really as powerful as Sulloway claims, it would likely still be folly to make statements about how "I knew X was a firstborn" or about how someone is a "typical laterborn" based on single biographical episodes (or perhaps even an individual's entire biography). This is a problem extending far beyond evolutionary psychology, and suspicions should be raised whenever a single explanatory variable is proposed as "the cause" of why a particular outcome was observed for a particular individual.

Can reasoning about ultimate causes lead us to understand what was Darwin's *real* motivation in his dispute with Wallace? Even if Sulloway's theory about birth order proves to be wrong and so something we should discard, this would not strengthen the case for Wright's account being "correct." Sulloway and Wright look at the same documents and see different proximate motivations: genuine selflessness on the one hand and strategic disingenuousness on the other. One can imagine more detailed argumentation (or, for that matter, new documents) that tips the scales in favor of one interpretation over the others, but it is much harder to imagine a valid adjudication of what Darwin was really thinking emerging as the result of further reflection about ultimate causes.

As such, I question whether we should expect evolutionary psychology to help us to figure out the motives of specific individuals in specific situations. What applying evolutionary psychology to individual lives too often reveals is not the explanatory power of the perspective, but its lack of explanatory restrictiveness. Whether Darwin acted "magnanimously" toward Wallace or "took him to the cleaners" is a question that is likely best answered by close attention to the historical details of the episode itself and not the details of our lives in the Pleistocene. Where the real possibility of valuable contribution by evolutionary psychology lies is in helping us understand why people might have the particular proximate motivations that their behaviors seem to indicate that they have (Buller 1999).

CONCLUSION

At the beginning of this chapter, I described some of the lavish praise that *BTR* had received upon its publication. The amount of evidence that Sulloway presents in support of his theory is considerable. Yet, I have presented evidence and arguments on a number of fronts for why his portrayal of birth order's influence in *BTR* is likely overstated. I am not saying that birth order effects do not exist, but indications are that they are smaller than the impression one gets from reading *BTR*. My conclusion is that both Sulloway's study of historical figures and his meta-analysis of the personality literature contain sufficient sources of possible bias that the effects he reports could be largely artifactual. As a result, the question of whether and how birth order affects our personality, attitudes, and behavior may not be much illuminated by any of Sulloway's quantitative studies presented in *Born to Rebel*, as real effects and possible sources of bias cannot be untangled. (Of course, criticisms can also be made of the limitations of studies reporting null findings, highlighting the difficulties of bringing social scientific debates on even specific empirical points to closure.)

If a pessimistic assessment of the theory does turn out to be correct, then those who had waxed extravagantly about the revolution that *BTR* would bring should perhaps feel a bit sheepish. Even granting the conventionalized extremity of book reviews, how should we take someone who predicts that a newly-published trade book will be as influential as the great works of Freud and Darwin? No matter: evolutionary psychology got another fifteen minutes in the popular-intellectual sun, and mass-media Darwinism has long since moved on to other attractions. Exactly twenty-three months after *Newsweek* had published its story on Sulloway, its cover story asked "Do Parents Matter?" to introduce Harris's controversial *The Nurture Assumption*. The *New Yorker* also ran a long, extremely enthusiastic story on Harris just as it had on Sulloway. Once again, journalistic enthusiasm was following the lead of some prominent evolutionary scholars. In the book's Foreword, Steven Pinker wrote: "Being among the first to read this electrifying book has been one of the high points of my career as a psychologist... I predict that it will come to be seen as a turning point in the history of psychology" (Harris 1998: xiii).

Like *BTR, The Nurture Assumption* advanced claims about human development—how individuals come to think and act as they do—that were dramatically at odds with received opinion. In briefest detail, Harris's main arguments are that what parents do has virtually no effect on the ultimate personality development of their children, except in extreme cases, and that children's interactions with their peers do have a profound effect upon development. Although evolutionary reasoning does not play as prominent of a role in her book as Sulloway's, she does suggest an adaptationist account of why natural selection might have produced a human disposition to be more influenced by peers than by parents. In Chapter 6, I consider Pinker's treatment of part of this account.

What is interesting about the juxtaposition of Sulloway's and Harris's theory is that both were hailed by (different) evolution-minded scholars as potentially revolutionary, even though they are *incompatible with one another*. Sulloway's theory emphasizes the importance of a child's place in the sibling structure and does not consider a child's place among one's peers at all, where Harris's theory gives the latter center stage. Sulloway sees personality as importantly shaped by what children do to maximize the attention and resources that they receive from their parents, while Harris grants little consequence to these sorts of influences on ultimate personality development and instead emphasizes the importance of what children do to win the respect and friendship of peers. The incompatibility of their theories explains why Harris devotes an appendix of her book to criticizing Sulloway's meta-analysis of the literature on birth order and personality. His conclusions, if correct, would undermine her own claims.[30]

This contradiction was not noticed by most of the journalists writing about *The Nurture Assumption*, who showed no awareness of the large stories that magazines had written about Sulloway two years earlier. If we were to dissect why *BTR* and *The Nuture Assumption* received so much coverage, I think their use of evolutionary reasoning plays an important part: in the popular market, Darwin sells. Science journalists also have the luxury of being able to move on every couple of months to whatever new revolutionary-evolutionary theory happens to strike their fancy, and they suffer few consequences when a theory that

[30] For his part, Sulloway (1998) said in an interview that his and Harris's theory "overlap in important ways." But he also states that Harris "is too single-minded when she denies the importance of systematic within-family differences." Also, the virtually complete absence of peers from *BTR*'s consideration of childhood environments also sits uneasily with the idea that the theories can be so easily reconciled.

they melodramatically promoted turns out not to be as well-supported as it initially appeared. It is extremely easy for a hit-and-run journalist or commentator to equate the skepticism of established scholars to an outsider's ideas with an elitist or disciplinary bias or an unwillingness to embrace anything new. In the case of skepticism toward evolutionary psychological theories, critical reactions can be depicted as ideologically blind expressions of political dogma, regardless of whatever arguments are provided.

Consider *Skeptic* magazine, whose unbounded enthusiasm for *BTR* I described at the beginning of this chapter. Michael Shermer, the magazine's editor and author of a popular book called *Why People Believe Weird Things*, considered different commentator's assessments of Sulloway's book. "[T]he reviews of Sulloway's book have been revealing. In general, scientists were very enthusiastic, nonscientists were more cautious about it, and journalists did not understand it." The "nonscientists" he refers to are a sociologist, historian, and a research psychiatrist.[31] Given some of the potential methodological problems we have seen with Sulloway's book, what we might find "revealing" in this pattern of reviews is how the scientists' high enthusiasm was unwarranted. But, of course, Shermer's assessment is exactly the opposite: the scientists' reactions are granted the position of privilege (even on social science topics), and the "nonscientists" are faulted for being cautious—or, should I say, *skeptical*—toward a set of theoretical claims that were putatively revolutionary in their implications but had not been subjected to normal peer review, much less independent efforts at testing or replication.

[31] Alan Wolfe, Roy Porter, and Nancy Andreasen, respectively.

Why were the "nonscientists" skeptical about *BTR*? Was it for the reasons they present in their review? No, Shermer chalks up the reactions of social scientists and historian to political and disciplinary bias:

> If Sulloway had discovered a cause of schizophrenia or depression, he would be praised to the high heavens because these are 'safe' subjects. Openness to radical ideas, especially when dealing with real-world events like political and religious revolutions, are not. We do not normally use science to study these subjects. Instead, we use social theory, textual deconstruction, and social-class analysis to politicize them. Sulloway has grafted a Darwinian explanation onto his data, and there is no greater sin in the eyes of sociologists and social theorists than to imply that our biology determines us in some way.[32] (p. 66)

Shermer concludes by charging "historians, sociologists, and many in the humanities" are guilty of being "cognitive creationists" because of their resistance to evolution-minded work like Sulloway's.

Earlier I used the term *selective skepticism* to describe an instance in which one urged tough-minded skepticism with regard to the claims of a disliked theoretical perspective while being credulous toward the claims of a favored perspective. I did so in noting the complaint of evolutionary psychologists that explanations of social behavior that propose adaptations are subjected to much more scrutiny by traditional behavioral scientists than are explanations that make no adaptationist claims. But here we have the editor of *Skeptic* indulging in a different flavor of selective skepticism. In this flavor, the Any-Darwin-Will-Do-Raspberry, normally tough critical standards are relaxed for a theory that is couched in the appropriate evolutionary argot, especially if the theory can be wielded like a truncheon to make a point about the pseudointellectualism of the social sciences or humanities.

[32] Actually, given their strong objections to the charge of biological determinism, there may be no greater sin in the eyes of *evolutionary psychology* than to imply that their perspective holds that our biology determines us.

Of course, we should not make too much of one loose cannon. But one need not look far to find evolutionary psychologists dismissing critics wholesale as politically biased, comparing their critics to Creationists, and using Darwinian psychological theories to "explain" critics' resistance (Thornhill and Palmer 2000; Lopreato and Crippen 1999; La Cerra and Kurzban 1995; Wright 1994b). An easy conclusion that sociologists could draw here is that the selective skepticism of evolutionary psychologists and their "skeptical" admirers is based on an irrational and uninformed hostility toward sociology. The fault is with *them*, not with *us*. To be sure, some of the fault is with them, for not consistently applying the principles of scientific discourse and critical thinking that they espouse. But, just as assuredly, we share the blame that the dialogue about behavioral explanations has come to be so poisoned that even valid criticisms that social scientists may raise about a theory like Sulloway's are quickly dismissed as posturing. For one thing, there are still too many sociologists who have a kneejerk negative reaction against any kind of Darwinian explanations. I discuss this further in the concluding chapter. For now, however, let me say that sociology should be seen as a field defined by the nature of its answers—i.e., that they are not "psychological" or "biological"—rather than by a set of interests, questions, and ways of framing questions. Unwillingness to engage evolutionary and other biological theories fairly damages the credibility of sociology and diminishes the extent to which sociologists are listened to outside of our disciplinary journals and meetings.

This said, when you look at Sulloway and Harris, what you have is two Darwin-throated cries of revolution, less than two years apart, which not only contradicted prevailing views about development but also contradicted each other. How should one expect those who are in the midst of devoting careers to the study of human development respond? Even when we dismiss as obviously untenable the seeming calls for immediate *acceptance* by writers like Shermer, what should we expect with regard to efforts at testing? Given that active researchers in any area often feel hopelessly overextended with their own projects, to what extent can we expect researchers to stop what they are doing to test the claims of a book like *BTR*? What about when one can open the book and immediately see what may be red flags of problems—for example, in *BTR*'s case, the doing of "Darwinian biography," the portrayal of "vote-counting meta-analysis" as a cutting-edge methodology, and the extrapolating of regression coefficients calculated from a historic, non-representative sample to the general population of the book's readers? In such circumstances, I think that we should be pleased at the efforts of testing that do get made, but we should be neither surprised nor contemptuous if widespread empirical attention from researchers in an area is not immediately forthcoming. Also, when researchers do gamely examine a widely-publicized, impeccably-blurbed, evolution-minded contribution to their area and come away disenchanted, we should not be surprised if they seem jaded in their first reactions to the next big Darwinian publishing event that comes along.

It is by now a running theme of this dissertation that the considerable publicity that evolutionary psychology receives may have important negative consequences for its intellectual development. Obviously, hosting contradictory attempts at revolution every couple of years is a good way for evolutionary approaches to gain publicity but not to gain credibility among active researchers in a substantive area. The retort that evolutionary approaches cannot be blamed for the excesses of journalists would be more convincing were it for the fact that both Sulloway's and Harris's books were published with considerable endorsement from well-established evolution-minded scholars. Because evolution-minded scholarship is not a monolith, contradictory views among its members are to be expected about what new theories seem most promising as scientific breakthroughs. But one thing that such contradictory enthusiasms illustrate is that even when other social scientists do believe that more should be done to incorporate Darwinian reasoning into their areas, deciding what work should serve as models or authorities is far from straightforward.

As scientists are well aware, however, crying breakthrough or revolution can quickly seem like crying wolf when claims do not pan out, and so one might pine for more even-temperedness when the weighty names of Darwinian scholarship decide to blurb books outside their area of expertise. In the last chapter, Steven Pinker offered a blurb of considerable flourish (but perhaps questionable taste) when he wrote that *A Natural History of Rape* would force intellectuals to decide between "established dogma and ideology" and "the welfare of real women in the real world." Evolutionary psychologists themselves may face a choice between "promoting themselves and the book sales of their colleagues" and "contributing to the health of evolutionary psychology as a scientific project."

APPENDIX: More detailed description of Freese, Powell, and Steelman (1999) study of birth order and social attitudes.

DATA AND MEASURES

Data

We use data from the 1994 General Social Survey (GSS), conducted by the National Opinion Research Center. GSS is a full probability sample of noninstitutionalized, English-speaking adults in the United States. In 1994, as part of a special module on family mobility, GSS respondents were asked to provide background information on each of their siblings, including their year of birth. To our knowledge, no other data set that contains information on a respondent's birth order combines GSS's large, nationally representative sample, high-quality data-collection techniques, and variety of questions on social and political attitudes.

A difficulty in testing birth order theories is that many individuals' early family lives do not lend themselves to easy classification as firstborns or laterborns. We use a subsample of GSS respondents that excludes only children, respondents with any step- or half-siblings, and respondents who report having a sibling born the same year as they. Only children differ from firstborns in that they do not compete with younger siblings for parental attention.[1] Step- and half-siblings imply varying relations in a family between children and caregivers that may complicate the allocation of parental resources and may unfairly undermine the expectations of birth order theories. Siblings born in the same year are

[11] Sulloway (1996: 23) writes that only children "represent a kind of 'controlled experiment'—what it is like to grow up unaffected by birth order or sibling rivalry," but he usually codes only children as firstborns in his analyses. Including only children as firstborns compromises the evolutionary theory that firstborns' social attitudes develop in response to rivalry from their younger sibling(s), which is why we exclude them here.

assumed likely to be twins, and twins unfortunately have received little consideration in the birth order literature. In auxiliary analyses, we retain only children and respondents with step- or half-siblings, but this has little effect on the overall patterns we observe (see Extra Table 6A-A).

After also excluding respondents who failed to report the birth year of any of their siblings, the sample used in our main analyses contains 1945 of the original 2992 respondents. The number of cases used in our analyses is sometimes considerably fewer than 1945 because the 1994 GSS used a split ballot design and most of the questions eliciting social attitudes were administered only to a randomly selected subset (1/3 to 2/3) of all respondents. Preliminary analyses using items administered to the whole sample suggest no systematic differences among respondents receiving different ballots.

Like the data used in Sulloway's and most other birth order studies, GSS allows comparisons between firstborns and laterborns from different families. Some have argued that birth order research ideally should use data that allow for the direct comparison of siblings (Retherford and Sewell 1991), although Ernst and Angst (1983) find that studies using inter- and intra- familial data yield similar results.[2] We are able to supplement our analyses of GSS data with intrafamilial comparisons by combining GSS data with data from the Study of American Families (SAF) (Hauser and Mare 1997). SAF attempted to interview a randomly selected sibling of 1994 GSS respondents; in all, 1,115 sibling interviews were conducted. SAF includes a small subset of the social attitude items used in GSS. When examining sibling pairs from SAF/GSS, we exclude step- and half-siblings and

[2] Ernst and Angst's (1983: 170-1) discussion of birth order studies that use parent ratings of siblings may be read as an exception to their overall conclusions on this point.

twins, and we also exclude cases in which both siblings interviewed are laterborns.[3] While the SAF data enable us to compare siblings' attitudes, two caveats should be made: SAF has a low response rate (43%) and the range of attitudinal items on SAF is much narrower than GSS.

Measures of birth order

Extra Table 6A-B presents means, standard deviations, and descriptions of all of the key variables used in this study. We measure birth order as a dichotomous variable indicating whether the respondent is the firstborn child in his or her family, as indicated by the year-of-birth information provided by respondents. This tactic is the most common way of measuring birth order in previous research. Sulloway's work has been criticized for using a measure of what he calls "functional" birth order, in which he makes case-by-case adjustments for instances in which early family environment is inconsistent with biological birth order—such as when, for example, divorce and remarriage lead a firstborn child to be raised with older step-siblings.[4] As mentioned earlier, we dropped all respondents with step-

[3] The number of cases in our analysis of SAF data is considerably fewer than 1,115 partly because of the exclusion of these respondents, but also, as mentioned above, because the split ballot format of the 1994 GSS led to some attitudinal items being asked to only a randomly selected subset of respondents. The low response rate on SAF resulted primarily not from refusals to participate but from GSS interviewers failing to get adequate contact information on the randomly selected sibling.

[4] Sulloway's evolutionary explanation of birth order effects depends crucially on this distinction between "functional" and "biological" birth order. However, the finding that biological birth order is unimportant once functional birth order is controlled is based on a test of only 29 biologically laterborn scientists who were raised as "functional" firstborns (1996: 465). Given the high rates of infant mortality and other sources of childhood instability in the eras predominantly represented in Sulloway's sample, it is almost certain that more than 29 cases of laterborns raised as "functional" firstborns exist among these scientists. This raises the possibility that the "functional" birth status of some sample members was investigated more thoroughly than others *precisely because they otherwise would have been exceptions to the study's general findings*. At least 3 of the 29 scientists—Louis Agassiz, Georges Cuvier, and Tycho Brahe—are probably among the most eminent 1% of all the scientists Sulloway examined, and they would have stood out as prominent exceptions to the overall findings had they remained coded as laterborns. If the probability of being recoded as a "functional" firstborn is correlated with the probability of otherwise being a (prominent) deviation from the overall pattern, then we

or half-siblings from our sample; by doing this, we attempt to maximize the correspondence between biological birth order and functional birth order.[5] In addition, we also tested other operationalizations of birth order: the number of older siblings (birth rank); the number of older siblings divided by the total number of siblings (relative birth rank); the number of older brothers; the number of older children of the same sex; and a trichotomous variable differentiating firstborns, middleborns, and lastborns. None of these alternative measures yielded patterns substantively different from those presented here.

Measures of social attitudes

GSS contains a large number of questions on social attitudes. We sought to test a respondent's social attitudes in six domains: (1) political identification, (2) opposition to liberal social movements, views on (3) race and (4) gender, (5) support for existing authority, and (6) "tough-mindedness." Initially, we chose 24 items and scales that represented these broad headings while remaining consistent with many of the specific assertions and historical examples presented in Sulloway's research.[6] These dependent variables are described in Extra Table 6A-B and in the text below, and their analysis comprises the primary focus of this paper. We later expanded our inquiry to include all (202) attitudinal items which may

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should not be surprised that Sulloway was able to report strongly significant differences ($p < .001$) between these 29 scientists and other biological laterborns.

[5] In Sulloway's historical data, another major cause of discrepancies between "biological" and "functional" birth order is infant mortality. We assume that measurement problems due to infant mortality are greatly diminished in a contemporary sample such as ours. Moreover, restricting the sample to only those respondents who reported having no deceased siblings does not affect the general pattern of results we report.

[6] Alphas for each scale are presented in Appendix B. The scales with the lowest alphas (individualistic views about racial equality, support for English-only measures) are based on variables that were presented together as a set on the GSS. We tried to use existing GSS scales when possible to avoid the possible criticism that our scale construction decisions were (unintentionally or otherwise) biased against the hypothesis. Certainly, low alphas can lead to attenuated estimates of real effects; however, three things should be pointed out about the scales with the lowest alphas. First, the estimated effects of birth order on these scales are opposite the predicted direction. Second, as will be shown in Table 2, other independent variables did exert a significant effect on these scales. Third, when we look at the items used to create the scales individually, we observe no significant effects in the predicted direction.

distinguish respondents on the basis of conservatism, support for existing authorities, or "tough-mindedness." We later summarize the results of this broader inquiry.

Political identification. We measure a respondent's political identification with an item in which respondents were asked to place themselves on a seven-point scale ranging from "extremely liberal" to "extremely conservative," as well as a similar item ranging from "strong Democrat" to "strong Republican." We also use support for George Bush in the 1992 Presidential election.

Opposition to liberal social movements. Consistent with the longstanding stereotype about the "liberal" laterborn and "conservative" firstborn, Sulloway provides historical evidence that laterborns have been both overrepresented among prominent liberal social reformers and underrepresented among those who have resisted liberal social change. We test whether firstborns are less supportive than laterborns of a variety of traditionally liberal movements and causes: abortion rights, environmentalism, free speech, social welfare programs, the effort to decriminalize marijuana, the right-to-die movement, and animal rights.

Conservative views on race and gender. Sulloway asserts that firstborns should be more resistant than laterborns to initiatives for racial and ethnic equality. Of all the social reform movements studied by Sulloway, the most disproportionate number of laterborns is observed among participants in the abolitionist movement (1996:152). Elsewhere, Sulloway describes firstborns as "particularly inclined toward racism" (p. 286; see also Sherwood and Nataupsky 1968; Lieberman and Reynolds 1978). Because this latter conclusion is drawn from a sample that is almost exclusively white, we restrict our sample to whites when testing for birth order differences in beliefs about racial equality. We measure attitudes toward racial reforms both in terms of attitudes toward African-Americans and attitudes toward immigration. For the former, we test whether the respondent believes the government is too generous to blacks, that whites should be able to segregate themsleves from blacks if desired, and that the economic differences between blacks and whites are caused by racial differences in "in-born ability" and "motivation or will power." For attitudes toward immigration, we test a respondent's opposition to providing benefits to immigrants and support for laws requiring government documents to be only in English.

To support his contention that firstborns have more traditional beliefs about gender than laterborns, Sulloway (1996: 154-158) presents evidence from a sample of female American conservatives and reformers. He notes also that Anita Bryant and Phyllis Schafly are firstborn women who have outspoken in their support of traditional gender roles, while Susan B. Anthony, Elizabeth Cady Stanton and other suffragette leaders were predominantly laterborns (1996: 154). We examine respondent support for a traditional division of labor between spouses, in which the husband is the breadwinner while the wife stays home and looks after their family. We also examine respondent beliefs about the appropriateness of mothers remaining in the labor force and the appropriateness of women seeking political offices.

Support for existing authority. As noted above, the idea that firstborns identify more strongly with authority than laterborns goes back at least to Adler (1928). Sulloway supports this idea with examples from both the French and American revolutions in which laterborns rebelled against monarchies while their firstborn siblings or sons remained staunch royalists. We measure attitudes toward obedience with an item asking how important the respondent believes it is to teach a child to obey, as compared to teaching the child to work hard, help others, be well-liked, or think for her/himself. Support for existing authority is measured by a respondent's confidence in those running major social institutions, including banks, the armed forces, organized religion, and Congress. We also use respondent patriotism ("How proud are you to be an American?") as an indicator of support for existing authority.

"Tough-mindedness." Following Eysenck's (1954) two-dimensional model of political attitudes, Sulloway (1996) argues that firstborns are not only more politically conservative but also more "tough-minded," i.e., less compassionate and more aggressive in their assessment of human affairs. As mentioned, Sulloway presents evidence that firstborns were disproportionately likely among French deputies to vote to execute Louis XVI, while laterborns voted to spare the King's life. Accordingly, we use a respondent's support for capital punishment to measure tough-mindedness. In addition, we measure tough-mindedness with an item asking whether the respondent believes that the justice system should be harsher in its sentencing of criminals, and an item asking whether the respondent feels that "it is sometimes necessary to discipline a child with a good, hard spanking."

Other independent variables

As noted above, our research seeks not only to examine the effect of birth order on social attitudes, but also to compare the influence of birth order with that of other variables that have received more sustained attention from sociologists. Consequently, after examining the bivariate relationships between birth order and our measures of social attitudes, we look at birth order in the context of multiple regression models that also include sex, age, race (coded as dummy variables for blacks and other nonwhites), parents' education, and sibship size. Because each of these variables has been posited to affect social attitudes directly, and because each may be correlated with birth order,[7] including these variables as controls also permits better estimates of actual birth order effects.

In addition, subsequent models employ more stringent controls. Because birth order theories typically place such emphasis on childhood environment, our next model adds controls for parents' occupational prestige (using recent recodes by Hauser and Warren [1997]), parents' marital status, the loss of a parent to death before age 16, childhood religion and the region of the country in which the respondent was raised. To account for the possibility that observed birth order effects may be caused by birth order differences in achievement, our final model adds controls for the respondent's education and occupational prestige.

RESULTS

Table 1 presents estimates of the effect of birth order on the different measures of social attitudes. All measures are coded so that positive coefficients are consistent with the hypothesis that firstborns are more conservative, supportive of authority, and "tough-minded" than laterborns. As described above, we provide results for four different models. Model 1 is simply the bivariate regression of the attitudinal measures on birth order. Model 2 holds constant the respondent's age, sex, race, sibship size, and parents' education. Model 3 adds controls for parents' occupational prestige and marital status, parental loss, the

[7] Ernst and Angst (1983) therefore advise that credible birth order research must, at a minimum, control for the respondent's sibship size and socioeconomic background. Hare and Price (1969) counsel that birth order studies should also control for age; at the same time, we remind the reader that with cross-sectional data we cannot distinguish age effects from cohort effects. Sieff (1990) suggests that controlling for sex is important because laterborn children may be disproportionately female; similarly, controlling for race is important because fertility varies across racial and ethnic groups.

respondent's religion, and the region of the country where the respondent was raised. Model 4 adds the respondent's current family income and education.

INSERT TABLE 5A-1 ABOUT HERE

If we look first at the results of the bivariate regression (Model 1), the data do not appear to support the hypothesis. The observed effect of birth order is indistinguishable from chance for 22 out of the 24 measures of social attitudes, and only one of the two significant effects is in the predicted direction: firstborns were more likely than laterborns to have supported Bush in 1992.[8] Meanwhile, contradicting Sulloway's findings about the "tough-mindedness" of laterborns, firstborn respondents are significantly more likely than laterborns to believe that the nation's courts are too harsh with criminals.

Introducing control variables, as in Models 2, 3, and 4, does not improve support for the hypothesis. In each of these models, significant effects are observed for only 3 of the 24 measures of social attitudes, and all significant effects are opposite the predicted direction. After controls are added the connection between birth order and support for Bush is no longer significant, while the relationship between birth order and the adequacy of criminal sentencing remains. In addition, laterborns are more likely than firstborns to support capital punishment, and laterborns also are significantly more patriotic.

Many of the non-significant estimates also run opposite the predicted direction. In the bivariate regressions, fewer than half of the estimates are in the predicted direction (9 of 24), while in Model 4 slightly more than half of the estimates are as predicted (14 of 24). Indeed, when asked to place themselves on a liberal-conservative continuum, firstborns identified themselves as more *liberal* than laterborns, although this result was not significant.

[8] In our data, firstborns were neither over- nor under-represented among Perot supporters.

Moreover, while it is plausible that systematic birth order effects could exist in some domains of social attitudes but not others, we observed inconsistent results within each of the six broad types of attitudes we defined.

Comparing bir th order to other independent variables

Above we noted that Sulloway's work poses a direct challenge to sociologists by asserting that birth order is a more important determinant of social attitudes than gender or social class. In Table 2, we compare the effect of being firstborn with controls used in Model 2: sibship size, age, sex, race, and parents' education, which we use here as a proxy for social class. While birth order is significantly associated with only 3 out of the 24 measures of social attitudes (all opposite the predicted direction), Table 2 shows each of the other independent variables is significantly associated with at least half (12) of the dependent variables. When we compare standardized coefficients (not shown), we find that parents' education is a more powerful predictor of attitudes than birth order for 22 of 24 variables (92%), race for 16 of 19 (84%); sex for 19 of 24 (79%); age for 18 of 24 (75%); and sibship size for 17 of 24 (71%). Consequently, our data strongly suggest birth order is not as important for understanding attitudinal differences as are these other variables.

INSERT TABLE 5A-2 ABOUT HERE

Several implications of these results are worth highlighting. First, by observing so many significant effects for other variables, we gain confidence that the null results we observe for birth order are not due to inadequate or unreliable measures of social attitudes. Second, the linchpin of Sulloway's claim that his Darwinian approach supplants conventional sociological analyses is his finding that birth order is 14 times more powerful a predictor of attitudes than social class, but our data indicate that parents' education actually influences attitudes much more strongly than birth order. Third, birth order appears even less valuable for predicting social attitudes than sibship size, which suggests that, at least relative to historical data, Sulloway overstates the influence of birth order not only compared to sociological mainstays like gender, race, and class, but also compared to other aspects of family configuration.

Finally, we should note that while the independent variables other than birth order significantly affect a broad range of social attitudes, only age does so in a seemingly consistent fashion (in terms of the direction of coefficients). In contrast, respondents with well-educated parents tend to be more liberal on most attitudinal measures than those with less-educated parents, yet they are also more likely to identify themselves as Republicans. Females and blacks tend to be more liberal than males and whites, but, among other things, females are more likely than males to oppose the legalization of marijuana and blacks are more likely than whites to believe that spanking children is sometimes necessary. Social scientists have known about such apparent inconsistencies for a long time, and considerable work has gone into explicating the nuances of attitude formation across various social divisions (Inglehart 1990, Brooks and Manza 1997). Yet these results may point to possible, deeper problems with birth order theories, which have tended to trade on broad characterizations of firstborns as "conservative" and "identifying with authority" (Sulloway 1996, but also Adler 1928 and Toman 1993). Put simply, such labels may not be fine-grained enough to map a thoroughly consistent relationship between a single independent variable and opinions on the complex social issues of contemporary society.

Comparisons within families

Like most other birth order studies, Tables 1 and 2 compare persons from different families, while birth order theories posit a process of differentiation that takes place within families. As described above, SAF data allow for the comparison of GSS respondents with a randomly selected sibling. These interviews included 9 of the 24 measures of social attitudes examined in Tables 1 and 2, including at least one measure from each of the six broad types we defined. Using the SAF/GSS data, we are able to test for birth order differences within families; however, we remind readers of the caveats we made earlier about these data.

Table 3 compares the mean responses of firstborn SAF/GSS respondents with the mean responses of their laterborn siblings. The means of most of the variables differ only slightly between firstborns and laterborns, and none of the observed differences is statistically significant.[9] Although the lack of significant results here may be partially attributed to the relatively low sample size, most of the observed differences are opposite the predicted direction. Only the differences in respondent attitudes toward free speech, the exclusion of blacks, and the importance of obedience in childrearing are consistent with the hypothesis (3 of 9, 33%). These results are consistent with Ernst and Angst's (1983) observation that inter- and intrafamilial data tend to yield similar findings on the effects of birth order and that, when results differ, studies using intrafamilial data tend to be less likely to observe significant birth order effects than comparisons of children from different families.

INSERT TABLE 5A-3 ABOUT HERE

[9] The mean differences between firstborns and laterborns were also not significant when Model 2 controls were added. To test whether a few outlying sibling pairs influenced our results, we compared the number of pairs in which firstborn sibling gave the more conservative response to the number of pairs in which the laterborn was more conservative, and this difference was not significant for any dependent variable.

Examining additional GSS items

Because the dependent variables discussed above represent only a portion of all the attitudinal items available in the 1994 GSS, it is possible that the results for the items we present are less favorable to hypothesized birth order effects than those using other items we could have chosen. We tested whether our selection was inadvertently biased by running regressions of all 202 GSS items that may be considered measures of conservatism, support for existing authority, and/or "tough-mindedness."[10] Table 4 summarizes the results of the bivariate regressions of these items on birth order, as well as the results of regressions using the controls employed in Models 2 and 4 above. The table shows how many results were in the predicted direction, how many were significant, and how many of the significant results were as predicted.

INSERT TABLE 5A-4 ABOUT HERE

Mirroring the results above, we find little support for the theory that firstborns are more conservative than laterborns. The number of significant effects is exactly what chance predicts for the bivariate regressions, and only slightly more than half for both models that add controls. Importantly, in none of the models are more than half of the significant effects in the predicted direction, suggesting that even if there were some small number of real birth order effects in the data that are not simply due to chance, theories such as Sulloway's offer no insight for predicting their direction. Because the measures of social attitudes are not independent of one another, we cannot use standard binomial confidence intervals to interpret deviations from 5% of items being significant or from a 50-50 split in the direction

[10] Even here, of course, researchers may differ about which items they would consider to be indicators of conservatism, support for authority, or "tough-mindedness." Over 97% of the 202 items examined were positively correlated with respondent self-identification as conservative (albeit not all significantly); those that did not were selected as indicators of supportive attitudes toward authority.

of coefficients. Even so, the number of variables in the predicted direction for Models 1 and 4 are both almost exactly 50%; that somewhat more than 50% of the items are in the correct direction for Model 2 is less convincing given the reversal of coefficients when controls are either added or dropped.

The possibility of strong birth order effects in these data is thrown into even greater doubt when we look at the second panel of Table 4, which compares the effect of birth order on all GSS attitudinal items with the other variables included as controls in Model 2. Although above we questioned the adequacy of broad labels like "conservative" or "liberal" for understanding the relationship between an independent variable and attitudes on complex social issues, here for purposes of comparison we ascribe a predicted direction to each variable based on previous research (e.g., Huber and Form 1973; Schuman, Steeh, and Bobo 1985; Hunt 1996). Looking first at the results for age and parents' education, we see that approximately three-quarters of all the estimates are in the direction we assigned, over half the estimates are significant, and over 40% of the estimates are significant as predicted (as compared to only 4% for birth order). Sex is significantly associated with 44% of the GSS items, and significant as predicted for 32%. Even for the control variable that exerts the weakest apparent influence on social attitudes—sibship size—we still observe twice as many significant effects as for birth order and three times as many effects that are significant in the predicted direction.

Interaction Effects

Up to this point, we have examined main effects of birth order and social attitudes, and find no evidence of the profound effects that Sulloway and others have predicted. Yet a key difference between Sulloway's and most earlier birth order theories is that Sulloway argues that birth order is also influential through its interaction with other independent variables.[11] Perhaps most plausibly, he (1996:133-136) proposes an interaction effect between birth order and age spacing between siblings. Drawing upon Hamilton's (1963) biological theory of inclusive fitness and kin selection, Sulloway claims that the most pronounced attitudinal differences occur when there is a moderate gap (2-5 years) between adjacent siblings.[12] Consequently, firstborns who have a sibling close to their age, or who only have much younger siblings, are expected to be less conservative than firstborns with a moderate gap to their next oldest sibling.

Table 5 presents tests for each of these possibilities. For firstborns with closely spaced younger siblings, the number of significant results is approximately consistent with what chance would predict (5.9%), and considerably less than half of the significant estimates are in the predicted direction (2 of 12). The results are similar for firstborns who have only distantly spaced younger siblings. Clearly, then, the data do not support the proposition that firstborns are more liberal when there is a small or large gap to their next oldest sibling.

INSERT TABLE 5A-5 ABOUT HERE

[11] The 1994 GSS does not contain information that would have allowed us to test two other interaction effects discussed by Sulloway, between birth order and shyness and between birth order and childhood conflict between respondents and their parents.

[12] Using Hamilton's theory, Sulloway argues that siblings who are more closely or distantly spaced are less costly (in an evolutionary sense) than those who are moderately spaced, and that as a result persons should feel a greater rivalry with moderately spaced siblings than with other siblings. Sulloway also makes claims about the effects of age spacing on laterborns (i.e., that laterborn children born after a long gap are much like firstborns and therefore are conservative); these were also not supported by tests using the 202 measures.

In addition, Sulloway presents evidence of interaction effects between birth order and social class, which we also test in Table 5.[13] His model of social attitudes reports a significant interaction suggest that upper-class firstborns are more conservative than lower-class firstborns (p. 507).[14] Meanwhile, in his model of the behavior of deputies during the French Revolution, he finds that upper-class firstborns were less tough-minded than lower-class firstborns (p. 323). Measuring social class both by parents' education and occupational prestige, we find no evidence of these patterns: the number of significant effects and the number of estimates in the predicted direction are fully in line with what chance would predict. Sulloway also argues that social class and birth order mediate the effect of parental loss through a three-way interaction effect (pp. 136-145). Lower-class firstborns, thrust into the role of surrogate parents, become even more doggedly conservative, while upper-class firstborns, who often do not have to bear this burden, become somewhat more liberal. This proposition is also not supported by our data: significant three-way interaction effects are in the predicted direction for 4 of 202 items when social class is measured by parents' education, and for 2 of 202 measures when class is measured by parents' occupational prestige.

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[13] In a footnote, Sulloway (1996:507) describes a model of social attitudes that includes an interaction between birth *rank* and sibship size. He provides no theoretical justification for why such an interaction should be included other than that the result is significant using stepwise methods in his sample. Because birth rank and sibship size are highly correlated (r = .64 in the GSS data), using the product of birth rank and sibship size is very much like using the square of sibship size, which makes more theoretical sense because it raises the possibility that the marginal effect of siblings on attitudes changes with each additional sibling (cf. Downey 1995). We find no evidence of an interaction of birth rank and sibsize in GSS data once the square of sibship size was controlled.

[14] The text of *Born to Rebel* is ambiguous about the direction of this interaction effect; we thank Frank Sulloway (personal communication) for clarifying this matter. Sulloway alternatively describes the interaction of birth order and social class on conservatism in terms of the birth order effects being much more pronounced for upper- and middle-class sample members than lower-class members. When we restrict the sample to only those respondents whose parent's occupational prestige is above the sample median, we find no additional evidence for the theory: excluding measures of tough-mindedness, estimates for 106 of 193 (54.9%) items are in the predicted direction, 17 (8.8%) are statistically significant ($p < .05$), and 7 (3.6%) are significant in the predicted direction.

Table 5A -1. Results of OLS and Logistic Regressions of Social Attitude Measures on Birth Order

	Model 1 Firstborn (bivariate[a])	*Model 2* Firstborn (adding basic controls[a])	*Model 3* Firstborn (adding other family background controls[a])	*Model 4* Firstborn (all controls[a])	Model 4 results in predicted direction?
Political identification					
Self-identification as conservative	-.057 (.069)	-.006 (.072)	-.021 (.071)	-.034 (.071)	No
Self-identification as Republican	.092 (.100)	.050 (.100)	.024 (.099)	.003 (.098)	Yes
Bush '92[d]	.220* (.104)	.193 (.111)	.155 (.113)	.141 (.114)	Yes
Opposition to liberal movements					
Opposition to abortion rights	.154 (.155)	.307 (.159)	.289 (.158)	.298 (.157)	Yes
Opposition to assisted suicide laws[d]	.039 (.139)	.229 (.150)	.194 (.154)	.206 (.154)	Yes
Opposition to legalization of marijuana[d]	-.167 (.142)	-.029 (.151)	-.078 (.155)	-.089 (.155)	No
Opposition to animal rights[c]	-.076 (.134)	-.005 (.141)	-.052 (.143)	-.032 (.144)	No
Opposition to environmental movement	.059 (.056)	.102 (.058)	.101 (.059)	.103 (.059)	Yes
Opposition to free speech	-.038 (.182)	.078 (.179)	.043 (.176)	.014 (.171)	Yes
Opposition to social welfare programs	.080 (.043)	.034 (.043)	.023 (.043)	.013 (.043)	Yes
Resistance to racial reforms					
Government too generous to blacks[b]	.083 (.059)	.093 (.061)	.076 (.060)	.065 (.060)	Yes
Exclusion of blacks[b]	-.003 (.062)	.048 (.060)	.024 (.060)	.014 (.058)	Yes
Individualistic beliefs about racial inequality[b]	-.142 (.076)	-.101 (.078)	-.127 (.076)	-.127 (.075)	No
Anti-immigration[b, c]	-.008 (.149)	-.026 (.155)	-.063 (.157)	-.090 (.158)	No
English-only laws[b]	-.079 (.074)	-.121 (.076)	-.129 (.077)	-.145 (.077)	No

Belief in traditional gender roles					
Against mothers working	.005 (.081)	-.020 (.079)	-.019 (.079)	-.021 (.079)	No
Traditional division of labor between spouses	.008 (.077)	.027 (.071)	.016 (.071)	.003 (.069)	Yes
Exclusion of women from politics[c]	-.019 (.152)	.153 (.162)	.150 (.165)	.133 (.166)	Yes
Support for existing authority					
Importance of obedience in childrearing[c]	-.095 (.110)	.051 (.115)	.001 (.117)	.010 (.117)	Yes
Confidence in social institutions	-.005 (.100)	.027 (.104)	.032 (.105)	.024 (.105)	Yes
Patriotism[c]	-.156 (.136)	-.292* (.145)	-.309* (.147)	-.332* (.148)	No
"Tough-mindedness"					
Tough on crime[d]	-.350* (.155)	-.401* (.164)	-.412* (.165)	-.424* (.166)	No
Capital punishment[d]	-.172 (.124)	-.269* (.134)	-.284* (.135)	-.305* (.136)	No
Corporal punishment[c]	-.001 (.112)	.114 (.117)	.092 (.119)	.086 (.119)	Yes

*p < .05, **p < .01, ***p < .001 (two-tailed). Standard errors in parentheses. Positive coefficents consistent with the hypothesis. N's range from 595 to 1894 (see Table 1).

[a]*Model 1* is the bivariate regression. *Model 2* controls for sibsize, age, sex, race, and parents' education. *Model 3* adds controls for parents' marital status and occupational prestige, parental loss, respondents' religion, and region where respondent was raised. *Model 4* adds controls for current income and education.

[b]White respondents only for racial equality items

[c]Ordered logistic regression of ordinal dependent variable

[d]Binary logistic regression of dichotomous dependent variable

Table 5A -2. Results of OLS and Logistic Regression of Social Attitude Measures on Birth Order and Other Variables

	Independent variables					
	Firstborn	Sibsize	Age	Female	Black	Parents' Education
Political identification						
Self-identification as conservative	-.006 (.072)	.039* (.018)	.007** (.002)	-.205** (.064)	-.526*** (.111)	-.027 (.028)
Self-identication as Republican	.050 (.100)	-.002 (.024)	-.005 (.003)	-.244** (.089)	-1.925*** (.153)	.102** (.039)
Bush '92[c]	.193 (.111)	-.009 (.028)	.001 (.003)	.107 (.101)	-1.940*** (.28)	.037 (.043)
Opposition to liberal movements						
Opposition to abortion	.307 (.159)	.099* (.040)	-.005 .005	-.099 (.144)	.165 (.248)	-.278*** (.063)
Opposition to assisted suicide laws[c]	.229 (.150)	.131*** (.035)	.010* (.004)	.450** (.139)	.700** (.212)	-.004 (.059)
Opposition to legalization of marijuana[c]	-.029 (.151)	.079* (.040)	.010* (.005)	.567*** (.135)	-.387 (.228)	-.086 (.058)
Opposition to animal rights[b]	-.005 (.141)	.094** (.035)	.006 (.004)	-.570*** (.127)	-.502* (.207)	-.141* (.056)
Opposition to environmental movement	.102 (.058)	.022 (.015)	.001 (.001)	-.073 (.052)	.140 (.088)	-.073** .023
Opposition to free speech	.078 (.179)	.036 (.045)	.025*** (.005)	.225 (.160)	.773** (.281)	-.546*** (.069)
Opposition to social welfare programs	.034 (.043)	-.018 (.010)	.005*** (.001)	-.195*** (.038)	-.289*** (.098)	.063*** (.017)
Resistance to racial reforms						
Government too generous to blacks[a]	.093 (.061)	-.002 (.016)	.002 (.002)	-.092 (.055)	N/A	-.080** (.024)
Exclusion of blacks[a]	.048 (.060)	.044** .015	.011*** (.002)	-.191*** (.054)	N/A	-.132*** (.023)
Individualistic beliefs about racial inequality[a]	-.101 (.078)	.0002 (.019)	.003 .002	-.190** .071	N/A	-.161*** .030
Anti-immigration[a, b]	-.026 (.155)	-.021 (.039)	.006 (.004)	-.215 (.137)	N/A	-.060 (.060)
English-only laws[a]	-.121 (.076)	-.043* (.019)	.010*** (.002)	-.195** (.068)	N/A	-.023 (.030)
Belief in traditional gender roles						
Against mothers working	-.020 (.079)	-.001 (.019)	.022*** (.002)	-.357*** (.072)	-.334* (.132)	.055 (.031)
Traditional division of labor	.027 (.071)	.006 (.018)	.022*** (.002)	-.203** (.065)	-.100 (.121)	-.079** (.028)
Women out of politics[b]	.153 (.162)	.114** (.038)	.020*** (.005)	-.037 (.147)	.035 (.248)	-.151* (.067)

Support for existing authority						
Obedience in	.051	.064*	.013***	-.080	.436*	-.221***
childrearing[b]	(.115)	(.029)	(.003)	(.103)	(.184)	.045
Confidence in social	.015	.026	-.001	-.071	.093	.044
institutions	(.095)	(.024)	(.003)	(.084)	(.149)	(.036)
Patriotism[b]	-.292*	-.113**	.021***	-.172	-.699**	-.114*
	(.145)	(.036)	(.004)	(.129)	(.214)	(.056)
"Tough-mindedness"						
Tough on crime[c]	-.401*	-.078	.004	.342*	-.549*	-.181**
	(.164)	(.041)	(.005)	(.150)	(.226)	(.062)
Capital punishment[c]	-.269*	-.083*	.003	-.291*	-1.291***	-.178***
	(.134)	(.032)	(.004)	(.122)	(.122)	(.050)
Corporal punishment[b]	.114	.068*	.003	-.443***	.708***	-.109*
	(.117)	(.029)	(.003)	(.107)	(.183)	(.047)

*p < .05, **p < .01, ***p < .001 (two-tailed). Standard errors in parentheses. Model includes an additional control for other nonwhites.
N's range from 595 to 1894 (see Table 1).
[a]Racial equality measures tested for white respondents only
[b]Ordered logistic regression of ordinal dependent variable
[c]Binary logistic regression of dichotomous dependent variable.

Table 5A -3. Intrafamilial comparisons of firstborns and laterborns on items common to the GSS and Study of American Families (SAF) questionnaires

	Average for firstborns	Average for laterborns	Is difference significant? (p < .05)	Is difference in predicted direction?	N
Self-identification as conservative	3.187	3.211	No	No	492
Self-identification as Republican	3.217	3.223	No	No	483
Opposition to free speech	2.000	1.810	No	Yes	274
Exclusion of blacks[a]	0.709	0.697	No	Yes	249
Individualistic beliefs about race[a]	1.596	1.780	No	No	382
Women out of politics	1.231	1.262	No	No	286
Obedience in childrearing	1.559	1.414	No	Yes	345
Confidence in social institutions	4.205	4.249	No	No	315
Corporal punishment	1.784	1.821	No	No	319

Significance tests use two-tailed t-test for paired observations. Variables coded such that the hypothesis predicts firstborn means to be higher than laterborn means.
[a]Averages for exclusion of blacks and individualistic beliefs about race are for white respondents only.

