THE PROBLEM OF PREDICTIVE PROMISCUITY IN DEDUCTIVE APPLICATIONS OF EVOLUTIONARY REASONING TO INTERGENERATIONAL TRANSFERS: THREE CAUTIONARY TALES

Jeremy Freese

Robert Wood Johnson Scholars in Health Policy Research Program, Harvard University

Department of Sociology, University of Wisconsin-Madison

jfreese@ssc.wisc.edu

To appear in *Caring and Exchange Within and Across Generations*, Suzanne Bianchi et al. (eds), Urban Institute Press (2007). Presented at Penn State Symposium on Family Issues, October 2006.

November 28, 2006.

Cox proposes that the economic analyses of resource transfers within families can be considerably enhanced by using evolutionary biological theory to generate expectations specific to relevant demographic categorizations of family members. Whereas wholly abstract economic theorizing can lead to surprising, productive insights about actors characterized merely as "person 1" and "person 2,"[1] evolutionary biology offers the possibility of furthering understanding by making specific use of information that "person 1" and "person 2" are "mother" and "father," "son" and "daughter", "biological child" and "stepchild," or even "paternal grandmother" and "maternal grandmother." Evolutionary biological theory and economic theory have historically drawn from one another in the development of theoretical tools, as the enterprises share considerable abstract affinities due to their common preoccupation with the logic of optimization (see, e.g., Gintis 2000). Moreover, family life is one of the areas in which the potential contributions of an evolutionary perspective has seemed strongest, working from the premise that the affective bonds of kin are rooted in genetically-based propensities that evolved by kin selection. In this light, one might wonder why utilization of substantive propositions from evolutionary biology in studying the economics of family life has been apparently so infrequent that Cox's essay is framed in nearly *terra incognita* terms.

In a previous essay (Cox 2003), Cox provides some reasoning for why the relevant economic theorizing previously might have not been ready for useful incorporation of "biological basics," but it is now. This may well be true for economics and its applications to the

[1] Indeed, such "person 1/person 2" formulations are themselves inappropriately concrete, as the analysis can be carried forth without presuming that the actors are persons, as the fruitful history of applying theories articulated in terms of individuals to organizations (e.g., firms, political parties) indicates. Indeed, rational actor theories seem often to work better for organizations than individuals (Satz and Ferejohn 1994; Clark 1997).

family. However, if one were to poll members of the Human Behavior and Evolution Society about why evolutionary theory is not more widely used in the social sciences, my wager is that most would cite the politics and politicization of academia. Many in my own field, sociology, conceive our field as defined less by the questions it asks than the content of answers it gives; while the boundaries of admissibly "sociological" explanations varies considerably, those that give substantial place to "biology" are outside (Freese, Li, and Wade 2003). From this follows readily that sociology and "biology" are engaged in an interminable zero-sum battle, in which incorporations of the latter diminish the relevance of the former, and thus many think work with "biological" explanations should proceed only with reluctance, as a matter of epistemological last resort (if even then, see Duster 2006). Worries about what those with malevolent politics might do with work that endorses the importance of "biology" has only contributed to the idea that, *almost as a matter of professional identity*, impartiality should be abandoned and alternative explanations should be given every advantage.[2] Fortunately for those who regard such sentiments as inconsistent with scientific ideals, a variety of external forces continue to erode the credibility of ignoring "biology" as a matter of orthodoxy, and so the intellectual environment for serious, authentic contemplation of the potential contributions of evolutionary biological reasoning in the social sciences continues to improve.

In the minds of some, turf and ideological concerns (and misunderstandings they cause) have been all that has held back a Darwinian revolution of the social sciences (Lopreato and Crippen 1999; Pinker 2002). An intellectual apparatus that combined an evolution-based theory

[2] Concerns about epistemic double standards are certainly not confined to sociology, and proponents of evolutionary approaches in psychology and anthropology have described feeling 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).

of preferences with standard economic reasoning about behavior given preferences—not unlike what Cox proposes—has already been one vision of what this "Darwinizing" intellectual force might look like (e.g., Kanazawa 2001a). Nonetheless, I think there is considerable reason for skepticism about the ultimate prospects of what deductive reasoning from evolutionary principles may offer to students of the family. While I applaud Cox's initiative, I describe the grounds for my skepticism here. Important to underscore at the outset, however, is that my arguments are not intended as comfort for those who would wish away the role of genetics in human affairs. There is by now ample reason to presume genetic commonalities and variation to be vitally important throughout much of social life—including resource transfers and other family dynamics—and my doubts are specifically with the prospect of deductive reasoning from evolutionary principles fulfilling much of its seeming promise to yield new, specific insights attributable to the historical consequences of selection for our species.

For an orienting illustration of the problems in developing predictions that prompt my concern, consider the conclusion of the draft of Cox's essay:

> I imagine myself being well into my dotage but happy and somewhat healthy, when one day I'm diagnosed with some awful, fatal disease—albeit one that can be cured at considerable expense, affording me an extra year of gratifying geezerhood. But here's the rub: the exigencies of economics dictate that by paying for the treatment I must sacrifice one year of my granddaughter's college education. I can't vouch that I'd be thinking of Hamilton's rule (if indeed I even would remember it) but I know what it would predict.

Of course, the example is not intended to be taken too seriously, but let us consider it seriously nonetheless. Cox is sufficiently confident that a transparent prediction of behavior can be made using evolutionary principles that he leaves the prediction itself implicit. As Cox seems certainly like the generous and loving type, we might be confident that he himself would forgo an extra year of life for the sake of his granddaughter's education, and working *inductively* from that intuition, it is easy to imagine that an evolutionary perspective *would have predicted this in the first place*. The scientific issue, though, is whether this is something an evolutionary perspective really would have allowed us to predict for the actions of hypothetical actor C in this situation.

Pretend first that we are operating from the reasoning sometimes associated with "human behavioral ecology," from which we would posit that C is basically a standard rational actor whose utility is determined by the extent to which actions prospectively maximize present inclusive fitness (Winterhalder and Alden Smith 2000). Given the current relationship between female education and fertility, it is not at all clear that C should be investing in his granddaughter's education in the first place, even if we imagine such education would contribute to her ability to provide for the children she does have and the quality of the mate she has them with. Regardless, by dying, C is forgoing not just the fitness opportunities from additional children he might sire—perhaps a bleak prospect at his advanced age, although we might want to know more about C before assenting to that—but also whatever fitness gains may accrue to other family members as a result of his actions during the next year. In short, even if the granddaughter's inclusive fitness is indeed increased by the extra year of education—not at all clear in contemporary societies—it is not enough to say she would benefit from the tradeoff to make sense, as the fitness price of the life of even a geezerly (but "somewhat healthy") altruist as not nothing.[3]

In any case, if we follow trends in Darwinian social science over the last twenty years, we might instead adopt an evolutionary psychological perspective, which would say what is relevant are the psychological mechanisms that were shaped under conditions of our ancestral past, and