Table 5A -4. Summary of Results of OLS and Logistic Regressions of All GSS Social Attitude Measures Relevant to the Hypothesis on Birth Order and Other Independent Variables[a]

	Predicted Direction	Total # of items	# items in predicted direction	# items significant (p < .05)	# items significant in predicted direction
Tests of the main effect of birth order on social attitudes					
Firstborn, bivariate regression (Model 1)	Conservative	202	100 (49.5%)	10 (5.0%)	4 (2.0%)
Firstborn, controlling for sibship size, age, sex, parents' education, and race (Model 2)	Conservative	202	117 (57.9%)	16 (7.9%)	8 (4.0%)
Firstborn, full controls (Model 4)[b]	Conservative	202	104 (51.5%)	15 (7.4%)	5 (2.5%)
Comparing the effects of Model 2 variables					
Firstborn	Conservative	202	117 (57.9%)	16 (7.9%)	8 (4.0%)
Age	Conservative	202	154 (76.2%)	120 (59.4%)	105 (52.0%)
Parents' education	Liberal	202	150 (74.3%)	106 (52.5%)	89 (44.1%)
Female	Liberal	202	139 (68.8%)	89 (44.1%)	64 (31.7%)
Black	Liberal	148	84 (56.8%)	54 (36.5%)	36 (24.3%)
Sibship size	Conservative	202	115 (56.9%)	38 (18.9%)	30 (14.9%)

[a]Includes all attitudinal items considered to test respondents' conservatism, support for existing authority, and/or "tough-mindedness."
[b]Full model controls for age, sibsize, sex, race, parents' education, parents' occupational prestige, parental loss, childhood religion, region where respondent was raised, respondents' income, and respondents' education

Table 5A -5. Results of Tests of Interaction Effects Between Birth Order and Selected Other Variables[a, b]

	Predicted Direction	Total # of items	# items in predicted direction	# items significant (p < .05)	# items significant in predicted direction
Tests of the interaction of birth order and age spacing					
Firstborn x Closely spaced younger sibling	Less Conservative	202	86 (42.6%)	12 (5.9%)	2 (1.0%)
Firstborn x Distantly spaced younger sibling	Less Conservative	202	77 (38.1%)	4 (2.0%)	2 (1.0%)
Tests of the interaction of birth order and other variables					
Firstborn x Parents' education	More Conservative[c]	202	106 (52.5%)	5 (2.5%)	3 (1.5%)
Firstborn x Parents' occupational prestige	More Conservative[c]	202	102 (50.5%)	10 (5.0%)	7 (3.5%)
Firstborn x Parents' occupational prestige x Parental loss	Less Conservative	202	116 (57.4%)	3 (1.5%)	2 (1.0%)

[a]Each of the potential interactions in the table was tested separately. Models also included age, sex, race, sibsize, parents' education, parents' occupational prestige and parental loss as controls.
[b]Includes all attitudinal items considered to test respondents' conservatism, support for existing authority, and/or "tough-mindedness."
[c] While all other models in the paper derive from predictions that birth order affects conservatism and tough-mindedness in the same direction, the prediction here is that upper-class firstborns are more conservative, but less tough-minded, than lower-class firstborns.

Extra Table 5A -A. Results of OLS and Logistic Regressions of Social Attitudes on Birth Order (Sample Including Only Children and Respondents with Step- or Half-Siblings)

	Model 1 Firstborn (bivariate[a])	*Model 2* Firstborn (adding basic controls[a])	*Model 3* Firstborn (adding other family background controls[a])	*Model 4* Firstborn (all controls[a])
Self-identification as conservative	-.070 (.059)	-.021 (.063)	-.020 (.063)	-.028 (.063)
Self-identification as Republican	.087 (.085)	.037 (.088)	.028 (.088)	.015 (.087)
Bush '92[d]	.153 (.090)	.106 (.100)	.096 (.102)	.084 (.102)
Opposition to abortion rights	.086 (.131)	.245 (.141)	.265 (.139)	.263 (.139)
Opposition to assisted suicide laws[d]	.016 (.120)	.173 (.133)	.164 (.136)	.175 (.136)
Opposition to legalization of marijuana[d]	-.065 (.122)	.001 (.135)	-.019 (.138)	-.038 (.139)
Opposition to animal rights[c]	-.054 (.114)	.034 (.127)	.036 (.127)	.068 (.128)
Opposition to environmental movement	.035 (.048)	.107* (.052)	.102 (.052)	.101 (.052)
Opposition to free speech	-.049 (.157)	.026 (.161)	.028 (.158)	-.010 (.154)
Opposition to social welfare programs	.073 (.038)	.027 (.039)	.026 (.039)	.018 (.039)
Government too generous to blacks[b]	.090 (.051)	.121* (.055)	.105* (.055)	.094 .054
Exclusion of blacks[b]	.028 (.053)	.079 (.054)	.059 (.053)	.056 (.052)
Individualistic beliefs about racial inequality[b]	-.121 (.065)	-.082 (.069)	-.105 (.067)	-.112 (.066)
Anti-immigration[b, c]	.069 (.126)	.019 (.138)	.018 (.140)	-.006 (.141)
English-only laws[b]	-.032 (.064)	-.105 (.068)	-.115 (.068)	-.128 (.069)
Against mothers working	.021 (.070)	-.040 (.071)	-.028 (.071)	-.031 (.071)
Traditional division of labor between spouses	.020 (.065)	.019 (.062)	.012 (.063)	-.008 (.061)
Exclusion of women from politics[c]	.059 (.129)	.187 (.142)	.188 (.144)	.170 (.145)
Importance of obedience in childrearing[c]	-.057 (.094)	.072 (.103)	.038 (.104)	.053 (.105)
Confidence in social institutions	-.053 (.086)	-.018 (.094)	-.005 (.095)	-.010 (.095)
Patriotism[c]	-.024 (.115)	-.255 (.129)	-.259* (.130)	-.280* (.131)

	Model 1 Firstborn (bivariate[a])	*Model 2* Firstborn (adding basic controls[a])	*Model 3* Firstborn (adding other family background controls[a])	*Model 4* Firstborn (all controls[a])
Tough on crime[d]	-.262	-.351*	-.352*	-.371*
	(.134)	(.147)	(.148)	(.148)
Capital punishment[d]	-.164	-.248*	-.246*	-.266*
	(.106)	.119	(.120)	(.121)
Corporal punishment[c]	-.037	.071	.085	.069
	(.095)	(.104)	(.105)	(.105)

*p < .05, **p < .01, ***p < .001 (two-tailed). Standard errors in parentheses. Positive coefficents consistent with the hypothesis. N's range from 762 to 2432.
[a]*Model 1* is the bivariate regression. *Model 2* controls for sibsize, age, sex, race, and parents' education. *Model 3* adds controls for parents' marital status and occupational prestige, parental loss, respondents' religion, and region where respondent was raised. *Model 4* adds controls for current income and education.
[b]White respondents only for racial equality items
[c]Ordered logistic regression of ordinal dependent variable
[d]Binary logistic regression of dichotomous dependent variable

Extra Table 5A -B. Means, Standard Deviations, and Descriptions of Variables in Study.

Variable	Description	Metric	Constituent GSS Items	Mean	SD	N[a]
KEY INDEPENDENT VARIABLES						
Firstborn	Respondents' birth order	0=laterborn to 1=firstborn	SBYRBRN1-9	0.31	0.46	1945
Sibsize	Respondents' number of siblings	Siblings	SBYRBRN1-9	3.00	1.95	1945
Age	Respondents' age	Years	AGE	45.24	16.42	1945
Female	Respondents' sex	0=male to 1=female	SEX	0.57	0.50	1945
Black	Respondents' race	0=non-black to 1=black	RACE	0.09	0.29	1945
Parents' education	Educational achievement of respondents' most educated parent	0=no high school diploma to 4=graduate degree	PADEG, MADEG	1.22	1.22	1860
Parents' occupational prestige	Occupational prestige of parent with most prestigious occupation	7=lowest to 81=highest	GMOMTSEI[c], GDADTSEI[c]	37.26	14.89	1843
Parental loss	Death of one of respondents' parents before the respondent was 16 years old	0=no to 1=yes	FAMDIF16	0.08	0.27	1945
DEPENDENT VARIABLES						
Political identification						
Self-identification as conservative	Placement on liberal-conservative scale	0=extremely liberal to 6=extremely conservative	POLVIEWS	3.19	1.39	1887
Self-identification as Republican	Party identification	0=strong Democrat to 6=strong Republican	PARTYID	2.90	2.01	1894
Bush '92	Voted for Bush (or would have voted for Bush) in 1992 Presidential election	0=vote for Clinton or Perot to 1= vote for Bush	IF92WHO, PRES92	0.37	0.48	1819
Opposition to liberal movements/causes						
Opposition to abortion rights[b]	Whether woman should be able to get a legal abortion if (1) her life is in danger, (2) baby may have serious defect, (3) she wants no more children, (4) family can afford no more children, (5) pregnancy the result of rape, (6) she does not wish to marry father, or (7) she wants abortion for any reason	0=should be legal in all these circumstances to 7=should be illegal in all these circumstances	ABANY, ABDEFECT, ABHLTH, ABNOMORE, ABPOOR, ABRAPE, ABSINGLE	2.29	2.44	1153
Opposition to assisted suicide laws	Belief that doctors should <u>not</u> be legally allowed to help patients with incurable diseases who want to end their lives	0=disagrees to 1=agrees	LETDIE1	0.26	0.44	1214
Opposition to legalization of marijuana	Belief that marijuana use should remain illegal	0=should be legal to 1=should stay illegal	GRASS	0.76	0.43	1245

Opposition to animal rights	Belief that animals do not have same moral rights as humans	0=strongly disagrees to 4=strongly agrees	ANRIGHTS	2.39	1.20	839
Opposition to environmentalism[b]	Whether respondent would be willing to (1) pay much higher prices (2) pay much higher taxes, and (3) accept cuts in standard of living to protect environment	0=very willing to do all of these to 3=not at all willing to do any of these	GRNPRICE, GRNSOL, GRNTAXES	1.54	0.75	843
Opposition to free speech[b]	Belief that persons who are admitted (1) atheists, (2) communists, or (3) homosexuals should not be able to (1) make a speech in respondent's community, (2) teach in college or university, and (3) have their books shelved in public library	0=all of these should be permitted to 9=none of these should be permitted	COLATH, COLCOM, COLHOMO, LIBATH, LIBCOM, LIBHOMO, SPKATH, SPKCOM, SPKHOMO	2.56	2.83	1131
Opposition to social welfare programs[b]	Belief that government has no responsibility to (1) improve the living standards of the poor or (2) help poor people pay their medical bills and that (3) the government tries to do too much to solve the country's problems	0=disagrees strongly with all items to 3=agrees strongly with all items	HELPNOT, HELPPOOR, HELPSICK	1.46	0.71	1301
Opposition to racial/ethnic equality						
Government too generous to blacks[b, c]	Belief that the government gives too much attention to blacks and should spend less money trying to improve the conditions of blacks	0=disagrees strongly to 3 = agrees strongly	BLKGOVT, NATRACEX, NATRACEY	1.73	0.76	773
Exclusion of blacks[b, c]	Belief that blacks should not push themselves where they are not wanted and that whites have a right to keep blacks out of their neighborhood if desired	0=disagrees strongly to 3=agrees strongly	RACPUSH, RACSEG	0.94	0.82	813
Individualistic beliefs about racial inequality[b, c]	Belief that blacks tend to have worse jobs and incomes than whites because most blacks (1) have less innate ability and (2) lack motivation, but not because blacks (3) have less educational opportunities and (4) are discriminated against.	0=disagrees with all items to 4=agrees with all items	RACDIF1, RACDIF2, RACDIF3, RACDIF4	1.82	1.20	1102
Anti-immigration[b, c]	Belief that illegal immigrants should (1) not be able to get work permits (2) not be able to attend public universities, and (3) not have their children qualify as American citizens if born in the US, and (4) belief that legal immigrants should not be able to receive welfare benefits upon	0=believes immigrants are entitled to all these to 4=believes immigrants are entitled to none of these	IMMFARE, UNDOCCOL, UNDOCKID, UNDOCWRK	2.75	1.19	703

	entering the country					
English-only laws[b, c]	Belief that English should be exclusive language used (1) in schools, (2) on election ballots, and (3) to conduct government business	0=disagrees with all items to 3=agrees with all items	ENGBALLT, ENGOFFCL, ENGTEACH	1.69	0.94	746
Support for traditional gender roles						
Against mothers working[b]	Belief that mothers should stay home (1) when they have child under school age and (2) after youngest child has started school, and that working mothers (3) have weaker relationships with children, (4) cause children to suffer and (5) cause family life to suffer.	0=disagrees with all items to 5=agrees with all items	FAMSUFFR, KIDSUFFR, MAWRKWRM, WRKBABY, WRKSCH	2.40	1.17	814
Traditional division of labor between spouses[b]	Belief that (1) women should look after home and family; (2) it is more important for wife to help husband's career than have one herself; (3) it is bad if husband stays home and wife works; and (4) it is better if husband is achiever outside home and wife takes care of family	0=disagrees with all items to 4=agrees with all items	FEFAM, FEHELP, HUBBYWK1, MRMOM	1.43	0.88	595
Exclusion of women from politics[b]	Belief that (1) men are emotionally better suited for politics than women, (2) that women should leave running US up to men, and (3) that respondent would not vote for female Presidential candidate from own party.	0=disagrees with all items to 3=agrees with all items	FEHOME, FEPOL, FEPRES	1.23	0.56	1150
Support for existing authority						
Importance of obedience in childrearing	Evaluation of how important it is to teach a child to obey versus to be well-liked, to think for her/himself, to work hard, or to help others	0=less important than all these to 4=more important than all these	OBEY	1.78	1.31	1290
Confidence in social institutions[b]	Confidence in persons running (1) Congress (2) the executive branch of	0=hardly any confidence in all institutions to 9=great deal of confidence in all institutions	CONARMY, CONBUS, CONEDUC, CONFED, CONFINAN,	4.75	1.59	1193

	the government (3) the US Supreme Court (4) the military (5) major companies (6) banks (7) education (8) organized religion and (9) medicine.		CONJUDGE, CONLEGIS, CONMEDIC			
Patriotism	How proud respondent is to be an American	0=not very proud to 3=extremely proud	AMPROUD	2.31	0.74	923
"Tough-mindedness"						
Tough on crime	Belief that courts are not harsh enough with criminals	0=believes courts are too harsh or about right to 1=believes courts are not harsh enough	COURTS	0.89	0.31	1852
Capital punishment	Attitude toward death penalty for those convicted of murder	0=opposes the death penalty to 1=favors the death penalty	CAPPUN	0.80	0.40	1824
Corporal punishment	Belief that spanking is sometimes necessary as punishment for children	0=disagrees strongly to 3=agrees strongly	SPANKING	1.90	0.88	1268

Additional items used for analyses in Tables 4 and 5: ABCHOOSE, ADMIRBLK[c], AFFRMACT[c], AMRANK, ANRIGHTS, ASKCRIME, ASKDRINK, ASKDRUGS, ASKFINAN, ASKFORGN, ASKMENTL, ASKSEXOR, ASNGOVT, BILINGED[c], BUSDECID, CHEMFREE, CHINA[c], CHLDCARE, COHABFST, COHABOK, COLAFFX, COMMUN, CONCLERG, CONGETH, DISCAFF/ DISCAFFY[c,d], DIVBEST, DIVIFKD1, DIVLAWX, DIVNOKD1, DRIVLESS, EPRES, EQINCOME, EQWLTH, ETHHIST[c], ETHORGS[c], FECHLD, FEJOBIND, FEPRESCH, FEWORK, GRNECON, GRNPROG, GUNLAW, HELPBLK[c], HITOK, HOMEKID, HOMOCHNG, HOMOSEX, HOMOSEX1, HOUSEWRK, HSPGOVT[c], IHLPGRN, IMMECON[c], IMMPUSH[c], IMMUNEMP[c], IMMUNITE[c], IMMWRKUP, JAPAN[c], JOBAFF[c], LETIN, MAPAID, MELTPOT[c], NATAID/ NATAIDY[d], NATARMS/ NATARMSY[d], NATCITY/ NATCITYY[d], NATCRIME/ NATCRIMY[d], NATDRUG/ NATDRUGY[d], NATEDUC/ NATEDUCY[d], NATENVIR/ NATENVIY[d], NATFARE/ NATFAREY[d], NATHEAL/ NATHEALY[d], NATMASS/ NATMASSY[d], NATPARK/ NATPARKY[d], NATROAD/ NATROADY[d], NATSPAC/NATSPACY[d], NOMEAT, OBEYTHNK, OBRESPCT, OWNTHING, PILLOK, POLABUSE, POLATTAK, POLESCAP, POLHITOK, POLMURDR, PORNLAW, PORNMORL, PORNRAPE, POSTMAT1, PRIVENT, PUBDECID, RACCHNG, RACFEW/ RACHALF/ RACMOST[c,d], RACHOME[c], RACMAR[c], RACOPEN[c], RACPRES[c], RDISCAFF[c], RECYCLE, RIMMDISC[c], SCHLETH[c], SECDOCS, SECPRVCY, SECTECH, SEXEDUC, SINGLPAR, SUICIDE1, SUICIDE2, SUICIDE3, SUICIDE4, SYMPTBLK[c], TAX, TEACHETH, TEENSEX, TEENSEX1, THNKSELF, TWOINCS, USINTL, USUN, WHOTEACH, WHTGOVT[c], WIRTAP, WORKBLKS[c], WORKHSPS, WORKIMM[c], WORKUNDC[c], WRKGROWN, WRKNOKID, WRKWAYUP, XMARSEX, XMARSEX1, XMOVIEX

[a]Almost all variation in number of available cases among different dependent variables is due to the split ballot format of the GSS and the administration of many questionnaire items to a randomly selected subset (1/3 to 2/3) of the total sample.
[b]Items were factor analyzed to confirm unidimensionality. Alphas (standardized) are: abortion =.89; environmentalism = .85; free speech = .72; government obligations = .75; spending on blacks = .64; exclusion of blacks = .69; individualistic beliefs about racial inequality = .55; immigration = .60; English-only measures = .58; working mothers = .80; traditional gender roles = .83; women in politics = .64; confidence in institutions = .73.
[c]Effects examined for white respondents only.
[d]Items with slightly different wordings on different ballots combined into single variable.
[e]Uses recodes by Hauser and Warren (1997).

6

DARWINIAN NUMEROLOGY

"In a world where science journalists are trying to function as scientists and scientists are functioning as book promoters, what is the average reader to think?"
—biologist David Sloan Wilson, review in *Skeptic*, 2000

A few years ago, and much to my surprise, I discovered that I was the Antichrist. This discovery was based on an initial realization, subject to subsequent research and testing, that the number 666 had occurred far more frequently in my life than could be explained away as "coincidence." I am the 6th of 6 children, my mother is the 6th of 12 children, and my mother's father was the 6th of 6 children. I was born on March 15th, 1971. (3+15=6+6+6; 1+9+7+1=6+6+6). My first and last names have six letters; my middle name, which I never use and won't repeat here, only has three, *but* my parents actually gave me this name with the intention of calling me by an irksome variant of it, which does have six letters. (6, 6, 6, just like Ronald Wilson Reagan). The house where I spent my childhood was by the intersection of 6th Street and 12th Avenue (6+12=6+6+6). My favorite old sweatshirt—the one I wore about half the winter days of my graduate school career—is emblazoned with the number 37 (37 is the largest prime factor of 666). If you take the first and middle names of all of my siblings and myself and transform them using technique of *gematria* (A=1, B=2,..., Z=26), their sum is 666. If you take the names of the members of my dissertation committee and do the same thing, their sum is—damn!—665.[1]

The examples are all genuine (although, as far as I know, I am not actually the Antichrist). While I don't recall why or how I came to start looking for transformations of 666 in my biography, once I started actively fishing for examples, they began popping up

[1] A Stata program for doing gematria transformations is available from the author (disclaimer: for entertainment purposes only).

everywhere. Repeated co-occurrences of a number can "wow" an audience and be used to suggest, if not some specific underlying cause, then at least that something mysterious is afoot. During his speech at the Million Man March, Louis Farrakhan cited various recurrences of the numbers 19 and 555 to connect the heights of various D.C. monuments, symbols of Egyptian history, and dates in the history of the slave trade (Brackman 1996). Wilhelm Fleiss, the eccentric correspondent of Freud, would use any additive, subtractive, or multiplicative combination of 23 and 28 that his ingenuity could muster to confirm his theories about the cyclical character of human psychophysiology (Sulloway 1979: 135-147). Sulloway directs attention to Martin Gardner's (1960) scrumptious point about Fleiss's work that whenever a and b share no common divisor, then $\pm Xa \pm Yb$ can be used to derive any number, which means that Fleiss would have been able (and was) to find "confirmations" of his ideas wherever he looked.

Evolution-minded inquiry will be accused of nothing so extreme or illogical here. What we consider in this chapter are three instances in which, to my mind at least, seemingly amazing properties have been attributed to individual numbers in claims about universal manifestations of human psychology. As such, and especially in secondary accounts, these numbers can take on magic-like qualities that give a mysterious character to the relationship between our evolved psychology and observed behavior. I start by examining Helen Fisher's contention that four years represents an evolved duration for human marriage. I question the evolutionary premises of her theory, but I also conduct a re-examination of the data on which her claims are based. I argue that, when properly considered, the cross-national data on divorce that she presents actually provide evidence against her theory, rather than supporting it.

The Fisher inquiry has led me to examine two other evolutionary claims that are based on a focal number whose recurrence on contemporary life has been suggested to manifest evolutionary pressures. One is Robin Dunbar's claim that 150 people represent a evolved, natural size constraint on human groups. The other is work, most closely associated with Devendra Singh, suggesting that .70 represents an evolved, natural preference for the ratio of the size of a woman's waist to the size of her hips. Below, we will see invocations of evidence for these claims that may remind the reader of the argument-by-repeated-occurrence logic that characterizes my earlier examples of numerology. In both these cases I do not do any original data analysis of my own, but I review the existing evidence and find that that it is not nearly as strong as some presentations of the theories have suggested. I also give reasons why one might question the validity of the evolutionary reasoning behind the claim to begin with. In other words, I suggest that all three cases might fail on both theoretical and empirical grounds.

At various points in the preceding chapters, it has been suggested that the popular attention given to evolutionary claims may hinder the development of less flashy, but more thoroughly grounded, efforts to integrate Darwinian thinking into the social sciences. Evolutionary approaches have long been dogged with the criticism that they provide too much "cocktail-party science": freewheeling speculation that is fun for people to pass along in casual conversation but that is either hopelessly untestable or cannot stand up to logical and empirical scrutiny. What I criticize here are "cocktail-party numbers": easy-to-digest, easy-to-remember, and easy-to-repeat numerical assertions about universal features of human nature that are easily cleaved of any consideration of the strength of the actual evidence for believing that this report about human nature is true.

Such cocktail-party numbers are not confined to statements about human nature, of course. Consequently, I should make plain that the lessons to be drawn from these examples are less criticisms of Darwinian approaches generally than they are criticisms of a particular kind of assertion about behavior made by some evolution-minded scholars and authors. Although they focus on claims regarding numbers, the three examples may serve as initial steps toward a critique of what might be called the vulgar universal: an attempted pansocietal generalization that is much more specific than is warranted by the available evidence. I believe that vulgar universals of the sort described in this chapter promote specific misconceptions about the relationship between evolved biology and behavior, one of the most important of which is the misconception that cognitive adaptations are much more precise in their operation than perhaps they are. This critique of overspecificity echoes Chapter 3's concern that evolutionary psychology has overestimated the innate specialization of the mind. In addition to this, vulgar universals may also evince an underestimation of the variation that exists across societies.

Consequently, although such universals may help an author to "wow" an audience in the short run, they are ultimately detrimental to evolution-minded thinking and hinder the useful incorporation of adaptationist reasoning by other social scientists. Because I am critical of the three examples below, I want to be strong in my advance warnings against throwing out the proverbial baby with the proverbial bathwater. Rooting out *bad, overly simplistic* thinking about evolution and behavior is important because it helps to supplant this with *better, more careful* thinking, not to eliminate consideration of evolutionary ideas entirely. Claims about universals that cannot be supported by the evidence only make the gulf between evolutionary social science and the rest of social science seem wider than it needs to be, and it makes efforts to forge fruitful connections between the two more difficult.

SCRATCHING THE FOUR-YEAR ITCH

> *"Marriage must incessantly contend with a monster that devours everything: familiarity."*
>
> —Honoré de Balzac, *The Physiology of Marriage*, 1829

When evolutionary anthropologist Helen Fisher conducted a study of divorce rates in sixty-two societies, she found a cross-societal tendency for those couples who do divorce to do so around the fourth year of marriage. She theorizes that this four-year peak in divorce rates is a manifestation of an evolved disposition for mates to part ways at this time—as she puts it, a "planned obsolescence" of the human pair bond (Fisher 1992: 114). In her view, human pair-bonding (which she equates with matrimony) evolved because it increased in various ways the reproductive success of those who practiced it, but she contends that none of these ways would necessarily spur an evolved tendency toward permanent bonds. Instead, she posits that, from an evolutionary standpoint, it is often advantageous for an individual to seek a new mate once a child from an existing union has passed through infancy, and she suggests particular neurochemical mechanisms that may facilitate the transition from one mate to another. In her view, the ideal lifelong marriage is a holdover of a special case adjustment to the development of agriculture, and the tension between "`til death do us part" and her proposed "four-year itch" reflects the influence of evolved dispositions that were selected for in our pre-agricultural past.

These contentions about a "four-year itch" are part of a broader, evolution-minded theory of serial monogamy that Fisher has developed in both social scientific (Fisher 1989, 1991, 1995, 1998) and more popular forums (Fisher 1987, 1992, 1996). Judging from her offering over the past few years, Fisher has moved toward being more of a "pop" anthropologist than an academic one. Even so, however, her claim about the four-year itch still circulates, and it has not been disputed in print. A range of scholars have recounted the finding of four-year peak in divorces (e.g., Small 1995; Zeifman and Hazan 1997; Lopreato and Crippen 1999; Burnham and Phelan 2000), and Miele (1996) suggests that it is a "law" of human divorce. When behavior geneticist David Lykken (1998) decided to give a talk entitled "How Can Educated People Continue To Be Radical Environmentalists," his first evidentiary salvo against the forces of educated ignorance invoked Fisher's theory and research:

> Romantic love, which anthropologists once thought had been invented by French poets in the middle ages, is now known to have characterized virtually every traditional society of which we have records... [W]hen our ancestors began producing those big-headed, altricial [=helpless] babies that needed several years of constant carrying and oversight... some sort of attachment had to be invented to persuade the fathers to help out. It turns out that, over all known societies that permit divorce, the modal length of marriage for those couples who eventually split is just four years; the fast-setting superglue of romantic infatuation lasts just long enough for Junior to be sturdy on his feet.

Indeed, perhaps precisely because of her use of popular forums, Fisher's claim about the four-year itch has a broader visibility. For example, a *Time* story on "polyamorists"—persons who are openly, simultaneously, sexually and romantically involved with more than one person at a time—discusses the finding: "[Fisher's study] revealed that people have a remarkable tendency to split up after just four years. The implication that polyamorists take from Fisher's work is that we aren't built for monogamy" (Cloud 1999: 91). The sidebar of a *Time* article by Barbara Ehrenreich (1999) puts Fisher at the vanguard of what is dubbed a "femaleist" movement in biology and social science, which Ehrenreich sees as resonating with the progressive ideals of feminism while adopting a more biologically-conscious approach to the study of gender and family.

After recounting Fisher's theory, I offer three different lines of criticism. First, I question whether the theory is actually consistent with the logic of natural selection; I argue that Fisher may have overemphasized the evolutionary benefits of serial monogamy and may not have adequately reflected upon the evolutionary costs. Second, I show that the evidence on ancestral birth spacing that Fisher presents in support of her theory actually, when properly interpreted, provides evidence against it. Third, I re-examine the UN data on which Fisher bases her empirical claims, and I find that the chief pattern she reports disappears when coding errors and questionable analytic decisions are corrected.

Fisher's Theory

> *"Among these hundreds of millions of men and women from sixty-two different cultures, individuals speak different languages, ply different trades, wear different clothes... and harbor different hopes and different dreams. Nevertheless, their divorces regularly cluster around a four-year peak"*
>
> --Helen Fisher, *Anatomy of Love*, 1992, p. 111

Long-term mating in humans is itself a bit of an evolutionary puzzle: Fisher (1992) notes that although 90 percent of birds form pair bonds (at least through a breeding season), only 3 percent of mammals do. Among humans, long-term pair-bonding is ubiquitous: not only do all societies have some institutionalized analogue to marriage, but also evidence suggests that the vast majority of individuals in every culture partake of this institution (van den Berghe 1979).

Fisher's theory tackles the questions of why human beings form long-term pair bonds with one another and why these pair bonds are often impermanent. She (1992: 73) takes monogamous pairing to be humans' "basic reproductive strategy." Fisher argues that many of the emotions we associate with love and romantic attachment are evolved adaptations to facilitate the development of monogamous pair-bonds. In granting this primacy to monogamy, she recognizes that the majority of the world's cultures allow and exhibit polygyny (and a few exhibit polyandry); however, she maintains that even in those societies that permit polygamy, the majority of individuals are never married to more than one spouse at a time (van den Berghe 1979; Frayser 1985). This contrasts with gorillas and other ape species in which females always join the harems of a dominant male (Fisher 1992). Fisher's emphasis on monogamy is also not meant to discount the high frequency of adultery: she uses "monogamy" to refer to long-term and interactionally intense associations like that of marriage, and she is careful to point out that her usage of monogamy by no means implies fidelity. Instead, like most evolutionary social scientists, Fisher argues that humans have developed a strong proclivity to avail themselves of extramarital mating opportunities in certain conditions.[2]

Fisher cites a series of factors during human evolution that she claims favored the development of a tendency toward monogamy over the harem-like arrangements of other primates. According to her theory, as hominids moved onto the savanna, they began to walk upright, which required a female to carry an infant in her arms rather than have the infant cling to her abdomen or ride on her back (as other primates do). Carrying an infant in her

[2] Theories of evolved dispositions toward adultery propose that adultery makes Darwinian sense for men because they can potentially increase their quantity of offspring and for women when they can potentially increase their quality of offspring. Men are predicted to be much more indiscriminate than women in the pursuit of extramarital affairs; it is thought to be in the Darwinian interest of women to have affairs chiefly when men have markers suggestive of "better genes" than their current husband (Fisher 1992; Buss 1994).

arms inhibited a female's ability to gather food, as well as to protect herself and the infant. Concurrently, the expanding brain size of hominids caused infants to be born increasingly early in developmental terms (so that their heads would still fit through the birth canal) and thus increasingly helpless. Fisher posits that both of these circumstances led females to have increased need for paternal support during the first few years of a child's life, and, consequently, females evolved dispositions to increasingly value indicators of willingness to provide paternal investment when selecting mates. Meanwhile, argues Fisher, the open space and scavenging life of the savanna made it increasingly difficult for males to collect the resources necessary to attract a harem or to protect a harem from danger. These pressures are theorized to have combined to spur the adaptation of a tendency toward pair-bonding, marked by males increasingly devoting their resources to one female partner and to their offspring with that partner. That is to say, the effort that males once expended in striving to gain a harem was now directed toward parental investment.

After providing this account of the evolutionary roots of pair-bonding, Fisher points out that none of the selection pressures thought to have spurred the evolution of pair-bonding would imply any evolutionary advantage to these bonds being permanent. Instead, Fisher (1992: 154) argues that "human pair-bonds originally evolved to last only long enough to raise a single dependent child through infancy." Once this time has passed (and if a second child is not born in the interim), she claims that there are often evolutionary advantages to both parents seeking new partners. Individuals may be able to "trade up" by deserting their present partner for one who is superior in an evolutionary sense. This is thought to occur both when a man leaves his wife for a younger woman (who has higher reproductive potential) and when a woman leaves her husband for a man with more resources. Having children by multiple spouses also increases the genetic variation of one's offspring, which Fisher argues would enhance the likelihood of one's genetic lineage surviving through the fluctuating environments of our evolutionary past. Additionally, because marriage has often been used as a way of forging alliances between families, Fisher conjectures that serial monogamy also may have increased the number of social ties between clans and thereby provided more extensive social resources to offspring. Fisher (1987: 33) proposes that humans "who [changed] mates bore genetically more varied children that grew up in environments of extended cultural resources. The young survived disproportionately, passing to modern humankind our nagging restlessness during long relationships [and] our penchant for divorce."

If natural selection favors pair-bonding but not permanent pair-bonding in humans, Fisher proposes that evolutionary forces can explain not only why couples fall in love and marry but also why they later often fall out of love and divorce. She claims that the four-year peak she finds in divorce rates across societies corresponds to what would be expected if pair-bonds had evolved to last just long enough to see a child through infancy. Consequently, she theorizes that the four-year peak is a remnant of an "ancestral breeding strategy" (Fisher 1995: 34), and she likens human pair-bonding to that of other species, such as foxes, who join with a mate only for a single breeding season and then separate afterward.

To bolster her case for the evolutionary roots of this cross-societal pattern in divorce rates, Fisher cites anthropological evidence suggesting that births during our ancestral past tended to be spaced about four years apart. She also notes that in hunter/gatherer societies, children begin spending long periods of time with peers rather than with their mothers and fathers at about age 4, which may suggest that children of this age may be coming to rely more on community members for care and less on their biological parents. On the neurophysiological level, Fisher follows Liebowitz's (1983) division of romantic love into lust and attachment, with distinct sets of neurotransmitters (e.g., serotonin, oxytocin, vasopressin) involved in each. Fisher (1995: 29) proposes that eventually, in long-term romantic relationships, "the brain's receptor sites for the endorphins and/or oxytocin, vasopressin, and/or other neurochemicals may become desensitized or overloaded, and attachment wanes, setting up the body and brain for separation."

It is important to note that in advancing these claims about the "four-year itch," Fisher does not deny the influence of cultural or institutional factors on aggregate divorce patterns, nor does she question existing sociological research on individual risk factors for divorce. She acknowledges that divorce rates can vary markedly across societies, as well as among different subgroups (social classes, racial/ethnic categories) within a society (Sweet and Bumpass 1987; Cherlin 1992). Fisher also recognizes that the average length of marriage can vary because of differences in divorce laws, the stigma surrounding divorce, and the prevalence of arranged marriages. She argues that the persistence of the overall four-year peak in divorce rates in the face of all these factors that induce variation suggests even more strongly the underlying pull of our evolved biology (Fisher 1989: 333). Fisher maintains that her theory is also consistent with the increased likelihood of divorce for couples in their 20's (prime reproductive and parenting years) and couples with no more than one child (Sweet and Bumpass 1987; Heaton 1990). Regarding the latter, she claims that the evolutionary advantages of parting ways after a couple's first child has finished infancy may be greatly diminished if a second child is born in the interim.

Fisher maintains that the *ideal* of lifelong monogamy arose out of the economic subordination of women to men that she traces to the invention of the plow in early agrarian society; the plow is also thought to have made marriages less egalitarian and more patriarchal (see also Lerner 1986). Fisher claims that the tendency for divorce rates to rise when women's economic opportunities increases (e.g., Ruggles 1997; Greenstein 1990) suggests that women are more likely to exercise their innate disposition for serial monogamy when they are not economically dependent on their husbands. This is part of a broader position, advocated by Fisher and other evolutionary social scientists, that maintains that the increasing prosperity of contemporary postindustrial societies is causing them to change in ways that reproduce aspects of ancestral social organization (Crawford 1998). Fisher (1999) predicts that various global trends are leading not only to increasing egalitarianism in marriage but also to a series of social changes that will ultimately raise the social standing of women because they increase the valuation of various biological advantages that women possess relative to men.

Does Fisher's Theory Make Evolutionary Sense?

A first issue in evaluating the theory is whether the adaptation that Fisher posits is truly consistent with the logic of natural selection. This is to ask whether a disposition to divorce (especially after the fourth year of marriage) would have yielded more benefits than costs for the intergenerational preservation of ancestral humans' genes. Posing this question need not implicate a general stance one way or the other about the ultimate merits of sociobiology; instead, it suggests only that one first step in evaluating a hypothesis putatively derived from a larger theoretical perspective might be to check whether the hypothesis makes sense when the orienting perspective is taken on its own terms. In other words, whatever one thinks of contemporary sociobiology, it should be apparent that, in order for a sociobiological explanation to have any chance of being correct, it has to be consistent with the core principle of all Darwinian approaches—that complex adaptations arise because they increase the evolutionary fitness of those who bear them. Additionally, if sociobiology is to offer the cumulative and nomothetic framework that its advocates propose (Lopreato and Crippen 1999, Buss 1995), then we might also expect Fisher's explanation to be reasonably consonant with sociobiological theories of related phenomena that have been offered by others.

If divorce was primarily a matter of leaving a current partner for a new partner of higher reproductive value—"trading up"—then evolutionary social scientists would expect serial monogamy to be favored by selection.[3] Males' evolutionary value as mates often increases as they get older and accrue more resources and status; consequently, evolutionary reasoning would predict that middle-aged men may systematically be able to lure a new mate with higher reproductive value than the spouse they married when they were younger. Sociobiologists frequently point to instances of a newly wealthy male deserting his current spouse for a younger "trophy wife" as evidence of the influence of evolved biological drives (e.g., Wright 1994). Yet, of course, not all divorces can be explained as instances of males "trading up" for younger partners; in our own society, women initiate the majority of divorces (granted, this could be the result of husbands behaving in ways that indicate increasing indifference to the marriage) (Buckle, Gallup, and Rodd 1996). For women, evolutionary reasoning does not provide any reason why women might regularly be able to

[3] See Frank (1988) for a alternative sociobiological theory about how the emotions surrounding romantic love have developed as a Darwinian solution to the rational commitment problems posed by the possibility of "trading up" partners.

acquire a spouse with higher mate value than their first spouse as they get older. Instead, a woman's reproductive potential (and thus mate value) declines as she ages, and while one can certainly point to instances of women "trading up" partners, natural selection works on systematic and recurrent tendencies. Consequently, and contrary to what Fisher argues, the possibility of switching to a better partner is unlikely to have served as a pressure spurring the evolution in women of a general disposition to detach from long-term pair bonds.

The problem here is made worse when we recall that Fisher theorizes that the optimal time for couples to part is just after their child completes her or his period of intensive parental investment, which Fisher takes to be approximately four years of age. From an evolutionary perspective, the mate value of a "divorced" female should be discounted even further if a male partner would need to bear responsibility for a new "stepchild." Moreover, the presence of a stepfather may harm the evolutionary fitness of the child: as we have already discussed, some of the most-respected work in evolutionary psychology has centered on the observation that stepchildren are many times more likely to be abused than biological children. Fisher (1989: 350) acknowledges this potential challenge to her theory, but argues the following:

> [I]n modern Western cultures—where children are normally the responsibility of only two adults and the costs of education and entertainment are high—the genetic disadvantages of stepparenting may be considerably higher than in paleopopulations where children grew up in play groups under the auspices of several adults and the costs of education and entertainment were low.

That is to say, stepparenthood may not have bore the same evolutionary costs for early humans as it does now, and so the incidence of child abuse may have been much lower then than it is now.

One may protest that this is doubtful speculation of Fisher's part. For one thing, to discuss parental investment in terms of "the costs of education and entertainment" is imposing our comfortable contemporary perspective onto the Pleistocene, where parental investment likely had much more to do with helping to provide basic necessities for survival. For another, we have plenty of reason to believe that stepparent abuse is a significant phenomenon beyond modern Western cultures. As Daly and Wilson (1999: 36) write:

> In the face-to-face societies of our ancestors, powerful central authority and social services beyond kin assistance were non-existence, and the situation for stepchildren was probably even worse than in peasant societies. According to one study of contemporary South American hunter-gatherers, the Ache of Paraguay, forty-three percent of children brought up by a mother and stepfather died before their fifteenth birthdays, compared to nineteen percent of those brought up by two genetic parents; apparently, deaths by assault and deaths due to deprivation of adequate care were both elevated... We hypothesize that it has been a general feature of such societies that stepchildren are variously disadvantaged—as they are among the Ache—and we know of no contrary evidence.

Note that the hypothesis in the last sentence directly contradicts the implications of Fisher's theory, suggesting that Fisher's and Daly and Wilson's work are not compatible with one another regarding the costs of stepparenthood among ancestral humans. Consequently, scholars may wish to give pause before presenting her theory and Daly and Wilson's work on stepfamilies together as exemplars of Darwinian perspective on family life (see Miele 1996; Lopreato and Crippen 1999).

As noted above, Fisher also proposes that serial monogamy could have been adaptive because of the greater genetic variety of offspring that would result from having children with multiple partners rather than just one. Such increased variation may well have evolutionary advantages in fluctuating environments. Yet, as I have noted elsewhere in this dissertation, a criticism of adaptationist explanations is that they often consider only the *benefits* of a proposed adaptation and give no thought to the possible fitness *costs*. In the case of an adaptation that promotes the dissolution of a pair-bond shortly after a child has passed through infancy, we have already seen that a possible cost is the potential negative effect of a stepfather on the child's well-being, as well as a presumed diminution of investment received from the child's biological father. Serial monogamy also presumably implies transaction costs in moving from mate to mate, both in terms of the effort involved in finding a new mate and (because of time lags between mates) the potential reduction in total brood size. Still another costs would be incurred if—as sociobiologists would predict for women—successive mates often imply a "trading down" in mate quality; in this regard, an evolutionist would have to compare the value of genetic diversity to the value of having all of one's offspring with the mate who had the "best genes." Taken together, these factors would seem to imply a fairly hefty set of costs to serial monogamy, especially for women. Fisher gives no compelling reason why genetic variation in one's lineage would be sufficient to outweigh these, and without such a reason, one can question whether her theory is truly consistent with the logic and expectations of a Darwinian approach.[4]

Is The Theory Consistent With Ancestral Data?

Fisher contends that her theory of serial monogamy not only makes evolutionary sense but also is consistent with available data on the mating patterns of both contemporary and ancestral humans. As I have already described, she claims that divorce rates in contemporary human societies tend to peak in the fourth year of marriage; I examine the

[4] As noted earlier, Fisher also suggests that serial monogamy may have been evolutionary beneficial because it would increase the extensiveness of social networks and the resources offered to children. For this to be credibly weighed as a "benefit", we would need more evidence that social ties remained well-preserved after couples separated and that the move away from the biological father's family into stepfamilies resulted in a net gain rather than loss for the child. No evidence for this is provided by Fisher.

empirical evidence for this claim in the next section. Fisher also maintains that her theory is supported by evidence that births among ancestral humans tend to be spaced about four years apart (Lancaster and Lancaster 1983). The correspondence between the four-year modal duration of marriage and the four-year interval between successive births is said to support the proposal that the former is a remnant of a ancestral breeding season that was about four years in duration.[5] She writes, "The modal duration of marriage that ends in divorce, four years, conforms to the traditional period between human successive births, four years. So I propose that the human tendency to pair up for a modal duration of four years reflects an ancestral reproductive strategy to pair up and remain together throughout the infancy of a single [helpless] child" (Fisher 1995: 34).

In other words, Fisher marshals support for her theory from evidence that the typical duration of marriages that end in divorce and the typical interval between successive births among ancestral humans are equal. But one can argue that, if properly considered, Fisher's theory does not imply this prediction.

To see why, we need to consider the period in a typical female's life-course from the start of the first marriage to the birth of their second child.[6] We can divide this period into seven components:

(a) from start of marriage to conception of first child

(b) from conception of child to birth of child

[5] Even if the four-year peak exists and is the manifestation of an evolved disposition, the claim that it is a remnant of a breeding season still seems very tenuous given that humans marry, conceive offspring, and divorce at all times of the year; put simply, human behavior (across all types of societies) is immensely more chronologically variable than that of species that engage in seasonal breeding.

[6] I say "typical" here rather than "average" because both sides of the equation are based on statements about modal occurrences. Because the distributions are both skewed to the right, we would expect that the mean duration of marriage and the mean birth-spacing interval are both greater than their respective modes, a point I return to later in this section.

(c) from birth of child to end of child's infancy (and, according to Fisher's theory, the end of the pair-bond if no second child has been conceived in the interim)

(d) from end of pair-bond to start of courtship with second partner

(e) from start of courtship to start of second marriage

(f) from start of marriage to conception of second child

(g) from conception of child to birth of second child

The length of time comprising (a) through (c) is the length of the individual's first marriage, while the length of time comprising (c) through (g) is the length of the interbirth interval for the female's first two children. In order for the equality of these intervals to count as evidence for Fisher's theory, the following equality must hold:

typical duration of marriage	=	typical interbirth interval
(a) + (b) + (c)	=	(c) + (d) + (e) + (f) + (g)

In this equation, we can immediately drop (c) since it is on both sides of the equation. The human gestation period is relatively constant over successive births, so (b) and (g) cancel each other out. Fisher does not provide any reason to expect that the period between marriage and conception differs between successive marriage, so (a) and (f) may also be presumed to be equal and cancel each other out. We are then left with:

$$0 = (d) + (e)$$

This result would seem to suggest that the only way in which evidence that the typical duration of marriage and the typical interbirth interval were both for years could be taken as supporting Fisher's theory would be if the second pair-bond began immediately upon the termination of the second. If (d) and (e) are positive in the typical case, then what Fisher's theory actually implies is that the typical interbirth interval should be *longer* than the typical length of the pair-bond. In actuality, if the typical duration of marriage and the typical interbirth interval are both four years, the interbirth interval is actually *shorter* than the typical length of marriage. This is because the Fisher's data on duration of marriage are measured in *completed* years: the "four-year peak" is a peak in which marriages end between 4.00 and 5.00 years of marriage, which we may assume implies roughly an average of 4.5 years.

What this means is that the four-year interval between births among ancestral humans should not be taken as supporting Fisher's theory that the alleged contemporary four-year peak in divorces is a "remnant of an ancestral breeding season," but instead it would seem to provide evidence *against* her theory. For birth spacing data to support her theory, we would expect either the birth spacing interval to be substantially longer than 4.5 years or the modal duration of marriage to be substantially shorter than 4 completed years.[7] Either way, the key point is that the modal spacing between ancestral births should be substantially longer than the modal duration of marriages-that-end-in-divorce. Fisher's own presentation of data suggests that this is not the case.

Indeed, this result is not inconsistent with breeding seasons of other species. Consider, for example, Fisher's own colorful description of the breeding seasons of foxes:

> "In February the vixen begins her mating dance. Typically several suitors dog her heels. At the peak of estrus one becomes her mate. They kiss and lick each other's faces, walk side by side, mark their territory, and build several dens as winter wanes. Then, after giving birth in the spring, the vixen nurses

[7] The source Fisher cites regarding interbirth interval (Lancaster and Lancaster 1983) reports an average interbirth interval among the !Kung of 4.12 years.