[3] Of course, a strict evaluation of the tradeoff would also need to consider that death is irreversible whereas a source of educational funding denied is not the same as education ultimately denied or even ultimately postponed.

calculations of current inclusive fitness are largely irrelevant (Tooby and Cosmides 1989, 1990, 1992). I find this perspective far more historically and psychologically plausible than that of human behavioral ecology, but, as behavioral ecologists are fond of pointing out, theirs is the perspective that leads more straightforwardly to precise, falsifiable predictions, at least when the quantities relevant for prediction can be specified. Evolutionary psychology posits specific psychological mechanisms that are often described as facilitating "love" and related emotional attachments that spur altruistic behavior toward kin. As a deductive matter, whether this love is strong enough to overcome preference for self-preservation is hard to imagine determining, especially since evolutionary psychologists have argued that the lack of analogy of ancestral parental investment to educational investment—particularly in daughters (Kanazawa 2001b)—would complicate predictions for behavior in this domain. To be sure, we can imagine trading off a year of life versus investing in a granddaughter's education may evoke psychological mechanisms honed to the fitness consequences of ancestral contexts of choice about one's own survival versus incremental benefits to a granddaughter's condition or future prospects. Evolutionary reasoning can help us to understand why it is not a biological puzzle that the human species might engage in kin altruism even at the expense of one's life, but the theory here is not sufficiently specific to offer a specific, falsifiable prediction of choice.[4]

(Additionally, in reflecting upon how we might see the decision dynamics making more sense when viewed in the light of evolutionary theory, one might consider how C's granddaughter would feel about the decision. We might imagine C being more willing to give up a year of his life for his granddaughter than his granddaughter is willing to accept the

[4] Or, as will be described in more detail later, we can make some simplifying assumptions and offer a prediction, but evidence consistent with or contrary that prediction provides little/no information about the merits of the theory.

beneficience. No simple application of Hamilton's rule can account for this, as an altruistic act is in the interest of the donor is even more in the interests of the recipient.)

Obviously, expecting a theory to predict particular choices from briefly sketched details is unfairly demanding. The question of larger relevance is the generation of predictions pertinent to *larger patterns of choices*—circumstances that would increase or decrease the probability of actors to invest in a family member at one's expense. Here again, however, making specific predictions is not as easy as it appears. (Or, more accurately, making predictions is quite easy, but figuring out the consequence of evidence consistent or inconsistent with those predictions is a task fraught with possibly insurmountable ambiguity.) I am going to elaborate my points by considering three separate examples involving investment of older relatives in younger relatives. The first, paternity certainty, is the subject of an exploratory case study by Cox; the second, on family structure and educational investment, is a sociological example not discussed by Cox but which further illustrates my general arguments; and the last, on differential investment in sons and daughters, concerns a literature reviewed by Cox to which I have previously contributed.

Given the self-consciously preliminary character of Cox's essay, my pessimism might seem premature—even spoilsport—but even if economists' enthusiasm for this area is relatively new, evolutionary approaches to human behavior by now have an extensive track record. At two different times, in the mid 1970s and the mid 1990s, there seemed rising enthusiasm for evolutionary thinking providing a "new science" that would serve as a flourishing (meta)theoretical foundation for considerable advances in behavioral science (Wilson 1975, 1978; Wright 1994; Pinker 1997). This has not come to pass, and enthusiasm for Darwinian approaches might even be in the waning part of a cycle of public intellectual interest. New efforts to bring evolutionary reasoning into economic studies of the family might profit from considering the cautionary lessons that can be drawn from part efforts.

1. PATERNITY UNCERTAINTY

The primary case study that Cox offers in exploring the potential usefulness of an evolutionary approach to intrafamilial transfers is consideration of “how paternity certainty might affect the propensity of maternal versus paternal grandmothers to care for children.” Paternity uncertainty avails itself to evolutionary theorizing because, if we suppose that fathers over the evolutionary history of our species always confronted a kind of uncertainty over whether a child was really their offspring unknown to mothers, this implies that genetic relatedness of father’s side relatives are attenuated by a factor reflecting this uncertainty when compared to their mother’s side counterparts.[5] By this reasoning, the category of “maternal” versus “paternal” grandmother is pertinent for predicting patterns of investment because, while both can be equally assured that their children are their own, they face unequal assurance that their children’s children are their own. A “paternity certainty hypothesis” about grandmothers’ investment might therefore be taken as having as its first implication that we should observe at least some patterns of greater investment in children by maternal grandmothers versus paternal grandmothers. Evidence indicates that such asymmetries do exist, but, as Cox notes, there are other routes to predicting greater investment by maternal grandmothers versus paternal grandmothers than paternity uncertainty. (An obvious set of alternative explanations discussed by Cox turns on mothers being more often the primary caregiver and possibly being more likely

[5] Paternity uncertainty implies also lower paternal versus maternal investment, but, as Cox recognizes, the evolutionary logic of parental investment would predict greater maternal investment even in the absence of paternity uncertainty over evolutionary time.

to seek and receive care from the grandparent with whom they most likely have the closest relationship, i.e., their own primary caregiver.)

When multiple hypotheses lead to the same theoretical implication, standard scientific procedure is to consider other theoretical implications, with an eye toward deriving those which would distinguish the hypotheses. Here, however, we run into an immediate and telling problem. Nowhere in his paper does Cox explicitly articulate his "paternity certainty hypothesis." Even if he did—and one can partially reconstruct his implicit hypothesis from his interpretation of his additional analyses—he would be merely selecting from an indefinite set of different possible "paternity uncertainty hypotheses" which have different implications. Evidence contradicting one of these paternity uncertainty hypotheses does not at all adjudicate between alternative hypotheses that involve paternity uncertainty and alternative hypotheses that do not. In other words, the clearest implication of paternity uncertainty from grandmother investment has alternative explanations, and more refined predictions come only by adding auxiliary propositions for which contrary evidence can be interpreted either as a failure of the hypothesis or just as a failure of particular auxiliary propositions. In the face of an indefinite but certainly quite wide range of possible findings about grandmothers and investment, those inclined to believe in the importance of innate dispositions evolved in response to paternity uncertainty will be able to defend persistence in this view, while those disinclined to this conclusion will be able to defend remaining unpersuaded.

In this specific case, the additional kinds of implications that Cox attempts to draw regarding the paternity uncertainty hypothesis are based mainly on the premise that we might also expect paternal grandmothers to respond to *cues* about their daughter-in-law's fidelity (and thereby the chances that she conceived the child with a male other than the grandmother's son).

In other words, the more certain a grandmother is that her son is the father of her alleged grandchild, the more she should invest. If we adopted a behavioral ecology perspective, we might offer a simplifying assumption that grandmothers make maximally accurate use of all available cues regarding fidelity and discount investment accordingly. From this, if we knew or posited $\Pr(father = cuckold \mid cues)$, we might even advance quite precise predictions about discounted investment. Such predictions, while themselves falsifiable, seem borne more of a "logical" than a "biological" perspective, unless we really believe it plausible that, in our actual evolutionary history, the relevant environmental cues have been stable enough long enough with enough consequences for fitness to support the development of a fitness detecting apparatus that finely honed.[6] Otherwise, we are left with the proposition of a psychology in which paternal grandmothers have a generalized propensity to reduced attachment in their grandchildren that is moderated by some sensitivity to environment cues.

One possible cue Cox considers involves indicators of the daughter-in-law's attitudes (her reported agreement with the proposition that "Marriage is a lifetime relationship and should never be ended under extreme circumstances"), while another is based on the child's physiology (degree of perceived physical resemblance to the father).[7] Whether a grandmother is sensitive to these particular cues, however, would seem to be the degree to which they invoke analogues that

[6] Only if we consider a highly specific kind of consequence to paternity certainty—along the lines of human behavioral ecology—might we (perhaps) get specific and singular predictions that would be otherwise hard to explain, but such predictions almost certainly would not withstand empirical scrutiny in their details, and, upon failing, would not gainsay less precise evolutionary predictions.

[7] Several studies exist regarding the proposition that, since it might seem often in the interest of children to advertise their paternity, one might predict that babies will actually look more like their fathers than their mothers (compare Christenfeld and Hill 1995 with the null findings of Brédart and French 1999; Bressan and Grassi 2004). Other work has looked at whether maternal relatives point out paternal resemblance (following Daly 1982) and whether resemblance increases paternal investment (e.g., McLain, Sellers, Moulton, and Pratt 2000; Bressan and Dal Martello 2002).

have indeed been reliable cues over evolutionary time.[8] In other words, lack of paternal grandmother sensitivity to hypothesized cues might reflect that they do not adequately resemble good cues to fidelity over the course of our evolutionary history, such that the expectation is a general tendency toward lower investment but not that investment will vary by the particular posited cue. For that matter, at least with regard to the attitudinal cues, alternative explanations that paternal grandmothers will be emotionally closer to those daughter-in-laws—and perhaps then more active in the grandmother role—who evince commitment to a lifelong marriage follow without recourse to paternity uncertainty or even evolved dispositions.

The point becomes plainer when we imagine, as Cox does in his conclusion (ms p. 55), definitive DNA evidence regarding paternity. Imagine if, even in the face of indisputable evidence that their son is the baby's father, paternal grandmothers still invested less than maternal grandmothers. Would this refute the proposition that such differences were the result of the genomic consequences of an evolutionary history of paternity uncertainty? No, no more than the observation that men forgo mating opportunities with an older but fertile women for a younger women they know cannot conceive children would refute the proposition that men have an evolved preference for youth in romantic partners (Symons 1995). Presently, abstract evolutionary hypotheses can have it either way—they offer no specific implication—regarding whether our evolved psychology would be expected to respond to explicit information about matters that could only be probabilistically inferred through indirect cues in the environments of our evolutionary past.

Indeed, if one looks over the evolutionary literature on genetic relatedness and resource transfers, different work can be often characterized as following one of two broad forks, both of

[8] Note that if paternal-side investment is contingent and consequential, paired females will have fitness incentives to evolve the capacity to fake cues of fidelity.

which may be available writ small in particular theoretical applications. On the one hand, the mind can be posited to have been sufficiently finely honed by selection that investments reliably track subtle situational cues. An example would be work on the influence of child's age and sex on the amount of grief parents feel if the child dies (e.g., Littlefield and Rushton 1986; Crawford, Salter, and Jang 1989). On the other hand, the mind can be posited to have developed coarse mechanisms; indeed, so coarse that they are readily manipulated into transferring resource to others who are not even genetic relatives. The slavish devotion displayed by many pet owners has been taken to illustrate how human dispositions to parental nurturance can be commandeered by the neotenous features of cuddly animals (e.g., Serpell 1986; Archer 1997; see also Wilson 1984: 126-7). While not impossible in principle, it is a bit unsettling to think of this work cumulating together toward a model of mind that dispenses just the right amount of love to a 7-year old girl vs. a 5-year old boy, and yet which is completely snookered by the large eyes and high-pitched mewl of a kitten.

More generally, "middle-level theories" from evolutionary biology may offer only few and imprecise deductive predictions before requiring ancillary propositions about which we have little deductive guide and which do not themselves speak much to the question of whether there exist evolved genetic predispositions shaped by the dynamic posited by the theory in question. The result is a considerably wide range of empirical states of the world that may be said to be consistent with the theory. This includes contradictory states: to give just one example, the theory of parent-offspring conflict (Trivers 1974) has been said to "predict" contradictory and potentially equally dubious positions regarding parental socialization—that children are almost completely impervious to socialization efforts (Pinker 1997: 447-448; cf. Trivers 1985: 163) and that children's interests are readily subordinated to parental manipulation (Surbey 1998). Yet, as much as the capacity of a theory to accommodate ancillary propositions that yield contradictory predictions may be frustrating, it does not mean the theory is "false." What it does mean is that we should not overstate the theory's deductive potential, or the extent to which the theories with which we work are honestly "falsifiable."

2. FAMILY STRUCTURE AND EDUCATIONAL OUTCOMES

Further insight into the deductive problems of an evolutionary perspective is apparent if we consider one of the few papers yielding positive conclusions about evolutionary psychology to have appeared in one of the top two sociology journals. The paper is not discussed by Cox, but concerns evolutionary psychology and parental investment, using educational attainment as a proxy for investment in a way similar to one of Cox's examples (ms p. 26; see also Cox 2003). In this paper, Biblarz and Raftery (1999; hereafter 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 social science 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, there is room for strong reservations about the theoretical conclusion that they draw. Their conclusion is based on four predictions attributed to "evolutionary psychology," discussed as if it were simultaneously a "perspective" and yet offered enough specificity to make singular, contrasting predictions to the other perspectives in this application. My argument is that these predictions do not follow unambiguously from evolutionary psychology and that alternative predictions can be at least 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. Indeed, I contend the issues separating the two paradigms cannot be resolved by BR's data or analyses.

To elaborate: 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.[9] In other words, this prediction proves ultimately unimportant for differentiating among

[9] BR cast economic theory as predicting that children in stepfamilies will do equally well as children in two-parent-biological families and better than children in single families, on the grounds that the key distinction is whether one has two parents providing complementary resources or not (citing Becker 1964, 1981). Yet, given that children who live in stepfamilies have likely spent some time in a single-parent family (before the custodial parent re-wed) and that divorce often represents a significant initial financial setback for the custodial parents, then the economic perspective could well be taken to predict that two-biological-parents will outperform stepfamilies. My general argument about testing predictions attributed to the evolutionary psychological perspective may perhaps also apply to predictions sometimes attributed to "economic theory," and in sociology is it common for implausible predictions to be attributed to economics and then refuted with glee. (I like to refer to this as a "economists think the darnedest things" trope in sociology.)

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...."[10] 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 known, the

[10] 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.