> her pups for almost three weeks while her 'husband' returns nightly to feed her a mouse, a fish, or some other delicacy. Through the vibrant summer days and nights, both parents guard the den, train the kits, and hunt for the voracious family. But as summer dies, father returns to the den less and less. In August, mother's maternal temper changes too; she drives [away] her kits and departs herself (Fisher 1992: 152)."

From this account, it seems apparent that the length of the pair-bond lasts only a few months (from February to sometime before August), while the length between the birth of successive broods is one year (from spring to spring). Again, then, we can see that if birth spacing data were to provide support for Fisher's theory, the modal spacing between births would have to be substantially longer than the modal duration of the pair-bond.

Matters are complicated by Fisher's elsewhere taking another 4-year interval from demographic data as putative support for her theory. As noted earlier, she reports that when children in hunter/gatherer societies are age 4 (that is, 4 completed years old), they tend to depend less on their biological parents and more on the community for their welfare. She proposes that this point in the child's life might correspond to the point when parental attachments would wane and parents would seek new partnerships. If this were the case, however, we would not expect this to be reflected by a four-year peak in divorces. Instead, the ancestral pair-bond would need to include the time between marriage and conception, the nine-month gestation period, and the 4-5 years between the child's birth and the end of intensive parental investment. In short, if ancestral parents really tended to part ways when their child was four years old, then, in order for contemporary divorce rates to reflect an ancestral breeding season, the divorce peak would have to be after five or six years of marriage instead of only four. Whereas the ancestral birth spacing data suggests that a four-year divorce peak occurs *too late* to support Fisher's birth spacing theory, the data on the end of infancy in hunter-gatherer societies suggests the four-year peak comes *too early*. Either way, *the possible recurrence of "four years" in the demographic record does not support Fisher's theory, it undermines it*.

Is The Theory Consistent With Contemporary Data?

In contemporary societies, there are obvious differences between the length of a marriage and the length of a pair-bond, if we think of the latter as an intense romantic attachment between a man and woman. Couples in most contemporary societies typically go through a period of very strong romantic attachment before getting married and even before deciding to get married. On the other end, relationships are often romantically over with long before a divorce is sought and legally granted. Across societies, there is also considerable variability in the typical length of courtship and premarital engagement, as well as in the speed with which disenchanted couples can obtain a divorce (e.g., Queen, Habenstein, and Quadagno 1985; Quale 1988; Pasternak, Ember, and Ember 1997: Miller and Browning 2000). All of this would seem to work against the realization of ancestral mating strategies in contemporary divorce statistics, even if these strategies really did exert an enduring influence on human behavior. Fisher (1989: 333) recognizes this and asserts that "the fact that distinctive patterns are found despite this level of error suggests that gross comparisons of the kind I am making can tolerate considerable bias and that actual human divorce patterns may be even stronger than the available data reflect." This section examines Fisher's empirical analysis to determine how distinctive the patterns she reports actually are.

Fisher's finding that divorces worldwide tend to peak during the fourth year of marriage is based on data from the United Nations Demographic Yearbook (hereafter, UNDY). The UNDY has published editions with supplemental statistics on marriage and divorce in 1958, 1968, 1976, 1982, and 1990. In each of these editions, the UNDY includes cross-tabulations of several variables related to divorce (i.e., the duration of marriages that end in divorce, the age of husband and wife at the time of divorce, and the number of children born in marriages ending in divorce). Tables are presented for the most recent available year for all countries that collect and submit this data. Fisher's sample consists of every nation/year for which divorce statistics are provided in the 5 Yearbooks. For example, Bulgaria has data in four of the Yearbooks, for the years 1967, 1974, 1980, and 1989; these are four observations in Fisher's sample. For each observation, Fisher determines the modal duration of marriages that ended in divorce, and then she constructs a histogram of these modes. This histogram is recreated in Figure 1.

FIGURE 1 ABOUT HERE

We can see in Figure 1 that, for this sample of countries and years, 4 years is the "grand mode"—that is, the mode of this distribution of modes. That the peak is at year 4 is the grounds for Fisher's contention that "mateships tend to disband during and around the fourth year of marriage" (1995: 33). Combined with the birth-spacing data discussed above, it is the evidentiary basis for her conclusion that "serial pairbonding evolved in hominid paleopopulations concurrent with selection for an increased female reproductive burden and… the modern cross-cultural modal duration-of-marriage-that-ends-in-divorce reflects an ancestral adaptive strategy to remain pairbonded at least long enough to raise an infant through the period of lactation" (1989: 350).

Looking at Figure 1, we can see that four years may be the most frequently observed mode in her sample, but the tendency for countries to have their distributions peak at this time is far from overwhelming. Only 40 of 188 (21%) observations have their mode in the fourth year, and less than half have their modes in years 3-5 (87 of 188, or 46%).[8] An examination of the divorce distributions of the individual countries and years in the sample also does not reveal any pronounced tendency for divorces to cluster at or around the fourth year. We can consider first the percentage of all divorces in a given country or year that occur in year 4. Of the data points in the sample, the percentage of divorces that occur in year 4 ranges from 0.07% (Italy 1980) to 14.71% (Brunei 1976), with a median of 7.03%. If we widen this interval to years 3-5, the percentage of all divorces that occur in this period ranges from 0.38% to 39.71% (Italy and Brunei again), with a median of 20.63%. These results mean that, for the median observations in Fisher's sample, only one of fourteen divorces occurs in the year that Fisher claims marks the end of the human analogue of a breeding season, and only one in five divorces occur in the three year interval around her predicted peak.

Another way of examining the strength of the tendency Fisher identifies is to look at the percentage of all marriages that end in divorces in year 4. Because the UNDY contains yearly information on the number of marriages for all participating countries, we can calculate this percentage for a particular country/year by dividing the number of divorces that occur after the fourth completed year of marriage by the total number of marriages performed in the same country 4 years earlier.[9] For the country/years in Fisher's sample, the percentage of marriages ending in divorce in year 4 ranges from 0.002% (Italy 1980) to

[8] "In year 4" in this paper will be used to mean the year after the fourth year of marriage (that is, the first year a couple is marriage would be year 0). This way of counting years of marriage is awkard, but is most consistent with Fisher's exposition.

[9] I actually divided the number of divorces in year 4 by the mean of the numbers of marriages performed 4 and 5 years earlier, as a marriage that ends in divorce in year 4 could have occurred either 4 or 5 calendar years previously. For example, if a couple married for four years divorces on July 1, 1990, their date of marriage could range anywhere from July 2, 1985 to July 1, 1986.

7.57% (Kuwait 1988), with a median of 1.21%. The percentage of marriages ending in years 3-5 ranges from 0.012% to 21.58% (Italy and Kuwait again), with a median of 3.73%. What this means is that the specific behavior—divorce after four years of marriage—that Fisher wants to attribute to our evolved, universal human nature is actually only exhibited by about one in eighty couples in the median country/year in her data. Whether we are looking at the distribution of modes in Figure 1 or the distributions of duration of marriage at time of divorce for individual countries and years, what we see is an immense amount of variation across societies. In no way does Fisher deny that cultural or environmental factors influence divorce patterns; the question is whether we can see any evidence for the "four-year itch" that she claims underlies this variation.

Another issue is Fisher's decision to focus on modes as she examines divorce patterns across societies. As a single number to summarize a distribution of data, the mode is not as commonly used as the median or mean, and it bears no necessary relationship to the center of the distribution.[10] Fisher's emphasis on modes would seem to demand more analytic justification. The only justification provided in any of her papers—"the mode is used because it illustrates the year of marriage in which the greatest number of divorces occurs" (Fisher 1989: 333)—seems more simply to define a mode than to justify why it is the appropriate summary statistic to use in her study.

Had Fisher chosen to use the median or mean rather than the mode, her results would have been dramatically different. Figure 2 juxtaposes Fisher's original histogram of modes with histograms of the medians and means of years married at the time of divorce for

[10] The modal year of divorce in the Netherlands Antilles dropped from 8 years in 1966 (184 divorces) to 1 year in 1971 (both data points in Fisher's analysis). Even in large countries, the modal year of divorce often fluctuates in ways more suggestive of chance than real trends; in France, the mode moves from 7 in 1954 to 3 in 1967 to 5 in 1971 to 4 in 1978.

the observations in her sample.[11] The figure shows that the median and mean years married at time of divorce tend to be substantially higher than its mode; this is precisely what we would expect given that the distribution of this variable is markedly skewed to the right for most of the country/years Fisher examines. Consequently, that the contemporary divorce data provides evidence for a "four-year itch" rather than an "eight-year itch" or "nine-year itch" requires that a convincing case be made for why using the mode reveals an underlying biological predisposition while using the median or mean does not, but no such case is provided by Fisher.

An entirely different issue is that using any of these summary statistics does not control for trends in the number of marriages in the individual countries. Almost certainly, the sort of inferences Fisher wishes to make require such controls, as well as numerous other methodological improvements. In this section of the paper, however, I show that Fisher's assertions do not hold even if we take her methodology on its own terms.

FIGURE 2 ABOUT HE RE

Yet, even if we accept that modes provide the most appropriate statistic for her analysis, closer inspection reveals problems with the claim that the distribution of modes peaks in the fourth year. A table in one of Fisher's articles (1989) presents the names of all of the country/years in an earlier version of her sample (not including data from the 1990 UNDY) and their modal years married at the time of divorce. Checking these numbers against the original UNDY data reveals eight (of 150) instances in which the mode was

[11] When the UNDY presents divorce stats by duration of marriage, it usually provides the number of divorces for a given country and year for each completed year of marriage from 0-9 years, and it presents combined totals of divorces after 10-14 years, 15-19 years, and 20+ years. Some countries report the information using different aggregations of years. For calculating the mean duration of marriage-ending-in-divorce for each country/year, multiple year totals were handled by substituting an intermediate point that was consistent with the most typical shape of the distribution of divorces by year.

incorrectly calculated.[12] Fisher also includes observations in her analysis in which the number of divorces is not presented for each completed year of marriage 0-10 but instead adjacent years are lumped together; comparing these cases with cases where data are provided by year-by-year suggests strongly that a better solution would be to exclude these cases from the analysis.[13] For two other data points, substantially different modes are indicated by different editions of the UNDY; these points are also probably best eliminated from further analysis.[14] Meanwhile, Fisher excludes two observations in which there is a tie for the most frequent years married at the time of the divorce (that is, the distribution is exactly bimodal); a more typical solution here would have been to divide the weight of each observation evenly across the bins corresponding to each mode. Correcting the errors and making these alternative analytic decisions yields the new histogram presented in Figure 3.[15] The peak of the histogram is no longer four years, but instead slightly more country/years have their mode in year 3 rather than year 4. This suggests that Fisher's conclusions may be undermined simply by a better handling of her own data.

FIGURE 3 ABOUT HERE

[12] The errors in the modes reported by Fisher (1989) are: Brunei 1976 (Fisher reports 4 years/the actual mode is 6); Costa Rica 1966 (3/3 and 8); France 1978 (4/6); Greenland 1981 (4/4 and 7); Netherlands 1975 (4/3); Netherlands 1981 (2/3); South Africa [blacks] 1967 (4/7); Sweden 1975 (9/8). Also, if we assume even very modest random fluctuations among years 10-14, the mode for Italy 1980 is in at least the tenth year, not 9 years as Fisher reports.

[13] Most UNDY data on divorce by duration of marriage for Byelorussian SSR, Ukrainian SSR, and the USSR (excluding these republics) combines total divorces in years 1-2, 3-4, and 5-9, making it impossible to determine the exact mode. Fisher's solution is to take the interval with the highest average number of divorces per year and distribute the histogram weight equally across these years. But if one applies this method of aggregating years to data on which full information on divorces by year is available, problems become apparent. In 1989, the USSR did provide divorce counts by individual year, and the modal duration of marriage-ending-in-divorce is 2 years, but had the USSR aggregated the 1989 data as it did in the past, Fisher would have recorded the mode as 3-4 years. Consequently, and given that Fisher excludes several other country/years when aggregation makes it impossible to determine the exact mode, the more reasonable solution would seem to be to exclude these cases.

[14] The data points in question are Brunei 1972 and Jersey/Channel Islands 1967.

[15] It may also have been desirable to weight each country equally when making the corrected histogram (rather than counting each reported year as a full observation), or weight observations by the country's population. Both were done in auxiliary analyses; I leave them out of the chapter because they are consistent with, but do little to elaborate, the presented results.

One could also question whether Fisher's sample is really adequate for making inferences about cross-national patterns, setting aside the question of whether any observed tendencies or uniformities are indicative of biological predispositions. Only 46 of the 199 (23%) countries or territories that provided population information for the 1990 UNDY reported detailed enough divorce statistics to be included in Fisher's sample.[16] Only 13 nations provide these statistics in all five editions of the UNDY that Fisher examines. Perhaps not surprisingly, the nations of Western Europe and the United States (hereafter referred to as "Western" nations) are more likely to have collected and submitted the appropriate data and to have provided this data in multiple years. Although, in the most recent edition of the UNDY that Fisher examines, Western nations contain only about 12% of the world's population, they comprise 38% of the observations in her sample. Meanwhile, only 4% (8 of 196) of the observations in Fisher's sample are from South American countries; only 7% (14 of 196) are from non-Soviet countries in Asia; and no African country is represented between the nations of the north coast and South Africa. A consequence of using such a non-representative sample is that the patterns that Fisher asserts as globally valid may only hold among Western societies.

Indeed, when we test this possibility, this is exactly what we find. Figure 4 presents separate histograms of modes for Western and non-Western countries. The figure suggests a clear difference: the peak years married at time of divorce tends to be earlier for non-Western nations than for Western nations (the difference in the means of these distributions is also significantly different [$p < .05$]). These results make it difficult to find warrant for the assertion of a unitary, evolved disposition exerting a prominent influence on the timing of divorce. While Fisher does acknowledge that cultural factors may play a role in the early

[16] Countries/territories with populations less than 25,000 were excluded from the total count.

divorce peaks of Muslim nations, divorce peaks were observed in the second year or earlier in non-Muslim countries as far-flung as Cuba, Japan, Israel, and Bulgaria. It therefore seems reasonable to wonder whether if Fisher had been able to obtain divorce statistics on every nation in the world, her data may well have pointed to a "two-year itch" instead. In sum, by treating a four-year peak as the manifestation of an evolved disposition and invoking culture to explain deviations from it, Fisher's analysis inadvertently privileges (or naturalizes) the statistically overrepresented Western nations and makes the non-Western nations appear as if they are more strongly influenced by religion or other cultural factors.

FIGURE 4 ABOUT HERE

This section has examined a evolutionary anthropological theory of divorce offered by Fisher which posits that humans have an evolved tendency toward serial monogamy marked in contemporary society by a peak in divorces after the fourth year of marriage. Taken together, the criticisms provided here would seem to undermine Fisher's theory, at least as she has presented it. This does not mean that a Darwinian explanation could not be devised that preserves much of her theory but strengthens it against the challenges presented here. For example, a theorist could draw a sharper line between pair-bonds and marriages and argue that marriage statistics reveal little about the real length of the pair bond. Different evidence might then be available suggesting a cross-cultural peak in pair-bond dissolution after the fourth or some other year. Alternatively, a theorist could argue that an underlying evolved disposition to divorce after a given period of time does exist but that it is so sufficiently conditioned by environmental or cultural factors that its operation cannot be detected from data as coarse as nationally aggregated divorce statistics. Of course, the *post hoc* character of any such revision should be noted in evaluating whether or not it is genuinely testable. At the very least, the criticisms I raise would seem to shift the evidentiary burden from skeptics to those seeking to preserve the theory.

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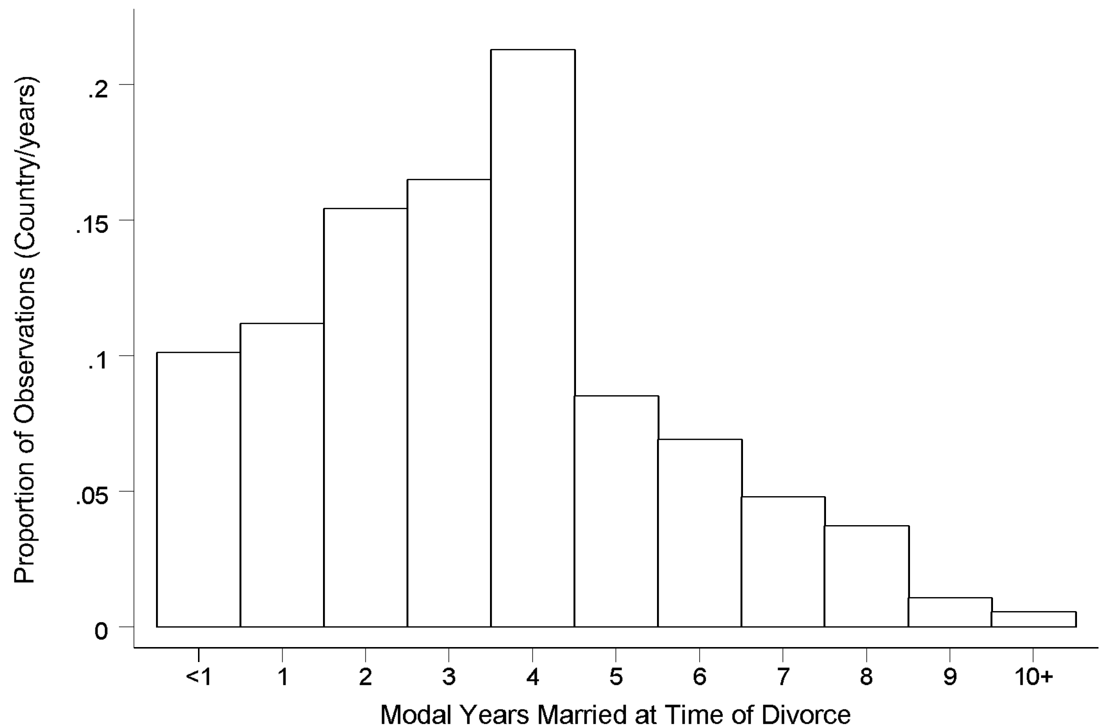

Figure 1. Replication of histogram presented in Fisher (1992) (N=188 [62 countries]).

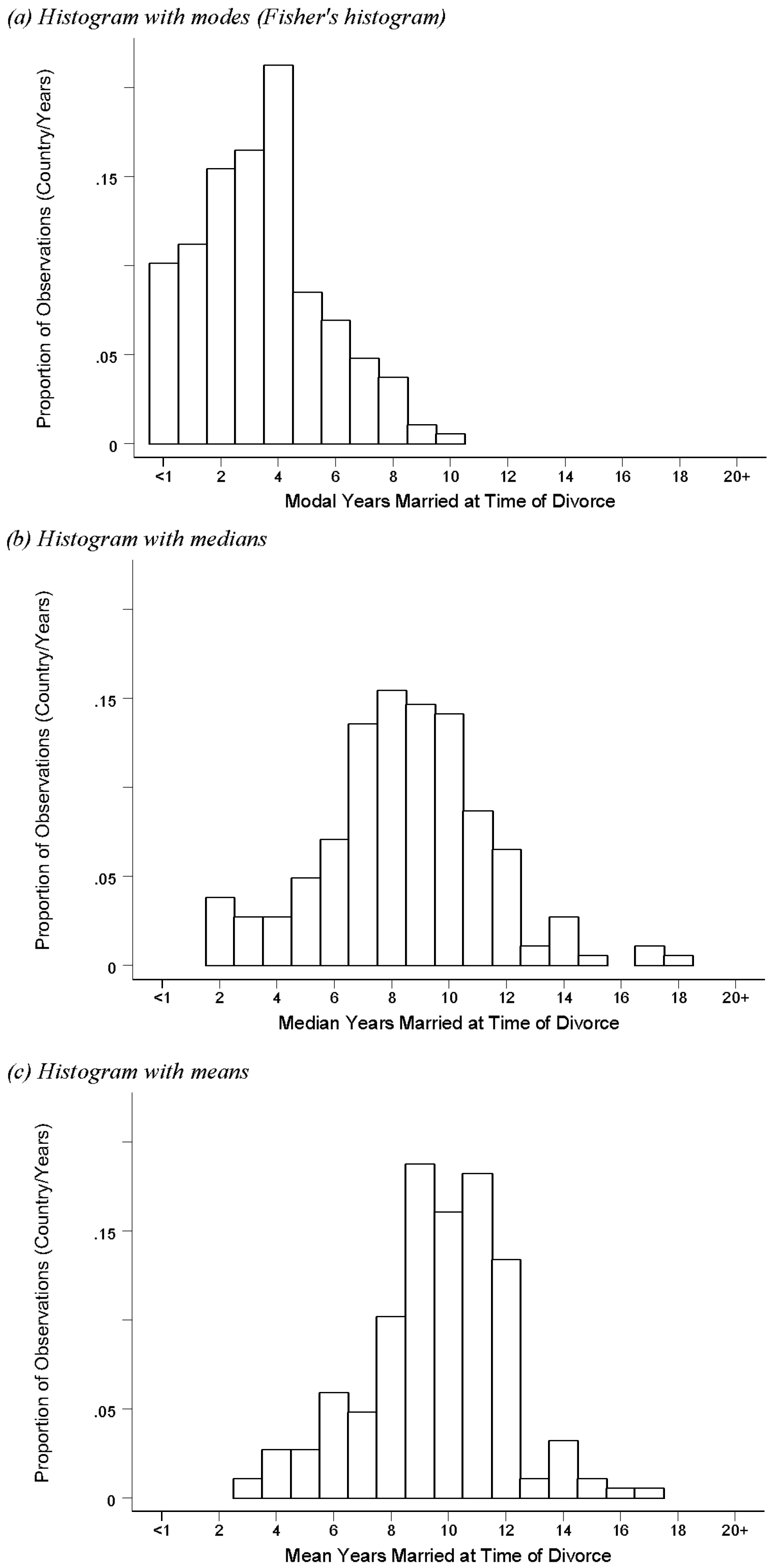

Figure 2. Comparison of histograms based on modal, mean, and median years married at the time of divorce.

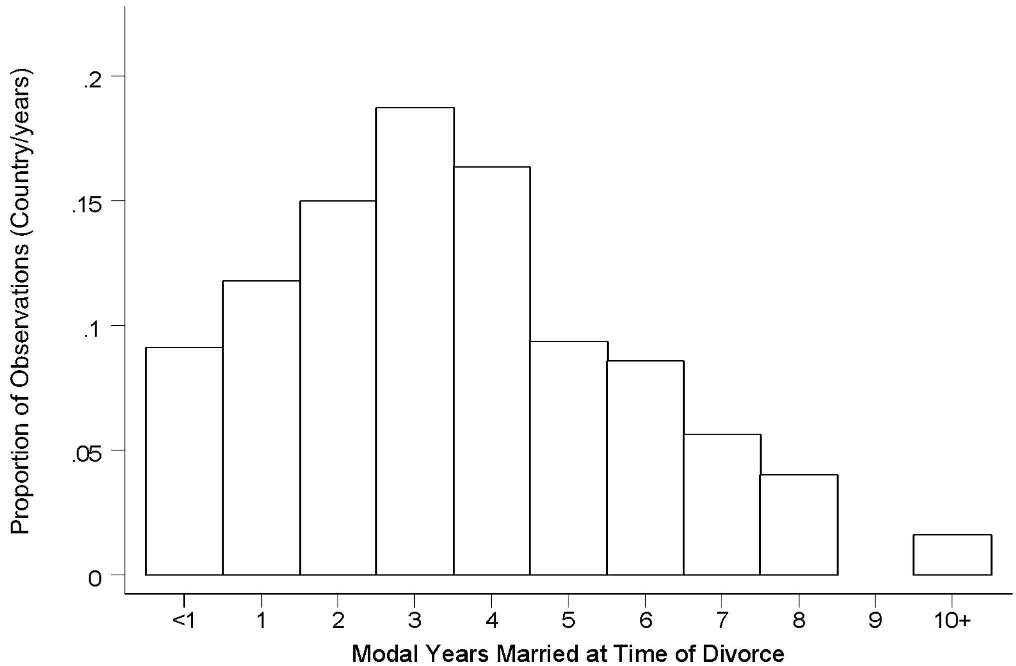

Figure 3. Histogram of modal years married at the time of divorce, with corrected data (N=187 [59 countries]).

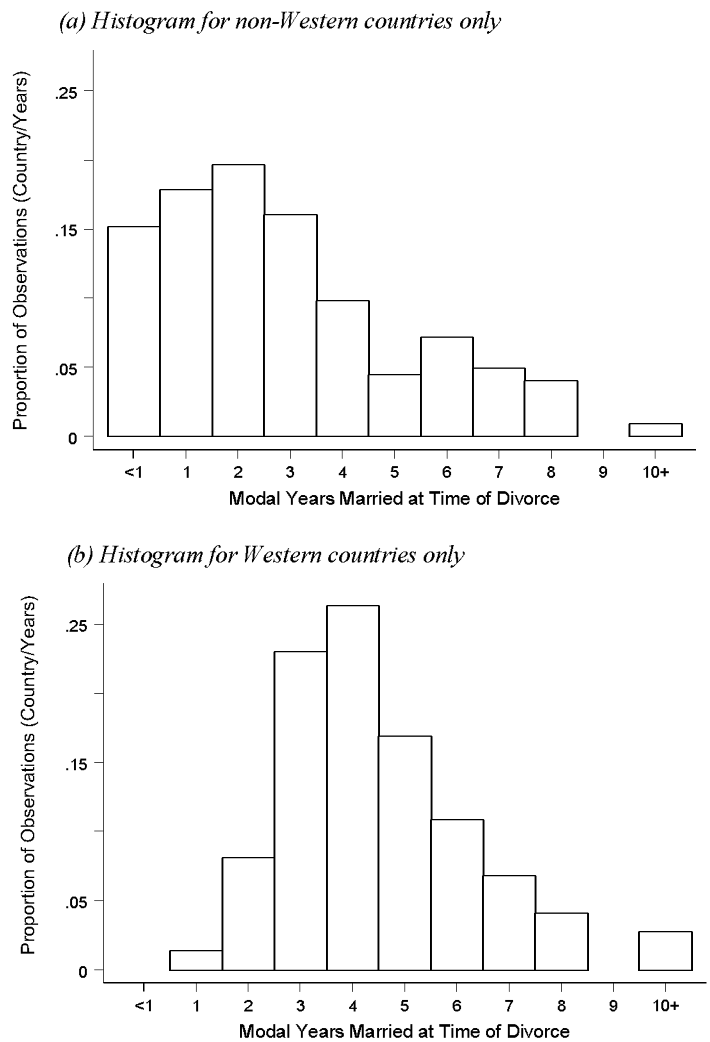

Figure 4. Comparing histograms of modes for non-Western and Western countries (N_{nw}=113 [40 countries]; N_{w}-74 [19]).

OVERRULING THE RULE OF 150

> *"The Rule of 150 suggests that the size of a group is another one of those subtle contextual factors that can make a big difference. In the case of the Hutterites, people who are willing to go along with the group, who can easily be infected with the community ethos below the level of 150, somehow, suddenly—with just the smallest change in the size of the community—become divided and alienated....*
>
> *If we want groups to serve as incubators for contagious messages... we have to keep groups below the 150 Tipping Point.... If we want to, say, develop schools in disadvantaged communities that can successfully counteract the poisonous atmosphere of their surrounding neighborhoods, this tells us that we're probably better off building lots of little schools than one or two big ones."*
>
> —Malcolm Gladwell, *The Tipping Point*, 2000 p. 182

The above appears in a chapter that is subtitled, "The Magic Number One Hundred and Fifty." As this passage suggests, Gladwell, a journalist well-known for his articles in the *New Yorker*, believes that there is real importance to whether groups are smaller or bigger than 150 persons, that adding just a few members around this threshold can make a big difference, and that this principle of group size has important social policy implications. Although he does not use a phrase as foreboding as the "Rule of 150," Matt Ridley also maintains that there are special properties of groups of this size. In his mind, it represents a constraint on our ability to maintain relationships based on face-to-face reciprocity, which he contends are the natural and ideal (his naturalistic fallacy, not mine) form of social organization. It is part of the evidence he marshals in a larger argument in favor of drastically reducing the power of the state and trying to recapture "a society built upon voluntary exchange of goods, information, fortune, and power between free individuals in small enough communities for trust to form" (Ridley 1996: 263).

Belief in the potential magic of the number 150 stems from research by Robin Dunbar, a British evolutionary psychologist, that is recounted in his Harvard University Press book, *Grooming, Gossip, and the Evolution of Language*. Briefly, the book argues that humans evolved the capacity for language use because of its function to maintain social ties and order within the group, the same function that he sees served by the extensive grooming of one another that many primates do (far more than hygiene requires). The move from grooming to gossip was necessitated by the increasing size of human groups. Part of his thesis turns on the idea that the reason that primates have comparably bigger brains than other animals is to keep track of the increased complexity of primate social life. If we take group size as a proxy for social complexity, then it stands to reason that among primates, we should then expect bigger brains to be correlated with bigger groups. The brain can be divided into three parts, of which the neocortex is the newest and the one most associated with why primates are "smart" (Dunbar 1996: 61-62). (Accordingly, the neocortex comprises a much larger proportion of overall brain volume in primates than other mammals, and humans have by far the largest relative neocortex size of any primate.) Dunbar found a strong relationship between the species' neocortex size and the size of the groups in which the species lived. More specifically, he found a strong linear relationship between the log of the ratio of the neocortex size to total brain size and the log of mean group size.

Dunbar's book is about the evolution of language in humans, and an important question for his theory as he sets it up is how well humans fit this pattern between neocortex size and group size. Dunbar could not actually include humans in his study, as our average group size is not just unknown but it is unclear what the appropriate human analogue for his measure of group size should be. But, as Ridley reports, "so tight is the correlation [between neocortex size and group size] that you can use it to predict the natural group size of a species whose group size is unknown" (Ridley 1996:69). Dunbar did just this: he plugged the human neocortex size into his estimated regression equation and calculated a predicted group size for humans of 147.8 persons, or about 150.

Then Dunbar began looking at human groups. He kept finding instances that to him suggested that 150 persons was somehow an optimal size for human groups and that groups larger than this were somehow too unwieldly to sustain as effectively. He provides these instances as evidence that this group size corresponds approximately the maximum level of social complexity that human minds have evolved to be able to handle. Below, I use the left column to describe some of the instances that Dunbar cites, and, in the right column, I provide commentary assessing the potential of this evidence.

1. Dunbar found that the average size of an intermediate-level hunter-gatherer grouping ("often referred to as clans") was "almost exactly 150" (p. 70). He noted that such groupings "appear to be the largest grouping in which everyone knows everyone else, in which they know not simply who is who but also how each one is related to the others" (p. 71)	*Clan size:* The mean here is actually based on only 9 societies (not 21 as Gladwell [2000: 179] reports) (Dunbar 1993: 684). Moreover, Dunbar could have used the midpoint of an available range for 6 other societies, which would have yielded a mean of 132.4, or he could have used the "clan" rather than the "subclan" for another group, which would have yielded a mean of 177.3. Moreover, Dunbar's readings of the primary sources used to arrive at these figures is challenged by Graber (1993), and Jarvenpa (1993) criticizes him for relying on old sources. Also, the unit of analysis of this intermediate grouping has no consistent definition, other than its being of "intermediate" size—sometimes it is a village and sometimes it is a clan or lineage group.
2. He found archeological evidence that the population of early villages of Near Eastern villagers was approximately 150, and he discovered that 150 is also the typical	*Specific groups:* One cannot discern how typical these examples are.

discovered that 150 is also the typical population of horticulturalist villages in the Philippines and Indonesia.

3. As mentioned by Gladwell above, Dunbar also found that the Hutterites "always split their communities when they reach a size of 150" (72). According to Dunbar, "The Hutterites say that once a community exceeds 150 people, it becomes increasingly difficult to control its members by peer pressure alone" (1996: 72)

Hutterites: The preciseness and historical mutability of 150 members as the criterion for "branching" seems overstated. The figures given by four sources: "about 150—although in recent years the split has been occurring even earlier, at about 130" (Bennett 1967: 51), "should not be larger than 150 and must not be larger than 200" (Deets 1975: 14-15), "120 to 150" (Flint 1975: 31), "130-150 persons" (Hostetler and Huntington 1980: 48). Peters (1965) lists the Hutterite colonies in North America and their populations; these ranged from 45 to 186. These same sources list social control (unwieldiness, factionalization) as only part of the reason groups split at this size; other factors are relative shortage of positions of management, excessive affluence, undesirable age distribution, and discontent due to increasing specialization of jobs.

4. Dunbar found evidence in the sociological literature that groups tended to become increasingly hierarchical when they became larger than 150-200.

Sociological principle: The most obvious cite in Dunbar's references for this contention is Coleman's *Introduction to Mathematical Sociology* (1964). I was not able to locate this assertion within Coleman's large book, which does not mean it is not there.

5. Dunbar was told that an "unwritten rule" of modern business organization was that businesses larger than 150-200 persons could no longer be organized on informal lines but

Business rule: Dunbar (1993: 687) presents the same assertion and cites (J.-M. Delwart, personal communication). It is difficult to discern how much weight should be attached to this.

instead required more formal management structures.

In his book, after presenting this initial evidence, he reports "*Once the significance of this finding had sunk in, I began to find other examples all over the place*" (p. 72, emphases added).

6. A friend informed him that when Brigham Young was preparing for the Great Trek that ended in Utah, he divided his flock of 5000 into smaller working groups of 150.

Mormon trek: Seemingly selective.

7. Research on the size of the social networks that people might be able to tap to ask a favor had found that the average network size was about 135 ("encouragingly close to our predicted 150").

Small world research: This was the observed mean size of the networks using the elicitation method. The researchers noted that the method had not tapped out the networks entirely, but that the network size could be predicted from the rate at which the production of new names through elicitation had diminished. The mean of this asymptotic network size was about 250 (Dunbar 1993: 687, citing Killworth et al. 1984).

8. The sizes of military companies in the Second World War ranged from Britain with 130 men to the United States with 223 men, "values which nestle comfortably around the predicted group size of 150, being within the permitted margin of error" (p. 75). Dunbar

Military companies: 7 out of the 10 company sizes for 20th century armies reported in Dunbar (1993: 686) have more than 185 members. Choosing the "company" as the relevant level of military aggregation also seems arbitrary.

writes, “[I]t is though military planners have discovered, by trial and error over the centuries, that it is hard to get more than this number of men sufficiently familiar with each other so that they can work together as a functional unit” (p. 75).

The right hand column suggests that there are a number of problems with the specific examples cited by Dunbar. They can be seen as problematic in several ways. First, some seem to overstate the strength of the correspondence to 150. Second, even taking this into account, several appear to be only partly accurate. Third, the examples seem sufficiently selective that we have no ground for judging the extent to which they provide evidence of a real pattern versus just coincidence. One might think that these were just casual examples chosen for the purposes of popular presentation, but Dunbar presents much the same evidence in an earlier scientific paper, and he conducts significance tests that compare these observed group sizes to the predicted group size. These various lines of evidence comprise Dunbar’s grounds for claiming that one can extrapolate from the regression line calculated for nonhuman primates to humans, and this extrapolation is a fundamental pillar of the larger theory that Dunbar tries to develop about language as a device for “social grooming” that was a prime mover in the evolution of human intelligence.

How far away from 150 would a group size have to be for Dunbar not to be able to cite it as evidence supporting his theory? In his earlier paper, Dunbar provides the abovementioned (and other) examples of groups that are presented as suggestive of human cognitive constraints on group size, and, in that paper, more complete ranges of the numbers involved are provided: “150-200,” “106.9,” “150... as the limiting size,” “197,” “up to 200 individuals, but rarely more,” “120-130,” “100,” “mean size of 179.6,” “maximum size of about 200,” “150 was a key threshold figure,” “50-500 individuals” “150 as the critical limit” “90-150,” “134 (although the variance around this figure was considerable)” “about 250.” These ranges would seem to provide a wide berth for finding instances of groups sizes that can be interpreted as supporting the theory. As I have noted, Dunbar sometimes uses significance testing to support his assertion that a group size is “close enough” to 150, e.g., his mention of early military companies being “initially 106 men, which is just at the bottom end of the statistical margin of error around the estimate of 150 given to use by the primate brain/group size equation” (1996: 75). The 95% confidence interval for the prediction he generates is 100.2-231.1.

If the “natural size” of human groups was a straightforward quantity, then if this size were within the confidence interval, this might be taken as evidence supporting the proposition that humans fit the pattern (or, more conservatively, it would not provide evidence against humans fitting the pattern). The data Dunbar presents are one step removed from this. In invoking them, Dunbar is (in my reading) trying to build a case for two assertions at once: first, that human groups have cognitive constraints on group size that fit the pattern extrapolated from other primates, and second, that the particular groups and group sizes he recounts are examples of the operation of these cognitive constraints.

Additionally, however, Dunbar may have calculated this confidence interval incorrectly. Janson (1993: 711, citing Sokal and Rohlf 1981) contends that the actual 95% confidence interval from Dunbar’s regression results is not 100.2-231.1, but 22.7-446.2. So if the size of early military companies had been 25 men or 450 men, they would have been nestled comfortably in the margin of error, but what would they really have shown about cognitive constraints on human group size?

In his response, Dunbar (1993: 725) does not contest Janson's correction, but instead writes:

> Janson is right to point out that my estimates of the 95% confidence limits around the confidence limits around the predicted value are technically incorrect... I should not... have used these as a basis for evaluating the statistical significance of the difference between observed and predicted values. I am, of course, very happy to use Janson's corrected estimates for this purpose: their wider range simply increases the statistical significance of the fit between observed and predicted values... Although it means that a wide range of possible values falls within the limits around the mean, this does not mean that the hypothesis is unfalsifiable. We are interested not in testing the theory per se, but in identifying possible equivalences in another taxon (i.e., humans).

The passage is notable for at least two reasons. First, in the sentence where Dunbar says he is "very happy" to use the corrected confidence intervals because it "simply increases the statistical significance", he seems to be arguing that by weakening the predictions of the model, Janson has actually strengthened the evidence for it.[17] Second, Dunbar here admits to a statistical error in his original confidence intervals, but then goes ahead and uses the original interval in making qualitative assessments of the fit of human group sizes to the predicted values in his book published three years later (e.g., "106 men, being just at the bottom end [of the margin of error]"). One wonders if his examples have been less

[17] In addition, Dunbar suggests that his recitation of groups with sizes of approximately 150 were not intended as support for this theoretical claims, but were only instead only an effort to identify "possible equivalences" in another taxon. Here, I can only encourage the reader to examine the relevant parts of the article or book and decide whether or not the invocation of the various 150's seems to be presented as evidence for the assertion that humans have a natural group size of about 150. Consider, e.g., this passage by Dunbar (1996: 76): "It is, of course, very easy to play the numerologist and find numbers to fit whatever size your theory requires. Nonetheless, the extent to which the values for these kinds of groups coalesce around 150 is impressive, particularly since all the examples I have discussed share similar bases in terms of social dynamics."

compelling to Gladwell and Ridley had the book reported that the real margin of error for predictions ranged from groups with 85% fewer members than predicted to groups with 200% more.[18]

In addition to questioning the confidence interval around predicted group size, we might also ask how much faith should we have in the prediction of 150 to begin with? The validity of predictions from regression depends on a variety of factors. Here are some that may reduce confidence in the prediction of 150:

1. *The measure of neocortex size.* Dunbar (1992) tries several different measures of neocortex size. Alternative measures would have produced alternative predictions. More seriously, critics claim that Dunbar's neocortex measure confounds the effects of neocortex size with that of brain size or body size, leading Dunbar to overestimate greatly the effect of neocortex size and likewise overestimate the group size predicted for humans (Deacon 1993; Falk and Dudek 1993; Holloway 1993).

2. *The measure of group size.* If group size is really supposed to be an indicator of cognitive constraints on the social complexity that an animal can handle, why use mean group size? Marc Hauser and collaborators (1993) argue that maximum group size would estimate the cognitive constraints on group size better. They also note that if one simply uses prosimian's *sleeping* group size in the regression rather than *foraging* group size, the predicted natural group for humans changes from 147.8 to 71.5 (or 58 if one eschews the Dunbar's log-log transformation of the variables).

3. *Extrapolating the regression line to humans.* Using Dunbar's measure, humans have a relative neocortex size about 30% larger than any other primate species (Dunbar 1992). This means that Dunbar's prediction about human group size is predicated on the assumption that the regression line remains linear well outside the range for which data was actually observed. As Dunbar recognizes "Strictly speaking, of course, extrapolation from regression equations beyond the range of the X-variables values on which they are based is frowned on. We can justify doing so in this case, however, on the grounds that our concern at this stage is exploratory rather than explanatory" (1996: 682). Still, that the predictions are outside the range of observed data should make us less confident that the real natural group size (if one exists) is close to 150.

[18] Paul Erlich (2000: 157) writes: "...Dunbar and anthropologist Leslie Aiello connected the need to groom, constraints on group size, and the evolution of language. Anatomically modern human beings, their research indicated, have a basic group size of 90-220 individuals, which brackets the number, 148, that was statistically predicted from the relationship between the neocortex-brain volume ratio and group size... [P]eople generally know how each person is related to the others, and Dunbar and Aiello's statistical prediction conforms quite well with observed group sizes of acquaintances in modern societies (130-150) individuals."

We began this section with Malcolm Gladwell opining about the social policy implications of the scientific finding that groups larger than 150 run afoul of a cognitive barrier. Shouldn't we be taking this biological fact into account when we design schools or factories, he asks? When we go back and look at the actual research underlying this finding, however, we can see that basing any serious policy upon this research would be premature. First, I pointed out some problems in the human social phenomena that Dunbar cites as suggestive of a natural group size of 150. Then, I argued that Dunbar's criteria for whether a particular group size was close enough to 150 to support his theory seems to be based on both a questionable interpretation of the meaning of confidence intervals and a possible computational error. Finally, I gave several reasons why the prediction of 150 itself may be undermined by considerable measurement and/or statistical error.

WHITHER THE WASP WAIST?

The front jacket of Nancy Etcoff's *Survival of the Prettiest: The Science of Beauty* features a photograph of the back of a woman wearing a classically elegant red dress. The photograph spans only from just below her shoulders to just above her buttocks; what it is meant to showcase is the woman's perfect "wasp waist" (a.k.a. her "hourglass figure"). The cover choice is presumably a reference to the evolutionary psychological research on waist-to-hip conducted mainly by Devendra Singh. Singh has amassed evidence for an evolved male preference for a waist-to-hip ratio of .70, which happens to correspond pretty closely to the measurements of supermodels like Kate Moss (33-23-35; WHR=[.66]), Cindy Crawford

(35-24-35; WHR=[.69]), and Anna Nicole Smith (39-27-39; WHR=[.69]).[19] These three examples also illustrate the key proposal that the preference for WHR remains constant as the woman's weight changes. It doesn't matter if you are so waifish as to inspire a public outcry (Moss) or so buxom as to inspire numerous late-night talk-show jokes (Smith), a waist-to-hip ratio of .70 is what men prefer.

The WHR of healthy, premenopausal women in developed societies typically ranges from .67 to .80 (Singh 1993). The evolutionary explanation of why selection favored men with preferences toward the lower end of this range cites advantages of low-WHR women over high-WHR women in both health and fertility. I discuss these in more detail after examining the evidence that a universal male preference for low-WHR women exists empirically. We should be clear that this research is typically framed in terms of the comparison of low-WHR to high-WHR, but that claims often suggest a specific preference for a WHR of .70 (which is low) are often seen. For example, Burnham and Phelan write, "Although subconscious, there's something about that 0.7 that only a gene could love." Below is how Buss (1999: 144; see also, e.g., Burnham and Phelan 2000:162-63) recounts the case for a universal preference for the .70 WHR:

> In a dozen studies conducted by Singh, men rated the attractiveness of female figures who varied in both WHR and total amount of fat... Regardless of the total amount of fat... men find women with low WHRs the most attractive. Women with a WHR of 0.70 are seen as more attractive than women with a WHR of .80, who in turn are seen as more attractive than women with a WHR of .90. Studies with line drawings and with computer-generated photographic images produced the same results. Finally, Singh's analysis of *Playboy* centerfolds and winners of U.S. beauty contests over the past thirty years confirmed the invariance of this cue. Although both centerfolds and beauty contest winners got slightly thinner over that period, their WHRs remained exactly the same, at 0.70.

[19] These measurements are taken from Internet fan sites, and confidence in their absolute accuracy should be circumscribed accordingly. Etcoff (1999: 192) reports that "sex bomb" Barbie's projected measurements are 36-18-33, or a WHR of .54.

> Is there any evidence that low WHRs are preferred across different ethnic groups? ...Singh and Luis (1995) presented line drawings of women differing in WHRs and body sizes to young Indonesian and Black men.... The results proved almost identical to those of the original studies. Men judged female figures who were of normal weight and had low WHRs (0.70) as the most attractive. Although further cross-cultural data are needed to confirm the hypothesis that WHR is a universal cue to female beauty, the existing evidence clearly points to this conclusion.

Reviewing the literature suggests that the evidence may once have pointed to this conclusion, but that it may not anymore. Yu and Shepard (1998: 321) were concerned that even though the low-WHR preference was thought to be one of the best-documented instances of a human male adaptation in mate preference, "every culture tested so far has been exposed to the potentially confounding effects of western media. Many of the remotest places on Earth have access to television, cinema and advertising posters displaying exceptionally gynoid females draped over desirable products such as cars and beer." The authors sought to replicate the findings among the Matsigenka, an indigenous people of Peru.

Yu and Shepard followed the usual methodology, which shows subjects a set of line drawings of a white woman wearing what looks like a black one-piece bathing suit. The females' face is the same in all pictures, but her weight and WHR are systematically varied. In their study, 6 drawings were used: 3 weight classes (underweight, normal, and overweight) and 2 WHRs (.70 and .90). Subjects are asked to rate or rank order the drawings on three dimensions: attractiveness, healthiness, and desirability as a spouse.

Yu and Shepard compared two Matsigenka populations, the Yomybato and Shipetiari, who only separated from one another in the last 20-30 years. The Yomybato live near the core of giant preserve that is otherwise restricted to scientists and officials and are, in the authors' reckoning, about as culturally isolated as anyone who can be studied today. The

Shipetiari live along a trade route outside the park and have had more contact with Western influences.