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—myself included—would grant that there seems almost certainly “something biological” about the greater investments of mothers than fathers, and that standard evolutionary psychological accounts provide 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; see also Goldberg 1973, 1993; Browne 2002). 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 (and others) 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 proposition 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 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 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 in principle 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.

Taking stock, the specific predictions that BR attribute to evolutionary psychology are only one of several sets that could have been as easily derived. It *just so happens* that these predictions happen to align with the observed findings, and thus the paper presents its results of providing support for evolutionary theory versus alternative "perspectives." The multiplicity of predictions is consistent with the skepticism about the deductive specificity of evolution-based theories, but this multiplicity does not necessarily point to some fundamental flaw or falsity in evolutionary approaches to social behavior. Indeed, the presence of competing testable predictions within a theoretical perspective may well be a sign of its vigor and health. As the issue is portrayed by one evolutionary psychologist:

> "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." Buss (1995:81)

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."

If a variety of predictions can be derived not just from psychology but from perspectives that do not posit genetically-based psychological adaptations (what Cox would call "nonbiological" explanations), then does this mean the enterprise of social science hypothesis testing is some kind of ill-reasoned illusory diversion? No, but it does suggest that trying to infer genetic causation from regressions of educational attainment on family structure may not be a very productive way of doing developmental psychology. Regarding specifically the relationship of evolved biology, parental investment, and socioeconomic attainment, illuminating the issues that separate evolutionary and sociological perspectives will also require both more and different data than what BR marshal. The two crucial questions may be (1) how and in what ways is attainment affected by different types of parental investments and (2) how do (evolved) biological and environmental factors interact in determining how and how much parents invest in their offspring. The potential contributions of survey analysis seem much greater for the first question than for the second: 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. Indeed, one can argue that no one seems to have a good understanding yet of either how to explicate methodologically or represent theoretically the interaction of "biology" and environment in the determination of social behavior, although certainly efforts are being made (more often with the idea of the interaction of genetic and environmental characteristics that each vary in populations [see Shostak 2003; Moffitt, Caspi, and Rutter 2005]).

3. THE TRIVERS-WILLARD HYPOTHESIS

An evolutionary hypothesis discussed at some length by Cox is the Trivers-Willard hypothesis (hereafter TWH). As evolutionary theories germane to resource transfers go, the TWH has the virtue of being seemingly straightforward in its reasoning and leading to seemingly nonobvious, seemingly falsifiable conclusions. The TWH follows from the reasoning that if (1) parental "condition" is heritable and correlated with reproductive success and (2) reproductive variance is greater for males than females, then there are fitness advantages for those of better "condition" who produce relatively more sons and those of lower rank who produce relatively more daughters. Trivers and Willard (1973) thus develop their hypothesis in terms of expected biases in sex ratios at birth, but they also note that similar implications follow for relative investment in children. As a final flourish of their three page paper, they also suggest that "socioeconomic status" might be the analogue of "condition" in developed societies.

The most straightforward application of the TWH of parental investment to the United States would be that, as measures of socioeconomic status increase, measures of parental investment should increasingly favor sons. In these terms, two separate studies find null results for the TWH for a considerable diversity of measures (Freese and Powell 1999; Keller, Nesse, and Hofferth 2001). Various means exist for continuing to assert the relevance of the TWH for contemporary developed societies while granting the null results of these studies. For example, one can assert that studies focused too much on an outcome too closely related to education, which Kanazawa (2001) argues is an especially distant kind of investment from a Pleistocene analogue (especially for daughters). Alternatively, one can argue that direct measures of investment are flawed and instead the question should be looked at using a distal outcome that reflects investment, viz., education (Hopcroft 2005). Depending on the strength of one's conviction that something like the TWH should be evinced somehow in contemporary developed societies, the hypothesis is either easily abandoned or easily preserved.

Part of what makes the Trivers-Willard hypothesis perhaps more "vampirical" than "empirical"—unable to be killed by mere evidence—is that the hypothesis seems so logically compelling that it becomes easy to presume that it must be true, and to presume that the natural science literature on the hypothesis is an unproblematic avalanche of supporting findings. In the paper in a leading economics journal cited by Cox, Edlund (1999: 1278) offers the brief, citation-free assessment of the literature as:

> "The Trivers and Willard hypothesis has been confirmed in a large number of studies of animal species (including humans) over the past 25 years. To my knowledge no study has found evidence against it."

In fact, the Trivers-Willard hypothesis of adaptive sex ratio variation is not at all well established in the animal kingdom. Adaptive sex ratio variation in birds—including but not limited to hypothesis that one could characterize as extensions of the logic of the TWH (see Frank 1990 for a review)—is discussed at length in a meta-analytic study as an example in which publication bias can lead to distorted conclusions (Palmer 2000). That author finds that "on closer inspection few, if any, compelling data exist for adaptive departure from a 50:50 sex ratio in any species" (p. 454; see Ewen, Cassey, and Møller [2004: 1277] for another meta-analysis of birds that concludes that "facultative control of offspring sex is not a characteristic biological phenomenon in breeding birds").

The reason that animal results are relevant is that one can easily get the opinion that the TWH is a well-established phenomenon in animals and only through some pseudo-dualistic "human exceptionalism" might one resist its applicability to humans. Instead, we face the opposite of the famous line from "New York, New York": if the hypothesis can't make it there, should we expect it to make it anywhere? That is: If the TWH has fared so inconsistently in the areas of its more direct application, then how likely should we expect it to matter importantly for application to parental investment instead of sex ratios and to the novel investment environments of contemporary developed societies instead of the environments of our evolutionary past.

Cox reports some studies that have findings for sex ratios that seem to provide some evidence for the Trivers-Willard hypothesis for sex ratios at birth in the United States (Norberg

2004; Almond and Edlund 2006). These findings may well suggest promise to the hypothesis. Still, caution is urged, especially as the Trivers-Willard hypothesis for sex ratios has provided inspiration for a burst of recent studies with positive findings. Kanazawa (forthcoming) has extended proposed a "generalized Trivers-Willard hypothesis," suggesting that:

> Parents who possess any heritable trait which increases male reproductive success at a greater rate (or decreases male reproductive success at a smaller rate) than female reproductive success in a given environment will have a higher-than-expected offspring sex ratio (more males). Parents who possess any heritable trait which increases female reproductive success at a greater rate (or decreases female reproductive success at a smaller rate) than male reproductive success in a given environment will have a lower-than-expected offspring sex ratio (Kanazawa forthcoming).

This generalized TWH has the virtue of being fecund in its testable implications, and Kanazawa and others have presented a series of findings which they interpret as supporting it.[11] Kanazawa (2005) predicts that body size confers a greater adaptive advantage for men than women, and thus taller and heavier people should be relatively more likely to have sons. Kanazawa and propose that persons with occupations suggestive of a masculine brain (e.g., engineers and mathematicians) will have more sons than persons with occupations with occupations with a feminine brain (e.g., nurses and school teachers) because children would be more successful if they were born with a relative masculinization of their brain consistent with their gender. Kanazawa (forthcoming) proposes that more beautiful people will be more likely to have daughters because beauty confers a greater reproductive benefit for women than men.

Nonetheless, one might question whether the "generalized Trivers-Willard hypothesis" accurately generalizes the TWH. The TWH derives as an implication of the greater reproductive

[11] The analytic strategies used in these papers have been recurrently criticized as exemplary of questionable methodological practice on the weblog of a prominent social statistician (Gelman 2006a, 2006b).

variance among males compared to females. In the Kanazawa (2006) application to beauty, the argument is that beauty is more pertinent for reproductive success for females than males, but a proportionally greater return for females would still not imply female-biased sex ratios unless that return was large enough to *more than offset* the greater returns to males for advantageous traits generally due to their higher reproductive variance. Roughly, if reproductive variance for males is x times that of females, then a trait would have to have more than the square root of x times as large an effect for females relative to other females than for males relative to other males in order to be favored by selection. If one looks again at Kanazawa's statement of the generalized TWH, it might read more like a general statement about what traits one would expect to be favored by selection than any statement that extends the logic of the original Trivers-Willard hypothesis. Ironically, by its inattention to the animating logical detail of the Trivers-Willard hypothesis—the tendency for reproductive variance of males of many species to be greater than that of females—the generalized TWH might yield applications that are not actually consistent with Darwinian reasoning.

In any case, because cues of beauty are considered in evolutionary psychology often to be taken as beautiful because they are cues to health (e.g., Symons 1995), *the prediction could have just as easily gone the other way*: more beautiful parents are of better condition and so should have more sons. Again, it just so happens that the prediction offered in the paper matches the results presented.[12]

The merits of the reasoning or reported support for particular applications aside, an important concern regarding the more general viability of the generalized TWH—and, for that

[12] As an autoanecdotal aside, when the findings of the Kanazawa study were told to me by a colleague who had read about the study in a newspaper, I presumed my colleague had it backwards and that the prediction made was that beautiful people would have more sons.

matter, the original Trivers-Willard hypothesis and the studies cited by Cox—for human sex ratios bears close consideration. Namely, if the TWH/generalized TWH resulted in strong influences on the probability of offspring being male or female, the hypothesis would have a plain implication for the correlation of sex within sibships. If stable traits are responsible for deviations in a parent's probability of their first offspring being a son from the population proportion of sons, then that same probability would apply to the second child as well. If a parent has an increased probability of having a daughter because they are smaller, less violent, have a more feminized brain, or are more beautiful, then the probability should be biased in roughly the same magnitude for subsequent children as well. Consequently, the generalized TWH implies a tendency toward positive correlation in the sex composition of children *within families*, even as the tendency toward relative equal numbers of males and females in the population remains.

Pretend the sex of a child is determined like the flip of a coin. The generalized TWH is then like postulating a population in which many individuals are flipping coins in which Pr(heads) differs substantially from Pr(tails). This variation between couples is sometimes referred to in the sex ratio literature as Lexis variation (Edwards 1966). As long as the biases cancel out in the aggregate, a group of wildly varying and biased coins can produce the same overall count of heads and tails as a group of homogeneous, fair coins.[13] Importantly, however, the same cannot be said regarding the distribution of *multiple* flips from those coins. Instead, Lexis variation implies greater positive correlation in sexes within sibships than would be observed in the

[13] Perhaps one wishes to believe the hypothesis predicts that Pr(child #2 = boy) is much affected by whether child #1 is a boy or girl, so that the overall number of boys and girls produced over a parent's fertility history corresponds more precisely to some TWH-influenced deviation from 50:50. This would have its own, opposite implications for the expectations about intrafamilial sex ratios—that pairs of same-sex offspring should be less frequent than pairs of opposite-sex offspring—which is also not supported by available data.

absence of Lexis variation. The original and generalized TWH would seem to imply that we should be observing more imbalanced sex ratios within families than would be expected if the sex of children within a family was independently determined, and the cumulative consequence of the generalized TWH is limited by the total amount of Lexis variation (and even that would assume that all Lexis variation is attributable to dynamics explicable as the TWH or generalized TWH).

While the study of sex ratios is considerable, data quality varies. Two large studies that make use of health service data that follow births from the same mothers report similar results, however. In a study of 815,891 children from the Danish Fertility Database, Jacobsen, Moller, and Mouritsen (1999: 3124), "no significant predisposition was found of couples or individuals to have children of a particular sex ('Lexis association')." In a study of 549,048 live and stillbirth children born in Scotland born from 1975 to 1988, Maconochi and Roman (1997: 1051) find that "the probability of a male infant was the same regardless of the genders of all other children born to the same mother." These data sources are both almost certainly more complete and accurate than the data sources used in any of the studies reporting support for Trivers-Willard hypothesis by Kanazawa or Hopcroft.[14] If for no other reason than improvement in database technology and the ability to track mothers over their fertility history, these data are also seem superior to previous data that had been used to produce estimates of Lexis variation (Edwards 1966, James 1975). Incidentally, the Maconochi and Roman (1997) also includes measures of both maternal and paternal social class, and they fail to find any evidence of a relationship between sex ratio and social class, meaning that these data should be counted among

[14] Note that some data for testing the TWH could seem to reveal patterns consistent with the hypothesis when the real effect is that of the child's sex upon maternal status, given evidence that fathers may be more interested in marriage and fatherhood for sons generally versus daughters (Morgan, Lye, and Condran 1988).

the null findings for direct studies of the original Trivers-Willard hypothesis for human sex ratios (cf, e.g., Hopcroft 2005, reporting positive findings for the hypothesis with inferior data). This is not to argue that sex ratios at birth cannot be influenced by postnatal environmental factors—not to mention, especially in some parts of the world, selective abortion or even infanticide—but the population data do not suggest significant nondeliberate variation in the sex ratio at birth by stable parental characteristics.

For this reason, readers may wish to have a generalized skepticism toward the generalized Trivers-Willard hypothesis, and may wish to be especially wary of the specter of a proliferation of findings attributing substantial differences in sex ratios to relatively indirect or unreliable measures of different phenotypic traits. The analytic techniques and data that characterize much preceding work leave considerable room for results to possibly reflect arbitrary or even incorrect analytic decisions that are difficult to evaluate based only on materials provided with publication. Moreover, given the large number of possible implications and numerous datasets or specifications by which they may be tested, the possibility of publication bias in which tests make it into print is large. At the very least, for the serial publication of positive (generalized or otherwise) TWH findings to warrant continued attention, it seems reasonable to hope that theorists will develop an account that reconciles these findings with the seeming lack of positive correlations of sex within sibships in large, high-quality data.[15]

[15] In this regard, generalized TWH enthusiasts might look to James's (2000) speculation that Lexis variation exists simultaneously with so-called "chaotic Poisson" variation that induces *negative* correlations of sex within sibships of equivalent magnitude to the Lexis variation, so that the two cancel each other out in large datasets. A challenge for those interested in the generalized TWH might thus be to deploy some of the ingenuity that has thus far gone into envisioning new applications of the theory to proposing an adaptive explanation for chaotic Poisson variation that would counterbalance the correlation of sex within sibships that the gTWH would otherwise imply.