Yu and Shepard found that the Yomybato rated the women with a .9 WHR as more attractive than the women with a .7 WHR. The Shipetiari rated the women with a .7 WHR as more attractive, but their preference for low-WHR women was significantly weaker than a sample of men from the United States. Interestingly, although the Shipetiari thought that low-WHR women were more attractive, they also thought they were *less* healthy than high-WHR women, in agreement with the Yomybato but in contrast to American men, who regard low-WHR women as looking both more attractive and more healthy. Yu and Shepard suggest that their findings not only indicate that the preference for the .7 WHR is not culturally invariant, but that they may even capture the WHR assessments of the Shipetiari changing in response to Western influence. As further evidence of this possibility, the authors looked at a third indigenous Peruvian population, this one ethnically mixed and more westernized than the Shipetiari, and they found that this populations' ratings of both the attractiveness and healthiness of WHR women was not significantly different from that of American men. Yu and Shepard conclude that "many 'cross-cultural' tests in evolutionary psychology may have only reflected the pervasiveness of western media" (322).

The Matsigenka practice slash-and-burn agriculture. As I have noted, evolutionary psychologists propose that the most important environments for the evolution of our domain-specific psychological mechanisms was those of the Pleistocene, in which human society was based on foraging or hunting-and-gathering. Wetsman and Marlowe (1999) published a study that examined male WHR preferences in a foraging society. Their subjects were the Hadza, who live in a partly savanna environment in Tanzania. Only about a third of 1,000 Hadza subsist only through foraging, but Wetsman and Marlowe restricted their sample to just these men.

Wetsman and Marlowe used the same methodology of having subjects rank line drawings as did Yu and Richards and earlier studies. They found that Hadza strongly preferred heavier women over thinner women, but that they did not systematically prefer either a .7 or .9 WHR. These results were statistically significantly different from a comparison sample of UCLA undergraduates, who exhibited the familiar American preference for thinness and wasp waists in women. In sum, while the Yomybato men showed a preference for women with higher WHRs, the Hadza men showed no preference one way or the other.

Wetsman and Marlowe are cautious in interpreting their results, saying "WHR preferences need to be tested in several other subsistence, nonstate societies before concluding that the trait is culturally invariant" (226). (This may go too far: it's not clear to me how a conclusion of cultural *invariance* would be restored by studying other societies; invariance would instead seem to require a study refuting their own results.) Their results do not imply that species-typical evolved adaptations cued specifically to WHR are not in play. Wetsman and Marlowe suggest that mate preference mechanisms could be sensitive to food scarcity: "WHR may only become relevant when food resources are plentiful enough that the risk of starvation during pregnancy and lactation for women is minimal" (226). This conjecture does not sit easily with the juxtaposition of their observations that "A foraging lifestyle means that surplus food is rarely available" and that foraging societies are "the closest link to societies in which WHR preferences evolved" (226). For an environmentally sensitive mechanism to evolve, the relevant environmental variations must recur, meaning that food surplus can only be a cue to a mechanism if it was a recurrent feature of the relevant adaptive environment.[20] This said, the merits of their conjecture can ultimately only be determined by further study.

As I said before, the evolutionary explanation for the preference for low WHR women has cited advantages for health and fertility, which effectively make low-WHR women more lucrative mates from a Darwinian standpoint than high-WHR women. As Singh (1993: 305) writes, "[T]hat WHR conveys such significant information about the mate value of a woman suggests that men in all societies should favor women with a lower WHR over women with a higher WHR for mate selection or at least find such women sexually attractive." So, if there is no invariant preference for low WHR, does this mean that natural selection of male preferences failed to track a stable and important cue to women's mate value? It is hard to say, because Wetsman's review of the literature suggests that the evidence of the adaptive value of the preference for low-WHR may be actually much weaker than one might gather from Singh's articles or the secondary treatments of this research.

As an illustration, let's consider three passages from Singh's (1993) most cited article on WHR. In each case, I will juxtapose the evidence of the benefits of low-WHR as they are presented by Singh with a second look at this evidence that the critique Wetsman provides:

EVIDENCE: "The relationship between WHR and reproductive potential can be inferred from the findings that body-weight-matched girls with lower WHR exhibit earlier

[20] Wetsman and Marlowe (1999: 226) write: "A foraging lifestyle means that surplus food is rarely available. Even when there is enough food to keep women alive, there may often not be enough to support fat deposition at a sufficiently high rate to adequately support ovulation and lactation. Weight of a potential partner becomes of primary concern, because this factor would be the best predictor of fertility for women. In Western industrialized nations where food is abundant, weight influences fertility to a reduced extent because there is little threat of starvation. WHR, therefore, becomes a reliable cue to fertility." The key question, however, would seem to be whether WHR was a reliable cue to fertility in the environment where the mechanism is supposed to have evolved. More completely, what we need to be asking is whether sufficiently large food surpluses occurred with sufficient regularity and were sufficiently reliable cues to fertility (net of weight, which they suppose to be the more fundamental cue) to cause the evolution of the environmentally sensitive mechanism.

pubertal endocrine activity, as measured by high levels of lutenizing hormone and follicle-stimulating hormone as well as sex steroid activity" (p. 294).

As Wetsman reports, the cited study (DeRidder et al. 1990) divided the girls into four quartiles, from lowest to highest WHR. Although the lowest quartile did have the highest levels of hormones, the highest quartile had the *second* highest levels of estrogens and lutenizing hormone. Second, the average age of the girls was under 11 (WHR decreases as girls go through puberty). The relationship of this finding to the reproductive potential and WHR of postpubertal women is not clear.

EVIDENCE: "A direct relationship between WHR and fertility has been reported recently; married women with higher WHR and lower body mass index (BMI) report having more difficulty becoming pregnant and have their first live birth at a later age than married women with lower WHR." (p. 294)

As discussed by Wetsman, in the cited study (Kaye et al. 1990), the difference in WHR between the women who did and did not have difficulty getting pregnant was 0.838 vs. 0.840. The difference in WHR between women who had their first live birth before 30 versus those having their first birth after 30 was 0.838 versus 0.843. These differences were *statistically significant* because the study included over 40,000 women, but their *substantive significance* for shaping mate preference mechanisms is doubtful, especially since the mean differences would seem smaller than what men can actually perceive. Also, Singh does not mention that the same study found that women who had 5 or more children had a higher average WHR than women who had fewer, and women who reported a family history of breast cancer had a lower average WHR than those with no such history.

EVIDENCE: "[L]ow WHR also accurately signals health as defined by absence of major diseases. A large number of studies have found that the risk-factor profile for major obesity-related diseases such as diabetes, heart attack, and stroke varies with the distribution of fat rather than the total amount of fat." (p. 295; see also table on same page; citations rely heavily on review by Barbieri [1990]).

Wetsman reports that obesity is typically presumed to have been rare in our evolutionary past, especially among reproductive-aged women. If correct, then the consequences of different kinds of obesity (i.e., being "apple-bodied" vs. "pear-bodied") could not have been a target of selection. Accordingly, studies of differences among obese women are of little value for substantiating adaptationist claims.

If we take the points of this critique together, they suggest that Singh's article may overstate the adaptive benefits of low waist-to-hip ratio for women. This raises the possibility that some evolutionary psychologists may not only have been premature in concluding that the preference was universal and that it was the product of a specialized adaptation, but perhaps also in concluding, at least from the original evidence, that the preference was adaptive in the first place.

CONCLUSION

At the beginning of this chapter, I suggested that these three examples of "magic numbers" might not only turn out to be factually incorrect but, in their repetition in secondary accounts, also may encourage certain misconceptions about evolution and human behavior. One is that natural selection has shaped psychological mechanisms to be more closely tuned to putative optima (a waist-to-hip preference of .70; a pair-bond length of 4 years) than I believe is typically warranted. Important to remember is that the smaller the discrepancy between the performance of an adaptation and its optimal performance, the weaker is the selection pressure for the further refinement of the adaptation (especially given pleiotropy, that the same gene can have multiple effects) (Kitcher 1985; Sober 1987). Certainly, natural selection can produce adaptations that are exquisitely fine-grained, but these suggest occasions in which the selection toward a specific complex optimum was strong and persistent (see, e.g., considerations of the adaptation of eyes, as in Dawkins 1996). More typically, and as other evolutionary scholars have suggested, we might expect psychological adaptations to look more like a broad heuristic than a finely honed rule (e.g., Ehrlich 2000). Also, the greater the fluctuation in environments in our evolutionary past, the more that an optimally attractive feature or optimal mating behavior may have itself fluctuated, reducing the selection pressure for precision to any specific optimum even further (Kitcher 1987; Lewontin 1987).

For social scientists, overly simple statements about universals are also troublesome because they set the bar too high for the sort of empirical insights that we should expect an evolutionary approach to produce. Some authors seems to believe that if only the social sciences would become "Darwinized," then suddenly we will begin uncovering all kinds of "laws" like the Rule of 150. Evolutionary psychologists have already been accused of making far greater promises about what the perspective can provide than they will be able to keep (Coyne 2000). In addition, simple universals encourage a simple model in which evolved tendencies produce consistent and specific effects and in which the interaction of transmitted culture with our cognitive structure is largely superficial. This only prolongs the view that asserting the importance of thinking about evolved cognition implies minimizing the importance of cultural variability and the immense differences that exist between the developed societies of the postmodern world and traditional agrarian or foraging ones. Given the massive changes that modernity has wrought in terms of the availability of nutrition, hygiene, beauty products, diet aids, etc., we should not be much surprised if women who appear to have hyperperfect faces or bodies by our own standards strike men from traditional societies as unhealthy or freakish. Given the massive variety of male-female arrangements that get classified together as "marriage"—and all the variety in surrounding customs thereof—we should not be surprised if there is no strong universal "signal" running through their noisy variability of cross-national data on divorce. Given the massive technological changes in the means of communication (e.g., writing, telephone, e-mail) and methods of keeping track of people (e.g., files, address books, Palm Pilots), we would have to hold to a very asocial view of cognition to suppose that our capacity for maintaining social ties effectively is constricted by a numerical limit that is the same as it was in our days on the savanna (see Clark 1997).

To be sure, claims about simple but specific universals provide an effective rhetorical tool in arguing against the idea that cultural variation is arbitrary and boundless, but this may come at the expense of promoting a view that fails to appreciate the consequences of the considerable variation among cultures that does exist. Confident pronouncements may sow cynicism when further scrutiny or additional study finds that the strong claims do not pan out. For example, when one looks at the stridency of some authors' accounts of the evolved, universal preference for a .70 waist-to-hip ratio, and then compares it to the actual supporting evidence that fueled this confidence, the authors end up looking less like sober scientists and commentators and more like advocates eager to embrace strong conclusions that support their view. More generally, secondary accounts of evolutionary social science are plagued by an overabundance of forceful writing about weak or incomplete research. The critiques in this chapter may encourage sobriety in the face of some of the near-zealous promotion of Darwinian theories by some of the perspective's proponents. The critiques may also provide cautionary tales about the importance of closely evaluating both the evolutionary logic of Darwinian explanations and the adequacy of their fit with the evidence offered in support.

A complaint about critics of evolutionary psychology is that they focus too much on popular treatments of the topic.[21] As I have already noted, the popular treatments in question are in many cases written by leading members of the field (Pinker 1997; Buss 1999) or highly commended by leading members of the field (reactions to Wright 1994a). The popular treatments have also received a wide reading among academics.[22] As the complaint goes, such popularizations may contain simplifications of thought that turn off readers more familiar with behavioral science research, but when one goes back to the journals and looks at the articles on which the popularizations are based, one finds careful "normal science" at work (Holcomb 1996). There is certainly some truth here, and research articles by evolutionary behavior scientists tend to be much more guarded and tentative than subsequent depictions of results by others. But the other side of the coin is that *popularizations of findings also hide shortcomings of the underlying research and can make it look much better than it is*. When Malcolm Gladwell reports that Dunbar calculated a predicted group size of 147.8 from a model of the neocortex ratios of other primates, the work looks much more solid than it does when one goes back to the original articles and sees the various statistical vagaries involved in this prediction. Recounts of the research on waist-to-hip ratio have typically presented its adaptive benefit as an established conclusion, and it is only when one looks up the sources cited by Singh (1993) does one realize how weak is the evidence presented in this seminal article for the value of a low ratio.

[21] Note that by popular, we are talking about *New York Review of Books*-popular, not *New York Post*-popular.
[22] Witness, for example, David Popenoe's (1996) nomination of *The Moral Animal* in *Contemporary Sociology*'s forum on favorite books of the previous 25 years.

How should one approach a book like *Mean Genes* (Burnham and Phelan 2000), which includes confident presentations of both the "four-year itch" and the preference for a .7 waist-to-hip ratio (as well as many other evolutionary hypotheses)? On the one hand, it is pitched as a Darwinian self-help book, written in a popular style and chock full of tips on how to harness the scientific insights of evolutionary psychology for personal improvement. On the other hand, the authors are an economic and a biologist with prestigious university affiliations. The jacket features a quote from Harvard anthropologist Irven DeVore assuring readers that the authors are "highly regarded scholars," and one from E. O. Wilson certifying the book as "well-grounded evolutionary biology." For the sociologist of science, we have episodes in which popular books enroll the endorsements of eminent others to aver that the book is based on good science, which in turn enhances the perceived authority of the findings that are boldly presented in its pages.

The parting message of this chapter is therefore twofold. First, one should be wary of simplistic claims about universal features of human behavior or human societies, such as those evinced by the claims regarding "magic numbers" that I have looked at in this chapter. We should instead expect that the universal statements we can make about social behavior will be relatively vague, and that increasing the specificity of statements will likely proceed hand-in-hand with increasing understanding of the breadth (but not boundlessness) of variation. Perhaps also the further that claims stray away from direct products of individual psychology to features that seem to depend importantly on larger group dynamics—as in the case of divorce rates and group sizes—the even more wary of the claim we should become. Second, we have seen more than once how the very features of popular presentations that contribute to the popular appeal of evolutionary approaches may ultimately be detrimental to these approaches' intellectual health. The repetition and veneration of cocktail-party universals sells books, but at the cost of stunting the development of truly interactionist insights into evolved biology and behavior, not to mention potentially diminishing the credibility of proponents if dramatic claims prove unwarranted.

7

MIDDLE-LEVEL THEORIES

AND THE MODERN STONE AGE FAMILY[1]

"Sooner or later, political science, law, economics, psychology, psychiatry, and anthropology will all be branches of sociobiology"
—Robert Trivers, Time, 1977[2]

If we are committed to the idea that Darwinian theory provides a veridical account of human origins, then it might seem to follow that any implication derived logically from basic principles of Darwinian theory must also be true. The compellingness of this reasoning has led some evolutionary scholars to speak in virtual certitudes about what evolution must have produced for humans to be what we are. Sulloway has been quoted as saying that "From a Darwinian perspective, it is just impossible that birth-order effects don't exist" (Horgan 1999: 190). Cosmides (1989: 196) has written that our capacity for social exchange implies that "The human mind must include inferential procedures that make one very good at detecting cheating on social contracts." What happens if we run the numbers and don't find any birth order effects or don't find any evidence of special facility for reasoning about social contracts? One might say, "That's the point. The 'what ifs' are moot because these things can't happen." But what if evidence ultimately does not support claims about a cheater-detector or about birth-order effects? Does this then throw the entire neo-Darwinian synthesis into doubt?

Of course not. We would instead conclude that there is a lapse in the logic of necessity somewhere between the idea of evolution by heritable variation and heritable

[1] This second half of this chapter title is borrowed from a subsection title in Pinker's How the Mind Works, which is in turn, of course, borrowed from the theme song of "The Flintstones."
[2] August 1, p. 54

reproduction and a conclusion like that firstborns are more politically conservative than laterborns. David Buss (1995) provides a good depiction of the different levels of deduction from the core ideas of inclusive fitness theory. These core propositions can be used to derive middle-level evolutionary theories, which are deductions from the basic theory that are formulated in general terms and that have potentially broad applicability. As an example, Buss uses Robert Trivers's theory of parental investment: (briefly) "the sex that invests more in offspring (often, but not always, the female) will evolve to be more choosy about mating, whereas the sex that invests less in offspring will evolve to be more competitive with members of their own sex for [access to the opposite sex]." Middle-level theories are then used to derive specific evolutionary hypotheses, which in turn are used to derive specific evolutionary predictions. The example David Buss provides from the theory of parental investment is the prediction that "[F]emales will select mates in part based on their ability and willingness to contribute resources." The specific evolutionary hypotheses can then be used to formulate specific evolutionary predictions, such as the "women have evolved preferences for men who are high in status." The falsification of a statement or prediction occasions close scrutiny of where the flaw in the reasoning might be.

This chapter will illustrate some of the problems that can emerge in the effort either to derive predictions from middle-level theories or to interpret existing patterns of behavior as fitting a middle-level theory. I examine some specific attempts to use middle-level theories to understand the interaction between parents and children in contemporary, developed societies. The study of family life is an area in which the potential contributions of an evolutionary perspective have seemed strongest, based on the premise that the affective bonds of kin are rooted in genetic-based propensities that evolved as a consequence of kin selection (see Chapter 2). I have already mentioned the research by Daly and Wilson (1988, 1999; see also Wilson 1987) that identified living with a stepparent as the largest known risk factor of being a victim of child abuse, a risk factor whose magnitude had not been identified prior to their research. The affective strength of kinship is evinced also in the recurrent and successful use of kin terms in political and religious rhetoric (e.g., Johnson 1987; Salmon 1998), and evolutionary psychologists have suggested that attempts to develop "fair" social institutions (e.g., with egalitarian or meritocratic reward structures) is continually undermined by a nepotistic tendency to act "unfairly" to favor one's relatives (Pinker 1997). Evolutionary considerations may also be implicated in the aversity that humans exhibit toward sexual activity with others with whom one is raised (e.g., Brown 1991, Ch. 5), and genetic evolution seems likely also to play a role in some sex differences in human behavior, most notably the tendency for females to invest more than males in the care of children (Trivers 1972).

Exploring the possible contributions of evolutionary theory to how sociologists understand family life was one of the initial interests that led me to embark on the larger project of this dissertation. Sociologists have a longstanding conviction that understands the transmission of social status from one generation to the next requires insight into what actually goes on within families and why parental treatment of children varies (Coleman 1966; Blau and Duncan 1967). As I have discovered, being willing to grant that there is "something to" evolutionary explanations regarding the family is one thing, but figuring out how evolutionary concepts might be used to develop new insights into family life is much more difficult. In Chapter 5, I raised questions about Frank Sulloway's theory of the effects of birth order. In this chapter, I look at applications of some of the theoretical ideas of

Robert Trivers, who has provided some of the most important foundations of the evolutionary biological theory of parental investment (Trivers 1972, 1974, 1985; Trivers and Willard 1973). If the wishes of some evolutionary psychologists were granted and the social sciences were to "Darwinize" overnight, Trivers would occupy the a similar sort of place in the canon of social theorists as Durkheim or Marx does today.

This chapter is structured very much like the last, in which an initial, more empirically-focused inquiry has spurred some additional investigation to flesh out a more basic topic in the evaluation of evolutionary approaches to human behavior. In the last chapter, we considered the problem of "vulgar universals" for the development of Darwinian social science, while in this one, we consider the problem of applying "middle-level" evolutionary theories to the complexities of contemporary family life. We begin with a study of the Trivers-Willard hypothesis, a conjecture that has been used to generate predictions about how parental status affects the tendency to favor children of one sex over the other. I report the results of a test of the hypothesis for families with adolescent children in the contemporary United States. In addition, I consider some of the theoretical considerations that complicate the application of the hypothesis to contemporary, developed societies.

After examining the Trivers-Willard hypothesis, we move to a consideration of some of the applications of Trivers's concept of "parent-offspring conflict" to describe aspects of the interaction of parents and children in modern societies. This discussion reveals how the same evolutionary dynamic can be interpreted in very different, and even contradictory, ways. Finally, I examine an article by the sociologists Timothy Biblarz and Adrian Raftery (1999), in which they use Trivers's theory of parental investment to generate predictions about the relationship between family structure and socioeconomic attainment. I will show that the specific predictions that Biblarz and Raftery provide are actually only one of several different sets of predictions that could have been just as defensibly derived, suggesting a fundamental obstacle for developing tests that pit the "evolutionary psychological perspective" versus more traditional social science frameworks. Taken together, the three inquiries comprising this chapter reveal some of the difficulties in applying Darwinian concepts to contemporary family life and suggest problems that confront attempts to incorporate evolutionary principles more thoroughly into the sociological study of the family.

THE TRIVERS-WILLARD HYPOTHESIS

"Even if I'm wrong, it will take them years to find out."
—Robert Trivers[3]

We move from parent-offspring conflict to a hypothesis that Trivers and Dan Willard published in *Science* while both were at Harvard in the early 1970's (1973). They begin with Fisher's (1958 [1930]) explanation of why sexually reproducing species so often have population sex ratios that are near 1:1. In a sexually reproducing species, because each organism has exactly one mother and one father, and approximately half the genes in the population as a whole are genes that organisms received from their mothers and half are genes received from their fathers. If a species naturally produced a population that was 60% female, then that 40% of the population that was males would provide 50% of the genes in the next generation, creating an evolutionary advantage for any genes that caused production of a greater-than-average number of sons. The evolutionary advantage would persist until the reproducing population was 50% female and 50% male. If the sex ratio swings back too

[3] Quoted by Hrdy (1987: 101) as an "offhand remark".

far, and the population of females dips below 50%, then natural selection would favor any genes that caused production of a greater-than-average number of females until the equilibrium at 1:1 was restored.

Consider now, however, that the reproductive success of males of many species tends to be positively and closely associated with their physical condition or social rank, in part because high-ranking males are more likely to procreate with more than one female. In polygynous human societies, for example, high-status males are more likely to have more than one wife (and many offspring), while low-status males are more likely to have no wives (and no offspring). The reproductive success of females is less variable than that of males because the reproductive potential of females is less strongly affected by the possibility of multiple mates. As a result, in many evolutionary environments, high-status males are expected to have a higher average number of offspring than their sisters, while low-status females have more offspring on average than their brothers.

If we assume that the rank of parents is correlated with that of their children, then it follows that high-status parents who have sons will have more grandchildren than high-status parents who have daughters. On the other hand, low-status parents with daughters will have more grandchildren than low-status parents with sons. Because these are exactly sorts of differences in rates of reproduction that drive natural selection, Trivers and Willard propose that we might expect species to have developed a mechanism by which members vary the sex ratio of their offspring in response to their rank in ways that would maximize their numbers of grandchildren and great-grandchildren. That is, we might predict that low-ranking parents would produce a disproportionate number of daughters and high-ranking parents a disproportionate number of sons.

Of more interest here, however, is that Trivers and Willard conjecture that their hypothesis *applies to parents' behavior toward children after birth just as it does to sex ratio*. They write:

> "If the model is correct, natural selection favors deviations away from 50/50 investment in the sexes, rather than deviations in sex ratios per se. In species with a long period of [parental investment] after the birth of young [e.g., humans], one might expect biases in parental behavior toward offspring of different sex, according to parental condition; parents in better condition would be expected to show a bias toward male offspring" (Trivers and Willard 1973: 91).

As I noted earlier, Trivers and Willard suggest that the hypothesis can be applied to humans by taking socioeconomic status as the proxy for parental condition, and they also apply their predictions about sex ratio to then-contemporary American society. It would seem reasonable, then, to draw the implication that the hypothesis is intended to apply to parental investment in contemporary, developed societies, as commentators like Wright have followed the lead of some researchers in doing.

At the same time, of course, we do have the issue that socioeconomic status does not have the same, strongly positive correlation with fertility as its analogue is assumed to have had in the Pleistocene. As such, it is unclear whether behaviors following the Trivers-Willard predictions would be adaptive in our current society. The hypothesis predates the "Darwinian anthropology" versus "evolutionary psychology" debate discussed in Chapter 2. I take it that most evolutionary social scientists would agree that in order for natural selection to produce behaviors consistent with the Trivers-Willard hypothesis, it would do so by shaping the design of cognitive mechanisms that influence parental investment. As I discussed in Chapter 2, a core principle of evolutionary psychology is that modern, developed societies have not existed long enough to reverse or substantially alter the cognitive mechanisms that evolved over the past thousands or millions of years. The relevance of this point requires that we "update" the Trivers-Willard hypothesis with the insights of evolutionary psychology, as the hypothesis (1973) predates the program of evolutionary psychology and even predates Wilson's *Sociobiology* (1975). Indeed, the idea that humans in contemporary societies value status *as if* it were still closely connected to fertility provides the linchpin of many contemporary applications of evolutionary approaches (e.g., Buss [1994] on sexual attraction; Wright [1994: 242-250] on gender stratification; Thornhill [1998] on aesthetics; Miller [2000] on sexual selection). Consequently, the leap that we should also expect the Trivers-Willard hypothesis to hold under contemporary conditions is small and consistent with the prevailing logic of the evolutionary approaches. In addition, Gaulin and Robbins (1991) cite evidence that they argue suggests the assumptions necessary for a Trivers-Willard mechanism to evolve do still hold in present-day North American society.

Previous Studies of the Hypothesis

Evidence for the hypothesis as it pertains to sex ratios is mixed, for other animals as well as for humans (see Hrdy 1987 for a review). In the most well-known animal study supporting the hypothesis, Clutton-Brock, Albon, and Guinness (1984) found that, among red deer, dominant-rank females produced significantly more sons than did lower-rank females (even though sex ratios were very little affected by the health of the mother or the quality of her range [Clutton-Brock, Albon, and Guinness (1982)]). Regarding the sex ratios of humans in developed societies, Trivers and Willard were correct that there is a positive association between sex ratio and socioeconomic status in the United States—as their hypothesis would predict—but this turns out to not be as clear-cut as one might think. For one thing, blacks in the United States tend to have lower sex ratios than whites, an effect that may reflect differences between European and African peoples generally more than current socioeconomic differences (Hrdy 1987; Khoury, Erickson, and James 1984). Also, sex ratio is possibly associated with birth order: the higher children are in their sibling order (i.e., a sixth-born is higher than a first-born), the more likely they may be to be daughters (Rostron and James 1977). If poorer families in many developed societies tend also to have more children, this could also imply spurious relationships between sex ratio and status that appear to support the hypothesis.[4] A large study of births in a racially homogenous (Scottish) population found no meaningful association between sex ratio and class once factors like birth order were controlled (Rostron and James 1977; see Essock-Vitale 1984 and Christopher 1984 for other studies that fail to find the predicted association between status and sex ratio).

The contradictory evidence on sex ratios has not discouraged efforts to apply the theory to parenting practices. Indeed, in her own review of the literature, Hrdy is much more enthusiastic about the evidence for Trivers-Willard effects on parental investment than on sex ratio:

> Whereas there is little evidence for sex bias at conception…, there is a great deal of evidence that parents discriminate against one sex or the other in terms of provisioning, medical attention, legacies, and simply time spent in association, and where these biases have been examined with the Trivers-Willard hypothesis in mind, they often appear to conform to specific predictions from the hypothesis. (Hrdy 1987: 131).

[4] If this relationship between birth order and sex ratio was the same in our ancestral past, when status was presumed to be substantially and positively correlated with fertility, this would appear to suggest biases in sex ratio against what the Trivers-Willard hypothesis would predict (larger, higher-status families having more daughters and smaller, lower-status families having more sons). Then again, Rostron and James (1997) also found that older mothers tended to give birth to a higher proportion of daughters than younger mothers, which could be taken as supporting the Trivers-Willard hypothesis if we speculate that older mothers would have a higher probability of death during the period of intensive parental investment (which we might expect to have an adverse effect on the child's subsequent status attainment).

A classic paper of early sociobiology by Mildred Dickemann (1979) uses the Trivers-Willard hypothesis to explain female infanticide among wealthy in India, China, and medieval Europe. Other work invokes the hypothesis to explain sex differences in infant mortality rates among the Mukogodo of Kenya (Cronk 1989, 1991), rates of parent-child interaction among the Ifalukese of Micronesia (Betzig and Turke 1986), and bridewealth payments among the Kipsigis (Borgerhoff Mulder 1987). Historical data from Germany (Voland 1988; Voland et al. 1991) and from Portugal (Boone 1984, 1986) has also since been cited in support of the hypothesis (see Cronk 1991). In contemporary North America, evidence for the hypothesis has been reported in studies from cradle to grave: Gaulin and Robbins (1991) report evidence for the hypothesis in a study of birth intervals and breast-feeding decisions, and Smith, Crawford, and Kish (1984) report results consistent with the hypothesis for a study of parents' inheritance decisions in Vancouver. Two other studies of inheritances, however, failed to support the hypothesis (Judge and Hrdy 1992; Betzig and Weber 1995).

At least one of the dependent measures used in Gaulin and Robbins's study is questionable (Freese and Powell 1999). Also, Dickemann's application of the hypothesis to female infanticide has been sharply and lucidly criticized by philosopher Philip Kitcher (1985: 315-329). Among other points, Kitcher notes that whatever fitness gains on Trivers-Willard lines are accomplished by infanticide are very likely more than offset by the high fitness costs associated with killing off half of one's offspring (see also Anderson and Crawford 1992 for another mathematical demonstration of this point). Kitcher also accuses Dickemann of one of the cardinal sins of "bad" reductionism: marring good, descriptive anthropological work by trying to force an interpretation based on a narrow theoretical dynamic.

My Own Studies, Subsequent Studies, and Subsequent Commentary on My Studies

Within sociology, quite a large literature has developed using the notion of parental investment especially as a way of describing the various ways that parents may act to help their children's socioeconomic futures. An open question is how much these existing sociological programs of research into parental investment might benefit from evolutionary perspectives on parental investment. The Trivers-Willard hypothesis was attractive to me in this regard because it is a hypothesis with clearly testable implications, and yet the predictions it makes are different from what a non-selection-thinking perspective in social science would likely predict. Brian Powell and I (1999) decided to focus our efforts on whether behavioral patterns consistent with the Trivers-Willard hypothesis could be observed among parents of adolescents in developed societies. This test is only summarized in the text here, but more detail is provided in Appendix A. For sociologists, adolescence is perhaps the period in which parental investment and its effects on children's outcomes have received the most attention. Adolescence also represents a period close to that in which offspring have their maximum reproductive value, having survived childhood but being just at the start of the many years in which they will be capable of reproducing,

Our study used data from two large, nationally representative surveys of American adolescents: the 1988 National Educational Longitudinal Study (NELS) and the 1980 High School and Beyond survey (HSB). These data were both collected by the National Center for Educational Statistics, and they have been used in dozens of other studies in addition to ours. The focal respondents in the NELS sample were eighth-graders and in the HSB sample were tenth-graders.[5] In addition to interviews with students, both studies attempted to interview one of the students' parents and also gathered information from and about the student's school. An advantage of this method of data collection is that being able to draw on both student and parent information makes our results less vulnerable to biases in either one.

We looked at measures of five different types of parental investment, in order to provide as broad a test as possible of the hypothesis. The five types also illustrate the diversity of ways that sociologists of family have conceived of possible parental investments in adolescent offspring (although, as I said before, our study focuses mainly on the educational aspects of these different types of investment) (see Appendix A for citations relevant for specific measures). *Economic investments* are financial expenditures parents make on behalf of their children, such as paying to send one's child to a private school or saving for college. *Interactional investments* measure parents' actual personal involvement with their children. *Supervisory investments* measure the effort parents spend monitoring their children's activities. *Social investments* involve knowledge that parents possess about their children's social networks. For example, when the parents of adolescent friends know one another, they may exchange information about their children's activities, help one another enforce family rules, and share responsibility for supervision. *Cultural investments* are provisions by parents for the child's acquisition of "cultural capital," socially valued (i.e.,

[5] The NELS study actually re-interviewed the same students as tenth-graders and twelfth-graders. We use the tenth-grade data to construct two of our measures of parental investment because the questions had not been asked in the eighth-grade survey. The HSB study included separate samples of tenth-graders and twelfth-graders. We looked at the twelfth-grade data and the results were imply the same substantive conclusions as the data we present here. NELS attempted to interview a parent of all respondents, while HSB only did so for a much smaller randomly selected sample of respondents.

elite) tastes and knowledge that contribute to attainment. We looked at 12 different variables for NELS; only 6 of these variables (mostly economic investments) had analogues in HSB.

For testing the Trivers-Willard hypothesis, the key independent variable is the interaction between child's sex and parents' status; if child's sex is coded as 0 for females and 1 for males, the hypothesis predicts that as status increases, the slope of the coefficient for this interaction should also increase. As is typical when working with interactions, we also estimate the main effects of child's sex and parents' status in our models. In actuality, we use two different measures of socioeconomic status: the education of the most highly educated parent, and parents' income. Our models also included additional controls for respondents' race/ethnicity, the marital status of the parents, the mother's age, and the number of siblings in the family, but, as it turned out, our results did not depend on whether any of these variables were included or excluded from the models.

The results of our model are summarized in Table 7.1. Because we used two different measures of status and 12 different measures of investment in one dataset and 6 in the other, our study comprised 36 different statistical tests in all. By chance alone, the probability of observing a significant effect in the predicted direction is 1 in 20.[6] We observed only one significant effect in the predicted direction in our study. In the NELS data, parents of lower education levels were reported to provide closer monitoring of girls relative to boys than were parents of higher educational levels. In contrast, we observed three statistically significant effects that were in the opposite direction from that predicted by the hypothesis. We tested a variety of alternative model specifications, and we looked for effects within different subsamples of NELS and HSB respondents. Nonetheless, our results

[6] Although of course the tests in this study are not independent.

uniformly failed to produce more significant effects supporting the hypothesis than what we would expect to observe purely by chance.

Table 7.1 Summary of results from Freese and Powell study of the Trivers-Willard Hypothesis

Dataset:	National Educational Longitudinal Study		High School and Beyond	
Measure of investment:	Child's sex × Parents' education	Child's sex × Family income	Child's sex × Parents' education	Child's sex × Family income
Economic investments				
Started saving for child's education	Null	Null	Null	Null
Total money saved for college	Null	Null	Null	Null
Private school	Null	Null	Null	Null
Educational objects in home	Null	Null	Null	Null
Interactional investments				
Talk with child about school	Null	Null	**Opposite**	Null
Involvement with child's school	**Opposite**	Null	--	--
Parent-teacher organization	Null	Null	--	--
Supervisory investments				
Monitoring of child's behavior	Null	**Confirming**	Null	Null
Social investments				
Know child's friends	Null	Null	--	--
Know child's friends' parents	Null	Null	--	--
Cultural investments				
Cultural classes	**Opposite**	**Opposite**	--	--
Cultural activities	Null	Null	--	--

Note: "Confirming" indicates a statistically significant result ($p < .05$) supporting the hypothesis; "opposite" indicates a statistically significant result in the direction opposite that predicted by the hypothesis; "null" indicates a nonsignificant result in either direction.

Kanazawa (forthcoming b) has criticized our study for using measures of investment related to edcuation, which he says an evolutionary psychological perspective would regard as "male-specific." In this view, the payoff of higher education is increased status and increased status in our ancestral past only accrued benefits in reproductive success for men, and consequently investment measures regarding higher education are inherently biased toward sons. He conducts another study using an alternate dataset (National Survey of Families and Households) and another measure of investment (how many activities parents do with children, which he regards as sex-neutral). He proposes that the results of these tests provide significant support for the Trivers-Willard hypothesis.

In our response (Freese and Powell forthcoming), we dispute Kanazawa's claim that the failure of the education measure is what proper reasoning from an evolutionary psychological perspective would have predicted all along. We object because we think that plainly, had we found patterns consistent with the hypothesis for these measures, this would have been taken as support for the hypothesis. We provide several Darwinian arguments that could have been used to justify this conclusion had supportive results been found. In presenting these, we are not criticize the use of induction to generate new theoretical insights, but we are critical of the presentation of inductively derived explanations as if they were what a deductive approach would have initially yielded.

In addition, we note that Kanazawa's critique still leaves several null findings from our original study unchallenged (Freese and Powell forthcoming). His critique also implies its own evolutionary predictions that are not supported by the data. If these investments are male-biased and parental investment patterns are strongly influenced by what was advantageous in the Pleistocene, then we would expect the parents to consistently favor males for these investments, but this was not observed in the NELS/HSB data—3 of 11 significant effects indicate bias toward sons, but 3 other significant effects indicate bias toward daughters (see Tables 3 and 5 of Appendix A). Kanazawa's critique would also seem to be contradicted by the construction of the dependent variable for his own study: his scale includes an item about parents "helping [their children] with reading or homework," which would seem as much an education-related measure as some of the measures of ours that he disputes. Finally, we re-examine his analysis and find that his own results are not robust: that they are statistically significant only within a very limited range of possible and largely arbitrary analyst's decisions. Consequently, we conclude that Kanazawa's comment does not provide any persuasive support that the TWH is important for understanding human parental behavior, at least in the contemporary United States.

Also, after our original study was published, another study of the Trivers-Willard hypothesis was conducted by Matthew Keller, Randy Nesse, and Sandra Hofferth (under review).[7] They looked at data from the 1997 Child Development Supplement of the Panel Study of Income Dynamics (*N*=2380 families). They examined the interaction of child's sex and four different measures of status (family income, parental education, whether respondents reporting having trouble feeding their family in the past year, and whether the family had received any governmental assistance while the mother was pregnant with the child in question). They looked at five dependent variables: the warmth of the parent-child relationship (both self-reported by the parent and as judged by an observer), the time the parent spent with the child (both in terms of raw minutes and as a percentage of the parents' available free time), whether and how long the child was breast-fed, and the child's birth weight (the last three measures allow comparison with the earlier study by Gaulin and Robbins [1991]). Note that none of these measures concern higher education. The results of the Keller et al. study are presented in Table 7.2. Of the 28 tests presented here, only two

[7] More information on this study will be added.

significant effects supporting the hypothesis were observed, and one significant effect opposite the predicted direction was observed. Just as we did, Keller et al. failed to find any support for the Trivers-Willard hypothesis in a contemporary North American sample.

Table 7.2: Summary of Results from Keller, Nesse, and Hofferth study of the Trivers-Willard Hypothesis, using Panel Study of Income Dynamics

	Interaction of child's sex and measure of status			
Measure of investment	Parent's education	Family income	Trouble feeding family in past year	Received public assistance
Warmth of relationship, self-reported	Null	Null	Null	Null
Warmth of relationship, observer rating	Null	Null	Null	Null
Time spent with child (unadjusted)	Null	Null	Null	Null
Time spent with child (adjusted)	Null	Null	Null	**Opposite**
Whether child was breastfed	Null	Null	**Confirming**	**Confirming**
Duration of breastfeeding	Null	Null	Null	Null
Birth weight	Null	Null	Null	Null

*adjustment for total amount of free time that respondent could have spent with child
Note: "Confirming" indicates a statistically significant result ($p < .05$) supporting the hypothesis; "opposite" indicates a statistically significant result in the direction opposite that predicted by the hypothesis; "null" indicates a nonsignificant result in either direction.

Additionally, I have conducted some additional, exploratory tests of the Trivers-Willard hypothesis regarding marital and fertility history, using the 1957-92 Wisconsin Longitudinal Study (WLS) and the 1995 marriage and family supplement to the Current Population Survey (CPS) *(see Appendix B to this chapter)*. If high-status parents favor sons over daughters, and low-status parents *vice versa*, then we might expect high-status parents to be relatively more likely to divorce after the birth of a daughter and low-status parents to be relatively more likely to divorce after the birth of a son. We might also expect high-status parents to be relatively more likely to stop having more children if their first child is a son (or first two children are sons), and low-status parents to be more likely to stop if there first child is a daughter (or first two children are daughters). Or, we might expect high-status parents to have a relatively longer interbirth interval after the birth and low-status parents to have a relatively longer interval after the birth of a daughter. The preliminary tests that I have conducted provide no more than very weak support for any of these predictions. Also, at best, weak support was found in these data for the Trivers-Willard predictions regarding sex ratio.

Other Issues

The above studies comprise an empirical challenge to the proposition that the dynamic proposed by Trivers and Willard is consequential to our understanding of parental investment in contemporary, developed societies. One can also question whether the Trivers-Willard predictions about parental investment in contemporary American society follow properly from evolutionary reasoning, as I suggest in this section.

Does behavior consistent with the Trivers-Willard hypothesis actually maximize reproductive success? The logic of the Trivers-Willard hypothesis may appear to be so straightforward that investments patterns consistent with its predictions might be thought to necessarily yield a fitness advantage whenever the specified conditions are met. Judith Anderson and Charles Crawford (1993), however, attempted to model the predictions of the hypothesis mathematically to determine its success at maximizing number of grandchildren in the context of other variables. They found that optimal investment patterns depended on a number of variables, including the dimensions of reproductive value affected by investments, the ages of sons and daughters in a family, whether the local population was growing or shrinking, and the extent to which variance in reproductive success differed between men and women. Although this kind of modeling approach requires various simplifying assumptions and can only look at a few variables at a time, the sensitivity of their model to the variables they did examine was enough to lead them to conclude that "it is reasonable to question whether the original Trivers-Willard rules of thumb would have been favored by natural selection in any current human population" (Anderson and Crawford 1993: 170). They go on to counsel that, "It would strengthen any test of the Trivers-Willard hypothesis if the investigators first demonstrate quantitatively, for the population under study, that the [hypothesized] rules of thumb being tested maximize numbers of grandchildren, or could have done so in the past" (Anderson and Crawford 1993: 170-71).

An evolutionary psychologist would likely argue that if we have cognitive adaptations in our heads that lead us to behave in ways consistent with the Trivers-Willard hypothesis, these adaptations acquired their form in response to the ancestral conditions of the Pleistocene and not those of modern life (Kanazawa forthcoming b). Anderson and Crawford's analysis shows that the applicability of the hypothesis may depend on the values of contextual variables from Pleistocene societies about which we can have no or only dim knowledge. Their analysis does make use of fertility schedules based on the !Kung of Africa—the favorite hunter-gatherers of evolutionary psychologists—and one could attempt to draw the other necessary inferences for the model from them. Yet, the assumption that !Kung demographics match the demographics of the EEA may be more tenuous than some evolutionary psychologists might readily admit (especially when we consider the variability among the world's hunter-gatherer peoples [Betzig 1998]), and any quantitative analysis based on the !Kung stands or falls with this assumption. As things stand, even if one was comfortable presuming that contemporary parental investments completely and thoroughly reflect Pleistocene imperatives, we would still have reason to doubt whether Trivers-Willard effects play any meaningful role in determining parental behavior.

Is socioeconomic status an appropriate proxy for parental condition? Trivers and Willard (1973) develop their paper as a hypothesis about the effects of parental "condition" on sex ratios, but parental condition itself is only loosely defined. Potentially, condition might be any variable that affects investment capacity and children's reproductive success. The examples they present are most suggestive of condition as physical health; that is, we might expect the strongest mothers to favor their sons and the weakest ones to favor their daughters. As I have noted, Trivers and Willard conjecture that their hypothesis may apply to humans in terms of socioeconomic status, such that higher and lower status serve as human society's counterparts to physical strength and weakness, at least in terms of the effects on intergenerational transmission of "condition" and reproductive success. Tests of the Trivers-Willard hypothesis in contemporary American societies (including my own studies) have typically used income or wealth as measures of condition. At the same time, some of the most impressive animal evidence comes from a study of the *dominance rank* of red deer, with high-ranking mothers producing a far greater proportion of sons than low-ranking mothers (see Clutton-Brock, Albon, and Guinness 1982, 1984). The same researchers, incidentally, did not find any Trivers-Willard effects for various indicators of physical health other than dominance rank.

Dominance rank is a central feature of life for many species, including our primate relatives. Dominant members of primate groups have preferential access to resources (also for dominant males, preferential access to estrus females), and rank determines priority for grooming and who gets to sit where (see, e.g., Dunbar 1996). Social hierarchies are of course a pervasive feature of human lives, and position in these hierarchies has an enormous influence on the course of people's lives. Various Darwinian social scientists have proposed that the need to reason strategically about and maneuver within social hierarchies has been an important selection pressure shaping the evolution of human cognition (e.g., Cummins 1998). In some traditional human societies, the relationship between one's "status" and "dominance rank" may be reasonably transparent.

Socioeconomic attainment is often taken as modern society's equivalent of the attainment of dominance rank among primates and the attainment of status among hunter-gatherers. Expecting evolved mechanisms to map neatly onto socioeconomic status is perhaps too much, given the obvious differences between ancestral and modern environments.[8] While socioeconomic status undoubtedly bears some relationship to the amount of dominance and deference behaviors individuals experience in their everyday lives, the relationship would seem to be far from perfect. Certainly, we can point to low-paying service-sector jobs in which heavy displays of deference are all in a demeaning day's work, but we should not overestimate the strength of the inverse relationship between occupational prestige and required deference (e.g., who has to be more obsequious to others more on the job, a junior executive or a construction worker?). Corporations have many employees who are well-paid but spend much of their day following the orders of others, while others make much less money managing a gas station and yet run the place like an ill-tempered tyrant.

[8] We must keep in mind that if we accept the premises of evolutionary psychology, then the cognitive mechanisms that produce behaviors consistent with the Trivers-Willard predictions must be wired to be sensitive to cues of the individual's status relative to others. A challenge for evolutionary psychology is to determine what cues are processed by status-dependent mechanisms in assessing one's social position, and then to explore how these cues translate into the status environments of contemporary societies. In order to predict the contemporary operation of status-dependent mechanisms, we would expect that the effectiveness of socioeconomic status as a proxy for rank in the EEA would depend on the correspondence of the *experience* of high status in the two environments on whatever dimensions of information are salient to the mechanism.

A related point is that the highest and lowest status members of modern human societies have much weaker social ties to one another than the highest and lowest members of a primate troop or a hunter-gatherer band. American cities, of course, tend to be structured in ways that minimize the residential proximity of rich and poor, and assortative associations tend also to minimize the extent to which the rich and poor share social circles (e.g., Massey and Denton 1993). Consequently, people's daily interactions may represent a very skewed sample from which inferences about one's socioeconomic standing are made. While most people have a general sense of their own family's status, individual's subjective assessments of their relative status may sometimes be considerably at odds with those obtained by measures of income, occupational prestige, or wealth (e.g., Simpson, Stark, and Jackson 1988). Although social science often favors nationally representative samples, there is no reason to suppose our cognitive mechanisms base their assessments on populations that start and stop at national boundaries, since there was nothing like the national level of analysis in the environments of the Pleistocene. In our test of the hypothesis for investment in adolescents, we looked at parents' income and education both as compared to the national average and compared to the other parent respondents from the same school, and we did not find any evidence supporting the hypothesis in either instance.

Also, modern life may be characterized by a proliferation of hierarchies that provide many of the trappings of rank (e.g., respect or control over resources). Strong hierarchies can exist within groups of individuals of the same occupation, as in the status differences between members of the same academic department or between more and less prestigious departments. Alternate hierarchies exist in which persons with mediocre jobs can rise up the ranks of their church or bowling league or volunteer organization, and their social identities may come to be much more heavily vested in these avocations than in their careers (Frank 1985). A consequence of variegated hierarchies in which individuals can hold multiple and discrepant positions is that status-dependent mechanisms can take some cues from domains that are only partially or weakly related to any general measure of socioeconomic status.[9]

Taken together, the upshot of the various potential conflicts between our ancestral past and postindustrial present is that conventional measures of socioeconomic status may not be good measures of status as it is relevant to the operation of any cognitive mechanisms that are dependent on status judgments. In other words, such judgments could be based on cues that are not well-detected by measures of annual income or accumulated wealth. Of course, this opens the door to a good deal of possible slipperiness: if effects consistent with the hypothesis are observed, then one could say that our minds attend to status cues accurately; if effects opposite the predicted direction are observed, one could say that features of contemporary life confuse our evolved mechanisms and cause them to behave maladaptively; and if no effects are observed, one can say that the disparity between past and present environments renders status-dependent mechanisms inoperative.

PARENT-OFFSPRING CONFLICT

Consider that children have a coefficient of relatedness of 1 to themselves and only .5 to any sibling (excepting identical twins). This implies that children should put their own genetic interests ahead of any one of their siblings, while parents' genetic interests are better

[9] Factors internal to actors that were once correlated with the achievement of rank among our ancestors may not be correlated with the attainment of socioeconomic status today. For example, very high levels of testosterone in men are associated with stronger dominance tendencies but also *lower* socioeconomic attainment. Dabbs (1992: 813) describes it as an "irony of androgens that testosterone, which evolved in support of a primitive kind of status, now conflicts with the achievement of occupational status." A status-contingent mechanism that took such internal factors as inputs could therefore produce behavior exactly the opposite of what we would predict in tests that used income or wealth as measures of status.

served by treating each of their children more equally. Consequently, children should want to commandeer a greater share of parental investment than parents are willing to give them. Parents are also more closely related to their own siblings, nieces, and cousins than their children are, so parent-offspring conflict extends also to investments outside the child's sibship. The individual members of a litter of kittens or puppies or piglets should be more concerned with getting enough milk for themselves than with making sure their siblings have enough, while the mother should be more interested in achieving an equal distribution among siblings. Indeed, the weaning process for many animals is a struggle in which offspring want to suckle more and longer than their mother is willing to let them. (It's in the mother's interests to cease feeding when the offspring has become sufficiently independent because she can then move to have another brood.) In sheep, for example, a mother will at first produce more than enough milk for a lamb, but later the mother will produce decreasing amounts of milk and will increasingly prevent her lamb's suckling attempts (Trivers 1985). Trivers used the phrase "parent-offspring conflict" to describe the divergence in the genetic interests of parents and children.

For humans, we have reason to think that parent-offspring conflict may begin in the womb. Although we might think of gestation as being the primordial cooperative project between mother and child, Haig (1993) presents evidence suggesting a prenatal tug-of-war in which the fetus works to extract more maternal resources than the mother's body wishes to give. Among other things, fetal cells embed themselves in artery walls and restrict the mother's ability to control the flow of blood to the placenta (to the fetus's advantage), and the placenta also secretes a hormone that tries to siphon off a larger ration of the mother's blood sugar (but, as happens so often in the "arms races" of evolution, this attempt is counteracted by the mother's body increasing its production of insulin). Obtaining extra resources can be to the fetus's advantage even if it slightly increases the mother's risk of dying in childbirth or of damaging her capacity to have more children.

What about after the child is born? Undoubtedly, there exists no shortage of conflicts between parents and children, especially between parents and adolescents. Moreover, in many of these conflicts, parents and children may appear to act at cross-purposes with one another, and in such a way that both parties can be characterized as behaving partly (or completely) selfishly. Does this mean that these conflicts can be usefully understood as the result of adaptations that arose from the selection pressures implied by children being twice as related to themselves as to their siblings? What about those instances when children seem to go along with their parents' wishes without any conflict—can the genetics of parent-offspring conflict illuminate these too? As just a few examples will demonstrate, the dynamic of "parent-offspring conflict" has inspired very broad interpretation and application. In response to which, we can ask: how much explanatory power does the genetics of parent-offspring conflict actually have for understanding patterns of interaction between parents and children in the postindustrial societies of the present? To what extent do these applications elaborate our understanding of behavior, as opposed to simply suggesting the potential totality of a Darwinian standpoint on social life?

Trivers's original paper

In Trivers's (1974) original paper on parent-offspring conflict, he provides a basic "rule of thumb" for how we might expect his theory to play out in childhood socialization. He writes:

> Parents are expected to socialize their offspring to act more altruistically and less egoistically than the offspring would naturally act, and the offspring are expected to resist such socialization. If this argument is valid, then it is clearly a mistake to view socialization in humans (or in any sexually reproducing species) as only or primarily a process of "enculturation," a process by which parents teach offspring their culture (Trivers 1974: 250).

In sum, to the extent that parents are capable of manipulating the behavior of their children, they would try to get them to behave more altruistically toward their siblings and other relatives than they would otherwise be disposed to do. Offspring may then have an evolved motivation to resist attempts at manipulation and to behave more egoistically. Consequently, we should not see socialization as being a totally innocent process of parents' attempting to mold or teach behaviors exclusively in the child's best (evolutionary) interests. Instead, part of socialization involves parental attempts to manipulate children into serving their own interests rather than the child's. Opening up to scrutiny the motives of some parental behaviors seems substantially better than a Pollyanna-ish view that parents are simply teachers or that they always looking solely after the best interests of the child.

Even so, one can quibble about specific details. For one thing, the instances in which parent and offspring genetic interests conflict probably comprise a relatively limited domain of activity compared to those where parent and offspring interests wholly coincide—that is, in helping the offspring to survive, gain resources, and reproduce—so we should not overestimate the role of parent-offspring conflict in shaping parental goals during socialization.[10] In many (most?) real situations, we would expect that what is in the child's genetic best interest is also in the parents'. In addition, the expectation that parents should want children to behave "more altruistically" than is in their genetic interests applies

[10] On a related front, Kitcher (1985) raises some additional objections that pertain to the question of how an offspring is supposed to discern which parental attempts at manipulation are in her/his best interests and which are not.

straightforwardly only to interactions with kin. Unlike altruism toward siblings or other kin, altruistic acts directed toward non-kin do not confer any greater benefit to parents than to children. As a result, the theory of parent-offspring conflict does not imply any evolutionary incentive for parents to try to get their kids to engage in more non-kin altruism than the children are naturally disposed to provide.

The larger problem, however, is that Trivers goes much further in his suggestions about the potential explanatory power of parent-offspring conflict. He writes that "Parent-offspring conflict may extend to behavior that is not on the surface either altruistic or selfish but which has consequences that can be so classified" (Trivers 1974: 251). After raising this possibility that the manifestations of parent-offspring conflict may be more extensive and subtle than they initially appear, he continues as follows:

> The amount of energy a child consumes during the day, and the way in which the child consumes this energy, are not matters of indifference to the parent when the parent is supplying that energy, and when the way in which the child consumes the energy affects its ability to act altruistically in the future. For example, when parent and child disagree over when the child should go to sleep, one expects in general the parent to favor early bedtime, since the parent anticipates that this will decrease the offspring's demands on parental resources the following day. Likewise, one expects the parent to favor serious and useful expenditures of energy by the child (such as tending the family chickens, or studying) over frivolous and unnecessary expenditures (such as playing cards)—the former are altruistic in themselves, or they prepare the offspring for future altruism. In short, we expect the offspring to perceive some behavior, that the parent favors, as being dull, unpleasant, moral, or any combination of these. One must at least entertain the assumption that the child would find such behavior more enjoyable if in fact the behavior maximized the offspring's inclusive fitness. (Trivers 1974: 251)

The passage trades on some easily evoked images of behavior, but it also invites immediate skepticism. Although we can all conjure up images of a child being fussy-because-she's-tired, should we accept as a general rule that well-rested children are less of a drain on parental resources than poorly-rested ones? Moreover, we can ask if the child's genetic interests are really better served by staying up late rather than going to bed early? Are parents really trying to get children to sleep more than is good for them, and children are protecting their own interests by resisting?

As a different matter, how does studying better prepare the child for future altruism? We might think that the main evolutionary benefit from the child's studying is in advantages afforded by educational achievement for survival, status attainment, and mating, but these serve the child's interests as well as the parents, so they cannot provide any selection pressure favoring resistance. Also, how is the child promoting his own interests with the "frivolous and unnecessary" expenditure of card-playing as opposed to studying?

At any rate, we seem to have moved quite afield from the genetic fact that parents are equally related to all of their children but children are less closely related to siblings than to themselves. We are also far from the initial implication that parents should encourage more kin altruism than children wish to perform. Moreover, phenomena are reshaped to fit the theory. The phenomenon here is that parents are regularly trying to get kids to do things kids find dull, unpleasant, and moral, but which are not obviously altruistic (i.e., studying). But, to make this phenomenon fit into the framework of parent-offspring conflict, we must accept that these activities are somehow laying the groundwork for future altruism. We now have to endow parents and children with the evolved capacities not only to recognize what actions are immediately altruistic but also to recognize those which prepare for future altruism, and we have to posit that parents evolved mechanisms that encourage the promotion of the preparatory activities while children have evolved the propensity to resist them. This seemingly requires that we adopt simultaneously adopt a stance *of both strong and weak adaptationism*: strong in that we have to buy the assumption that all the necessary mutations responsible for the abovementioned adaptations arose and had sufficient time to be selected to near-fixation, yet weak in that we have to buy also that a seemingly easier design solution did not arise as an evolutionary possibility—namely, the evolutionary elimination of whatever psychological link underlies the supposed connection between dull activities now and kin altruism later. If children's future kin altruism could not be manipulated by the performance of seemingly unrelated activities in their youth, then parental strategies that involved expending resources trying to get them to do these activities would be selected against rather than selected for.

Another matter to consider is whether the intergenerational tensions that Trivers attempts to explain as a consequence of the genetics of parent-offspring conflict are *specific* to parent-offspring interaction or are a more *general* feature in adult-child interactions. Societies like ours are full of non-kin, adult socializing agents: teachers, Girl Scout leaders, little league coaches, fictive uncles and aunts, celebrities who do public service announcements. The value that these agents attempt to encourage in children are broadly compatible with those of the parents themselves, so much so that parents and agents see themselves as partners in the larger project of making better moral citizens of children. Children, for their part, are not necessarily more or less susceptible to attempts to influence that come from adults who are not their parents. These broader patterns of adult-child interaction are less obviously explicable using the dynamics of parent-offspring conflict, although of course other evolutionary explanations can be devised. A related matter is the extent to which the moral messages from parents or other socializing agents pertain to encouraging altruism. Other moral themes, such as the value of delaying gratification to receive future rewards, follow less easily from the theory of parent-offspring conflict but seem also to meet resistance from children.

How Parent-Offspring Conflict Works

Taken together, these problems would seem to make the application of parent-offspring conflict to contemporary family life a more complicated matter than Trivers apprehended. We might alternatively propose that to whatever extent an evolutionary perspective is to contribute to understanding disputes between parents and children over bedtimes, studying, and chicken-tending, the explanation must be less direct and more nuanced than the simple invocation of parent-offspring conflict that Trivers suggests. Since we are talking about a paper that is over 25 years old, it might seem like I am trying to make a lot of hay out of an exuberant first formulation of an idea, rather than how the concept is current used by evolutionary psychologists. (Moreover, a 1985 book by Philip Kitcher provides a more extensive critique than I have of Trivers's application of parent-offspring conflict to the dramas of human adolescence.)

So let's fast forward to some more recent endeavors. In *How the Mind Works*, Steven Pinker (1997) discusses the value of Trivers's concept of parent-offspring conflict for understanding the development of personality. Once more, parent-offspring conflict would seem to be taken to predict that parents would try to shape children's personalities in ways that serve the parents' genetic interests but not the child's. Pinker also attributes to the theory a prediction about how the socialization efforts of parents pan out:

> Trivers reasoned that, according to the theory of parent-offspring conflict, parents should *not* necessarily have their children's interests at heart when they try to socialize them. … [P]arents may try to persuade children that staying home to help at the nest, allowing themselves to be sold in marriage,

> and other outcomes that are good for the parent (and hence the child's unborn siblings) are in fact good for the child. As in all arenas of conflict, parents may resort to deception and, since children are no fools, self-deception. So even if children acquiesce to a parent's rewards, punishments, examples, and exhortations for the time being because they are smaller and have no choice, they should not, according to the theory, allow their personalities to be shaped by these tactics.
>
> Trivers went out on a limb with that prediction. The idea that parents shape their children is so ingrained that most people don't even realize it is a testable hypothesis and not a self-evident truth. The hypothesis has now been tested, and the outcome is one of the most surprising in the history of psychology." (Pinker 1997: 447-8)

Pinker here apparently credits Trivers with having predicted years in advance one of the most surprising findings in the history of psychology. For those who complain that evolutionary approaches don't produce anticipated predictions, what more could you ask?

The finding here is what Judith Rich Harris (1998) presents in her book *The Nurture Assumption*: evidence mainly from behavior genetics that is taken to suggest that parents have no (or virtually no) influence on shaping the personalities of their children. Instead, to the extent that personality traits are not explained by genetic variation, Harris sees them shaped almost entirely by childhood interactions with peers.[11] Important for our purposes here, the strong influence of peers is matched by a relative imperviousness to whatever shaping efforts parents make. Instead, the best parents seem to be able do is choose good neighborhoods, move if their child falls in a bad crowd or become a target of peer abuse, and try to facilitate the child's success in peer groups.

But did Trivers really go "out on a limb" and predict that children's personalities would not be shaped by parents' socializing efforts? In his original 1974 paper, Trivers writes that "socialization is a process by which parents attempt to mold each offspring in

[11] While Pinker's account emphasizes Harris's suggestions about the socializing role of competition among peers—which might seem to put the focus on dynamics that cause peers to differ from one another—the greater part of her exposition concerns children conforming to group norms, emulating high-status peers, and adopting the beliefs, attitudes, and aspirations of their peers.

order to increase their own inclusive fitness, while each offspring is selected to resist *some* of the molding..." (p. 260, emphasis added). In his 1985 book *Social Evolution*, Trivers argues that "so far as we know, personality and conscience are formed early in socialization, probably during the first five years of a child's life. We expect the child to develop during this time internal representations of its parents' viewpoints as well as its own" (p. 163). Trivers thus portrays children as acceding partly to parental influence, even while calling attention to an antinomy between parents' socializing efforts and children's resistance.

Trivers does present adolescence as a time when children "reorganize their personalities in such a way as to reflect their own self-interest more exactly" (Trivers 1985: 164). Yet, it is even here difficult to read Trivers as anywhere actually predicting what Harris claims is the case—that parents have virtually no influence on the personalities of their children, including in childhood whenever the children are outside of the home. Moreoever, others who have written about parent-offspring conflict, like Robert Wright in *The Moral Animal* (1994: 168-69), take it for granted that parents have a strong socializing influence over their children, and *they give Trivers credit for explaining this* in his parent-offspring article. In other words, no matter how research on the influence of parents on children turns out, popularizers of evolutionary psychology can give Trivers credit for having predicted it all along. Such *post hoc* crediting may have rhetorical value for selling evolutionary psychology, but it does not jibe well with what Trivers actually wrote.

When we set the question of credit-for-clairvoyance aside, there is also reason to doubt Pinker's assertion that Harris's empirical claims are consistent with the theory of parent-offspring conflict. Once we move beyond the suggestiveness of the phrase "parent-offspring conflict," it is hard to see the connection between parents' having little influence on children's personality traits and a genetic dynamic that would spur offspring to resist parents' efforts to get them to be more altruistic to their siblings. If we take it as likely that parents' and children's genetic interests coincide in practical situations considerably more than they diverge, then it would suggest that many of parents' socializing efforts were (evolutionarily) genuine attempts to increase their offspring's fitness. For that matter, why should parent-offspring conflict lead us to expect children to be more open to influence from one's peers (with whom they share *no* common genetic interests) than one's parents (with whom genetic interests are only in partial conflict)?[12]

Additionally, Harris's conclusions about the minimal influence of parents are far from accepted wisdom within psychology. The *Newsweek* cover story on Harris's book quotes prominent developmental or educational psychologists weighing in with judgments like "She's all wrong" (Frank Farley), "I'm embarrassed for psychology" (Jerome Kagan), and "By taking such an extreme position, Harris does a tremendous disservice" (Wendy Williams) (Begley 1998). Kagan has asserted that only a selective reading of the evidence could lead to the conclusion that parents tend to have no important long-term effects on their children (see, e.g., his debate with Harris in *Slate* magazine). In other words, what Pinker provides his readers is an erroneous history of a dubious evolutionary explanation for a "finding" that may well be exaggerated. Regardless of how ongoing debates about the extent of parental influence are resolved, we should be skeptical of how closely any finding follows from the theory of parent-offspring conflict and skeptical that Trivers deserves credit for predicting it.

[12] To be fair to Harris, her own evolutionary explanation is broader than Pinker suggests and draws on more dynamics than just parent-offspring conflict (Harris 1998: 118-122). (I think Harris makes a good case for why we wouldn't expect evolution to produce children who are completely programmable by their parents, but it's far less obvious to me how any of her arguments would lead us to expect the very negligible influence of parents on kids that Harris claims is empirically the case.)

What's Freud Got To Do With It?

> *"Had Freud better understood Darwin, for example, we might all have been spared the confusion engendered by his bizarre misconstrual of parent-offspring conflict as an instance of sexual rivalry."*
>
> —Martin Daly and Margo Wilson, review essay, 1998[13]

In his own writings, Trivers (1974, 1985) made a number of efforts to use parent-offspring conflict to explain various of Freud's defense mechanisms. In her review of developmental psychology for *The Handbook of Evolutionary Psychology*, Michele Surbey favorably recounts Slavin's (1985) efforts—following closely on Trivers—to give Darwinian footing to Freud's theories of repression:

> Maintaining long-term bonds with kin, such as those that develop between child and parent, may necessitate the suppression of self-interests of one or the other party for prescribed periods of time. This way a child's self-interest can be submerged during the period of dependence, yet remain intact and accessible for guiding behavior in later stages of life when parental investment is no longer crucial to offspring fitness. This is why Slavin [1985] suggested that failures of repression occur during adolescence (the initiation of reproductive life), and it may also be why repressed childhood experiences are often revealed in adulthood, when we would expect the mechanisms of repression to let up somewhat.
>
> ....[R]epression acts as a psychological arbitrator maintaining a tenuous equilibrium between the fitness interests of parent and offspring during an extended childhood. According to Slavin [1985], childhood wishes and motivations that are acceptable to the parent and maintain family cohesiveness remain in a child's consciousness, whereas those that are not congruent with parental views are repressed and exist primarily at the level of the unconscious. At adolescence, the individual's self-interest begins to emerge again as it gains independence and, as a sexually mature organism, is able to pursue its own fitness interests through the production of its own offspring. (Surbey 1998: 388).

According to this reasoning, children are thought to repress manifestations of their genetic self-interest because they need to do so to remain in the good graces of the parent on whom

[13] "The Evolutionary Social Psychology of Family Violence," p. 431

they are dependent, but then, when the child grows into adolescence, children have a diminishing need to repress things and are increasingly able to assert their own genetic interests more strongly. One problem with the theory is that we have little reason to think that children engage in some remarkable suppression of selfishness when they are young. To make sense in the context of parent-offspring conflict, Slavin's theory would entail that children are more altruistic when they are young (as they repress their own interests to conform better to their parents' will) and become less so as they get older. Such a prediction is seemingly contradicted by both casual observation and the literature on the development of altruistic behaviors (see Schroeder, Penner, Dovidio, Piliavin 1995: 126-156).

The view presented here about the emerging independence of adolescence also deserves scrutiny. Surbey presents much the same argument again later in her essay (this time, in the context of discussing Kevin MacDonald's (1988) work on evolution and identity formation):

> The redefinition of one's identity, as separate from that of one's parents, represents the undoing of the process of identification during childhood, and is the psychological equivalent of achieving fitness independence. At adolescence, an individual's fitness no longer depends on the investment and fitness of a parent, and the adaptive value of psychological identification with a parent wanes. The search for and achievement of an individual identity during adolescence may signify the end of the last major manifestation of parent-offspring conflict (Surbey 1998: 394).

The emotional movement of children away from their parents is taken as a "manifestation" of parent-offspring conflict. Earlier, we saw that parent-offspring conflict was often discussed by reference to weaning, where the offspring tries to prolong the period of intensive investment and the parent tries to terminate it. Juxtapose the examples, and we can see that having parent-offspring conflict in your theoretical toolkit lets you have it both ways:

if parental resources are solicited by children but denied by parents, then this can be taken as an example of children trying to get more from their parents than parents are willing to give, but if parents offer resources but are refused by children, then this can be taken as children asserting their independence from attempts to manipulate by parents.

It is worth repeating that all of this explanatory mileage is gained from a dynamic that, in other hands, might seem to imply little more than that parents have a greater interest in a child's sibling's welfare than the child does. In the examples that I've cited, parent-offspring conflict can account for all of the stereotypical tensions between parent and child (e.g., going to bed vs. playing cards), the complete rejection of parental influence by children, and the sublimation of the child's interests to the parent's will. Given such malleability, one may question how much parent-offspring conflict, without some additional disciplining apparatus, deserves credit for predicting anything at all about human behavior beyond the existence of selfish tensions between parents and children.

PARENTAL INVESTMENT AND SOCIOECONOMIC ATTAINMENT

> *"One of the issues that is difficult to explain to people who are unfamiliar with evolutionary psychology is that it is not a monolithic set of hypotheses that yields one invariant prediction about each phenomenon. I am sometimes asked, for example, 'What is the evolutionary explanation for homosexuality?' or 'What is the evolutionary explanation of female orgasm?' One characteristic of a healthy science is that, on the cutting edge, there are competing hypotheses that vie for attention."*
>
> –David Buss (1995:81)

As the above quote from Buss suggests, evolutionary psychology is a more diverse undertaking than is commonly recognized. Alternative evolutionary psychological accounts exist for and within the same domain of phenomena, and, in these instances, evolutionary psychologists are like other natural or social scientists in that they seek empirical means of adjudicating among competing Darwinian hypotheses. Consequently, a potential problem whenever a single set of predictions is attributed to the "evolutionary psychological" perspective is that, very possibly, alternative predictions could also be derived with no less parsimony from evolutionary psychological principles. The potential for misuse here cuts both ways. On the one hand, scholars hostile to evolutionary approaches can claim that the perspective should be rejected on the basis of falsified predictions, when alternative Darwinian explanations consistent with the observed data are available or easily devised. On the other hand, scholars more favorable to the evolutionary psychology can suggest that the perspective should be favored over non-Darwinian alternatives because its predictions are more consistent with observed data, even though other, less consistent predictions could have been derived instead.

The problem was illustrated by an article in the *American Journal of Sociology* by Timothy Biblarz and Adrian Raftery (hereafter BR). BR use four large, nationally representative datasets to look at the relationship between family structure—that is, whether the respondent was raised mainly in a home with both biological parents, just one parent, or a biological parent and stepparent—and educational and occupational attainment. They found that children raised by both biological parents or by single mothers tend to do better than children raised by single fathers or in stepfamilies, controlling for other factors. They found also that these differences remained constant over the 30-year period spanned by their data.

BR compared these results with predictions that they had derived from evolutionary psychology and various other explanatory idioms more common within sociology (socialization/learning theory; control theory; economic theory; an explanation asserting selection bias; and an explanation emphasizing exposure to marital conflict). They concluded that "[a]mong six candidate theoretical frameworks, the findings are most consistent with an evolutionary view of parental investment" (1999: 323), As such, the paper appears to provide an empirical victory for evolutionary psychology over more conventional sociological alternatives; the victory is made more impressive by the study's use of a classic hypothesis testing framework, well-known social science datasets, and sophisticated methods of analysis.

BR's study stands as an important contribution to our understanding of the empirical character of family structure effects on attainment. Nevertheless, I have strong reservations about the theoretical conclusion that they draw. I do not dispute their data analysis, but I do dispute the four predictions that they attribute to evolutionary psychology. Below, I show that these predictions do not follow unambiguously from evolutionary psychology and that alternative predictions can be just as easily generated. As a result, their results should not be taken as comprising a real test of the evolutionary psychological perspective versus more conventional social science perspectives on attainment. Instead, as I discuss further in Chapter 8, the issues separating the two paradigms cannot be resolved by BR's data or analyses.

Re-Examining The Predictions Of Biblarz And Raftery

In characterizing the evolutionary psychological perspective, BR offer three cross-sectional (in their words, "static") predictions about the effect of family structure on outcomes, as well as one "change" prediction about how the magnitude of family structure effects have changed over the 30 years they examine. Although BR employ a different method of presentation, these predictions can be equivalently expressed as follows:

1. (Static) Children raised in two-biological-parent families will have higher attainment than children raised in any alternative family structure.

2. (Static) Children raised in a single-mother family will have higher attainment than children raised in a single-father family.

3. (Static) Children raised by a single parent of a given sex will do better than children raised by a parent of that sex and a stepparent. (Because of insufficient data on father-headed alternative families, BR are only able to test the prediction that children from single-mother families will outperform children from mother-stepfather families.)

4. (Change) The observed family structure effects on attainment will be constant over the 30 year period examined.

The other five frameworks they examine make (at least some) different predictions. When comparing these frameworks with their results, BR (1999: 356) conclude that "[e]volutionary parental investment theory was the only one where static and change predictions were both borne out by the data." Let us now look closer at each of the predictions that BR attribute to evolutionary psychology:

1. Children raised in two-biological-parent families will have higher attainment than children raised in any alternative family structure. As BR discuss, the tendency for children from two-biological-parents to outperform those from alternative family structures (taken together) had been well-documented prior to their study. We should perhaps then not be surprised that five of their six candidate frameworks predict this known pattern, and the one that does not—their rendition of the economic perspective—fails soundly on its other predictions as well. In other words, this prediction proves ultimately unimportant for differentiating among the theoretical alternatives BR present; perhaps ironically, it is also the least contestable of the predictions that BR attribute to evolutionary psychology.

Even so, their specific justification of this prediction for evolutionary psychology can be questioned. They argue that evolutionary psychology "would predict that children from two-biological-parent families will have an advantage over those from other kinds of families. The father's average resource contribution will be less than the mother's, but not by much, because humans have high male parental investment, and so children will benefit from the presence of the biological father"(Biblarz and Raftery 1999: 326). The claim that humans have high male parental investment is a statement about humans *relative to other species*. In more than 95% of mammalian species, including most primates, males provide *no* (or virtually no) direct care for an offspring after it is born (Clutton-Brock 1991: 132). Thus, the inference that human fathers' parental investment in humans will differ "not by much" from mothers' simply does not follow: even if males only invested 10% of the effort that females did in children after they were born, we would still be a species with relatively high levels of male parental investment.

2. Children raised in a single-mother family will have higher attainment than children raised in a single-father family. BR (1999: 325-26) ascribe this position to evolutionary psychology because, they contend, "the evolutionary perspective on the family gives more weight to the role of the mother than that of the father in determining children's fates...."[14] BR then devote a paragraph to explaining the evolutionary rationale for predicting that mothers will invest more of themselves in their offspring than fathers. Empirically, as is well

[14] BR's reference to "determining children's fates" is slippery; in evolutionary biology, "fate" is almost always considered in terms of reproductive success, not the idiosyncratically human "fates" of educational and socioeconomic attainment. As I discussed earlier in this chapter, in trying to generalize biological theories to humans, Darwinian social scientists sometimes try to draw theoretical parallels between an animal's health or dominance rank and human socioeconomic status.

known, the asymmetry in parental investment between mothers and fathers holds strongly across all human cultures (and, for that matter, among all mammals) (e.g., Rossi 1984). For humans, of course, many explanations of this asymmetry have been offered that do not give center stage to evolved sex differences in biological dispositions (e.g., Chodorow 1978). (At the same time, many social scientists would grant that there may likely be something biological about the greater investments of mothers than fathers, and that the evolutionary account may be a plausible explanation of why this is so.)

In any event, does it actually follow from evolutionary psychological tenets that if mothers have an innate tendency to invest more in children than fathers, then children from single-mother families will attain higher average positions in societies than single-father families? Not necessarily. Evolutionary psychologists readily acknowledge that across all human societies, men control a vastly greater share of economic, political, and cultural power than women; indeed, hypothesizing about the supposed origins of patriarchy is a lively topic in the field (Smuts 1995; Hrdy 1997; Miller 1998; Wright 1994; see also Goldberg 1973, 1993). Presumably, then, an evolutionary psychologist would grant that a plausible consequence of a patriarchal social organization is that, *ceteris paribus*, a unit of paternal investment will have a greater positive effect on attainment than a unit of maternal investment. If the difference between male and female social efficacy is large, then a moderately-invested single-father may still be able to do more to advance his children's futures than a maximally-invested single mother.

In other words, we have a tradeoff between superior male societal power and superior female parental investment—phenomena both given extensive consideration by evolutionary psychologists. Whether one predicts that children of single mothers or children of single fathers will have higher attainment does not directly follow from an evolutionary psychological perspective, but instead requires ancillary assumptions about the relative weight each side of the tradeoff should be given for determining attainment in a given society. Put another way, had BR done their analyses across a range of societies and found the opposite result (that single-father families outperform single-mother families) in some, most, or all of them, *this would in no way disconfirm an evolutionary psychological view of investment and attainment*. Instead, disconfirmation would require at least a demonstration that the results cannot be explained by the unequal distribution of power between the sexes.

Given that BR subtitle their paper "Rethinking the 'Pathology of Matriarchy'" (that is, rethinking the idea that single-mothers pose a substantial detriment to their children's attainment), it is worth noting that some well-known work of Darwinian psychology does not share their enthusiasm for single motherhood (at least in the environments of our evolutionary past). Evolutionary psychological work on neonaticide/infanticide has emphasized the contributory effects of father absence, with the argument that a new mother's having to raise the infants without a father's help may have strongly contributed to create circumstances in which the infant's chances of survival and reproductive success were so low that killing it and saving resources for other opportunities was favored by selection (Daly and Wilson 1988: 63-64). Darwinian work on sexual development has theorized that the putatively strongly detrimental effects of father absence in our ancestral past may have led to the evolution of a tendency for children raised in fatherless homes to engage in more quantity-based mating strategies (marked by earlier age at first intercourse, greater promiscuity, and higher propensity for divorce) than those who grow up in two-parent homes (Draper and Harpending 1982; Belsky, Steinberg, and Draper 1991).

3. Children raised by a single parent of a given sex will do better than children raised by a parent of that sex and a stepparent. In presenting evolutionary psychology's predictions, BR write: "Children from single mother families will also have advantages over those from stepfather/biological-mother families. The stepparent's concern with his own reproductive fitness is in competition with the stepchildren for the mother's resources, increasing the risk of abuse to children in families with a stepparent" (Biblarz and Raftery 1999: 326). They cite the well-known research of Daly and Wilson (1996; see also Daly and Wilson 1988, 1999). What Daly and Wilson have demonstrated beyond rational dispute is that children with a stepparent are several times more likely to be victims of child abuse and child homicide than are children raised by a single biological parent or both biological parents. They also report that "step-relationships are, on average, less investing, more distant, more conflictual, and less satisfying that the corresponding genetic parent-child relationships" (Daly and Wilson 1996: 79).

Daly and Wilson can thus perhaps be read uncontroversially as predicting that children in stepfamilies will do worse than those in two-biological-parent families, but predicting that *being raised by a stepparent is worse than having no second parent at all* is an entirely different matter. This requires that the net contribution of the stepparent to the child's socioeconomic attainment is *negative*. But Daly and Wilson do not claim that *most* stepchildren suffer abuse at the hands of a stepparent. Moreover, they acknowledge that many stepparents provide *positive* investments in their stepchildren, and they see such investments as entirely consistent with their evolutionary psychological contentions. They argue that step-marriage should be considered partly in terms of the mother procuring investment for the would-be stepchild as part of the terms of the marriage. In the same paper BR cite, Daly and Wilson (1996: 80, emphases added) write:

> Stepparents assume their obligations in the context of a web of reciprocities with the genetic parent, who is likely to recognize more or less explicitly that stepparental tolerance and investment constitute benefits bestowed on the genetic parent and child, entitling the stepparent to reciprocal considerations.
>
> In this light, *the existence of stepparental investment is not so surprising*. But the fact of such investment cannot be taken to imply that stepparents ordinarily (or indeed ever) come to feel the sort of commitment commonly felt by genetic parents. *Evolutionary thinking suggests that stepparental affection will tend to be restrained....*

The claim here is not that stepparents are, on average, bad for children, but rather that their investment will be "restrained" in comparison to biological parents. One could therefore use the above paragraph to argue that an evolutionary psychological perspective predicts that even though children from mother-stepfather families will attain less than children from two-biological-parent families, they will attain *more* than children from single-parent families. That BR derive the opposite prediction is based on their ancillary assumption that the average stepfather does/causes more harm than good, a conclusion that necessarily follows neither from Daly and Wilson's work specifically nor an evolutionary psychological perspective more generally.

4. The observed family structure effects on attainment will be constant over the period examined. BR's data span a period in which there was a near reversal of the ratio of alternative family structures resulting from the death of a parent to those resulting from divorce. Comparing the 1962 and 1992-4 samples, BR estimate that the percentage of alternative families that were the result of parental death decline from 68% to 33%, while the percentage of alternative families that were the result of divorce rose from 28% to 62%.

BR assert that this demographic transition implies different predictions for the different candidate frameworks about how the magnitude of family structure effects have changed over time. They claim that the evolutionary psychological perspective predicts no change, and they justify this contention as follows:

> From the evolutionary perspective, divorced and widowed single mothers have the same level of their own fitness tied up in the children, and so both types of mothers would have the same level of impetus to invest highly in their children. The presence of a nonbiological parent would negatively impact children, regardless of whether the biological father had died or the parents had divorced. The change in cause structure over time should not alter the implications for children of basic family forms. (Biblarz and Raftery 1999:330)

From an evolutionary standpoint, a mother's incentives to invest in her offspring are not affected by whether her marriage ends in divorce or death. However, BR's argument fails to recognize both (a) that the evolutionary incentives for the non-cohabitating biological father to invest in his offspring are similarly unaffected and (b) that the father's capacity to invest is certainly affected by *whether he is estranged or dead*. An evolutionary view would predict that a living, estranged father would still have concern for his child's well-being and may serve as a useful source of investments, of various sorts, over the child's development. BR's failure to acknowledge this is perhaps particularly striking given their speculations about the role of families in helping their children obtain "favoritism in hiring" or other "special favors" in translating educational achievement into socioeconomic success (p. 357). Tapping informal networks to help a child may be precisely the sort of help that (living) non-cohabitating fathers can perhaps provide with nearly equal facility as cohabitating ones.

As a result, we would expect that as the ratio of divorced mothers to widowed mothers increases, the negative effects of alternative family structures would not remain constant but *decrease* over time. BR's prediction of no change requires that we presume that the effect of having an estranged father is the same as the effect of having a dead one; this prediction does not obviously follow from evolutionary psychological premises, and it also sits uneasily with their earlier claims regarding human male's high parental investment.

The Problem of Promiscuous Predictions

Evolutionary psychology is regularly criticized for its explanatory malleability. To some, every pattern of behavior seems as though it can be given a Darwinian explanation, regardless of whether it actually exists empirically. We have just seen that the specific predictions that BR attribute to evolutionary psychology are only one of several sets that could have been as easily derived. This multiplicity of predictions does not necessarily point to some fundamental flaw in evolutionary approaches to social behavior. Indeed, as the quotation from Buss at the beginning of this section attests, the presence of competing testable predictions within a theoretical perspective may well be a sign of its vigor and health. In any event, evolutionary psychology is by no means unusual as a theoretical perspective in being able to generate multiple predictions, and the problems of hypothesis testing among flexible theoretical perspectives stretches well beyond both evolutionary psychology and conventional sociology. For example, Laibson and Zeckhauser (1998: 26) complain that "the promiscuous prediction problem... plagues mainstream economics. Both behavioral [economics] models and standard economics models are often so flexible that almost any outcome can be explained by them."

This said, the importance of generating genuinely *testable* hypotheses must be emphasized. Critics of evolutionary approaches often express frustration at the extent to which Darwinian explanations often differ not in their—often *post hoc*—empirical characterizations of present-day behavior but rather in their conjectures about the lives of our Pleistocene ancestors, which are of course far less amenable to real testing. When evolutionary psychology offers specific and genuinely testable hypotheses about social phenomena, these may be empirically engaged and either confirmed or disconfirmed. Conversely, pitting more monolithic characterizations of the evolutionary psychological perspective against sociocultural- or economics-based alternatives will likely be less fruitful.

CONCLUSION

This chapter began with a discussion of the Trivers-Willard hypothesis. Regarding its application to human parental behavior, I suggested that the persistence of this hypothesis within evolutionary social science has occurred despite the weakness of the evidence for it, the strength of the evidence against it, and the logical difficulties entailed in its application to contemporary societies. I proposed that not only does the available empirical evidence not support the Trivers-Willard hypothesis in contemporary populations, but a careful consideration of the reasoning of the hypothesis might lead us to wonder whether we should really expect to observe it for human parental investment as a function of socioeconomic status. From this, I moved to a consideration of "parent-offspring conflict" and its application to humans. I argued that the concept has been stretched well beyond its scope and that it can be invoked in seemingly contradictory ways. Finally, I moved to an examination of a recent attempt to apply evolutionary theories about parental investment to the longstanding sociological problem of how and why family structure affects socioeconomic attainment. I demonstrated that the predictions derived by Biblarz and Raftery happened to fit the data but by no means were the only or even the most defensible set of predictions one could have derived from an evolutionary perspective. Ironically, a possibly better derivation of predictions would have yielded results less consistent with an evolutionary perspective, meaning that the apparent fit of between theory and data may be as much a function of the contestable theoretical interpretation of the authors than any real consonance.

In all three instances, we have theoretical ideas that have been pressed too far. "Parent-offspring conflict" can be casually invoked to explain any instance of conflict between parents and children. The "Trivers-Willard hypothesis" is a clever idea that happens to be not well supported by actual data on humans, especially in contemporary, developed societies, and yet it might be cited whenever impoverished parents are observed to favor daughters or wealthy parents are observed to favor sons (e.g., Dickemann 1979; Cronk 1991). Biblarz and Raftery attempt to give greater specificity to the predictions of evolutionary psychology than they actually imply. The result in all cases may be "confirmations" of evolutionary theories that are mainly confirmations in the eye of the beholder and that, in imposing the Darwinian template, may distort the empirical character of the phenomenon under consideration. Critics have long suggested that it is the very power of the adaptationist perspective that may be its greatest failing, as this power leads to an inadequate restrictiveness on what are proposed to be adaptations (e.g., Lewontin 1979, 1984).

If we wanted to look beyond family dynamics, we could have seen other examples of Trivers's concepts being far too easily applied. As I suggested in Chapter 3, Cosmides and Tooby may have been misguided in their use of "reciprocal altruism" as a starting point to explain the putative existence of the innate "cheater-detector" mechanism. The idea that “self-deception enhances the deception of others” stands ready for any Darwinian social scientist who must explain why the “evolutionary” motive for a given behavior is inconsistent with the act’s apparent motive (see MacDonald 1998a, 1998b). Additionally, of course, ideas other than Trivers’s are also easily misapplied. Various specious uses of Zahavi’s (1975, 1977) handicap principle spring to mind, such as Diamond’s (1992: 192-204) invocation of it to explain “why do we smoke, drink, and use dangerous drugs” (a phenomenon that should seem sufficiently broad to elude any unitary explanation—Darwinian or otherwise).

One can protest that I am calling attention to a problem that is in no way confined to evolutionary approaches to social behavior. All theoretical programs in social science have concepts that some practitioners will stretch beyond their appropriate domain of usage; if anything, evolutionary programs may have an advantage in this regard because the grounding of conceptions like “parent-offspring conflict” in a specific aspect of selection allows one to better judge when the concept is being used improperly. Many social science programs have ideas that were proposed early on and that persist despite of a dearth of any good evidence for them. And certainly, many social science theories can generate multiple predictions when one attempts to apply them to particular phenomena (it is the ability to proliferate predictions after the fact that makes so many theoretical perspectives in social science—not just evolutionary approaches—seem as though they can explain anything *post hoc*).

In sum, evolutionary psychology has some of the same theoretical problems that are observed in some other quarters of social science. If I were arguing that these theoretical problems warrant the dismissal of evolutionary psychology but not of other theoretical programs with similar programs, then my critique would clearly be out of line. But this not what I seek to argue: nowhere in this dissertation do I wish to be seen as arguing that some body of theory is "good" and free from any of the problems that make some other body of theory "bad." True Believers in any extant perspective in the social sciences are likely best regarded with suspicion. But whenever such problems exist, the maturation of a theoretical perspective requires an effort to move beyond them. Evolutionary approaches would be better off if there was a more careful consideration of the scope of some of its central concepts. They may also benefit from greater care by practitioners in their derivation of explanations of known phenomena and predictions about the unknown. In saying this, however, one should not doubt that more careful conceptual development would also profit many other programs in the social sciences.

Implicit in the idea that evolutionary psychology and traditional social science share problems of theoretical development is that the various vagaries of theory and method that make social science appear "soft" would not somehow be magically solved by the Darwinizing of the social sciences. Evolutionary psychologists cannot claim the mantle of being superior methodologists than their peers; indeed, the extensive theoretical training of evolutionary psychologists may sometimes come at the expense of the honing of data analytic skills. The foregoing chapters also suggest that even if we believe that evolutionary approaches are theoretically on the right track by seeking a more intimate tie to evolutionary biology, we should not presume that these approaches are more rigorous in their theoretical reasoning than other social scientific programs. While some readers may feel that I am only belaboring the obvious here, I think many of the natural scientist and public advocates of evolutionary psychology do harbor precisely this illusion that adopting an Darwinian stance will be some kind of miraculous panacea for theoretical reasoning in the social sciences.

Certainly popular presentations of the evolutionary psychological perspective suggest this (Wright 1994; Pinker 1997), and one sees it also in some presentations directed at sociological audiences (Lopreato and Crippen 1999). What such commentators need to realize to merit being taken seriously is that doing careful social science, like downhill skiing or watercolor painting, is harder than it looks. The difficulty is inherent in the complexity of the human mind and social life, and it implies a thicket of puzzles for any approach that seeks to understand human behavior and society without mischaracterizing or oversimplifying them.

Although developing theoretical ideas and perspectives is of course important, the social sciences might be well served by an increased emphasis on the importance of good descriptions of the social world. The push to make papers "more theoretical" can lead to Procrustean distortions of phenomena in the effort to fit them into the argot of a preferred theoretical perspective. It is both daunting and depressing to think of the journal pages consumed each year by analyses that seem designed from the outset to provide support for the authors' existing theoretical conceptions. Not only does such work recurrently fail to live up to the ethos of hypothesis testing, but it also too often leads to a presentation of findings that cannot stand up to alternate model specifications or other sensitivity analyses. In other words, efforts to marshal evidence for one's own theoretical "camp" in its battle against some other "camp" can prevent the clear-eyed observation of the real patterns that do or do not exist within empirical data.

Again, this is no call for an end to theory, and, specifically, I believe that the social sciences need to be more open to the theoretical insights that evolutionary perspectives may provide, regardless of the shortcomings of some of the Darwinian efforts discussed in this dissertation. One can believe this, however, and also believe that the social sciences should accord more esteem to the considerable effort required to provide a careful description of some phenomenon. Moreover, while I do not wish to seem too discouraging about the project of devising testable predictions for theory in the social sciences, it is a task that may require more caution than it may often receive, especially the task of devising genuine predictions that pit a favored theoretical perspective against alternatives (see, e.g., Lloyd's [1999] critique of the "crucial experiments" about cheater-detection that we examined in Chapter 3). We must be wary of the possibility of theory-aided observation turning into theory-obscured observation. Such warnings may be especially important in considering the merits of evolutionary hypotheses, as the contentiousness of debate may spur considerable efforts of advocacy on both sides. Indeed, as I discuss further in the concluding chapter, the data that sociologists often use (i.e., social surveys) may be sufficiently removed from the issues of innateness implied by evolutionary hypotheses that explanations can be easily devised to fit explanations in either a Darwinian or more conventional sociocultural framework.

APPENDIX 7-A: Details on Freese and Powell (1999) study of Trivers-Willard hypothesis

DATA AND METHODS

Data

To test the Trivers-Willard hypothesis, we rely primarily on the National Educational Longitudinal Study of 1988 (NELS), a general-purpose survey of 24,599 eighth-graders sponsored by the National Center for Educational Statistics. NELS was designed to provide information on a wide variety of factors thought to affect children's development, educational performance, and life outcomes. Most of our measures are constructed from the 1988 survey, but two are taken from a 1990 follow-up of the original respondents. NELS participants were chosen using a stratified, two-stage probability sample in which first schools were randomly selected and then students were randomly selected within these schools.[1]

The NELS data are particularly appropriate to testing sociobiological hypotheses of parental investment for several reasons. First, the data set includes many different measures of investment, drawing upon information gathered from the students and their parents.[2] Using different measures drawn from different sources increases the assurance that our findings are less vulnerable to biases in student or parent responses or problems with any single measure of investment. Second, as a study of eighth-graders, the NELS data capture students at a relatively advanced stage of the period of intensive parental investment, and just at the onset of their own potential reproductive careers. As noted above, this is also the age

[1] Winship and Radbill (1994) discourage using sampling weights in certain circumstances (1994), but weights are necessary here because the weights are a function of at least one of the dependent variables (enrollment in private school). Analyses using unweighted data yield similar results.

[2] Most research indicates that, under conditions similar to those here, adolescents' and parents' reports of behavior are reasonably consistent with behavioral data gathered through other means (see, e.g., Davies and Kandel 1981; Swearingen and Cohen 1985). Some work has suggested that parents overestimate their

in which sociobiologists expect the highest level of attachment of parents to children. Third, the large sample size not only permits the use of multivariate analyses and the testing of interaction effects, but also increases the likelihood that even very modest influences on parental investment will be statistically significant; consequently, the data allow a relatively liberal test of the hypothesis.