Cox recognizes ambiguities in testing the TWH when he discusses using educational attainment as a proxy for investment, but he concludes “we have Trivers and Willard to thank for pointing the way toward a potentially noteworthy empirical finding.” This is a recurrent theme in Cox’s essay, that even if evolutionary hypotheses are incorrect, they stimulate inquiry that can lead to new empirical discoveries. So long as all researchers are as reflective and contemplative of alternative explanations as Cox, I agree. The worry, however, is that enthusiasm (or, just as bad, antipathy) for evolutionary hypotheses might sometimes lead to misleading deductions of hypotheses or various kinds of publication biases that result in a cumulative literature that distorts our ultimate portrait of the underlying empirics. Additionally, enthusiasts might be more inclined to conduct tests that correspond to their intuitions rather than push for those that are less intuitive. The gambit that new hypotheses, wherever they come from, are useful if they press us to ask questions we would otherwise not ask is worthwhile only to the extent the questions answered fairly and competently, and my hope is that subsequent researchers who follow Cox’s call will share also the genuine curiosity with which he proceeds.

CONCLUSION

Barring some unexpected evidentiary shift in favor of intelligent design or the creation of humanity by an alien race with a twisted sense of humor, the human species is the legacy of a long history of processes of evolution by selection. Every one of us is the product of an astonishing winning streak: our parents survived and reproduced, their parents survived and reproduced, and so on, stretching back before humans were humans and even before mammals were mammals. “Evolutionary psychology,” in the broadest sense, is falsifiable only by a falsification of the application of the theory of evolution itself, at least as applied to the human genome. In other words, even if you adopt an extremely environmentalist stance toward human behavior, implicit in that stance is that some history of evolution by selection is responsible for building a species about which that stance could be true. For this reason, I reject the terminology of "biological" versus "nonbiological" causes, because we are biology all the way through, and any account that makes reference to the import of human psychology—e.g., the import of preferences—is biological.[16] Explanations that draw on "socialization," "culture," or "learning" are postulating changes in actor's psychology; psychological change is biological change.

The fact of evolution by selection implies that evolutionary social science has two distinct projects. The *historical* project involves taking the observable facts of human affairs, as best we can assemble and refine them, and developing an account of the history of the species that coincides with these facts. Evolutionary theorizing of this sort is regularly criticized as storytelling, and storytelling it often is—but the narrative history of our species is a story, and the stories we tell may be better or worse approximations of this real story, for which consistency with available information is our guide. The *deductive* project involves taking knowledge about our history and about the logic of selection processes and using that to develop new verifiable insights about human life. The deductive project feels more like doing science, and avails itself to revered forms of scientific publication, but much of my argument has been that the development of substantive evolutionary-reasoning-based predictions about contemporary developed societies is an enterprise fraught with ambiguity once one moved past fairly simple, vague, and uncertainly contingent generalizations (e.g., mothers tending to invest more children

[16] Turkheimer (1998) provides an interesting discussion that includes a division between a "weak" sense of a phenomenon as "biological" and a "strong" sense, although neither corresponds to the idea that a phenomenon is innate in the sense of a phenotype we would expect to develop similarly across a very broad range of developmental environments, which appears to be how Cox uses the term.

than fathers; parents tending to invest more in biological children than stepchildren). To be sure, the historical and deductive projects have and will continue to offer possibilities of productive iteration, where empirics that narrow that range of possible histories can lead to predictions that, in turn, lead to new insights that strengthen confidence in particular historical accounts. Nonetheless, my belief is that, given the complexity of human life and the specificity with which we wish to understand it, the direction of genuine knowledge production is and will continue to be vastly lopsided in the direction of observations about us now informing reconstruction of our past development as a species.

What this implies for Cox's project requires us to be mindful of what Cox is seeking. At least in my reading, Cox is displeased with models that treat actors as abstract entities that could just as easily be firms, robots, or simulacra, and wishes to extend knowledge to take advantage of knowledge about categorizations that can be applied to the real actors in families ("mother" and "father" instead of "person 1" and "person 2"). In this respect, Cox seems to want to posit basic category-related psychological propensities from actors, and then to work forward from these propensities to empirical implications. Evolutionary theory provides a means of stimulating thought about what these propensities might be. I agree, but my reading of various evolutionary psychological literatures makes me worry that what is often brought out are a researcher's existing intuitions about the social world, only perhaps with the illusion added that such inferences are purely deductive rather than very much inductive. Anything that elaborates and adds coherence to our understanding of the apparent psychological propensities of actors in our population is good, so long as with this understanding we do not import misleading assessments of what we have learned.

What social scientists sometimes imagine that evolutionary psychological theorizing can provide is a way of doing developmental psychology on the cheap. They want the analogue of being able to look at choices and determine not just revealed preferences but also a revealed biography of how the actor came to have those preferences. They want evolutionary psychological theory to allow researchers to use surveys of adults to settle disputes about what about our psychology reflects the genetic commonalities of a shared "human nature" and what reflects "socialization" or "culture" or "structural forces." That variants of evolutionary psychology are like other perspectives in providing many possible and sometimes contradictory predictions when applied to surveys of adults suggests this inferential errand should be regarded with skepticism. More than this, individual researchers should consider to what extent they wish to be in the business of understanding the provenance of psychological traits versus trying to understand the population consequences of patterns of traits as they are. (This division is reinforced by the recognition that the extent to which traits are genetically-based has no particular relationship to their malleability, which means more generally that there are no grounds for foreboding claims about genes imposing hard limits on the possibility of social interventions [Wahlsten 1990].)

If one does take the stance that it matters less for one's own purposes if particular evolutionary hypotheses are true—in the sense of providing insights into the actual evolutionary history of our species and its genomic consequences—than if they are testably useful, then it should prompt reflection about what about the hypothesis is useful and toward what end. Much about resource transfers in families may well be illuminated by working more with category-based preferences while remaining indifferent to how those preferences came about. For whatever reason, if evidence indicates that there is "something about" certainty in the genetic relatedness of kin, we can do much to document the contours and consequences of that pattern, hopefully toward useful generalizations about the circumstances that make revealed preferences for genetic relatives stronger or weaker. The extent to which analysis proves to require detailed reference to Darwinian principles, and thus specific engagement with how the behavioral tendencies originate, is a subsequent question. When analyses do wish to proceed with an indifferent or agnostic stance toward questions of provenance, this stance should be explicit.[17]

Barbara Ehrenreich (2000: 88), a Ph.D. biologist who is often mistaken for a sociologist, once wrote that

> [T]here are people who reject any attempt to apply evolutionary theory to human behavior, and, as far as I'm concerned, they can go back to composing their annual letters to Santa Claus. Obviously, humans have been shaped by natural selection (though it's not always so obvious how).

Ehrenreich works as a journalist and thereby has the luxury of being able to banish the "how" to parentheses. For scientists working on human behavior, "how" is the whole thing, and intuitions of the strong relevance of our evolutionary history go nowhere without a systematic proposal about how this relevance should be incorporated into our thinking about social life. Yet, considering the diversity of contemporary interpretations of what our evolutionary past has to say about social life in the present, Ehrenreich's statement just that it is "*not always* so obvious how" is perhaps even charitable. Given

[17] It should also be symmetric, rather than an analysis offering casual socialization or other environmental explanations but then suddenly declaring a principled agnosticism when speculations turn to genes. Indeed, to me this is one of the chief virtues of greater awareness of Darwinian theorizing: they call attention to the extent to which the concluding sections of some social science literatures are replete with casual, almost throwaway, speculations about the socialization or cultural origins of behaviors, offered without any real prospect of subsequent evaluations as a way of gesturing toward providing a developmental explanation, almost as a kind of serial secular origin myths. More explicit statements that work does not and is not intended to speak to the origins of psychological traits or behaviors, when this is the case, would be more honest and ultimately more scientific.

this, those interested in the implications of postulates about human psychology should be able to work forward from postulates without having to take any stance on developmental origins, much less their ultimate origins in our past. Those interested in reconstructing evolutionary history should be respected for their interest in taking on the task of trying to develop an empirically and logically consistent account of our past. Those who do wish to try working deductively forward should be expected at least to be reflective and specific about the historical and psychological assumptions required to arrive at their predictions, and such predictions should be scrutinized logically as closely as they are empirically.

Economists, especially, have a proud history of being relatively unconcerned about the realism of the assumptions of their models if these models provide clear, verifiable implications (Friedman 1953). As illustrated in the preceding examples, the situation here differs in two important respects. First, economists traditional tolerance for vastly simplified models is certainly not a tolerance for *vague* models; the problem I point to above is that evolutionary hypotheses are often articulated in a way that elides the suppositions about selection implicit in that prediction, with the consequence that the capacity of the theory to follow different suppositions to a contradictory prediction is obscured. Second, the reasoning that leads most easily to "evolutionary" predictions is a model that posits that human beings act in accordance with what maximizes inclusive fitness today (i.e., a standard rational actor model with inclusive fitness serving as the lone term in the utility function). This model implies processes that are not just psychologically and historically unrealistic—which economists might otherwise be willing to tolerate—but also results in predictions, when held to any level of specificity, that are patently inconsistent with the facts of fertility and familial behavior in contemporary, developed societies.

For example, one might look at declining rates of childbirth and see qualitatively some analogue to the tradeoffs of quantity for "quality" that make sense from a fitness-maximizing standpoint, but there is absolutely no reason to think that the low level of fertility today are optimal from the sense of a strategy for maximizing frequency of genes in the population several generations hence. For this reason, one is led to deriving predictions based on the idea that we possess evolved dispositions that influence behaviors today but not in any optimizing sense, and here the content and magnitude of these dispositions is exactly the point of vagueness at which it becomes easy for people to interpret known empirics or their sense of the social world as being straightforward predictions from Darwinian theory. In the absence of explicit articulation of the psychological and historical suppositions that give rise to their predictions, putatively deductive applications should be recognized for their heuristic value, including as a way of explicating intuitions one already has about the world, but this work should not be mistaken for explanation.

REFERENCES

Almond, Douglas, and Lena Edlund. 2006. “Trivers-Willard at Birth and One Year: Evidence from U.S. Natality Data 1983-2001.” Working paper. Columbia University, Department of Economics, New York.

Angrist, J. D. and W. N. Evans. 1998. "Children and their parent’s labor supply: evidence from exogenous variation in family size." *American Economic Review* 88:450-77.

Archer, John. 1997. "Why Do People Love Their Pets?" *Evolution and Human Behavior* 18:237-259.

Becker, Gary S. 1964. *Human Capital*. New York: National Bureau of Economic Research.

Becker, Gary S. 1981. *A Treatise on the Family*. Cambridge, MA: Harvard University Press.

Belsky, Jay, Laurence Steinberg, and Patricia Draper. 1991. “Childhood Experience, Interpersonal Development, and Reproductive Strategy: An Evolutionary Theory of Socialization.” *Child Development* 62:647-670.

Biblarz, Timothy J. and Adrian E. Raftery. 1999. “Family Structure, Educational Attainment, and Socioeconomic Success: Rethinking the "Pathology of Matriarchy".” *American Journal of Sociology* 105:321-365.

Brédart, S. and French, R.M., 1999. “Do babies resemble their fathers more than their mothers? A failure to replicate Christenfeld and Hill (1995).” *Evolution and Human Behavior* 20**:** 129–135.

Bressan, P. and Dal Martello, M.F., 2002. Talis pater, talis filius: perceived resemblance and the belief in genetic relatedness. *Psychological Science* 13**:**213–218.

Bressan, Paola and Massimo Grassi. 2004. Parental resemblance in 1-year olds and the Gaussian curve. Evolution and Human Behavior. 25: 133-141.

Brown, Gillian R. and Joan R. Silk. 2002. "Reconsidering the null hypothesis: is maternal rank associated with sex ratios in primate groups?" *Proceedings of the National Academy of Sciences* 99:11252-11255.

Browne, Kingsley R. 2002. *Biology at work: Rethinking sexual equality*. New Brunswick, NJ: Rutgers University Press.

Buss, David M. 1995. “Evolutionary Psychology: A New Paradigm for Psychological Science.” *Psychological Inquiry* 6:1-30.

Buss, David M. 1995b. “The Future of Evolutionary Psychology.” *Psychological Inquiry* 6:81-87.

Cameron, E.Z. 2004. "Facultative adjustment of mammalian sex ratios in support of the Trivers-Willard hypothesis: evidence for a mechanism." *Proceedings of the Royal Society of London Series B - Biological Sciences* 271:1723-1728.

Chodorow, Nancy. 1978. *The Reproduction of Mothering: Psychoanalysis and the Sociology of Gender*. Los Angeles: University of California Press.

Christenfeld, Nicholas J. S. and Hill, Emily A., 1995. Whose baby are you?. *Nature* 378: 669.

Cox, Donald. 2003. "Private Transfers within the Family: Mothers, Fathers, Sons and Daughters." In *Death and Dollars: The Role of Gifts and Bequests in America*, edited by Alicia H. Munnell, and Annika Sunden (168-197). Washington, D.C.: Brookings Institution Press.

Daly, Martin. and Margo Wilson. 1982. Whom are newborn babies said to resemble?. *Ethology and Sociobiology* 3**:** 69–78.

Daly, Martin and Margo Wilson. 1983. *Sex, Evolution, and Behavior* (2nd ed.). Boston, MA: Willard Grant.

Daly, Martin and Margo Wilson. 1988. *Homicide*. New York: Aldine de Gruyter.

Daly, Martin and Margo Wilson. 1996. "Violence Against Stepchildren." *Current Directions in Psychological Science* 5:77-81.

Daly, Martin and Margo Wilson. 1999. *The Truth about Cinderella: A Darwinian View of Parental Love*. New Haven, CT: Yale University Press.