Measures of Parental Investment

The Trivers-Willard hypothesis predicts that parents of low socioeconomic status will invest more heavily in daughters than sons, while high-status parents will invest more heavily in sons than daughters. Sociobiology tends to treat parental investment as a unidimensional concept, encompassing everything from the production of children *per se* to all behaviors toward children throughout their development. Sociologists, on the other hand, have considered there to be several, qualitatively different, means of investment through which parents may expend resources to positively affect the futures of their children. Parents may be willing to expend enormous resources for their children's benefit in some ways while being much less generous in others. Similarly, some forms of investment may be marked by a systematic favoring of one gender over another, while others are not. Focusing on one type of resource may obscure evidence supporting the Trivers-Willard hypothesis. Consequently, to provide as broad a test as possible, we analyzed how child's sex and parents' status influence five general categories of investment: *economic*, *interactional*, *supervisory*, *social*, and *cultural*. These categories should not be taken as mutually exclusive, for undoubtedly there is some overlap among them. Table 1 provides summary statistics and descriptions of all the

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investment in their children relative to children's own reports; in this circumstance, we would expect the intercepts of our models to be affected but not the interaction effects at issue here.

measures of parental investment used in our study, as well as of the measures of socioeconomic status discussed in the next section.

TABLE 7A-1 ABOUT HERE

Economic investments. Economic investments may be the most salient way in which parents differ in their level of investment: parents differ in the amount of money they have to invest, but they also choose to spend more or less of this money on their children. We concentrate here on measures of parents' spending on their children's education.[3] In the NELS questionnaire, parents were asked whether they had saved any money for their children's future education; those who reported saving money were asked how much they had saved. We use both as measures of economic investment. We also use whether or not parents send their children to a private school. While parents may send their children to a private school for a variety of reasons, the utilization of private schools very often implies salient financial considerations. Another measure of economic investment we use is the number of educational items families have in their homes, such as reference books and periodicals.[4] The presence of these objects in the home is associated with both superior achievement within school and higher incomes as adults, even for children of similar socioeconomic backgrounds (Teachman 1987; Downey 1995; Fejgin 1995). As with private schooling, the acquisition of educational objects implicates considerations beyond the economic, yet the objects also entail expenses which parents could have foregone.

[3] Investments in education benefit children not only in terms of their occupational future. Access to more privileged educational institutions (e.g., private schools, elite colleges) also increases the likelihood that the child's peers will be from high-status families, and, more importantly from a sociobiological perspective, increases the likelihood that the child will marry and reproduce with a high-status mate.

[4] When a personal computer is included among educational objects in the home, the relevant results do not change.

Interactional investments. Financial expenditures may greatly enhance children's futures, but several studies also show that children benefit from greater personal involvement by parents in their education (Muller 1993; Sui-Chu and Willms 1996; Carter and Wojtkiewicz 1997). Parents may provide instruction and guidance to their children by talking with them regularly about their educational experiences. We measure the amount of parent-child interaction about education by how often children talk to their parents about course selection, school activities, and class material. Parents may be more or less actively involved in their child's school, which we measure in terms of whether parents have gone to a school event, attended a school meeting, or visited the child's classes in the current school year. Parents may also participate more or less actively in the school's parent-teacher organization, which allows them to receive information about curricula and perhaps also to influence school policies or teacher expectations in their child's favor. We measure affiliation with a parent-teacher organization in terms of membership, meeting attendance, and involvement in the organization's activities.

Supervisory investments. Just as parents are more or less closely involved in their child's schooling, they also more or less closely supervise their child's activities outside of school. In rational choice theory, effort spent monitoring and supervising the activities of other persons is a cost no different than spending money (Becker 1981; Hechter 1987). Parental supervision is positively associated with improvements in children's self-reliance, happiness, sense of social responsibility, and educational success (Baumrind 1989). Here, we use a scale of parental supervision that measures how much effort parents spend trying to find out what their child does outside of school, who the child's friends are, and how the child spends her or his money.

Social investments. In developing the concept of "social capital," Coleman (1988, 1990: 300-321) has argued that overlap between the interactional networks of parents and children may provide important educational benefits in its own right (see also Lee and Brinton 1996; Teachman, Paasch, and Carver 1996, 1997). When parents know their children's friends, they are better able to judge the merits of the child's peer group and may also have an additional source of information about the child's feelings and possible problems. When the parents of adolescent friends know one another, they are able to exchange information about their children's activities, help one another enforce family rules, and share responsibility for monitoring and supervision. We measure social capital both as the number of the child's friends that the parent can identify by first name or nickname, and the number of these friends' parents that the child's parent knows.

Cultural investments. Finally, parents who regularly provide their children with the opportunity to participate in elite culture activities (e.g., theatre, museums) are considered to be investing in the "cultural capital" of their children (Bourdieu 1977). As elite tastes and cultural knowledge are socially valued, children with high cultural capital are theorized to be more likely to enjoy educational and occupational success, and are more likely to marry a high-status partner. Studies have largely supported this thesis (Dimaggio 1982; Dimaggio and Mohr 1985; Kalmijn and Kraaykamp 1996). We operationalize Bourdieu's investment in cultural capital in terms of the child's enrolling in art, music, dance, language, or computer classes outside of school and the child's visiting art, science, or history museums.

Measures of Socioeconomic Status and Additional Controls

Although Trivers and Willard (1973) contend that their results may be applied to humans in terms of position on a "socioeconomic scale," they do not specify how to measure socioeconomic position within a society. Because socioeconomic status has long been considered a function of a family's income and parents' education, we use both as measures of status. Family income is measured as the total income (in dollars) earned by the family from all sources in 1987, the year before the survey was given. Education is measured on a scale of progressive achievement indicating whether the most highly educated parent graduated from high school, attended college, graduated from college, or received an advanced professional degree.

We included four additional controls in our model so that possible Trivers-Willard effects are not obscured by potentially confounding variables.[5] First, we control for the number of siblings the child has (including all full- half- and step- siblings), as it is conceivable that favoritism toward males or females may be affected by family size. Second, we control for the age of the child's mother. Steelman, Powell, and Carini (1998) show that children tend to benefit from having older parents; including mother's age in the model controls for the possibility that these benefits are unequally distributed between sons and daughters. Third, to take into account the family structure effects on investment described above, we include a control for whether the child resides with both of her or his original parents. Fourth, because both parental investment and gender preferences may vary by race/ethnicity, we include dummy variables indicating the racial/ethnic background of the child (White, Black, Latino, Asian, or Native American).

Because both our measures of socioeconomic status are taken from the parents' questionnaire, we could use only those cases in the data set for which both student and parent questionnaires are available, which excluded 1,948 of the original 24,599 cases

[5] As it turns out, the substantive conclusions of our paper are unchanged by the inclusion or exclusion of these control variables.

(7.9%). Another 1,463 cases (5.9%) were dropped because parents either did not complete the income or education questions, or their questionnaires were missing information on one of the control variables.[6] This left 21,188 cases in our sample. In the models estimated below, cases were also deleted if respondents did not complete any of the questions used to construct the measure of parental investment used as the dependent variable; consequently, sample sizes for the regressions are usually less than 21,188.

Supplemental analyses

To ensure that our findings are not idiosyncratic to one dataset, we supplement our analysis of NELS data with an examination of the 1980 High School and Beyond (HSB) study. Also conducted by National Center of Educational Statistics, HSB is a large, nationally representative survey of adolescents that uses a format and sampling procedure similar to NELS, with three important differences. First, while the base-year respondents in NELS are eighth-graders, HSB interviews high school sophomores and seniors. Our analysis of HSB here uses the tenth-grade respondents only.[7] Second, whereas NELS attempted to interview parents of all student respondents, in HSB only a randomly selected subsample of parents was interviewed. Third, because HSB does not contain information on the sex of the child respondents' siblings, we were unable to conduct all of the supplementary analyses for HSB that we present for NELS.

All of the measures of economic investment that we tested from NELS have clear counterparts in HSB. Three measures—whether parents had started saving for their child's future education, how much parents had saved, and whether parents have enrolled their child in a private school—are based on virtually identical phrasing in HSB and NELS; the

[6]Because there is a comparatively large number of missing cases in NELS for mother's age, missing values on this variable were imputed from other independent variables.

remaining measure, educational objects in the home, differs only in that HSB asks about fewer objects. To examine interactional and supervisory investments, we include measures of how frequently parents talk with their child and how closely they monitor their child's activities. Although substantively similar to measures we use from NELS, these are based on different questionnaire items. Unfortunately, HSB does not contain any measures of social or cultural capital comparable to those in NELS. Means, standard deviations, and descriptions of the HSB measures of parental investment are provided in Table 2. Sample sizes of these measures vary widely because we rely on the much smaller sample of parent questionnaires for our two measures of savings for college.

TABLE 7A-2 ABOUT HERE

In our analysis of HSB, measures of parents' income and education are drawn from the student surveys rather than parents' reports. This was done so that the full sample could be retained when analyzing the measures of investment drawn from student or school information.

In auxiliary analyses in which the sample was restricted to only those cases in which parent interviews were available, substantively identical results were obtained regardless of whether student or parent reports were used to measure education and income.

RESULTS

Main Effects of Sex, Education and Income

Table 3 presents OLS and logistic regression estimates for two models of parental investment. The main-effects model estimates the effects of child's sex and parents' status on

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[7] Analyses using the 12th grade sample yield no additional support for the hypothesis.

parental investment, while the interaction-effects model tests whether the relationship between child's sex and investment varies with increased status. The Trivers-Willard hypothesis is tested by the second model, but a brief consideration of the main effects is worthwhile, as the results indicate that our measures of parental investment behave similarly to those used in other studies. Like Carter and Wojtkiewicz (1997), we find that girls receive a higher expected level of investment than boys for several of our dependent variables. Girls have more interactions with their parents about school, are more heavily supervised by their parents, have a greater investment in social capital, and are more likely to have taken cultural classes outside of school than boys.[8] These apparent advantages do not carry over to economic investment, however: parents of boys are more likely than parents of girls to have begun saving for their child's future education, and on average they have also saved more money.

TABLE 7A-3 ABOUT HERE

Consistent with Muller and Kerbow (1993) and Muller (1993), the results for the main effects model also indicate that education and income strongly and positively affect the provision of many different forms of parental investment, including those not directly related to available material resources.[9] All of our measures of parental investment are positively affected by increases in parents' education. Meanwhile, the only measure of investment not positively affected by increases in family income is parents' monitoring of their children.

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[8] While we focus on the Trivers-Willard hypothesis, others might offer sociobiological explanations for some of the *main* effects of gender on investment that we observe. For example, parents may invest more in the cultural capital of daughters than sons because this is perceived as more important for daughters in luring a mate. Parents may supervise or invest more in the social capital of daughters to better ward off unapproved matings (which, in an evolutionary sense, have a greater cost for parents of daughters than parents of sons). None of these change the *interaction* effects between sex and status that Trivers and Willard predict.

[9] Comparing standardized coefficients (not shown), the effect of education on all the interactional, social, and cultural measures is significantly stronger than the effect of income. Education and income more equally affect economic investment; not surprisingly, financial investments in children are strongly impacted by

That the effects of income on this dependent variable differ so markedly from the other measures of investment raises the possibility that monitoring (at least as operationalized here) may not be an appropriate measure of parental investment; instead, it may reflect other qualities of parents, such as a tendency toward authoritarian behavior or a more general propensity toward regulation.

Testing the Trivers-Willard hypothesis

The Trivers-Willard hypothesis predicts that increases in education and income should yield greater returns in parental investment for sons than for daughters. The interaction effects model in Table 3 tests the Trivers-Willard hypothesis by adding interaction terms that test whether the effects of increases in parents' status differ for boys and girls. The measures of investment used here are constructed so that the coefficients of the interaction terms will be positive when effects are consistent with the hypothesis, and negative when inconsistent.

As may be seen in Table 3, however, very few of the interaction effects are significant. Contrary to the expectations of the Trivers-Willard hypothesis, we observed few differences between boys and girls in the effect of either parents' education or income on the amount of investment received. In the interaction of sex with education, significant interaction effects were observed for only two (of 12) measures of investment, and both of these were in the direction *opposite* of that predicted by Trivers and Willard.[10] As parents' education increases, the expected increase in parental involvement in schooling is larger for daughters than sons.

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parents' earnings.

[10] A TOBIT regression of total dollars saved for college on the independent variables that treated those cases in which parents had saved no money as left-censored ($\tau = 0$) did not yield a significant Trivers-Willard effect ($p_{\text{male x education}} = .83$, $p_{\text{male x income}} = .53$).

Likewise, increases in education also yield a larger increase for daughters than sons in the number of cultural classes taken.

The hypothesis fares only slightly better when we consider the interaction between child's sex and family income. Again only two of the twelve interactions we tested are significant; only one is consistent with the hypothesis. As Trivers and Willard would predict, income increases affect parents' monitoring of sons more positively than it affects their monitoring of daughters.[11] This is the only significant effect supporting the hypothesis we observed for our twelve measures of parental investment and two measures of status.[12] Given the number of tests we conducted, we cannot rule out the possibility that this single effect supporting the hypothesis is simply the result of chance.[13] In addition, we suggested earlier that the monitoring variable may be a poor measure of investment because, unlike the other measures, monitoring was not positively affected by income.

Additional tests using NELS

Up to this point, our analyses provide strong reason to question the applicability of the Trivers-Willard hypothesis to these data. Few significant effects were observed, and those that were found were more often than not opposite the direction that the model predicts. To check if these findings were robust to alternative specifications, we conducted a variety of additional tests. First, because (as discussed above) the children who face the most

[11] When income is measured in logged dollars, the interaction between child's sex and family income significantly affects the number of child's friends known by the parent in the direction predicted by the Trivers-Willard hypothesis ($\beta = .668$, $p = .048$). However, the effect of the interaction of sex and income on the monitoring of child's activities is no longer significant ($p = .214$).

[12] Analyses measuring status in terms of parents' occupation (0 = unemployed to 4= upper professional [e.g., doctor, lawyer]), see Downey 1995: 753) also failed to reveal any significant interaction effects supporting the hypothesis.

[13] Moreover, it is unclear whether the observed interaction is actually consistent with what Trivers and Willard would predict. Instead, for all levels of income, parents spend more effort monitoring their daughters than their sons. The gap between daughters and sons narrows as income increases, but the difference is reduced by less than 40% from the lowest to highest income quintiles.

direct competition for parental resources are those with opposite-sex siblings, we tested the Trivers-Willard hypothesis on a NELS subsample that excluded all child respondents with either no siblings or with only same-sex siblings.[14] The results of these analyses are presented in Table 4. Looking at the interaction effects model, we find significant interactions between child's sex and parent's education for four variables: educational objects in the home, involvement in child's schooling, monitoring of the child's activities, and cultural classes taken. None of these significant interactions are in the direction predicted by the hypothesis. The only interaction between child's sex and family income that is significant is for the number of cultural classes taken, and this too is opposite the predicted direction.

TABLE 7A-4 ABOUT HERE

Second, we considered the possibility that the Trivers-Willard hypothesis may only differentiate those at income extremes (perhaps because such a large majority of Americans perceive themselves as "middle class"). Comparing those families with the lowest annual incomes (less than $10,000) to those with the highest (greater than $75,000), we find no significant differences in investment that support the hypothesis. Along similar lines, we examined whether parental behavior is sensitive to local-level differences in status rather than national-level differences. When we compare families whose incomes are below the average of the other NELS respondents from their school to those whose incomes are above average, we again find no significant effects that support the hypothesis.[15, 16]

[14] Information on the sex composition of sibships was included only on the 1990 NELS follow-up survey. Accordingly, when restricting the sample to children with opposite-sex siblings, we could use only those cases for which 1988 and 1990 data were available.

[15] We also looked at several other measures of economic investment: the amount of money parents expected to pay for their child's education, parents' willingness to go into debt to finance their child's education, how early parents' had started saving for their child's future education, and whether parents had enrolled the child in a preschool or Head Start program prior to kindergarten. None revealed any significant interactions between parents' status and child's sex.

[16] In addition to looking at measures of parental investment, we also looked at other items which,

Supplemental analyses also indicate that our results are not substantively affected by the inclusion or exclusion of any of the added control variables.[17] The results do not substantively change when the sample is restricted to only those child respondents who live with both of their original parents (eliminating all families with single- or step-parents). Separate analyses of White, Black, Hispanic, and Asian respondents indicate that the patterns observed in Tables 3 and 4 are reasonably consistent across all of the groups.

Supplemental Analyses using HSB

As mentioned earlier, we investigated the possibility that our findings were idiosyncratic to the NELS data set by conducting a similar analysis of HSB. Table 5 presents the estimated effects of a main effects and interaction effects model on measures of parental investment from HSB. Comparing the main-effects model in Table 5 with that of Table 3 (in which the same regressors were examined using NELS) shows that results are generally consistent across datasets. Certainly, both datasets evince strong main effects of income and education across various measures of investment. More important, however, when we look to the interaction- effects model in Table 5, we see little support for the Trivers-Willard hypothesis. No significant interactions between child's sex and family income are observed. The interaction of sex and education is significant for only one variable, frequency of talk between parent and child; however, this effect is in the direction opposite of that predicted:

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while not measures of investment themselves, could be seen as proxies. Of these, the child's positive regard for her/his parent yields significant results in the direction opposite the hypothesis, while parents' educational expectations for the child yields significant results in the predicted direction.

[17] Significant effects supporting the Trivers-Willard hypothesis may be obscured by our using the model to estimate the interaction of child's sex with two different measures of socioeconomic status. We tested this possibility by performing analyses in which the two interaction terms were estimated in separate models. For the full sample, we observed two substantively consequential divergences from results presented in Table 3: the interaction between income and child's sex is significant for frequency of talk about school, but the sex-by-income interaction is no longer significant for monitoring of the child's activities. For the sample restricted to children with opposite sex siblings, the results were substantively identical to those presented in Table 4.

increases in education yield a larger expected increase in frequency of talk for daughters than sons.

TABLE 7A-5 ABOUT HERE

As with NELS, we performed a number of auxiliary analyses to check the robustness of our findings (not shown). Dividing the data into subsamples based on race and parents' marital status yields no additional support for the hypothesis. Similarly, we find no support for the hypothesis when only those with the highest and lowest incomes are compared, or when parents' education and income are measured relative to the mean of the other HSB respondents from the same school. Meanwhile, when parents' education is used as the only measure of status in the model (as was done with NELS in Table 4, Model 1), the interaction of child's sex and education is significant in the expected direction for whether the child attends a private school. This is the only significant result we observed in our analyses of HSB data that supported the Trivers-Willard hypothesis. Given that we tested several models with six dependent variables and two measures of status, this lone result could be due to chance.

Table 7A -1. Means, Standard Deviations, and Descriptions of Measures of Parental Investment and Socioeconomic Status for Analyses using the National Educational Longitudinal Survey, 1988

Variable Name	Description	Metric	Mean	SD	N	Source
MEASURES OF PARENTAL INVESTMENT						
Economic Investment in Child's Future						
Started saving for child's education	Whether parent(s) have begun saving for their child's education after high school	0=no, 1=yes	0.50	0.50	18417	Parent
Money saved for college	Amount of money parents have saved for child's future education	Thousands of dollars	5.52	5.26	8542[c]	Parent
Private school	Child attending a private school in 8th grade	0=child in public school, 1=child in private school	0.20	0.40	21188	School
Educational objects in home[a]	Presence in home of (1) place to study, (2) daily newspaper, (3) regular magazine, (4) encyclopedia, (5) atlas, (6) dictionary, (7) more than 50 books, (8) pocket calculator, (9) typewriter	Number of items	6.95	1.67	19874	Student
Parental Involvement in Child's Education						
Talk with child about school[a]	Frequency child talks to parents about (1) school activities, (2) course selection, (3) things studied in class, and talks to (4) mother or (5) father about planning high school program	0 = has not talked to parents about any of these to 10 = has talked more than three times this year to parents about each of these	6.67	2.38	20188	Student
Involvement with child's school	In the current school year, parents have (1) attended a school event, (2) attended a school meeting, (3) spoken with child's teacher or counselor, (4) visited child's classes.	0=parent has not done any of these to 4=parent has done each of these	2.15	1.21	15417	Student
Parent-teacher organization[a]	Parent (1) belongs to PTO, (2) attends PTO meetings, (3) takes part in PTO activities	0=parent does none of these to 3=parent does each of these	0.98	1.13	20280	Parent
Supervision of Child's Activities						
Monitoring of child's behavior[a]	Parents try to find out (1) who child's friends are, (2) how child spends free time, (3) where child goes after school, (4) where child goes at night, (5) how child spends her/his money	0=parents try 'not at all' to find out any of these to 15=parents try 'a lot' to find out each of these	9.78	3.74	12555	Student[b]

Variable Name	Description	Metric	Mean	SD	N	Source
Investment in Social Capital						
Knows child's friends	Number of eighth grader's friends parent knows by first name or nickname	Number of friends known (maximum 5)	3.56	1.69	20305	Parent
Knows child's friends' parents	Number of friends' parents child's parent knows	Number of friends' parents known (maximum 5)	2.67	1.69	20305	Parent
Investment in Cultural Capital						
Cultural classes[a]	Student has attended classes outside school in (1) art (2) music (3) dance (4) language, (5) computers	0=no classes to 5=classes in all of these areas	0.49	0.76	19245	Parent
Cultural activities[a]	Student has gone to (1) art, (2) science, (3) history museums	0=has not visited any museums to 3=has visited each type of museum	1.51	1.26	19723	Parent
INDEPENDENT VARIABLES						
Family income	Family income from all sources, 1987	Dollars X 10000	4.16	3.79	21188	Parent
Parents' education	Educational level of most highly educated parent	0=Did not finish high school to 4=At least an M.A. or equivalent	2.07	1.15	21188	Parent
Male	Child's sex	0=female, 1=male	0.50	0.50	21188	Student

[a]Items were factor analyzed. Alphas (standardized) are: talk about school = .72, parent-teacher organization = .73, monitoring behavior = .84, parental control = .82 educational objects = .62, cultural classes = .53, cultural activities = .80, evaluation of parents = .82.

[b]Items from NELS follow-up questionnaire, 1990 (sophomore year)

[c]Item asked only of those parents who indicated that they had saved some money for their child's future education.

Table 7A -2. Means, Standard Deviations, and Descriptions of Measures of Parental Investment and Socioeconomic Status for Analyses using High School and Beyond, 1980

Variable Name	Description	Metric	Mean	SD	N	Source
MEASURES OF PARENTAL INVESTMENT						
Started saving for child's education	Whether parent(s) have begun saving for their child's education after high school	0=no, 1=yes	0.48	0.50	2390	Parent
Money saved for college	How much money parents have saved for their child's future education	Thousands of dollars	3.04	3.28	1016[b]	Parent
Private school	Child attending a private school in 10th grade	0=child in public school, 1=child in private school	0.13	0.36	20667	School
Educational objects in home[a]	Presence in home of (1) place to study, (2) daily newspaper, (3) encyclopedia, (4) more than 50 books, (5) pocket calculator, (6) typewriter	Number of items	4.60	1.29	19387	Student
Talk with child[a]	Frequency child talks to parents about personal experiences and planning high school program	0 = does not talk with parents about either of these 4=talks with parents "every day" about personal experience and "a great deal" about planning high school program	3.45	1.44	20152	Student
Monitoring of child's behavior[a]	Parents (1) keep close track of how child is doing in school and (2) almost always know where child is and what child is doing	0=neither parent does either of these to 3=parents do both of these	2.51	0.79	19954	Student
KEY INDEPENDENT VARIABLES						
Family income	Estimated family income from all sources, 1980	Dollars X 10000	2.03	0.99	20667	Student
Parents' education	Educational level of most highly educated parent	0=Did not finish high school to 4=At least an M.A. or equivalent	1.76	1.22	20667	Student
Male	Child's sex	0=female, 1=male	0.49	0.50	20667	Student

[a]Items were factor analyzed. Alphas (standardized) are: monitoring of activities = .54, talk with parents = .63, educational objects = .54
[b]Item asked only of those parents who indicated that they had saved some money for their child's future education.

Table 7A -3. Unstandardized Coefficients from Regressions of Parental Investment Measures on Child's Sex, Parents' Education, and Family Income. (National Educational Longitudinal Study, 1988)

	Main Effects Model			Interaction Effects Model				
Measure of Investment	Male	Education	Income	Male	Education	Income	Male x Education	Male x Income
Economic Investment in Child's Education								
Started saving for child's education[a]	.071* (.031)	.282*** (.017)	.096*** (.006)	-.040 (.101)	.291*** (.026)	.138*** (.014)	.011 (.035)	.008 (.017)
Total money saved for college	.266** (.097)	.456*** (.054)	.521*** (.001)	.017 (.337)	.366*** (.095)	.524*** (.028)	.082 (.124)	-.027 (.039)
Private school[a]	-.016 (.038)	.364*** (.019)	.131*** (.005)	-.196 (.123)	.268*** (.043)	.073*** (.012)	.018 (.037)	.013 (.011)
Educational objects in home	.032 (.021)	.371*** (.011)	.057*** (.003)	.066 (.069)	.368*** (.017)	.064*** (.006)	-.015 (.023)	.005 (.008)
Involvement in Child's School								
Talk with child about school	-.460*** (.032)	.394*** (.017)	.043*** (.005)	-.640*** (.109)	.375*** (.026)	.044*** (.009)	.023 (.035)	.022 (.012)
Involvement with child's school	.054** (.019)	.216*** (.010)	.028*** (.003)	.271*** (.059)	.253*** (.014)	.026*** (.005)	-.072*** (.020)	.001 (.007)
Parent-teacher organization	.013 (.015)	.183*** (.008)	.045*** (.003)	.019 (.040)	.184*** (.013)	.042*** (.005)	-.014 (.015)	.008 (.006)
Supervision of Child's Activities								
Monitoring of child's behavior	-.949*** (.066)	.226*** (.035)	.005 (.009)	-.834*** (.242)	.299*** (.055)	-.012 (.018)	-.126 (.081)	.006* (.003)
Investment in Social Capital								
Know child's friends	-.313*** (.022)	.248*** (.012)	.022*** (.003)	-.379*** (.068)	.235*** (.018)	.023*** (.006)	.010 (.022)	.010 (.008)
Know child's friends' parents	-.166*** (.022)	.219*** (.012)	.036*** (.003)	-.209** (.064)	.210*** (.020)	.035*** (.006)	.098 (.022)	.007 (.008)
Investment in Cultural Capital								
Cultural classes	-.381*** (.098)	.152*** (.005)	.028*** (.002)	.025 (.027)	.201*** (.010)	.043*** (.004)	-.097*** (.011)	-.028*** (.004)
Cultural activities	.029 (.017)	.277*** (.009)	.034*** (.003)	.078 (.048)	.296*** (.013)	.038*** (.005)	-.025 (.018)	.003 (.007)

p < .05, ** p < .01, *** p < .001 (two-tailed). Robust standard errors in parentheses. N's range from 8542 to 21188 (see Table 1). Models include additional controls for respondent's race/ethnicity, marital status of parents, mother's age, and number of siblings in family.
[a]Logistic regression of dichotomous dependent variable, all others use OLS regression.

Table 7A -4. Unstandardized Coefficients from Regressions of Parental Investment Measures on Selected Independent Variables, Children with Opposite-Sex Siblings Only. (National Educational Longitudinal Study, 1988)

	Main Effects Model			Interaction Effects Model				
Measure of Investment	Male	Education	Income	Male	Education	Income	Male x Education	Male x Income
Economic Investment in Child's Education								
Started saving for child's education[a]	.084 (.047)	.302*** (.025)	.102*** (.010)	-.055 (.155)	.304*** (.037)	.145*** (.019)	.017 (.050)	.022 (.019)
Total money saved for college	.062 (.148)	.477*** (.082)	.498*** (.022)	-.078 (.516)	.363** (.128)	.508*** (.038)	.076 (.187)	-.048 (.060)
Private school[a]	-.006 (.064)	.393*** (.032)	.146*** (.008)	-.304 (.204)	.274 (.059)	.088 (.015)	.037 (.059)	.023 (.020)
Educational objects in home	.076* (.031)	.357*** (.016)	.058*** (.004)	.326* (.102)	.379*** (.024)	.066*** (.007)	-.090* (.035)	.012 (.011)
Involvement in Child's School								
Talk with child about school	-.464*** (.048)	.393*** (.026)	.035*** (.008)	-.685*** (.152)	.353*** (.038)	.040** (.013)	.044 (.050)	.014 (.018)
Involvement with child's school	.054 (.028)	.204*** (.014)	.029*** (.004)	.231* (.090)	.234*** (.021)	.026*** (.007)	-.068* (.031)	.008 (.010)
Parent-teacher organization	.052* (.023)	.180*** (.012)	.046*** (.004)	.050 (.065)	.187*** (.017)	.039*** (.007)	-.021 (.025)	.017 (.009)
Supervision of Child's Activities								
Monitoring of child's behavior	-.927*** (.083)	.256*** (.043)	-.001 (.012)	-.547* (.258)	.355*** (.061)	-.010 (.022)	-.212* (.089)	.059 (.031)
Investment in Social Capital								
Know child's friends	-.322*** (.033)	.243*** (.018)	.028*** (.005)	-.295* (.114)	-.243*** (.026)	.032*** (.008)	-.009 (.036)	-.001 (.012)
Know child's friends' parents	-.192*** (.033)	-.209*** (.018)	.043*** (.005)	-.153 (.107)	.215*** (.027)	.046*** (.009)	-.001 (.035)	-.004 (.001)
Investment in Cultural Capital								
Cultural classes	-.391*** (.015)	.165*** (.081)	.026*** (.003)	.032 (.041)	.210*** (.013)	.041*** (.005)	-.105*** (.016)	-.023*** (.006)
Cultural activities	-.039 (.026)	.276*** (.013)	.041*** (.004)	.109 (.074)	.292*** (.019)	.049*** (.007)	-.021 (.028)	-.002 (.010)

p < .05, ** p < .01, *** p < .001 (two-tailed). Robust standard errors in parentheses. Models include additional controls for respondent's race/ethnicity, marital status of parents, mother's age, and number of siblings in family. Sample sizes range from 3671 to 9014.
[a]Logistic regression of dichotomous dependent variable, all others use OLS regression.

Table 7A -5. Unstandardized Coefficients from Regressions of Parental Investment Measures on Child's Sex Parents' Education, and Family Income (High School and Beyond, 1980).

	Main Effects Model			Interaction Effects Model				
Measure of Investment	Male	Education	Income	Male	Education	Income	Male x Education	Male x Income
Started saving for child's education[a]	-.215* (.097)	.432*** (.047)	.245*** (.056)	-.649 (.247)	.371*** (.065)	.185* (.081)	.123 (.092)	.109 (.108)
Total money saved for college	.249 (.232)	.349** (.110)	.531*** (.128)	-.104 (.626)	.283 (.168)	.511** (.180)	.124 (.218)	.042 (.249)
Private school[a]	-.215* (.097)	.432*** (.047)	.245*** (.056)	-.474** (.156)	.293*** (.034)	.229*** (.045)	.090 (.047)	.073 (.063)
Educational objects in home	-.014 (.019)	.247*** (.008)	.258*** (.011)	-.032 (.051)	.246*** (.011)	.255*** (.015)	.003 (.016)	.007 (.021)
Talk with child	-.456*** (.022)	.178*** (.010)	.099*** (.013)	-.313*** (.055)	.199*** (.014)	.116*** (.019)	-.044* (.020)	-.031 (.025)
Monitoring of child's activities	-.083*** (.013)	.047*** (.006)	.027*** (.007)	-.101** (.032)	.050*** (.008)	.020* (.010)	-.006 (.012)	.014 (.014)

*p < .05, ** p < .01, *** p < .001 (two-tailed). Robust standard errors in parentheses. Models include additional controls for respondent's race/ethnicity, marital status of parents, and number of siblings in family. N's range from 1016 to 20667 (see Table 2).
[a]Logistic regression of dichotomous dependent variable, all others use OLS regression

APPENDIX 7 -B: Exploratory Research on the Trivers-Willard Hypothesis and Sex Ratio, Birth Spacing, and Birth Stopping

Predictions

As noted, Trivers and Willard (1973) present their theory as an extension of Fisher's explanation of why most sexually reproducing species tend to produce sex ratios near 1:1. Reproductive success is often associated with physical condition or social rank, and this association is stronger for males than females. As a result, males of above-average condition tend to have more offspring than their sisters and females of below-average condition tend to have more offspring than their brothers. When the condition of parents and offspring have been correlated over evolutionary time, this leads to the prediction that parents may have developed the capacity to vary the sex ratio of offspring in response to condition. Translating this into socioeconomic status as suggested by Trivers and Willard:

H_1: *High SES parents will have a greater proportion of male offspring than low SES parents.*

Trivers and Willard cite work suggesting that such sex ratio differences exist in the United States at the time of their writing, although Hrdy (1987) points out that the apparent effect could be the result of confounding variables like birth rank and race. She cites a large study of births in a racially homogenous (Scottish) population that found no meaningful association between sex ratio and class once factors like birth order were controlled (Rostron and James 1977).

Although Trivers and Willard wrote before the work of Symons (1979) and others, we can consider their hypothesis in light of the insights of evolutionary psychology by conceiving that if the proposed dynamic does affect parental investment it does so through the psychological mechanisms governing parental investment. We may expect it to operate by manipulating parental affect and/or interest in the child's welfare. Wright (1994) has this in mind when he suggests that the hypothesis works "[underground] by shaping human feelings, not by making humans conscious of its logic." A classic trade-off in evolution is between the value of effort spent on investing in one's existing children and effort applied toward the production of additional children. More broadly, this is the tension between offspring *quality* and offspring *quantity*. We may expect that the more valuable one's existing offspring are, the more likely one is to enact a more quality based strategy by not having any additional children, at least not until existing children have passed the point of receiving intensive parental investment.

H_2: *Relative to low SES parents, high SES parents will be more likely to stop having children (at least for an extended period of time) after the birth of a son than the birth of a daughter.*

On the other hand, divorce usually results in reduced investment by the male parent and research suggests that it exerts a substantially negative influence on various life outcomes for children. Consequently, the Trivers-Willard hypothesis may also lead us to predict that:

H_3: *Relative to low SES parents, high SES parents will be less likely to divorce when they have sons instead of daughters.*

Gaulin and Robbins (1992) suggest that the Trivers-Willard hypothesis can also be tested by examining the interval between the birth of successive children. Although I do not buy their proposition that the length of the interval before the birth of a child can be used as a measure of investment in a child of that sex, the interval after the child's birth does seem a plausible measure. Accordingly:

H_4: *Relative to low SES parents, high SES parents will have a longer interbirth interval after the birth of a son than the birth of a daughter.*

Data

Data for this study come from the June 1995 Current Population Survey (CPS) and the 1957-92 Wisconsin Longitudinal Survey (WLS). CPS is a large, nationally representative monthly survey used to compute official statistics on employment; every 5 years the CPS includes a supplement obtaining marital and fertility histories of respondents. The June 1995 supplement was administered to female respondents aged 15-65 (N=32115 with one child or more). WLS is a longitudinal survey of 10,317 persons who graduated of Wisconsin high schools in 1957. Data were collected from respondents or their parents in 1957, 1964, 1975, and 1992. The cumulative data file was used to reconstruct the respondent's marital and fertility histories. Tests of hypotheses 2 and 3 exclude respondents for which the first child was not born during the respondent's first marriage. For both datasets, listwise deletion was used for cases with missing values on any of the variables in the model.

Note: In these exploratory analyses, I do not use event history methods which would be more appropriate for testing some of these hypotheses. However, as discussed below, I have taken steps that minimize the problems that would introduced by censoring.

Measures of Socioeconomic Status. Following Gaulin and Robbins (1991) and Kanazawa (forthcoming), the analyses presented below compare a low SES and high SES group. Because these authors found significant Trivers-Willard effects using the simple comparison of groups rather than a continuous measure of status, this strategy seemed to comprise a more liberal test of the hypothesis. For the tables below, respondent's education is used as the measure of status. Respondents with a high school education or less were coded as low SES, while respondents with at least a college education were coded as high SES.[1] All respondents with some college but no degree were excluded from the analysis.

For the CPS, mother's education was the only indicator of status that could be used. For the WLS women, supplementary analyses were also run in which first husband's education was used to measure status (the analyses below consider only first marriages). For the WLS men, supplementary analyses also looked at both current occupational prestige (low SES if SEI <= 55 and high SES if SEI >= 70) and occupational prestige at the time of the birth of the first child as measures of status (low SES if SEI <= 30 and high SES if SEI >=45).[2]

In addition, analyses for all samples and measures of status tested were done in which the measure of status was treated as a continuous measure and no respondents were excluded. None of these various supplementary analyses resulted in overall substantive conclusions regarding the Trivers-Willard hypothesis that differed from that presented below.

Results

Hypothesis 1. The hypothesis predicts that high SES parents will be more likely to give birth to sons than low SES parents. To test whether SES affected the probability of a mother giving birth to a son versus a daughter, I estimated a logistic regression model of child's sex on SES. Separate models were estimated for the sex of respondent's first child, the sex of the respondent's second child if the first child was male , and the sex of the respondent's second child if the first child was female .

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[1] All respondents of the WLS have at least a high school diploma, as high school graduates comprised the sampling frame.

[2] The SEI values chosen put approximately 2/5 of respondents in the low SES group and 2/5 in the high SES group. If child was born before the respondent's first job (e.g., while the respondent was in college), the prestige of first job was used.

The results of these analyses are presented in Table 1. For left to right, the table includes separate panels for CPS respondents, WLS women, and WLS men. For each panel, I present the present the predicted probabilities of the child being a son, for both a model with no controls and a model controlling for years married at birth of child, parents' age at birth of child and (for the analyses of second children) the spacing between the first and second child.

For both the WLS men and women, no significant differences were observed, nor was there a consistent tendency for the effects to be in the direction predicted by the hypothesis. For the CPS sample, a significant effect (p=.04) in the predicted direction was observed for the bivariate model of the respondent's second child, given that the first child was male. This effect was reduced to marginal significance (p=.06) when controls were added. In the other two conditions of the CPS sample, the effects were also in the predicted direction for the model with controls, but they were not significant. Given the number of statistical tests conducted, these results provide at best weak support for the hypothesis.

Hypothesis 2 and 3. Hypothesis 2 tests whether there is a Trivers-Willard effect on birth stopping decisions. If high SES parents prefer sons to daughters, then they may be more likely than low SES parents to stop having children for a prolonged period after the birth of a son than the birth of a daughter. Meanwhile, low SES parents may be more likely to stop for a prolonged period after the birth of a daughter (or two daughters) than the birth of a son (or two sons). Hypothesis 3 tests whether there is a Trivers-Willard effect on whether parents stay together or separate. High SES parents might be predicted to be relatively more likely than low SES parents to divorce after the birth of a daughter (or two daughters) than after the birth of a daughter (or two sons).

Multinomial logit models were used to analyze the parental decision to separate, stop, or have another child after the birth of the first child, and, in the case of those couples who did have another child, after the birth of the second child.[3] A seven-year "decision window" was used: parents who separated in the seven year period were coded as "separated", those who stayed together but did not have a child within those seven years were coded as "stopping." Parents who had another child and separated were coded according to which happened first. To avoid problems with censoring, the sample was restricted to only respondents for whom this seven year period had ended. For the analysis of decision after the second child, this means that the sample was restricted to only those respondents whose second child had been born at least seven years previously.[4]

Table 2 presents the results. For the three samples, the top half of the table presents predicted probabilities of stopping and separation after the birth of a son and the birth of a daughter. The bottom half presents the predicted probabilities of stopping and separation after the birth of two sons or two daughters. The Trivers-Willard hypothesis is tested by the interaction of child(ren)'s sex and SES. Each model contains interaction terms that test the hypotheses regarding both stopping and separation. The table indicates whether these terms were in the predicted direction and whether they were significant. For the decision to stop vs. have another child, five out of six tests were in the predicted direction, but none were significant ($p > .10$, two-tailed). For the decision to separate vs. have another child, four of six tests were in the predicted direction, and an effect was marginally significant for the

[3] These are nested multinomial logit models, although they were estimated separately.
[4] Analysis of the decision after the second child was restricted to those respondents who were married to their first spouse and who had their second child within seven years after the birth of their first child.

decision to separate after the birth of the second child (p=.08, two-tailed).[5] While a preponderance of nonsignificant results were in the predicted direction, these results do not provide positive support for the hypothesis.

Hypothesis 4. Following Gaulin and Robbins (1991), we might expect high SES parents to have a relatively longer interbirth interval after the birth of a son than the birth of a daughter, compared to low SES parents. I test the hypothesis for the interval after the birth of the first child and the birth of a second child.[6] To minimize problems of censoring, the CPS sample was restricted to women 42 years old or older, and whose fertility history was thus likely to be complete.

Table 4 presents the results of the analysis. In the table, the interbirth interval is measured in logged months; analyses using the untransformed interval produced similar results. Again, the Trivers-Willard hypothesis is tested by an interaction term. For the interval after the first child, the key interaction is between SES and the comparison of daughter vs. son. For the interval after the second child, the key interaction is between SES and the comparison of two daughters vs. two sons.

For WLS males, the interbirth interval is significant and in the predicted direction (p = .03, two-tailed). This was the only significant effect observed; only three out of the six effects were in the predicted direction. Again, these results can only at best be said to provide weak support for the hypothesis.

[5] This result was not significant when the model was estimated as a logit of separation vs. either stopping or having another child.

[6] In the table, I look at whether the interval after the second child is affected by the sex composition of the two children. In supplementary analyses I looked at whether the interval after the second child was affected by the sex of the second child only, but I found no evidence of any effect.

Taken together, these four interrelated tests provide at best weak support for the Trivers-Willard hypothesis. In other words, I find no persuasive evidence of Trivers-Willard effects on sex ratio, fertility history, or marital history in two large contemporary samples of US adults.

Table 7B -1. Socioeconomic status and predicted probabilities of first and second child being a son

	CPS Females			WLS Females			WLS Males		
	Low SES	High SES	*p*	Low SES	High SES	*p*	Low SES	High SES	*p*
First child									
p(son), bivariate	.522	.517	NS	.513	.504	NS	.522	.498	NS
p(son), model with controls	.517	.527	NS	.514	.500	NS	.523	.496	NS
N		14190			3051			2488	
Second child, first child male									
p(son), bivariate	.510	.544	.04	.522	.490	NS	.498	.520	NS
p(son), model with controls	.509	.545	.06	.523	.487	NS	.496	.524	NS
N		6144			1385			1121	
Second child, first child female									
p(son), bivariate	.511	.509	NS	.503	.500	NS	.505	.514	NS
p(son), model with controls	.509	.516	NS	.502	.506	NS	.508	.509	NS
N		5570			1325			1066	

Predicted probabilities obtained using logistic regression. All p-values less than .10 reported, two-tailed. Model with controls includes years married at birth of child, parents' age at birth of child and (for second child analyses) spacing between first and second child. These variables held at their mean in the calculation of predicted probabilities. Weighted results and robust standard errors used for CPS data.

Table 7B-2. Predicted probabilities of separating or of having no more children in 7-year period following birth of first or second child

	CPS Females		WLS Females		WLS Males	
	Low SES	High SES	Low SES	High SES	Low SES	High SES
After first child						
p(stop | son)	.141	.163	.065	.039	.079	.057
p(stop | daughter)	.145	.159	.053	.053	.076	.057
Is effect in predicted direction?	Yes (NS)		No (NS)		Yes (NS)	
p(separation | son)	.077	.064	.017	.007	.017	.018
p(separation | daughter)	.080	.062	.017	.029	.015	.010
Is effect in predicted direction?	No (NS)		Yes (p=.08)		No (NS)	
N	11981		3017		2485	
After second child						
p(stop | two sons)	.448	.475	.259	.243	.330	.356
p(stop | two daughters)	.451	.521	.267	.214	.355	.340
Is effect in predicted direction?	Yes (NS)		Yes (NS)		Yes (NS)	
p(separation | two sons)	.094	.093	.044	.022	.031	.032
p(separation | two daughters)	.098	.072	.033	.015	.036	.032
Is effect in predicted direction?	Yes (NS)		Yes (NS)		No (NS)	
N	9654		2687		2175	

Predicted probabilities obtained using multinomial logistic regression. All p-values less than .10 reported. Probabilities of event occurring within seven years of birth of child. Models include controls years married at birth of child and parents' age at birth of child, which were held at their means in the calculation of predicted probabilities. Models after birth of second child also control for spacing between first and second child and for families with one son and one daughter. Weighted results and robust standard errors used for CPS data.

Table 7B-3. OLS Estimates of Regression of Socioeconomic Status and Child's Sex on Subsequent Birth Spacing

	CPS Females		WLS Females		WLS Males	
Logged spacing after first child	b	SE	b	SE	b	SE
Low SES × Daughter	.045	.035	-.029	.051	.098	.046
Low SES	-.039	.026	-.067	.037	-.109	.033
Daughter	-.059	.029	.024	.046	-.079	.037
Yrs married at birth of first child	.025	.004	.045	.008	.060	.008
Parent's age at birth of first child	.002	.003	.014	.004	.011	.004
Is key interaction in predicted direction?	Yes (NS)		No (NS)		Yes (p=.03)	
N	6947		2803		2281	
Logged spacing after second child	b	SE	b	SE	b	SE
Low SES × 2 Daughters	.018	.076	-.018	.105	-.153	.105
Low SES × 1 Son & 1 Daughter	-.068	.066	-.004	.094	-.028	.092
Low SES	.033	.042	-.026	.076	.049	.076
2 Daughters	.011	.075	.083	.097	.057	.086
1 Son 1 Daughter	.063	.061	-.022	.088	.064	.076
Years married at birth of child	.044	.006	.032	.011	.042	.013
Parent's age at birth of child	-.005	.004	.015	.007	.017	.006
Is key interaction in predicted direction?	Yes (NS)		No (NS)		No (NS)	
N	4153		1898		1338	

CPS sample restricted to women 42 years old or older. Weights and robust standard errors used for CPS data. "NS" indicates $p > .10$.

8

CONCLUSION

"In a solitary chamber, or rather cell, at the top of the house... I kept my workshop of filthy creation... and often did my human nature turn with loathing from my occupation, whilst, still urged on by an eagerness which perpetually increased, I brought my work near to a conclusion."

—Mary Shelley, *Frankenstein*, 1831 (Chapter IV)

We are currently in the middle of a great burgeoning of interest in Darwinian approaches to social behavior. Concurrent to this, sociology endures criticisms that it is intellectually weak, theoretically incoherent, and too politically biased to be publicly credible. Some of the harshest such critiques of sociology have come from proponents of evolutionary psychology or related approaches. These Darwinian critics have, with great rhetorical flourish and to often large reading audiences, presented their own perspective as scientific and revolutionary—"a new science"—while the existing alternatives, sociology prominent among them, are depicted as tired and epistemologically squishy. Consequently, we should not be surprised that some have suggested that one way that sociology can reinvigorate itself is by directly incorporating insights of evolutionary psychology as a theoretical foundation, a starting point for talking about both individual social behavior and the microfoundations of macrosociological events. In blunt terms, some argue that sociology needs to get over its "biophobia" and "Darwinize" itself (Ellis 1996; Lopreato and Crippen 1999; Thornhill and Palmer 2000).

At the same time, it is clear that at least for some sociologists, this Darwinization may seem a fate worse than disciplinary death. Mere mention of the word "sociobiology" can conjure up considerable ill-will among some, who associate the term with sexism or racism or some other politically unseemly agenda. These hostile reactions, however, only fuel the perception that sociologists as a whole are so biased against evolutionary approaches that they are unable to evaluate Darwinian claims scientifically or fairly. Even when one moves beyond the political disputes, the logic of evolutionary psychology—for that matter, the logic of natural selection itself—is easily misunderstood, especially if it has not been given careful study. Because many evolutionary psychologists and traditional sociologists differ almost all the way down to first principles in how they believe social behavior should be understood (i.e., a focus on ultimate causes versus an absence of consideration of such causes), the prospects of scholars talking past each other is high, even when genuine dialogue is desired.

What we have then are two different groups of scholars, thinking about and doing research on some of the very same topics, but with considerable misunderstanding and hostility between them. My larger, and ongoing, project is to try to come to some understanding of what role thinking about human genetic evolution should play in thinking about social behavior and social organization. My orienting conviction has been that sociology needs to do a better job of articulating its relationship to human's evolved biology, or else it risks appearing increasingly irrelevant to what is evoking debate and excitement outside its disciplinary boundaries. A joke that some evolution-minded scholars tell about sociology is that it is "guarding the corpse"—the corpse being a "politically correct" view of the human psyche that science has long forsaken.

With such goals is mind, the obvious place to begin seemed to be with the work that has been generating all the attention. The preceding five case studies are the product of my efforts to wrestle with some specific potential contributions of evolutionary psychology to social behavior. The studies that were begun earliest (i.e., that recounted in Chapter 5 and the beginnings of Chapters 6 and 7) were centered on my substantive interest in the sociology of family life. Over time, the inquiry expanded to include a work that was considered foundational to evolutionary psychology (i.e., Chapter 3, on the cheater-detection research of Cosmides and Tooby) and a work that gained enormous attention as a controversial entry of evolutionary psychology into a politically charged area (i.e., Chapter 4, on *A Natural History of Rape*). The works examined in these case studies make contentions in many different areas of traditional sociological interest, including social cognition, social exchange, rape, the social "functions" of rape trauma, the effects of early childhood environments, political radicalism, social attitudes, the achievement of eminence, the effects group size, divorce, sexual attraction, adolescence, parental investment, family structure, and socioeconomic attainment. Some of the theoretical claims I have examined have also been used, either by the original authors or by others, as the basis for recommendations about social policies.

The expansion of the case studies reflects at least two developments in my thinking as this project has progressed. The smaller of these developments has been an increasing interest in the program of evolutionary psychology as a phenomenon of study in its own right. We have at hand an enterprise that has commanded considerable public attention and has been the subject of several highly successful trade-book treatments that hail it as revolutionary (most notably Wright 1994 and Pinker 1997), while being regarded suspiciously or dismissively by many academic social scientists, so much so that some proponents may feel like they have had to make career sacrifices and endure the calumny and condescensions of a collegiate "confederacy of dunces" in order to pursue their preferred theoretical perspective (see, e.g., Kenrick 1995; Tooby and Cosmides 2000; Thornhill and Palmer 2000; Segerstråle 2000). A question that has interested me here is how the "growing pains" of evolutionary psychology as a theoretical program is affected by aspects of this antimony between its public popularity and its difficult relationship with some other quarters of social science. At various points in the case studies, I point to instances in which the public attention to evolutionary psychology may be detrimental to its ultimate intellectual development, but fuller development of ideas in this regard must be postponed as a potential topic for future research.

The larger development has been an increasingly ambivalent sense that while our evolved biology has an important role to play in the understanding of social behavior, there are considerable logical, theoretical, and/or empirical problems with many of the current efforts of Darwinian social science, especially with that work that has gained the most attention. One can be convinced that there is "something to" evolutionary arguments regarding parenting, sexuality, or kinship, and yet still regard there to be formidable deficiencies in what is being most prominently proffered under the evolutionary banner. These concerns have also grown from reading some of the critiques of the "new science" of evolutionary psychology, many of which have been published since this project began (e.g., Angier 1999 322-354; Buller 2000; Buller and Hardcastle 2000; Lloyd 1999; Shapiro and Epstein 1998; Shapiro 1998; D.S. Wilson 1994, 1999; essays in Hardcastle 1999, Rose and Rose 2000). The case studies are thus mainly critiques of these specific proposed contributions to Darwinian social science. Another consequence of this shift-in-orientation is that the case studies do not confine themselves to the specific work under examination but also try to connect these criticisms of specific work to more general issues and problems of evolutionary psychology. A unifying theme of these is how easy it is for evolutionary work to overreach what they might be reasonably expected to provide.

Taken together, these case studies suggest reason for careful evaluation of evolutionary psychological hypotheses, and they illustrate a variety of different ways that work in this area can be engaged by sociologists. First, there is the analysis of empirical data, whether in the form of an original empirical effort to test a Darwinian hypothesis (as in Chapters 5 and 7) or a replication of some earlier study (as in Chapter 6 and attempted in Chapter 4, as well as in some of the experimental conditions of Chapter 3). Second, there is the analysis of evidence that has been marshaled as supporting a theory, and the questions of its methodological soundness and its fit to the point it is claimed to support. For the hypotheses of Darwinian social science, this consists of the evidence that the hypothesized behavioral pattern exists, that the behavior is produced by proposed mechanisms, and that the behavior served the hypothesized adaptive function in our ancestral past. In our case studies, we have seen that the evidence on any or all of these points is sometimes not as strong as some (and especially secondary) presentations would suggest. Third, there is the analysis of the evolutionary reasoning behind some hypothesis and the proposed application of these hypotheses to contemporary society. Although doing so has led to some long forays into what may seem like arcana of evolutionary reasoning, wrestling with Darwinian claims at this level also shows that criticism of particular theories is not based on a skittishness about thinking evolutionarily. Additionally, in some instances, evolutionary psychological theories contain within them sociological theories about aspects of the organization of social life, either now or in our ancestral past (as we saw in Chapter 3 with the reduction of social laws rationing benefits to social exchanges).[1] On the whole, the results of the case studies may

[1] Indeed, the social reasoning of evolutionary psychological explanations is something that I wish I had been able to pursue more deeply in this dissertation. While evolutionary psychology can (as I discuss shortly) criticize social science for often not being sufficiently concrete about the psychological presuppositions of their theories, so too perhaps can a sociologist criticize evolutionary psychology for not being plain enough about

indicate that constructing evolutionary explanations that stand up to critical scrutiny may be more difficult than it may at first appear.

As it stands so far, then, the thrust of my project has yielded largely critical fruit. If I end now, the project will seem as though it has only been negative. It would be as if I have gone off and read all of this evolutionary psychology only so I could return and tell sociologists that there are many problems with evolutionary psychology, which is what most sociologists probably already suspected anyway, even if their reasons for skepticism may not be particularly well thought out. The point of my critiques has not been that sociology should continue to ignore biology (as it mostly has), even while the biology of behavior is a matter of increasing public interest and interest in other disciplines. Instead, if anything, I think that sociologists need to do more: more to engage the evolutionary claims about social behavior that are made by others, and more to develop new ways of thinking about how propositions about evolved psychology can be incorporated into theories of social behavior. This "more" may also consist of challenging whether some of the epistemological assumptions of evolutionary psychology are appropriate for sociological inquiry, especially those that may be seen as imposing a homogeneity on the diversity of phenomena captured by terms like "rape," "cheating," or "liberal social attitudes." Yet, wish as some might, Darwinian approaches to social behavior are not going to go away, nor, despite the flaws of some current efforts, should they. Advances in neuropsychology, genetics, psychopharmacology, behavioral endocrinology and related areas are also only going to sustain the public desire for scientific insights into the relationships of body, mind, behavior

what their theories presume about the social organization of life in the pertinent ancestral environment. Moreover, because so little is known about Pleistocene social life, evolutionary reasoning is often reliant on "thought experiment"-like reasoning about what social dynamics for early humans would have been like and what sort of behaviors would have been adaptive and maladaptive in this environment.

and society, and there might be some role for an explicitly Darwinian framework for understanding these relationships, even if its eventual form is difficult to presently discern.

By way of concluding this dissertation, then, I wish to consider explicitly some of the ways that evolutionary thinking might prove valuable for sociologists. I begin by trying to draw lessons from two of evolutionary psychology's most prominent criticisms of sociology. These are that many sociologists have been too politically biased and that many sociologists have given inadequate attention to the psychological presuppositions of their theory (as well as what these presuppositions imply about human evolutionary history). In both cases, I believe that the evolutionary critique has merit (although, that said, the claims of political bias can also be easily overstated), and I provide a mix of exhortations and suggestions for sociological improvement on both fronts. I also discuss the testing of evolutionary psychological predictions, for which I draw lessons from the preceding case studies.

In the second half of the chapter, I consider aspects of the sociological use of evolutionary psychological ideas. I begin by proposing that sociologists can make valuable use of propositions about behavioral propensities, including those provided by evolutionary psychology, while remaining indifferent to whether the evolutionary account is correct and even indifferent to what about the proposed propensity is innate. Evolutionary thinking may thus be one source of ideas for the sociological study of the social consequences of psychological propensities, without sociologists needing to take up the study of the logic of selection in order to be able to evaluate the theoretical reasoning of the evolutionary hypothesis. Next, I describe some evolutionary approaches to culture. Then, I discuss the study of social manipulations, which may operate in part by exploiting evolved features of human psychology, and therefore provide one place for conversation between sociologists and evolutionary psychologists. I argue against some evolutionary psychological reasoning about manipulation that, if unchallenged, would abrogate potential sociological contributions to understanding behavior. Additionally, I consider some ideas about how evolutionary psychology may contribute to the design of effective social interventions. In an effort to give some more positive examples than what the case studies have provided, at various places in this chapter, I give specific attention to what my sociobiological travels have led me to regard as one of the most promising avenues of Darwinian social science (especially as it might intersect with sociology): work that explores how evolved propensities may underlie human prosocial behavior (e.g., altruism and cooperation).

TOWARD A DE-POLITICIZATION OF DARWIN?

"I'm starting with the man in the mirror, I'm asking him to change his ways."
—Michael Jackson, 1987

As we have seen at various points in this dissertation, proponents of evolutionary approaches have launched many harsh critiques of the rest of social science, including some directed at sociology specifically. In asking how sociology can benefit from evolutionary psychology, a first place to look for lessons might be in these critiques. One of the most prominent and frequently repeated charges that evolutionary critics have leveled against sociology is that the discipline is too ideologically biased to evaluate the proposed contributions of evolutionary approaches fairly (van den Berghe 1990; Tooby and Cosmides 1992; Kenrick 1995; Salter 1996b; Ellis 1996; Lopreato and Crippen 1999; Thornhill and Palmer 2000). The charge has substantial rhetorical value for placing some critical assessments of evolutionary approaches "outside" science. Still, one would be hard pressed to deny that unreflected-upon hostilty exists among some sociologists, which may stem from the widespread belief that evolutionary approaches to behavior are necessarily linked to reactionary politics and to an endorsement of the status quo (see Segerstråle 2000 for a chronicle of some churnings of this debate). Evolutionary psychologist David Buss (1995: 85) has written, "It is my observation that those who most stridently accuse evolutionary psychologists of being ideologically driven are themselves strongly driven in their thinking by ideology." A more blanket accusation is that sociologists typically base their evaluation of claims more on consonance with political beliefs than on consonance with the available evidence (e.g., Thornhill and Palmer 2000). The case studies have suggested that evolutionary psychology's distrust of other social scientists may sometimes lead them to the dismissal of seemingly valid criticisms as products of ideological bias (see Chapters 4 and 5). This is a problem for evolutionary psychology, but it is also one for sociology. To the extent that such dismissals—and the terms of the debate on which the dismissals are based—are given weight by broader audiences, sociologists risk losing its capacity to offer a credible and cogent voice in these debates.

The role that ideology and advocacy should play in sociology or any other social science is a matter of disagreement among practitioners and a source of seemingly eternal contention. A more pragmatic and tractable issue, however, is what sociologists can do when entering into debates in areas where their credibility may be challenged on the grounds of ideological bias. The retort that one's findings are a product of one's politics may be inevitably available in such a contentious area (and regardless of whatever one's findings and politics are), but one can also take steps to avoid giving added credence to such allegations. In this dissertation, I have set as a ground rule that arguments made in the evaluation of a theoretical claim or other aspects of a work of evolutionary scholarship will be strictly divorced from the apparent political implications of the claim if it were shown to be true. Scholars should also be careful about imputing political motives to authors, beyond the positions that the authors themselves explicitly express, and they should not use attributions of insidious motives as the grounds for dismissing theoretical claims.[2] Failure to do these things only makes sociologists vulnerable to the criticism that they cannot separate the pursuit of truth from the pursuit of politics.

Phrases like "the pursuit of scientific truth" suggest the value of trying to move debates to empirical grounds whenever possible, where sociologists can use their training in data analysis to help settle debates about the observable character of social life. In the next section, I discuss further the role of empirical evidence in some of these debates. First, however, one can note that for those claims that can be engaged through quantitative data analysis, the sociologist faces the possible complaint that "you can manipulate numbers to show whatever you want." Here, credibility is enhanced by the use of publicly available, secondary data, when possible, as this mitigates the possible criticism that the data collection process itself may have introduced bias for or against a hypothesis (as we saw in Chapter 5, where we could not rule out interviewer effects for the data that Sulloway collected from historians). Also, by being explicit about the analytic decisions one has made working with data and by performing and describing sensitivity analyses that examine how the results are affected by these decisions, the analyst may allay suspicions that one is only reporting results consistent with one's pre-existing theoretical commitments (I perform many such alternative analyses in my tests of Sulloway's theory and the Trivers-Willard hypothesis). While this may be good counsel about data analysis in any circumstance, the atmosphere of distrust

[2] This said, sociology has also served a valuable role in challenging attempts to appropriate the mantle of science in constructing polemics (e.g., Fischer et al. 1996).

unfortunately common in the study of Darwinian explanations of behavior makes it even more important, and important for scholars approaching the field from any standpoint.

Of course, the point is not just *to appear* to be judicious and open-minded when evaluating evolutionary explanations, but *to actually be so*. On this score, sociologists have too easily accepted the premises that Darwinian approaches are inherently conservative and antithetical to progressive plans for social reform. To be sure, some conservative and libertarian observers of evolutionary approaches have argued this in one form or another (Ridley 1996; McGinnis 1997), and some evolutionary psychological work has freely drawn policy implications that would seem to have a decidedly conservative bent (see Chapter 4). But sociobiologists have, from the beginning, fought the charge that their perspective implies a genetic or biological determinism, and the evolutionary psychological focus on the sensitivity of mechanisms to environmental conditions—and the unpredictability of responses to environmental novelties—would seem to make the perspective even more easily reconciled with the prevailing political sensibilities of sociologists (Buss 1999; Buss and Kenrick 1998; Cosmides and Tooby 1992). We should not be surprised, then, that there have been explicit calls to Darwinian arms from the left: "It is time for the left to take seriously the fact that we are evolved animals, not only in our anatomy and our DNA, but in our behaviour too. In other words it is time to develop a Darwinian left" (Singer 1999; see also Barbara Ehrenreich's [1999] discussion of a femaleist movement among women interested in biology).

My point is not to trade the argument that Darwinian thinking inherently implies a conservative politics for the view that such thinking inherently implies a liberal one. Instead, I want only to show that there is no monster lurking in the shadows of evolutionary psychology that immediately and incontrovertibly implies some political stance.[3] Instead, there is much room for interpretation and debate, as there typically is in political matters (the debates over the existence and political meaning of a possible "gay gene" provides an interesting related example from behavior genetics [see Burr 1996]). Consequently, sociologists of any stripe do not have to feel like they are compromising some principles if they approach with an inquiring spirit the potential contributions that Darwinian perspectives may offer. Moreover, the credibility of any ideological position is surely damaged if its supporters can be portrayed as afraid of some avenues of inquiry, and so a better alternative would seem to engage and examine areas of controversy.

Additionally, if mainstream social science was more open to considering Darwinian explanations, it might improve the separation of the wheat from the chaff in this field. As these case studies have indicated, there are many potential pitfalls in applying evolutionary concepts to behavior, and a more complete dialogue with social science may help provide a means to separate solid and cautious work from claims that may be overly dramatic, especially in the realm of that work which receives considerable public attention. To the extent that the response of mainstream social science appears uniformly negative to proposed Darwinian contributions, then the objections to particularly objectionable work has less of a chance of being seen as genuine critique, as opposed to *a priori* judgment. In addition, a latent theme of this dissertation has been the suggestion that evolutionary scholars have not done a good enough job of critically evaluating the work of some of their peers. Such potential for (public) self-criticism might be circumscribed by the self-perception that many evolutionary social scientists may have of being "a small, beseiged group, almost like a secret

[3] This is further evinced by some evolutionary biologists, evolutionary psychologists, or persons who have shown strong interest in evolutionary psychology having ties to Marxian activism, from John Maynard Smith to Herbert Gintis.

society" (Segerstråle 2000: 15).[4] If mainstream social science helps to foster such an us-versus-them view of debates over evolutionary explanations, then it is harder to chastise Darwinian social scientists for being reluctant to criticize "their own", even when failing to do so contributes to further polarization and misunderstandings of their enterprise.

TESTING EVOLUTIONARY PSYCHOLOGICAL PREDICTIONS

As noted in the preceding section, the value of moving debates to empirical grounds when possible should not be underestimated. Even so, my experience with the case studies and other readings leads me to be pessimistic about the possibility of empirical research adjudicating many issues at the heart of these debates, at least anytime soon.[5] Even when empirical points are not in dispute, potential interpretations are varied and difficult to disentangle. For example, parties could agree on the following points: (1) that stepparents across a wide variety of cultures are more likely to abuse a child than a biological parent; (2) that many stepparents display considerable loving behavior toward and attachment to their stepchildren; (3) that humans display considerable loving behavior toward individuals who are not their biological kin (not just other children but, even more notably, pets; (4) that cultures attach meanings to whether a child is biologically one's own, and even (5) that many cultures have some stereotype of the "wicked stepparent." Yet, one can still disagree greatly on the extent to which the cultural manifestations of (4) and (5) reflect some biological mechanism of "parental love" versus exhibit a causal effect of their own on parental behavior.

[4] The quoted phrase is Segerstråle's characterization of the description provided in W.D. Hamilton's first presidential address to the Human Behavior and Evolution Society.
[5] Perhaps neurology and endocrinology will provide further insight on some questions, by providing more specific information about the proximate physical mechanisms implicated in affective states and behavior.

Where empirical evidence may be able to contribute most strongly is against either polar position: culture as causally determinative for understanding adult-child relationships versus culture as causally inert. To continue the example, the magnitude of the discrepancy between abuse rates by stepparents and biological parents and the cross-cultural evidence suggesting the potential universality of this pattern (see Daly and Wilson 1999 regarding both) may provide convincing evidence against one who would want to deny any potential role for evolved mechanisms of parental love. To whatever extent that traditional social science tends to adopt unreflectively such an extreme stance toward its subject matter, as I will discuss further below, the potential contribution of evolutionary psychology for calling such extremity into dispute should be emphasized. Similarly, to whatever extent potential biological causes are unreflectively ignored by social scientists, evolutionary psychology provides an important contribution by pointing to the need for these lacunae to be examined or at least acknowledged.

The problem comes once one moves away from either extreme position and recognizes that some interaction between biological and cultural forces is at work. For once one is willing to grant something to each side, it becomes much less apparent how one might empirically decide how the interaction between the two works. Even less visible is what empirical inquiry can contribute to deciding how much emphasis each should receive from social scientists. Recognizing that an interaction exists is not the same as figuring out how to handle it, and, although I discuss efforts that have been made integrate sociobiological and cultural thinking below, these efforts are still quite nascent.

Empirical studies certainly may assist in evaluating specific theoretical conjectures, but we should be much aware of the limits of its contributions here, especially given the data that social scientists commonly work with (e.g., surveys). For the typical sort of analysis conducted by sociologists, at least, consistency with the predictions of an evolutionary psychological hypothesis *per se* need not be taken as evidence that the behavior under study is genetically specified to the degree that the Darwinian hypothesis proposes. Consider the test of Sulloway's hypothesis regarding birth order and social attitudes that we conducted in Chapter 5. We did not there find evidence supporting the hypothesis, but even if we had, this would not have meant that an alternative explanation of birth order effects could not have been developed (admittedly, *post hoc*) that did not draw as closely on evolutionary reasoning as did Sulloway's theory. Likewise, *in*consistency with some evolutionary psychological hypothesis does not imply that some other, equally domain-specific hypothesis is not the correct explanation. Had we observed a large number of birth order effects opposite of Sulloway's theory, this would not been some irrecoverable blow to the edifice of evolutionary psychology. Instead, we might expect that some alternative evolutionary psychological explanation would be devised (admittedly, *post hoc*) that would make the effects we observed appear to follow straightforwardly from Darwinian principles. Our test may then be seen as providing evidence for or against Sulloway's specific evolutionary theory, but not as evidence for or against evolutionary psychology against its more conventional social scientific alternatives, which may seem counterintuitive since Sulloway's theory is an evolutionary psychological theory.

In Chapter 6, I discussed an article by Biblarz and Raftery that attributed predictions about the relationship between family structure and socioeconomic attainment to six different candidate theoretical frameworks, including evolutionary psychology. The results were most consistent with the evolutionary psychological perspective, which was taken as support for this perspective against the more traditionally social scientific alternatives. I attempted to demonstrate, however, that the specific predictions that Biblarz and Raftery attribute to evolutionary psychology are only one of several sets that could have been as easily derived. As I said then, such "promiscuous predictions" (Laibson and Zeckhauser 1998: 26) do not necessarily point to some fundamental flaw in evolutionary approaches to social behavior. We doubtlessly could have contrived alternative predictions from the other, more conventional perspectives that Biblarz and Raftery examined, including in all likelihood some that fit the observed pattern of results. What the exercise did illustrate, however, was the difficulty of devising defensible tests that are proposed to adjudicate between evolutionary psychological hypotheses and more traditional social scientific alternatives.

Instead, regarding specifically the relationship of evolved biology, parental investment, and socioeconomic attainment, illuminating the issues that separate evolutionary and sociological perspectives requires both more and different data than what Biblarz and Raftery marshaled. There are at least three crucial questions: (1) how and in what ways is attainment affected by different types of parental investments; (2) how do (evolved) biological and environmental factors interact in determining how and how much parents invest in their offspring; (3) whatever these evolved biological factors are, is the evolutionary psychological theory the correct historical account of why we evolved this way instead of some other? The potential contributions of their survey analysis are most obviously relevant only to the first of these. For the second and third questions, my strong suspicion is that just about anything that survey data of this sort might tell us about differential parental investments could be rendered consistent with framework(s) that make close reference to our evolutionary past and with framework(s) that do not. These associations we observe between family structure and socioeconomic attainment may indeed be the product of two propensities regarding parental investment: that mothers invest more in children than do fathers and that parents invest more in biological children than stepchildren. Even so, the skeptic can immediately retort that, even if this analysis does suggest that these propensities exist, it does nothing to show that they are genetically based; moreover, even if the propensities are genetically based, the analysis does nothing to show that the evolutionary psychological theory is the correct explanation of why.

Critics of evolutionary approaches often express frustration at "promiscuous predictions" and often *post hoc* character of evolutionary explanations. To be fair, however, we must keep in mind that such malleability also characterizes many other social scientific frameworks (see, e.g., Laibson and Zeckhauser 1998 regarding rational choice theory). When evolutionary psychology offers specific and genuinely testable hypotheses about social phenomena, these may be empirically engaged and either confirmed or disconfirmed. The conclusions from such tests cannot be used to make sweeping generalizations about evolutionary psychology as a whole, although perhaps they may serve as some collateral evidence regarding similar claims that do not have such obvious testable implications. Meanwhile, pitting more monolithic characterizations of the evolutionary psychological perspective against sociocultural- or economics-based alternatives will likely be less fruitful, as the particular predictions attributed to each framework may often be available for challenge.

I do not have some magic answer for these problems to offer in this conclusion, but, for both sociologists and evolutionary psychologists, these problems underscore the importance of reflecting upon and being explicit about what a particular empirical inquiry can and cannot determine. With regard to Darwinian explanations for behavior, there is the additional complication that the specific claim about evolutionary history is actually a step removed from the more common point of divergence between evolutionary psychology and more traditional social scientists, which concerns *the characterization of the innateness of the behavior that the evolutionary explanation implies*. Put another way, the Darwinian explanation itself may often be a bit of a red herring in discussions in which the real bone of contention is what about a behavior is "in the genes." For example, regarding parental investment, the disagreement may be more about how closely the psychology of parental investment behavior is specified genetically, rather than about whether some particular evolutionary historical account itself is correct. In testing evolutionary psychological theories, this distinction between the questions of what-is-specified-genetically and how-was-it-specified-genetically only increases the potential confusion about the conclusions that can be drawn from a particular empirical analysis, and researchers need to be clear what their own analyses are intended to approach.

THE IMPORTANCE OF PSYCHOLOGY FOR SOCIOLOGY

> *"We still need to produce the* mechanism *by which conditions—certain arrangements of microsituations—motivate human actors to behave in certain ways. This mechanism should explain both why they behave as they do in specific situations and why they maintain certain dispersions in microbehaviors among themselves, across time and space, thereby making up the macropatterns of social structure."*
> —Randall Collins, "On the Microfoundations of Macrosociology," 1981

This chapter has discussed how the evolutionary psychological critique gives sociology reason to consider how it wishes to conduct inquiry in the face of charges of political bias, but the critique also speaks to sociology in other ways. Importantly, the critiques remind sociologists that our models must be *psychologically honest*, in the sense that sociologists should endeavor to make plain the extent to which sociological theories (at whatever level of aggregation) are dependent on specific psychological assumptions and what these psychological assumptions are (Tooby and Cosmides 1992; for renditions of this point by sociologists, see Cicourel 1974; Collins 1981). Depending on the theory or its applications, these could be assumptions about the motivation of others, their biases, their capacity to obtain information from the surrounding environment, their capacity to process such information, their capacity to remember, and their reaction to attempts at manipulation, to give just a few examples. Some kind of need for sociality is one common theme (Maryanski and Turner 1992). For example, one effort to make explicit the premises about human motivation of several prominent sociological theories reported the following postulated human needs: "for sense of group inclusion," "for sense of trust," "for sense of ontological security," "for sense of facticity," "to avoid diffuse anxiety," "to sustain self-conception" and "for symbolic/material gratification" (Turner 1987: 24). When such presumptions are made explicit, they can more easily be subjected to scrutiny, and they may be refined and elaborated by the findings of psychology.

Lack of precision about the interaction of mind and social environments leaves social science open to Tooby and Cosmides's (1992) charge of "incoherent environmentalism." Writing about social life is often plagued by an imprecise language of action that draws upon a congeries of different idioms, which is easy for critics to lampoon:

> Contemporary social commentary rests on archaic conceptions of the mind. Victims burst under pressure, boys are conditioned to do this, women are brainwashed to do that, girls are taught to be such-and-such. Where do these explanations come from? From the 19th century hydraulic model of Freud, the drooling dogs and key-pressing vermin of behaviorism, the mind-control plots of bad cold war movies, the wide-eyed obedient children of Father Knows Best (Pinker 1997: 57).

Along the same lines, sociologists have also long been criticized for imprecise and "oversocializing" language for talking about how members come to exhibit normatively appropriate behavior (Wrong 1961), of which recent examples are not hard to come by, e.g., "All societies train their members to balance closeness and distance, the interests of self and other" (Scheff 1994: 41). Either the computational idiom of cognitive psychology or the utility-maximization idiom of rational actor theory might provide sociology with a more precise language for talking about individual action and one that makes it easier to draw connections with other disciplines (for a review of sociological rational choice theory, see Hechter and Kanazawa 1997; for a discussion of the contributions of cognitive psychology to the study of culture, see DiMaggio 1997). It might provide the means for more thorough discussions of the mechanisms implied by commonly invoked explanatory concepts like "learning," "culture," and "socialization," especially when compared to the behavioristic models that have informed much of sociological exchange theory (following Homans 1961, 1964).

At the same time, even though all this points toward the potential value of sociologists attending more to cognitive considerations, it remains to be seen whether and to what extent this potential value would be further enhanced by an evolutionary perspective on cognition. In other words, sociologists might decide that they need more psychology, but not more evolutionary psychology.[6] Again, Darwinian approaches provide a critique that is perhaps particularly useful when pitted against an extreme view that there is no specificity to mechanisms of the mind. Once the role for such specificity is granted, then the value of reasoning about our evolutionary past for uncovering what is this specificity is less apparent.

[6] Also, sociologists may be able to contribute importantly to explicating the sociality of cognition: the distribution of knowledge among multiple individuals and the distribution of devices that serve as cognitive artifacts (D'Andrade 1995; Howard 1995; Clark 1997).

Also, we should keep in mind that in the study of social organization and social institutions, some abstraction from the actual psychological complexity of actors is necessary. One of the defining features of evolutionary psychology as a new science of the mind is its emphasis on the domain-specificity of mental mechanisms to the level of particular adaptive problems. To the extent to which the mind is domain-specific, it could provide a grounds for understanding "natural" features of social categorization and cognition (how our mind divides up the world, how it reasons within those divisions) (Hirschfeld and Gelman 1994; Howard 1995 provides a review of social cognition from a sociological perspective). As I described in Chapter 3, however, this extreme view of the modularity of mind has met with protest, including the criticisms that it is incompatible with developmental neurobiology and that there is not enough genetic information available to specify the very large number of cognitive adaptations that the model implies (Karmiloff-Smith 2000; Buller and Hardcastle 2000; Ehrlich 2000; Buller 2000). Conversely, then, a more modest architectural view of how evolution has shaped the mind would seem more warranted, but this still permits much room for the proposal that the mind intrinsically processes information about some types of information about some types of situations and social relationships differently than others (D'Andrade 1995; DiMaggio 1997).

Even if the highly modularized view were correct, however, some simplification of it would be required for building tractable models of social dynamics. In this regard, one promising direction for new theoretical work might be that which attempts to use evolutionary psychology to elaborate rational choice models of the actor, by contributing to the formulation of basic propositions regarding human preferences: who we prefer as mates and friends, what we prefer as foods and sources of pleasure (Hodgson 1993; Hirshleifer

1977). Kanazawa (forthcoming) explicitly attempts to do so while drawing on the insights of Coleman's (1990) sociological version of rational choice theory. Importantly, evolutionary psychology also may provide valuable insights for understanding mean sex differences in values and preferences (Kanazawa forthcoming; Buss 1999). A challenge for such efforts is to show how basic preferences may be rooted in our evolutionary history and yet still be malleable to construction in and through social processes (see, e.g., DiMaggio 1990; Bourdieu 1985).

Evolutionary insights might be used to try to understand why social actors deviate from the predictions of a rational choice theory. Evolutionary psychologists and others have attempted to harness Darwinian reasoning to improve our understanding of such classical topics in the study of the economic actor as attitudes toward risk (Daly and Wilson 1988; Hawkes 1990), judgments under uncertainty (Cosmides and Tooby 1996), shifts in valuation (Kenrick et al. 1994), and time horizons (Rogers 1994). Robert Frank (1988) has tried to use evolutionary considerations to explain the rational functions of seemingly "irrational" emotions like anger and love. In his view, these emotions allow actors to make and enforce commitments that an actor who was perfectly rational and known to be perfectly rational would not. One example is the effectiveness of threats, where a reputation for being a hothead may make a threat seem credible even when it would be enormously (irrationally) costly to the threatening party to actually carry out (for a discussion of credible commitments in the framework of the "new institutionalism" of social science, see Ingram and Clay 2000). For sociologists, the benefits of proposed refinements rests on how well they can point the way toward new understanding of phenomena.

In addition, however, the evolutionary psychological emphasis that humans are animals may help to underscore the fallibilities of real actors relative to these abstractions: for example, while evolutionary reasoning may augment a version of rational choice theory, it might also contribute to understanding why some amount of inconsistency and capriciousness of action relative to this theory is inevitable. An evolutionary psychology, with its emphasis on the multiplicity of selection pressures involved in the creation of the mind, should lead to some skepticism toward the fit of any "metanarrative" of the mind—any simple model based on few assumptions—for fitting individual behavior precisely. Although, as noted, the strong modularity of evolutionary psychology can lead to this being taken too far, some evolutionary psychologists have shown enthusiasm for limited, heuristic-based models of adaptive behavior, such as that captured by the concepts of "bounded rationality," "ecological rationality," and "social rationality" (see the "adaptive toolbox" approach of Gigerenzer et al. 1999). The potential value of evolutionary considerations for an individual-level model of action also calls into question the common assumption that the same model can be used for both individual and organizational actors (see Cook and Whitmeyer 1992: 117).

To say that a sociological theory must be *psychologically honest* is not to provide a paean to methodological individualism, but it does suggest the benefit to sociology of thinking more carefully about the place of individuals within its theories. Sociologists are known for their emphasis on the "emergence" and "irreducibility" of social phenomena, and their skepticism toward the "reductionistic" explanations of others (see critiques by van den Berghe 1990; Lopreato and Crippen 1999). These emphases have historical roots: sociology has long had to defend its disciplinary independence from the potential usurpation of other disciplines, including psychology. The history of social psychology in sociology has been shaped "by the need to answer charges of psychological reductionism" (Morgan and Schwalbe 1990: 149; see House 1977). Going even further back, Durkheim's *Rules of Sociological Method* is said to have been written specifically "as a tactic to secure a chair for sociology at the Sorbonne against the opposition of the psychologists" (Rose 2000: 120).[7]

While this stance has been valuable for establishing disciplinary independence, however, it has also left sociology vulnerable to the charge that sociologists do not believe that their theories have to be consonant with the facts of psychology and biology. In arguing for evolutionary psychology over the Standard Social Science Model, Cosmides, Tooby, and Barkow (1992) write: "It is, perhaps, one of the astonishing features of intellectual life in our century that cross-disciplinary consistency should be treated as a radical claim in need of defense." Clearer articulation of the microfoundations of its theory, as well as of its justification for when and why these microfoundations are insufficient to explain macrophenomena, may strengthen the response of sociologists to this kind of criticism (for a lucid discussion of the inadequacy of methodological individualism in the context of Marxian analysis, see Wright, Levine, and Sober 1987).

As another matter, critiques by evolutionary psychology also remind sociology that its models must be *evolutionarily honest*. Although many sociologists may evince considerable skepticism about evolutionary psychology, we should not be skeptical about evolutionary theory as a correct account of human origins. Consequently, whatever model of the actor that sociologists find most useful in their own inquiries, we are presuming an evolutionary history that produced in humans a mind for which our model is able serve as an

[7] Durkheim's *Rules* serves as Tooby and Cosmides's (1992) principal sociological source in the formulation of their Standard Social Science Model characterization of prevailing thought in social science.

adequate abstraction. This implies that every proposed psychology is an evolutionary psychology, but this does not by itself mean that sociologists need to reflect deeply on the lives of our savanna dwelling ancestors in order to develop practically adequate models of human behavior. Indeed, in the case studies, we found reason to evince a cautious stance toward claims that certain models of the actor are evolutionarily implausible or impossible (Chapters 3, 4, and 6). I assert that the capacity to make such judgments about evolutionary plausibility is limited and cause for caution, but not that it cannot be done. Evolutionary grounds do make it difficult to justify a species-typical model of mind in which parents are indifferent to the outcomes of their children or in which people's behavior is partly driven by the whisperings of some innate "death instinct." The problem is that deciding what follows straightforwardly from evolutionary principles and what is more controversial is often not easily determined, which is especially unfortunate for that which cannot be adjudged empirically. In any event, such plausibility judgments should never be a substitute for actual behavioral observation: if a human behavior pattern exists, of course, then the psychological machinery responsible for producing it must have been evolutionarily possible.

This last point has resonated in my own mind by thinking about each half of the term evolutionary psychology as one of a pair of "dueling canvases" onto which the knowledge afforded by inquiry is painted. On the "evolutionary" canvas we have knowledge of the course and conditions of human evolutionary history, while on the "psychology" canvas we have knowledge of the innate structure of the human mind. The canvases are linked such that painting on one of them allows us also to fill in some of the other: whatever we learn about evolutionary history constrains the possible designs of the mind, while whatever we learn about the mind constrains what could have been possible evolutionary histories. One can posit a profound role for the innate structure of mind and yet still see only a modest role that working on the "evolutionary" canvas has to play for illuminating the content of our "psychology." One can see the contribution going vastly more the other way, from an explication of our psychology to an understanding of what our evolutionary history must have been like (see Grantham and Nichols 1999). This provides a role for evolutionary psychologists as part of the general pursuit of understanding, but limits the explicit contributions of the Darwinian approach for the rest of psychology and the rest of social science.

Part of the difficulty for evolutionary approaches in this regard is that psychological functionalism is often possible without specific reference to our Pleistocene past. Tooby and Cosmides (1989) take as a prominent orienting exemplar for the program of evolutionary psychology the (psychological) functionalist approach of David Marr's work on vision, but, as critical work on evolutionary psychology has pointed out, Marr's work actually makes little use of specifically evolutionary reasoning (Shapiro and Epstein 1998). Consider the following passage about the relationship between mood and attention (Schwarz 1998: 245):

> [O]ur moods may reflect the state of our environment, and being in a bad mood signals a problematic situation, whereas being in a good mood signals a benign situation. If so, we may expect that the motivated tactician's [i.e., the human actor's] thought processes are tuned to meet the situational requirement signaled by these feelings. When negative feelings signal a problematic situation, the individual is likely to attend to the details at hand, investing the effort necessary for a careful analysis. In contrast, when positive feelings signal a benign situation, the individual may see little need to engage in cognitive effort, unless this is required by other current goals.

If the psychological finding described in this paragraph is general, then the above argument could be easily recast in adaptationist terms, simply by suggesting that the relationship between mood and cognitive effort is sufficiently consequential that those whose psychologies worked in this functional way outreproduced those whose psychologies does not. An evolutionary psychologist could argue that such an extension improves the theory by making the evolutionary assumptions of the explanation explicit, while a critic could respond that it only adds unnecessary and untestable propositions. As I suggest in the next section, for many uses to which cognitive propositions may be put to use by sociologists, a psychologically functionalist metaphor like the "motivated tactician" (Schwarz 1998) may be useful, while one may be indifferent to (while not dismissive or dubious of) the correctness of a specific evolutionary historical account for why the metaphor works.

FORM-TO-CONSEQUENCE THINKING

In the remainder of this chapter, I wish to make some brief proposals toward how sociologists may make use of evolutionary ideas as a starting point in work of their own, including discussions of the study of cultural evolution and social manipulations. Perhaps ironically, however, my first point is that sociological inquiry can take away ideas from evolutionary approaches but can still remain indifferent to the question of the extent to which things are "genetically based" or "culturally based." Daly and Wilson (1999) have provided clear evidence that the stepparent-stepchild relationship is more conflict-laden than the relationship between biological parents and children. We can attempt to untangle the debates about the innateness of this effect, but we can also remain *indifferent* to these and instead try to further unpack the factors that affect the difficulties of step-relationships. Does the age of the child matter? Does the proximity of the other biological parent matter? In so doing, our interest in children may be explicitly provoked by Daly and Wilson's research, marking an important contribution of evolutionary approaches—as a source of inspiration for ideas or questions for research to pursue. Yet, as presaged by our earlier discussion of testing evolutionary predictions, our inquiry might neither depend upon or speak to the validity of any specific assertions about the genetic basis of differential treatment of stepchildren and biological children.

By "indifference," I mean that the researcher recognizes the possibility of a broad range of evolutionary psychological, sociocultural, or other theories could be invoked to explain why a population exhibits the propensity under investigation, but the researcher proposes that, for the purposes of one's current inquiry, there is no need to privilege any particular theory of its origins, beyond perhaps giving credit to any theory that provided inspiration for the inquiry. A large class of potential inquiries for which such a stance of indifference may be useful and prudent are those that examine the social consequences of some psychological propensity. Evolutionary psychologists often engage in form-to-function reasoning (Tooby and Cosmides 1992), from the apparent form of a psychological mechanism to hypotheses about its ancestral function—what I described in the last section as the move from the "psychological" canvas to the "evolutionary" one. The tactic I am describing would proceed from the form of psychological to its contemporary consequences, or from the "psychology" perspective to a third canvas that contains our portrait of some aspect of social life (which is, of course, not to say that an examination of social life does not at the same time provide many useful insights into the character of our psychology) (for a review of comparative social psychology, see Miller-Loessi 1995). Meanwhile, the appropriateness of work on the "evolutionary" canvas is not called into question, but one acknowledges that the success of their effort has little bearing and is not directly affected by one's research project.

To consider one example, Bowles and Gintis (1999) discuss experimental and anthropological evidence for the existence of a behavioral disposition they call *strong reciprocity*: "a propensity to cooperate and share with others similarly disposed, and a willingness to punish those who violate cooperative and other social norms—even when such sharing and punishing is personally costly" (see also Hoffman, McCabe, and Smith 1998; cf. also social psychological work on "concession tactics" in bargaining, see Lawler and Ford 1995: 246-47). Where strong reciprocity differs from models of reciprocal altruism or cooperation influential among both evolutionary psychologists and economists is in its emphasis on the motivation to punish defectors even when the costs of punishment cannot be offset by higher payoffs in the future.

One line of evidence for this propensity comes from "ultimatum game" experiments. An anonymous subject (the "proposer") is allowed to make a "one-shot" proposal to split $10 in some manner with a second anonymous subject (the "recipient"), after which the recipient will then have the option to either accept or reject the proposal. If the proposal is accepted, both parties get the agreed-upon split; if it is rejected, neither party gets anything. Because the actors do not know each other's identities and this is a one-time interaction, a strict rational choice theory would predict that the "proposer" should make offer a split very favorable to himself (e.g., $9.99 vs. $.01) and the "recipient" should accept it, since some payoff is better than none. In practice, recipients regularly reject what they perceive to be "unfair" offers (especially offers below $3), even though it means that they get nothing. Indeed, proposers typically offer equal or nearly equal splits, partly (but only partly) because they anticipate that if they make a low offer it will be rejected. We can say "partly (but only partly)" because of "dictator" game experiments where the recipient is simply given the split offered by the proposer, rather than having the option of accepting or rejecting it; subjects are less likely to offer equal or nearly-equal divisions in the dictator game than the ultimatum game, but a substantial number still do (see Gintis 2000; Hoffman, McCabe, and Smith 1998). A similar dynamic is observed in experiments involving contributions to public goods, where subjects are willing to incur costs to punish noncontributors even when nothing monetary can be gained from doing so (see Bowles and Gintis 2000b).

Once such evidence for a behavioral propensity is on the table, there are several directions that one can proceed. One is to continue elaborating the conditions that affect the strength of the willingness to punish (e.g., Hoffman et al. 1994). Another is to determine the extent to which the tendency for strong reciprocity is observed across cultures (Gintis 2000). Still another may be to try to develop an evolutionary rationale for how a propensity for strong reciprocity may have come to evolve (Bowles and Gintis 2000a).

A different direction that might be of more interest to sociologists, however, might be to take the propensity as given (at least as a regular feature of the members of a particular society) and ask what effects it might have for social life. Bowles and Gintis (1999; drawing upon Gilens 1999) apply the idea of strong reciprocity to rising public sentiment against welfare. They propose that:

> "egalitarian policies that reward people are independent of whether and how much they contribute to society are considered unfair and are not supported, even if the intended recipients are otherwise worthy of support, and even if the incidence of non-contribution in the target population is rather low. This would explain the opposition to many welfare measures for the poor, particularly since such measures are thought to have promoted various social pathologies. At the same time it explains the continuing support for social security and medicare in the United States, since the public perception is that the recipients are "deserving" and the policies do not support what are considered anti-social behaviors. The public goods experiments are also consistent with the notion that tax resistance by the nonwealthy may stem from their perception that the well-to-do are not paying their fair share."

While the basic orientations of humans may be benign and even generous, they can be motivated to punish those who are perceived as not doing their fair share or who are seen as violating prosocial norms. Consequently, welfare is not supported when its recipients are perceived as shirking or an engaged in normatively disapproved behavior. By itself, this conclusion may not seem a particularly profound observation about welfare; what makes it more interesting is the possible connection to a more basic psychological propensity, a propensity that can also be theorized to have consequences for other domains. For example, Bowles and Gintis (2000b) apply strong reciprocity to their discussion of social capital and community governance.

Their analysis also implies that "human selfishness" is not needed to explain public sentiment against welfare. Further, if studies do show that strong reciprocity is a deeply-rooted propensity, then an implication is that those interested in maintaining the welfare state might do better to devise ways to work with the disposition to strong reciprocity than to try to change punitive attitudes toward shirkers or norm-violators: "Egalitarians have been successful in appealing to more elevated human motives precisely when they have shown that dominant institutions violate norms of reciprocity" (Bowles and Gintis 1999). The obverse, whose implications are not traced out by Bowles and Gintis, is that those interested in seeing the decline of the welfare state, for whatever reason, may find much to be gained from fostering the image of welfare recipients as norm-violators. In other words, the image of the welfare recipient becomes an important, contested part of the political debate about welfare precisely through its relationship to a basic psychological propensity. Among other things available for sociological inspection are the structure of competing representations of welfare recipients, and the attempt to understand why some representations have prevailed over others. We have then a possible locus for a contest of social manipulation between political opponents, with what comprises an effective move in the contest explicable partly in terms of basic psychological mechanisms, a sociological and historical story that has a psychological propensity as part of its necessary cast of characters.

One can imagine further inquiries that are centered on tracing out the implications of a fundamental motivation to punish those seen as violating the terms of prosociality. Additionally, the application of social reciprocity to social problems might also provide a source of insights for further psychological experiments that may elaborate our understanding of strong reciprocity. In the same way, then, that evolutionary approaches propose that "selection thinking" can point the way toward insights into the operation of mechanisms that would not otherwise be noticed or discovered, so too may sociological thinking about the consequences of mechanisms.

Of course, the idea of taking a few psychological propositions and tracing out their social effects in some domains is itself nothing new. In the face of proliferating Darwinian hypotheses and public interest in them, however, what is important to recognize is that these effects can often be examined with indifference to their origins, avoiding contentious issues if they are not directly relevant to the researcher's focus anyway. Strong reciprocity provides another exemplar, to my mind, because its theoretical underpinnings are so closely tied to the evolutionary game theoretic work on reciprocal altruism and cooperation (Trivers 1971; Axelrod 1984). As such, the study of its consequences may serve as an example of sociological inquiry beginning from premises developed through evolutionary thinking.

As another example, we may consider the marriage squeeze (Guttentag and Secord 1983). Marriage patterns in many societies reveal optimal matching of preferences in marriage markets results in husbands being a few years older than wives. When birth rates are rising, then younger cohorts are larger than slightly older cohorts, which leads to an imbalance in sex ratios of persons of "marrying" age that is favorable toward men. Alternatively, when birth rates are declining, then younger cohorts are smaller than slightly older cohorts and there is a marriage squeeze that favors women over men. Other events, such as wars and migrations, can also alter sex ratios, usually in ways that favor men over women. Guttentag and Secord (1983) have famously argued that periods where the marriage market favors men lead to loosening of the standards of sexual behavior for women, while periods where the marriage market favors women lead to a tightening of the marriage market. Evolutionary psychologists can provide theoretical explanations both for why optimal matching yields older men and younger women, rather than the reverse, and why social changes can be more easily represented as a raising or lowering of the sexual standards of women, rather than changes in the standards of men (Buss 1994 talks specifically about the marriage squeeze in this light). Someone interested in marriage markets might thus find the evolutionary perspective useful. However, whether ultimate causes is something that the research needs to address, or whether it is something to be "bracketed out," depends on the purposes of the research.

To say that sociologists can often be indifferent to questions of ultimate origins is not to imply a disingenuous stance, in which sociologists plead indifference whenever they might have to "fess up" to evolutionary psychologists being right. The idea is not to provide a principled grounds for "the intellectual equivalent of closing the shutters and shouting for the police," as Douglas Kenrick (1995: 56) describes the reaction of some academics to evolutionary psychology. Instead, as I have already argued, sociology has many reasons why it should not be afraid of giving cognitive adaptations their due, and, when relevant, sociologists should approach the different ways that genes and environments can interact with more of an inquiring spirit than the discipline has a history of doing. Yet, we must also recognize that there are several interrelated questions here: what is the behavioral pattern? what is the psychological mechanism? what are its effects? in what way is the mechanism genetically specified? what is the evolutionary history responsible for this genetic specification? In examining specific issues, we want neither to claim that our data can speak to questions that they cannot nor do we want to claim that our theories rest of particular positions regarding innateness or evolutionary history if they do not.

A desire to find "lines of indifference" that sociologists can draw is partly driven by practicalities. While I feel it is vital for sociology to explore its psychological and biological assumptions, once we start talking about the interrelationship of evolved biology, psychology, and sociology, the number of potentially relevant disciplines for a chain of theoretical reasoning becomes quickly enormous. Over the course of working on this project, I have regularly found myself wishing I knew more about anthropology, biology, cognitive science, demography, economics, game theory, genetics, history, human sexuality, mathematics, neuropsychology, philosophy of mind, philosophy of science, political science, primatology, social psychology, social theory, statistics, zoology, and, most recurrently, sociology. Whatever else we think about the role that "selection thinking" should play in a discipline like sociology, a practical matter is that training in one topic takes away from training in another, and for working scholars reading time is a precious commodity that can only be scattered so widely. It is easy for someone who believes that sociology should attend more to biology to pronounce, as Lee Ellis (1996: 25) does, that a Ph.D. in sociology "should never be conferred if a sociologist cannot demonstrate some graduate level proficiency" in evolutionary theory, genetics, and neurology/neurochemistry. But, such a premise seems so unworkable that it may make the entire premise of greater cross-disciplinary integration seem hopeless. A partial solution may be a clarification and circumscription of the problems considered in any inquiry, including the extent to which our inquiry speaks to ultimate causes.

CULTURE AND CULTURAL EVOLUTION

> *"To deny the relevance of sociobiological theory to human behavior, one must either attack neo-Darwinian theory as a whole (not a promising enterprise), or be prepared to show how models that take culture into account actually generate more satisfactory hypotheses about human behavior without violating the assumption of natural origins. That the latter can be done is an argument we will defend repeatedly."*
>
> —Robert Boyd and Peter J. Richerson, *Culture and the Evolutionary Process*, 1985

The wide differences between the explanatory idioms that are standard practice for evolutionary psychology and traditional social science may seem to imply that the active use of insights from the former requires a rejection of most or all of the latter. While calls for "Darwinizing" social science might seem to advocate just such a step (e.g., Lopreato and Crippen 1999; Thornhill and Palmer 2000), the desire for a more modest stance requires greater efforts toward integration. A central point of consideration is how evolutionary insights can be used in conjunction with a theoretical perspective that also gives an extensive role to culture, and a place to begin here is with how evolutionary psychologists themselves have considered culture. As noted, evolutionary psychologists have used the phrase Standard Social Science Model (or its acronym, SSSM) as a shorthand for the theory of mind that they believe implicitly or explicitly guides most of the nonevolutionary work in the human sciences. As I discussed in Chapter 1, this model has its origins in the work of Boas and other early anthropologists (and in Locke's depiction of the mind as a "blank slate" before that), and it is exemplified by Durkheim's ([1895] 1962) stance that human nature consists only of "very general attitudes" and "vague and consequently plastic predispositions" that society subsequently "molds and transforms."

We have seen that evolutionary psychologists reject the premise that the mind is comprised of vague or general dispositions, proposing instead a plethora of specialized innate mechanisms that are linked to very specific domains of human behavior (see Chapter 2). As noted, evolutionary psychologists have criticized the laziness with which concepts like "culture" and "learning" are used by anthropologists and sociologists (Tooby and Cosmides 1992). One criticism is that explanations that rely on some notion of learning (as in learning the norms of values of a society) are incomplete without an account of the underlying psychological mechanisms that are involved. Buss (1995: 13) states this point bluntly:

> "'Culture', 'learning', and 'socialization' do not constitute explanations, let along alternative explanations to those anchored in evolutionary psychology. Instead, they represent human phenomena that require explanation. The required explanation must have a description of the underlying evolved psychological mechanisms at its core."

As noted, evolutionary psychologists speculate that learning or acquiring culture is not a relatively unitary process but instead will turn out to be an array of heterogeneous and largely independent processes that are controlled by different cognitive mechanisms (Tooby and Cosmides 1992). The evolutionary psychological critique resonates with the demands of others that "scholars must clarify the cognitive presuppositions behind their theories of what culture does and what people do with it, and the fundamental concepts and units of analysis" (DiMaggio 1997: 263). Efforts underway in this regard have been summarized for anthropology (D'Andrade 1995) and sociology (DiMaggio 1997), with the efforts of anthropology being much more developed than those of sociology.

An important implication of this view is that sociobiologists and evolutionary psychologists maintain that learning is *biased*: some things can be learned more easily than others, and the ease with which something can be learned is a directly a function of the structure of our evolved minds (Lumsden and Wilson 1981). A commonly cited example is that children apparently can be conditioned to fear things that likely posed real and present dangers in our evolutionary past—spiders, snakes, heights, and darkness—much more quickly than they can be conditioned to fear arbitrary objects, like automobiles or electrical outlets (Seligman and Hager 1972; Marks 1987). Likewise, if an evolved language acquisition device does help children swiftly acquire fluency in natural human languages, then we would expect language acquisition to be much slower or even impossible for an infant abducted by alien speakers of an extraterrestrial tongue. The obvious extension of this reason to human culture is that while cultural phenomena may exhibit considerable independence from whatever "leash" is provided by our evolutionary biology, some cultural variants may be more easily produced and sustained by others. For example, horror films may make use of conventions and plot devices that are consequences of the particular history of the genre, but the success and repetition of some devices rather than others may draw upon evolved tendencies to find some things (darkness, snakes) more intrinsically scary than others.

Evolutionary psychology criticizes other social sciences for granting too much explanatory power to socialization or to culture. They contend that many of the behaviors that social scientists have claimed are "taught" to individuals during socialization are actually manifestations of our evolved human nature. Pinker (1997: 57) provides a mocking laundry list of examples here:

> "Mainstream social critics can state any absurdity if it fits the Standard Social Science Model. Little boys are encouraged to argue and fight. Children learn to associate sweets with pleasure because parents use sweets as a reward for eating spinach. Teenagers compete in looks and dress because they follow the example set by spelling bees and award ceremonies. Men are socialized into believing that the goal of sex is an orgasm. Eighty-year old women are considered less physically attractive than twenty-year olds because our phallic culture has turned the young girl into the cult object of desire. It's not just that there is no evidence for these astonishing claims, but it is hard to credit that the authors, deep down, believe them themselves." [8]

As Pinker's list suggests, the easiest and most frequent targets of scorn from evolutionary psychologists are culture-specific explanations that scholars have provided for phenomenon that are universal or nearly universal features of human societies. Another commonly-cited example in this vein concerns the disparity in homicide rates between men and women. In the United States and Canada, men perpetrate roughly five times as many murders as women. Daly and Wilson (1988; Wilson and Daly 1985) have offered an evolutionary account of why this disparity occurs. They contrast their theory with various attempts to explain the difference in terms of such things as "the theme of masculinity in American culture" (Wolfgang 1978: 87; see also Wolfgang 1958: 163) and the emphasis on "softness and gentleness" in North American female "cultural conditioning" (Chimbos 1978). Daly and Wilson (1989) contend that any explanation that draws on specific features of American or Canadian culture must be incorrect, as they have examined records on many different societies and have found a wide difference between the sexes in every one. In fact, they claim

[8] Pinker goes on to suggest that the scholars who offer them may do so more out of deference to the moral authority of the SSSM (because of its perceived liberal implications) than because they believe these explanations to be correct: "These kinds of claims are uttered without concern for whether they are true; they are part of the secular catechism of our age."

that male and female rates of homicidal violence are actually *closer to one another* in contemporary North America than in any other society they have examined.

The evolutionary psychological critique provides a challenge for those sociologists who study socialization (Corsaro and Eder 1995), perhaps especially for those who may adopt an extreme position regarding the socialization of men and women into gender roles (e.g., work following from Chodorow 1978; see also Blum 1997; Udry 2000; Colapinto 2000 for discussions more on the proximate biological causes of sex differences in behavior). More generally, evolutionary psychologists recommend a much more circumscribed role for culture than what it presently commands in many areas of social science. Even so, few wish to follow anthropologist Laura Betzig's (1997: 2; see also Betzig 1998) proclamation that "I, personally, find culture unnecessary," but instead evolutionary psychologists have sought to provide a place for culture in their theorizing.

A specific contribution of evolutionary psychology may be its notion of "evoked culture," in which some cultural differences are seen as the product of the interaction of environmental differences with the particular environments in which a group resides (Tooby and Cosmides 1992). For example, among the San of the Kalahari desert, Cashdan (1989) has observed that the extent of egalitarianism in the group is closely related to the variance in the food supply. The higher the amount of variance, the more egalitarian the group, which evolutionary psychologists have suggested is the result of psychological mechanisms that are invoked in high variance circumstances and that promote communal food sharing behavior (Buss 1999: 404-406; Cosmides and Tooby 1992 discuss a similar phenomenon among the Ache of Paraguay).[9] At the same time, "selection thinking" may not seem necessary for

[9] Lenski (1966), among others, have provided discussions of egalitarianism and generalized reciprocity among hunter-gatherers that do not find it necessary to postulate specialized cognitive mechanisms in order to account

deriving the conclusion that potential food shortages spurs risk-averse behavior. A more dramatic contribution of evolutionary psychological thought would be to reveal relationships between environmental variables and cultural phenomena that could not be so easily explained in rational-choice-like terms. Gangestad and Buss (1993) suggest just such a relationship when they report a positive association between the prevalence of parasites in a culture and the emphasis that culture places on physical attractiveness (which can be explained in evolutionary terms by physical attractiveness being more strongly correlated with health in societies with high pathogen prevalence). If more associations like this are found and shown not to be spurious, then it might suggest a valuable role for evolutionary psychology in understanding some of the ways that the norms of societies vary.

Evoked culture, however, cannot account for the bulk of what is commonly considered culture, which, albeit variously defined, comprise practices and knowledge that are communicated from person to person by imitation or instruction—"an ensemble of customs and technologies" (Cavalli-Sforza 2000: 173; Tooby and Cosmides [1992] refer to this as "epidemiological culture"). The chapter on "Evolutionary Approaches to Culture" in the *Handbook of Evolutionary Psychology* compares no fewer than seven full-scale attempts to model the relationship between evolved biology and culture, and even this ambitious review could be faulted for being incomplete (Janicki and Krebs 1998). These models all take for granted that humans evolved the capacity for culture because of the fitness advantages it provided to our ancestors. Some have attempted to use concepts and mathematical tools from evolutionary biology to try to create models of cultural transmission.

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for the phenomenon. An evolutionary psychology could retort, perhaps, that they are putting psychological flesh on the self-interest that Lenski sees as adequate to explain much of the observed behavior.

Most famously, in deliberate analogy to the gene, Dawkins (1976) proposed the concept of *meme* to refer to the basic unit of cultural selection; his examples of memes include "tunes, ideas, catch-phrases, clothes fashions, ways of making pots or building arches" (p. 192).[10] If we think of evolution by natural selection as a process of the differential replication of elements over time, in which the probability of replication is affected by characteristics of the element, then Dawkins argues that the replication of memes in a culture can be thought of in the same way as the replication of genes in a gene pool. Memes are conceived as parasitizing the brains of their human hosts, and the most successful memes are those whose features best facilitate their spread from one person to another. In the same way that a virus contributes to its spread by causing its host to sneeze, memes are thought to induce transmission by affecting behavior in ways that spur their own replication (like a religious call to proselytize to others [see Brown 1999 on the use of the "meme" concept to criticize religion]). For evolutionary scholars, memes provide a way of understanding why people may hold ideas that cause them to behave maladaptively, just as a parasite can cause its host to behave maladaptively. Memes have generated considerable enthusiasm among some, who see "memetics" as a way of using the "universal acid" of Darwinian selection to dissolve some of the mysteries of culture (Lynch 1996; Blackmore 1999). Yet, many of the more gushing proclamations of the "new science" of memetics are easily criticized (Midgley 2000), especially those that are carried forth in virtual innocence of the long history of other approaches to cultural analysis and work on cultural innovation and diffusion (for a sociological review of the literature on diffusion, see Strang and Soule 1998).

[10] Although Dawkins's "meme" concept might seem hopelessly "popular," especially when juxtaposed with more serious models, it has inspired a number of continuing, serious efforts that see Dawkins writing as foundational to their own (e.g., Blackmore 1999).

While simple ideas about "memes" are unlikely to prove useful for the study of the diffusion and distribution of cultural practices, more serious interest may be warranted those models that apply the logic and mathematics of selection or epidemiology to rates of change in the frequency of cultural variants (Boyd and Richerson 1985; Durham 1991; Sperber 1996). These authors may use the concept of "meme," but unlike the popular enthusiasts of memetics, they do not take the analogies to genes or viruses too seriously. Importantly also, these treatments give as much consideration to the disanalogies between cultural and genetic evolution as they do to what the two processes share. Dual inheritance theories see culture and genes as "providing separate (but linked) systems of inheritance, variation, and fitness effects—and hence of distinct but interacting evolutionary change." (Smith 1999: 31; the most prominent examples are Boyd and Richerson 1985; Richerson and Boyd 1992). Genetic evolution can be seen as shaping cultural evolution through its effects on human psychology, which leads to biases favoring certain cultural forms over others. Cultural evolution, on the other hand, has affected genetic evolution by providing culture-based solutions to some adaptive problems while creating others. The cultural development of dairying, for example, apparently created a selection advantage from those who could better tolerate lactose, which is suggested today in the reduced incidence of lactose intolerance in persons whose ancestors are from regions where dairying occurred (Jones 1992: 285-86).

Dual inheritance or other "coevolutionary" models may provide a particularly promising point of integration for sociologists because some grant a powerful role to the effects of social norms. For one thing, social norms can diminish behavioral variation within a group, reducing the effects of natural selection on individual differences, while preserving variation between groups with different norms (Sober and Wilson 1998). Analyses of mathematical models also reveal that sanctions for violating prosocial norms, especially when accompanied by prospective sanctions for those who fail to punish, can promote the proliferation of altruistic behaviors within the group, by making the cost of defection from the norm greater than the cost of producing the prosocial behaviors (Boyd and Richerson 1992; cf. sociological work by Heckathorn 1990, 1993). As Sober and Wilson write (1998: 152): "Virtually *any* behavior can become stable within a social group if it is sufficiently buttressed by social norms. The costs and benefits that are naturally associated with the behavior are simply overwhelmed by the rewards and punishments that are attached by the social norms" [citing Boyd and Richerson 1992]. Theorists propose that processes of cultural evolution may have favored norms and other cultural practices that allow society to outcompete its neighbors or allow groups within a society to dominate others (Boyd and Richerson 1985; Sober and Wilson 1999).

While norms can certainly have maladaptive consequences for individual and groups, conforming to norms can often produce adaptive behaviors that would be beyond the individual to derive on his own, yielding perhaps a bias for conformity as an intrinsic part of human psychology (Boyd and Richerson 1985; Simon 1990; Henrich and Boyd 1998). Boyd and Richerson (1985) also discuss the advantages of attempting to mimic traits of those who display culturally-defined symbols of higher prestige and the potential consequences of this dynamic for the cultural evolution of systems of symbols.[11] Not only may sociologists be heartened to see some evolutionary approaches evincing strong appreciation of the importance of culture and social norms, but developments in these areas might prove useful for the sociological study of conformity and social control, at the least as

[11] At the same time, sociological studies have shown that the tendency to use more prestigious models is not just an individual-level matter but also describes some of the conduct of firms (e.g., Fligstein 1990; Han 1994).

a way of understanding why people conform and why they imitate the behavior of the models that they do. Coevolutionary models, which consider the group-level consequences over time for enforcement of norms, may prove much more promising in this regard than more paradigmatic evolutionary psychology, in which, according to one critic, "the committment to individualism is so strong that there is no need to discuss society as anything more than what individuals do to each other in the course of maximizing their inclusive fitness" (Wilson 1999: 281).

A general contribution of coevolutionary models is in providing a more formal way of understanding why granting a powerful role to culture and norms need not contradict a Darwinian account of human origins. Yet, while doing so, such work also points to the potential importance of our biological evolution for understanding why some cultural variants are more readily observed than others. Thinking of genes and culture as separate systems of inheritance may also provide means for thinking about tensions between these systems, manifested as an tension between personal desires (reflecting genetic fitness) and behavioral expectations in social contexts (reflecting cultural fitness). The models do not provide some answer to the question of how much culture is constrained by the structure of our evolved psychology and how much it is autonomous of it; Boyd and Richerson's (1985) model is deliberately broad enough to accommodate a broad range of possibilities depending on features of our evolutionary history whose plausibility can only be inferred from knowledge of our psychology and culture (again, reasoning from details of mind and society to a better picture of our evolutionary history). The utilization of the mathematical models developed by dual inheritance theorists for studying contemporary cultural diffusion may be hindered by the existence of other models already. Coevolutionary models are also designed to consider changes in culture and group fitness over longer timespans that is typical for sociological studies of diffusion (see Strang and Soule 1998, especially pp. 283-285 on formal models). As a consequence, coevolutionary models may provide a useful overall context for understanding the relationship between culture and our evolved psychology, as well as perhaps a source of stimulating insights for investigations of cultural dynamics (e.g., the discussions of "indirect bias" and "frequency-dependent bias" in cultural transmission), but may be otherwise more limited for guiding or generating inquiry by sociologists.

SOCIAL MANIPULATIONS

When I discussed the potential sociological study of strong reciprocity, I suggested that understanding basic psychological mechanisms may help us to understand contests of manipulation among organized interests seeking to mobilize public opinion to diverse ends. The appeals of those who want to continue the welfare state and those who wish to end it are both designed with the intention of influencing our evolved and enculturated minds. The study of social manipulations-in-action may therefore become another point of contact between the sociological interest in social consequences and the evolutionary psychological interest in cognitive mechanisms. Social influence provides one way that competing institutional interests can be introjected into the often relentlessly individualistic tendencies of evolutionary analyses. For such an exchange to be productive, however, some misleading thinking will need to be cleared out of the way, which exaggerates the potential contributions of evolutionary psychology to the issue and underestimates the possible contributions of sociology. Prominent here is the use of specious evolutionary reasoning to rule out the possibility of social manipulations of broad types altogether.

We saw a suggestion of this in the discussion of parent-offspring conflict in Chapter 7, in which Pinker credited Trivers with predicting a relative imperviousness of children to the efforts at manipulation by their parents. If parents developed the ability to manipulate children to produce behavior more consistent with the parent's genetic interests than with the child's (in those instances where the two diverge), then one could propose that selection would favor whatever counteradaptations in children would make them impervious to such manipulation. Consequently, one might conclude that in the equilibrium, any successful parental manipulation of children that systematically contradict the children's evolutionary interests will be eliminated by selection (which Pinker ties to Harris's [1998] empirical arguments).

Even more dramatic declarations about the capacity of evolution to provide defenses against social manipulations exist, however:

> "For human sexuality to be 'socially constructed' and independent of biology, as the popular academic view has it, not only must it have miraculously escaped these powerful pressures, but it must have withstood equally powerful pressures of a different kind. If a person played out a socially constructed role, other people could shape the role to prosper at his or her expense. Powerful men could brainwash the others to enjoy being celibate or cuckolded, leaving the women for them. Any willingness to accept socially constructed gender roles would have been selected out, and genes for resisting the roles would take over." (Pinker 1997: 467)

Poof! Steven Pinker might have just seemed to make the "social construction" of gender roles disappear, with just a few magic sentences and a little wave of his Darwinian wand at feminist academics. Indeed, if we grant that any such socially constructed role could likely be shaped by others to profit at its bearer's expense, Pinker's reasoning here would seem sufficiently general to demand dispatching with role theory altogether, on the grounds that social roles require an openness to manipulation by others that could not possibly evolve. As Barbara Herrnstein Smith (2000: 138-39) writes of this passage, "Pinker evidently believes, and believes that he has just proved, that human beings have an innate, naturally selected resistance to the social construction of gender roles—or, indeed, given the logic of his argument, to the social construction of *any* roles."[12]

Yet, human beings can be observed behaving in ways that are hard to explain without reference to the expectations of roles that were not of their own creation, and that involve obligations that seem to benefit another party at least as much as they benefit the actor. If we take the existence of roles as given, and if we grant also the possibility that roles can be shaped to advantage the more powerful members of society over the less powerful, then it would seem that there must be something wrong with Pinker's reasoning. Pinker's argument is logically similar to those that describe the evolutionary stalemates that ensue between predator and prey species. The traits of those members of a predator species that are most adept at catching prey may be favored by natural selection, but selection also favors the traits of those of the prey species who are most adept at eluding the predators. Perhaps the predator gains a temporary advantage, but then the selection pressures upon the prey species for become more intense, and, given sufficient time, natural selection favors the development of the requisite adaptations for the prey to "catch up." Likewise, one might argue that to whatever extent humans could be manipulated into some behavior that works against their

[12] Regarding Pinker's use of "socially constructed," Herrnstein Smith (2000: 143) writes: "Pinker is either being obtuse here or taking advantage of the multiple meanings of 'sexuality' and 'biology' to foist a manifestly absurd claim on some of his critics. Although the idea of 'social construction' can be invoked and applied crudely (as can any other idea, including 'natural selection'), the point usually made by those citing it in these connections is not that people's physiological traits, erotic feelings or carnal activities ("human sexuality" in any of these senses) are independent of the evolutionary history of the species or of those people's individual genomes ("biology" in either of those senses) but, rather, that conventional divisions, associations and normative attributions of various traits, feelings, activities, and roles—especially in accord with simple male/female or homosexual/heterosexual dichotomies—are not simple products or direct "expressions" of either that history or those genomes."

genetic fitness, natural selection would favor the evolution of cognitive adaptations that make the mind impervious to such manipulations.

However, when we consider humans as predators, Tooby and DeVore suggest that the evolved high intelligence of humans allows them to short-circuit this process of evolving new methods of attack and defense. Instead, humans can devise new means of attack and defense many order of magnitude faster than other species can evolve new defenses genetically.[13] The human advantage is increased further by the evolution of the abilities of humans to share the good tricks that they learn with kin and neighbors and to pass these tricks (including how to make weapons and tools) along culturally to their offspring. Moreover, human beings do not have any more capacity than can any other mammal to evolve by natural selection defenses against the predations of fellow humans, but instead the attack and defenses of human war has (at least in its modern incarnation) entailed a spread of rapid technological innovation whose trajectory is independent of any ongoing change to the human genome.

The foregoing logic can also be applied to the manipulation of one human being by another. Human beings regularly say and do things that are attempts to get another person to produce some desired behavior (sometimes the manipulator believes doing the behavior is in the other person's best interest, and sometimes not) Tactics of manipulation are *tools* that can be deliberately fashioned with reference to the perceived nature of the human mind in the same way that a poison-tipped dart is deliberately shaped with reference to the perceived

[13] A great exception here may be contemporary medicine's battle against bacteria, in which bacteria have been increasingly able to use their overwhelming numbers and short generations to evolve resistance to antibiotics faster than scientists can develop new ones. A magazine quotes one medical professor as saying, "You don't know how the race is going to go. You could be a strategic optimist and say the basic science is bound to save us in the nick of time. Or you could be a defensive pessimist and say we're going to be in a post-antibiotic era for a while" (Allen 1999).

nature of human physiology. Given that manipulations are attempts to produce an *effect*, they embody at least a wishful hypothesis about how such an effect could be *caused*.   Take for example the union organizer who decides that human nature (or at least the nature of the workers he deals with) is such that unionizing efforts will succeed only if they appeal to the self-interest of the worker rather than trying to persuade workers by reference to the good of all.

As importantly, tactics of manipulation are *technologies* in the sense that knowledge of what works and what does not can be spread among persons. Union organizers communicate with each other and develop a body of shared knowledge about what tends to work, just as there are consultants and an accumulated body of ideas available to help company executives convince their workers not to unionize. As with any other technology, effective tactics of manipulation can also be preserved by transmission from members of one generation to the next. Insights that have been gained into the craft of human manipulation are evinced in the occupational training of lawyers, teachers, salespersons, television producers, advertising executives, managers, clergy, poets, and politicians, to give just some of the most obvious examples.

Importantly, one can no more evolve a wholesale defense against all exploitative manipulations than a species could evolve a wholesale defense against all predation. Moreover, human cognitive capacities allow novel attempts at manipulation to be generated spontaneously and continuously, such as the endless new gimmicks that advertisers come up with in the effort to sell their product. Human inventiveness can also be put to the purposes of defending against manipulations and for teaching others such defenses, as when a college professor endeavors to teach students how to "think critically" about materials they read.

Genetic evolution has given us the cognitive capacities that allow for back-and-forth contests of manipulation and defense against manipulation. Yet what is fallacious about Pinker's argument above is that the logic of genetic stalemates between predators and prey over evolutionary time cannot be used to draw conclusions about the outcomes of contests of human manipulation. The argument errs because the invention and refinement of technologies of manipulation can occur on a time scale orders of magnitude faster than the genetic changes for a defensive cognitive adaptation can evolve through natural selection. If genetic evolution cannot respond fast enough to keep up with changes in available technologies of manipulation, then we cannot claim that certain manipulations are categorically impossible because genetic selection would have provided defenses against them.

Granted, evolved dispositions may well make it easier for a human being to manipulate people into doing one activity rather than another; our evolved taste for sweets and fats may make selling donuts to be an intrinsically easier task than selling wheat germ. Efforts at manipulation may appeal to features of human nature that are the products of natural selection, such as when a company tries to sell its products with advertisements that associate the product with the attainment of sex or status.

What this implies is that evolutionary analysis may play a role in understanding the response of actors to attempts at influence of various sorts, but that there are sharp limits for an evolutionary analysis innocent of an accompanying sociology or history. In the case of gender roles, we do not have some singular, conscious manipulator working sinister to get people to behave according to prescribed gender roles, but societies do have normative and cultural systems that may be structured in a way that facilitate their own reproduction.

Within such a system, evolved psychological differences between the sexes may imply that females, on average, may exhibit some socializations biases relative to males, and the character of these biases may be consistent with differences between men and women that are observed across cultures. However, the way of conceiving of such differences is not to deny that relevance of socialization to gender roles, or to talk about these biological differences imposing "limits" on what kinds of relationships may obtain between men and women, but to see how psychological mechanisms interact with different environmental influences on the life course. These environments can include cultural messages that effectively work as social manipulations toward certain behaviors that exacerbate influences that exist.

This gives ample space for evolved differences, but it is not the argument that Pinker makes above. Instead, Pinker's conclusion would seem to be that natural selection makes any aspect of the social construction of gender roles impossible. An alternative position might be that the behaviors associated with gender can be subject to considerable social manipulation, even while this social manipulation is mediated by cognitive mechanisms of mind that may differ importantly between the sexes. Both views deny any *tabula rasa* conception of mind and point to the importance of understanding the cognitive structure of men's and women's minds. The latter position, however, allows for the possibility of much more variation in how gender roles are manifested across individuals, across societies, and over time, but it does not entail that gender roles must be "independent of biology" or that attempts at social influence are to be considered "brainwashing" if they are to be considered at all.

In any event, we should be reluctant to state that some action is so contrary to human nature that it is impossible to manipulate people to do it, as it may be instead a matter of our not being clever enough to think of what the effective manipulation would be (see the discussion of biology and inevitability in Chapter 2). The creativity of manipulation is precisely that a behavioral result may be obtained from others by altering the appearance or incentives of the event from the other's perspective (see, e.g., Cialdini 1985). Going back to Pinker's statement above, men might not be able to convince other men to be celibate just by telling them to, but one can imagine possibly (and partly) effective manipulations based on religious appeal or on connecting the achievement of status with sexual restraint. Attempts at social influence operate by trading upon features of human psychology, and understanding what manipulations work and why may be augmented by reference to our evolutionary past (which I discuss further in the next section), but claims about the character of our evolutionary past should be much more gingerly used to make declarations about the limits of social influence. Relative *resistance* to certain cultural variants may prove a much more useful metaphor for an evolutionarily-informed study of social behavior than the metaphor of *limits*.

CONSUMPTION AND SOCIAL INTERVENTION

> *"Instead of being dominated by the natives and by nature, like the unfortunate Lape'rouse staking his life every day, the cartographers in Europe start gathering in their chart rooms... the bearings of all lands... What is the consequence of this change of scale? The cartographer* dominates *the world that dominated Lape'rouse. The balance of forces between the scientists and the earth has been reversed; cartography has entered the sure path of a science; a centre (Europe) has been constituted that begins to make the rest of the world turn around itself."*
>
> —Bruno Latour, *Science in Action*, 1987, p. 224

By assembling the information gathered by explorers and analyzing it, the cartographers Latour describes in the above passage were able to create maps that then permitted explorers to gain greater and greater control in their dealings with the previously uncharted nature that had dominated them. If we think of social manipulations as representing technologies that are based at least implicitly on a model of the actor, then the clearer the image of the relevant parts of human nature possessed by those designing a manipulation, the more effective might be the manipulation that is designed. For the applied psychological science of marketing, we might imagine a potentially increasing asymmetry between those with access to detailed information about consumer behavior and consumers themselves: corporations and marketing firms coming to dominate the behavior of the consumers on whom they are dependent.

Consequently, that such manipulations may operate by exploiting features and failings of an evolved human nature might be particularly useful for the fledgling development of the sociology of consumption (Corrigan 1997; Ritzer 1999). Burnham and Phelan (2000: 246) suggest that the exploitation of instincts evolved for the Pleistocene by profit-seeking agents in contemporary societies contributes to “drug addiction, obesity, gambling, and bankruptcy,” among other problems that can be recharacterized as an inability of individuals to curb deleterious individual consumption. Regarding diet, they go on to write:

> “Humans and other primates, for example, love fruits because they are naturally loaded with sugar. Food manufacturers pander to our sweet tooth. While an orange is 10% sugar, some breakfast cereals have been pumped up to more than 50%... Similarly, fast-food pushers did not create our taste for fatty, salty, caloric-laded foods, they simply exploit our existing desire by producing a product with exaggerating features.”

In such a view, the mismatch between ancestral and present environments creates sources of vulnerability that may be exploited by increasingly capable profit-seeking agents, so capable, perhaps, that their chief obstacles may be the marketing appeals of other products rather than the will of consumers. What is the structure of these manipulations, how do they interact with innate psychological mechanisms, and how might they be better resisted (Rushkoff 1999)? The evolved conception of mind confronted with novelties to which it was not adapted may provide a grounds for a better critical understanding of the tactics used by marketing to induce consumption.

Robert Frank (1999) has used Darwinian ideas to inform discussions about how the evolved desire for status leads to a human preoccupation with *relative*, rather than absolute, standing, which, in turn, leads some to work ever-longer hours in the pursuit of ever-more-luxury items that can serve as icons of their place in society (i.e., Veblen's "conspicuous consumption"). Frank proposes that our Darwinian obsession with relative status helps to explain why wealthier individuals within a society report being happier than poorer individuals, but increasing the wealth of a society as a whole does not increase the overall reported happiness. With respect to luxury items, Frank views the problem as a social dilemma in which society would be better off if everyone spent less money on them (and more on various public goods, such as better roads), but the appetite for relative standing is sufficiently keen that the interests of any one individual are in continuing to spend as much as one can (for a review of sociological work on social dilemmas and cooperation, see Kollock 1998). Accordingly, Frank (1999) argues that changing the income tax to a steeply progressive consumption tax would have the socially beneficial result of reduced consumption by all.

Frank's proposal is an example in which a social manipulation based on psychological principles is proposed as an intervention that would be beneficial for society as a whole. When sociologists talk about the manipulation of social actors, the design of effective and beneficial social interventions is often in mind. Like other manipulations, such interventions are often predicated on models of the actor, and I have already suggested that greater attention to the findings of psychology might help sociology to develop improved conceptions of the actor; such attention might then, in turn, contribute to the design of more effective social interventions. Evolutionary psychologists have said that an increased awareness of the Darwinian underpinnings of behavior can assist in the design of programs to provide socially desirable goals (Tooby and Cosmides 1992; Buss 2000).[14] In Chapter 4, I reported that the policy suggestions offered by Thornhill and Palmer for the prevention and treatment of rape had been dismissed by others as being naive. Such missteps, however, should not make us unreceptive to the possibilities that evolutionary insights about psychological mechanisms may offer for creating social programs. Indeed, one role for a sociology that makes greater use of psychological findings (evolutionarily derived or otherwise) may be to contribute to figuring out how such results can be used to serve public interests.

Evolutionary approaches have promoted considerable theoretical and empirical work on the conditions that support or undermine cooperation (see most famously Axelrod 1984). Buss (2000b: 21) writes, "by exploiting our knowledge of the conditions that promote cooperation, people might be able to mitigate some of the destruction inflicted by competition." Likewise, Dugatkin (1999) offers some specific suggestions about how

[14] Some evolutionary psychologists go too far, in my opinion, when they suggest that selection thinking is necessary to design effective social interventions. Buss and Kenrick (1998: 1018) quip that "trying to change behavior in ignorance of the nature of our evolved mechanisms is like making one's way through a gauntlet blindfolded." Tooby and Cosmides (1992: 40) also maintain that the evolutionary perspective on the mind "offers the only realistic hope of understanding enough about human nature to eventually make possible successful intervention to bring about human outcomes." To buy the argument that a more advanced psychology is required to "eventually make possible" successful intervention seems to suggest that no successful interventions have ever existed, or that the ones that do exist do so only by luck. Additionally, it suggests that the principal failure to achieve some ameliorative goal rests on an inadequate psychology, as opposed to an inadequate commitment of resources and public will toward this goal. Advances in psychology may help with the design of social programs, but effective programs may also rest on simple psychological assumptions but large public commitment.

evolutionary insights about cooperative behavior in animals may help to foster cooperative behaviors among humans. As we saw before, Bowles and Gintis (1998, 2000b) suggest that insights into the basic propensity for strong reciprocity may have positive effects both for understanding how to foster community governance. Recent evolutionary work has also stressed the interaction of the institutional structure in which actors find themselves with human dispositions regarding cooperation. Richerson, Boyd, and Panciotti (forthcoming) provide a specific application to improving the development of successful solutions to common-pool resource problems (see Ostrom 1990). We can expect further developments in the study of what makes people cooperate or refuse to cooperate, and a specifically social approach to such findings may contribute to putting them into workable practice (see Hoffman, McCabe, and Smith 1998; Richerson, Boyd, and Paciotti forthcoming).

In addition, given that our minds adapted in environments that were characterized by relatively small-scale, egalitarian societies in which individual actors had considerable autonomy (see Maryanski and Turner 1992), evolutionary social scientists have proposed we might expect recurrent social problems to emerge from the "mismatch" between complex modern societies and these environments. As a result, some have suggested that social improvements may rest in recreating some of these characteristics as best as possible given the other demands of modern society (Crawford 1998b; Buss 2000b). In one intriguing recent suggestion along these lines, Richerson and Boyd (1999) contend that complex societies, especially in their hierarchical and stratified character, must possess "institutional work-arounds" that allow them to work reasonably effectively despite their apparent conflict with the social organization of ancestral environments. If this is the case, they argue that "the techniques that complex societies use to organize institutions like armies should reflect compromises with an exploitation of social instincts. Variations in the effectiveness of institutions in different societies of the same general level should often reflect better and worse work-arounds" (Richerson and Boyd 1999). Richerson and Boyd list some of the devices that they see providing these workarounds, including ways of exploiting symbolic systems and organizing hierarchies, and then they apply their theory to the relative effectiveness of different armies in the European theatre of World War II. They argue that the greater effectiveness of the German army can be traced to better work-arounds that built solidarity among groups of soldiers and that resulted in more effective chain-of-command.[15] They write: "The irony is cruel but instructive. The criminal, reckless, totalitarian, Nazi regime managed to find the most successful formula of the period for meeting the conflicting demands of national command and control and the need to provide for the felt needs of individual soldiers" (Richerson and Boyd 1999).

One can imagine further tests of their theory by comparing effectiveness in other types of institutions (e.g., firms). Indeed, to my reading, their work seems strongly resonant with "welfare corporatist" ideas about how corporations can adopt structures of small, teamwork-oriented workgroups that maximize the committment that the corporation receives from workers and increase their ultimate productivity (Lincoln and Kalleberg 1990). In comparing firms in the United States and Japan, Lincoln and Kalleberg (1990: 240) find "preliminary support for the welfare corporatist hypothesis of a universally applicable commitment-maximizing organizational form." What Richerson and Boyd's theory suggests is that the reason for such universal applicability is the congruence between the organizational form and the social instincts that developed in our ancestral environments.

[15] Among the data sources they use are Shils and Janowitz 1948. Richerson and Boyd also discuss the organization of the Israeli Defense Force in the 1960's and 1970's as another example of effective military organization that can be interpreted in terms of effective institutional "work arounds."

The intriguing possibility is that whereas earlier in this section we discussed how the discrepancy between ancestral and current environments may be exploited in ways that increase how much we consume, the strategic minimization of aspects of this discrepancy might be used by corporations to increase how much we produce.

CONCLUSION

> *"Empty materialism consists in stating that, of course, everything is material, including social-cultural things, and in leaving it at that. Well and fine; but as long as you do not begin to reflect about the material existence of these things, as long as you keep invoking cause-effect relationships among them without even trying to imagine what material processes might bring about these relationships, you are merely paying lip-service to materialism."*
>
> —Dan Sperber, *Explaining Culture*, 1996, p. 11

What should sociology do about Darwin? The most prominent lesson from the case studies that provide the bulk of this dissertation is that the answer is not some wholesale adoption of the methods, theories, and arguments of Darwinian approaches. These chapters have revealed numerous problems with work presented as forceful arguments for evolutionary psychology, with work hailed by eminent others as revolutionary, and with work seen as providing exemplary empirical foundations of evolutionary psychology. They also suggest some of the difficulties in the application of Darwinian principles and concepts to the complexities of modern life. In the case studies, I have engaged the work on an empirical level, drawing testable implications from theories and then moving to test them. I also have challenged the strength of the evidence previously presented in support of the evolutionary explanations examined in these case studies. More than this, I have sought to engage much of this work on its own theoretical grounds, by scrutinizing the viability and assumptions of its reasoning from a purely evolutionary perspective. To the extent that the work examined here has been used to provide dramatic claims about the achievements of the "new science" of evolutionary psychology, the dissertation suggests that this new science has been oversold. Beyond this, the case studies may perhaps serve as examples of their own of the different ways in which evolutionary explanations may be approached in an effort to evaluate their merits.

The case studies do suggest that the hubris evinced in some treatments of evolutionary psychology is not warranted, but my conclusion is not that this is something for sociologists skeptical of evolutionary approaches to crow about. The theoretical and methodological problems that I point to in some works of evolutionary psychology, as well as some of the empirical failures that we have observed, are not intended as claims that nonevolutionary social science is not laden with problematic work of its own. Nor do they provide grounds for categorical skepticism toward Darwinian claims. Indeed, the prevalence of kneejerk reactions against Darwinian ideas only increases the perception that social scientists cannot judge evolutionary claims fairly. Fortunately, however, there appears to be much greater tolerance for evolutionary approaches than there was in the years following Wilson's *Sociobiology*, so we have reason to hope for improved dialogue on what evolutionary contributions may be most valuable for social scientists.

The preceding two paragraphs are indicative of an ambivalence that has run throughout much of this dissertation. On the one hand, I believe that there is a considerable amount of questionable work presented under the banner of evolutionary psychology, and some of this work has received an enormous amount of public attention—attention that has sometimes also included direct derision of sociology. Much evolutionary work seems especially deficient when it attempts to make larger claims about social processes and policy, the very matters to which the sociological imagination is most attuned. On the other hand, despite the shortcomings that have been described in the case studies, I believe that sociologists should not be dismissive of the potential contributions of evolutionary approaches. I have cited the harm of such dismissiveness for the credibility of sociology, but there is also the potential harm for sociology substantively.

In other words, I believe that there might be important contributions of evolutionary approaches to be found and used by sociologists, even if they are not well-represented among the case studies I examined in detail in this dissertation. (Moreover, even if evolutionary approaches do not provide a useful model for behavior for sociologists, the points of this critique do not apply to other projects exploring biology and behavior, like that of efforts of behavior genetics, behavioral endocrinology, or behavioral pharmacology [see Booth, Carver, and Granger 2000].) One of my greatest worries about this dissertation is that it will be taken as attempting to deny any importance to biology for understanding behavior. As a result, I should be clear that I am not placing my scientific bets with those who would deny that there are some behavioral differences between the sexes, that there is something basic about human nepotistic preferences, or that humans have various intrinsic cognitive biases regarding in-groups and assessments of risk. Yet, such proposals are much simpler, more modest, and more cautious than the theses advanced by much current Darwinian social science, including especially much of the evolutionary psychology that garners the most attention.[16] The cumulative lesson from the case studies may be more about the problems

[16] Another cause of tension here might be that to my mind some of the most convincing work in evolutionary psychology has few obvious implications for sociology. Two examples are work on the relationship between facial and body symmetry and attractiveness (Thornhill and Gangestad 1994; Thornhill, Gangestad, and Comer 1995; Gangestad and Thornhill 1998) and work on evolutionary factors that contribute to food aversions (Fessler 2000).

with selection thinking that reaches too far and claims too much than about selection thinking *per se*. Yet, the stridency of some Darwinian presentations, combined with the harsh reactions they receive from many, makes it difficult to figure out a more judicious and rewarding path to follow.

In this concluding chapter, I have mentioned some work and offered some ideas that I think may provide more promising directions, but this falls short of providing an positive programmatic outline for evolutionarily-inclined sociologists. As a final flourish, I wish I had sufficient insight to pull a rabbit out of my theoretical hat that would resolve my own ambivalence and provide an answer for how sociology should proceed. In spite of this failure of creativity, however, I do believe that just such a positive program that allows ample space for the exploration of biological propositions is needed in sociology, and I have suggested a number of different desiderata here. Such a program needs to strike a balance between the real psychological complexity of human actors and the abstraction necessary to derive propositions about aggregate outcomes. In this regard, I have expressed enthusiasm for the possibility of evolutionary approaches building onto the framework of a rational choice theory, both by providing a theory of preferences and as a way of understanding some of the systematic "non-rationalities" that actors exhibit. With such additions, biological propositions need to be seen as a *starting* point for talking about the effects of different environmental conditions on individual behaviors and of different institutional structures on aggregate outcomes, rather than seeing the invocation of biology as an explanatory *end*. Coevolutionary models also stress the importance of considering population-level features of culture, including social norms.

I have also argued for the importance of a further clarification of what are the issues regarding biology or innateness for some particular inquiry. Evolutionary accounts themselves are likely to be less pertinent for sociological questions than the implications of the accounts about the character of the mechanisms underlying behavior and the extent to which these mechanisms are specified genetically. Advances in cognitive psychology, neuropsychology, and genetics will be important sources of insights here, pointing toward the need for greater interdisciplinary communication and collaboration between social scientists and scientists in these fields. At the same time, however, practical limitations require sociologists to find ways of defining relationships to the biological sciences that do not force sociologists to become experts in human biology as well as human society.[17] I have suggested the social inquiries may often be able to examine the consequences of observable behavioral dispositions while remaining indifferent to questions about why the disposition exists. As we move to larger units of analysis, inquiries may depend increasingly less on the specificity of psychological presuppositions, making a stance of indifference even more sensible. Even so, evolutionary frameworks may provide an important source of inspiration regarding potential features of human psychology, and this role as a wellspring should be recognized even when inquiries do not directly engage the propositions that separate evolutionary and more traditional approaches.

Taking these various points together implies several different actions that different sociologists can take in debates about the value of evolutionary insights into human nature. One is moving to examine and test evolutionary hypotheses that have been proffered in a scholar's substantive domain, perhaps especially those that receive considerable public

[17] This is not to say that it would not be beneficial for sociologists to have at least some greater familiarization with biology (evolutionary and otherwise) as part of their training, perhaps comparable to the training in physical anthropology that is required of many graduate students in cultural anthropology.

attention and seeming assent. Another is attempting to incorporate biological propositions into their work, with emphases on testability and on the interaction of biology and environments. A third is providing defensible grounds for bracketing out such debates as something that can be justifiably set aside for the purposes of particular lines of inquiry. On any front, the cross-disciplinary consistency of sociology with psychology and biology needs to be affirmed, even as the reasons why the consistency does not imply an easy reductionism are refined and re-articulated. As biological inquiries continue to reveal more about human genes and human brains, the clarity of such a commitment to consistency will allow sociology to gain strength from these new developments, rather than the need for sociology seeming to be diminished by them.

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