Draper, Patricia and Henry Harpending. 1982. "Father Absence and Reproductive Strategy: An Evolutionary Perspective." *Journal of Anthropological Research* 38:255-273.

Duster, Troy. 2006. "Comparative perspectives and competing explanations: taking on the newly configured reductionist challenge to sociology." *American Sociological Review* 71:1-15.

Edlund, Lena. 1999. “Son Preference, Sex Ratios, and Marriage Patterns.” *Journal of Political Economy* 107(6): 1275-1304.

Edwards, A. W. F. 1966. “Sex ratio data analysed independently of family limitation.” *Annals of Human Genetics* 29: 337-347.

Ehrenreich, Barbara 2000. "How "Natural" is Rape?" *Time*, pp. 88.

Ellis, Lee and Steven Bonin. 2002. "Social Status and the Secondary Sex Ratio: New Evidence. on a Lingering Controversy." *Social Biology* 49:35–43.

Ewen, John G., Phillip Cassey, and Anders P. Møller. 2004. "Facultative primary sex ratio variation: a lack of evidence in birds?" *Proceedings of the Royal Society of London Series B - Biological Sciences* 271:1277-1282.

Frank, Steven A. 1990. "Sex allocation theory for birds and mammals." *Annual Review of Ecology and Systematics* 21:13-55.

Freese, Jeremy and Brian Powell. 1999. "Sociobiology, Status, and Parental Investment in Sons and Daughters: Testing the Trivers-Willard Hypothesis." *American Journal of Sociology* 106:1704-43.

Freese, Jeremy and Brian Powell. 2001. "Making Love out of Nothing at All?: Null Findings and the Trivers-Willard Hypothesis." *American Journal of Sociology* 106:1776-1788.

Freese, Jeremy, Jui-Chung Allen Li, and Lisa D. Wade. 2003. "The Potential Relevances of Biology to Social Inquiry." *Annual Review of Sociology* 29:233-256.

Friedman, Milton. 1953. *Essays in Positive Economics*. Chicago: University of Chicago Press.

Gelman, Andrew. 2006a. “Amusing example of the fallacy of controlling for an intermediate outcome, or, the tyranny of statistical methodology and how it can lead even well-intentioned sociobiologists astray.”

http://www.stat.columbia.edu/~cook/movabletype/mt-tb.cgi/420. (Accessed November 28, 2006)

Gelman, Andrew. 2006b. “Problems in a study of girl and boy births, leading to a point about the virtues of collaboration.” http://www.stat.columbia.edu/~cook/movabletype/archives/2006/08/more_on_girl_an.html. (Accessed Novemeber 28, 2006.)

Gintis, Herbert. 2000. *Game Theory Evolving*. Princeton, NJ: Princeton University Press.

Goldberg, Steven. 1973. *The Inevitability of Patriarchy*. New York: Morrow.

Goldberg, Steven. 1993. *Why Men Rule: A Theory of Male Dominance*. Chicago: Open Court.

Hopcroft, Rosemary. 2005. "Parental status and differential investment in sons and daughters: Trivers-Willard revisited." *Social Forces* 83:1111-1136.

Hrdy, Sarah Blaffer. 1997. “Raising Darwin's Consciousness: Female Sexuality and the Prehominid Origins of Patriarchy.” *Human Nature* 8:1-49.

Hrdy, Sarah Blaffer. 1999. *Mother Nature: A History of Mothers, Infants, and Natural Selection*. New York: Pantheon Books.

Jacobsen, R., H. Møller, and A. Mouritsen. 1999. "Natural variation in the human sex ratio." *Human Reproduction* 14:3120-3125.

James, William H. 2000. "The variation of the probability of a son within and across couples." *Human Reproduction* 15:1184-1188.

Kanazawa, Satoshi and Griet Vandermassen. 2005. "Engineers have more sons, nurses have more daughters: an evolutionary psychological extension of Baron-Cohen's extreme male brain theory of autism and its empirical implications." *Journal of Theoretical Biology* 233:589-599.

Kanazawa, Satoshi. 2001a. “De Gustibus Est Disputandum.” *Social Forces* 79:1131-1163.

Kanazawa, Satoshi. 2001b. "Why We Love Our Children." *American Journal of Sociology* 106:1761-1775.

Kanazawa, Satoshi. 2005. "Big and tall parents have more sons: further generalizations of the Trivers-Willard hypothesis." *Journal of Theoretical Biology* 235:583-590.

Kanazawa, Satoshi. 2006. "Violent men have more sons: further evidence for the generalized Trivers-Willard hypothesis." *Journal of Theoretical Biology* 239:450-459.

Keller, Matthew C., Randolph M. Nesse, and Sandra Hofferth. 2001. "The Trivers-Willard hypothesis of parental investment: no effect in the contemporary United States." *Evolution and Human Behavior* 22:343-360.

Laibson, David I. and Richard Zeckhauser. 1998. “Amos Tversky and the Ascent of Behavioral Economics.” *Journal of Risk and Uncertainty* 16:7-47.

Lopreato, Joseph and Timothy Crippen. 1999. *Crisis in Sociology: The Need for Darwin*. New Brunswick, NJ: Transaction Publishers.

Maconochie, Noreen and Eve Roman. 1997. "Sex ratios: are there natural variations within the human population?" *British Journal of Obstetrics and Gynaecology* 104:1050-1053.

McLain, D.K., Setters, D., Moulton, M.P. and Pratt, A.E.. 2000. Ascription of resemblance of newborns by parents and nonrelatives. *Evolution and Human Behavior* **21**: 11–23.

Miller, Geoffrey F. 1998. “How Mate Choice Shaped Human Nature: A Review of Sexual Selection and Human Evolution.” In *Handbook of Evolutionary Psychology: Ideas, Issues, and Applications,* edited by C. Crawford and D. L. Krebs (87-130). Mahwah, NJ: Lawrence Erlbaum Associates.

Moffitt, Terrie E., Avshalom Caspi, and Michael Rutter. 2005. “Strategy for Investigating Interactions Between Measured Genes and Measured Environments.” *Archives of General Psychiatry* 62:473-481.

Norberg, Karen. 2004. “Partnership Status and the Human Sex Ratio at Birth.” *Proceedings of the Royal Society B: Biological Sciences* 271(1555): 2403-2410.

Pinker, Steven. 1997. *How the Mind Works*. New York: Norton.

Pinker, Steven. 2002. *The Blank Slate: The Modern Denial of Human Nature*. New York: Viking.

Pollard, Michael S. 2003. "Emerging Gender Indifference?: Demographic Indicators of a Changing Gender System." Department of Sociology, Duke University.

Rossi, Alice S. 1984. “Gender and Parenthood (American Sociological Association, 1983 Presidential Address).” *American Sociological Review* 49:1-19.

Satz, Debra and John Ferejohn. 1994. "Rational Choice and Social Theory." *The Journal of Philosophy* 91:71-87.

Segerstråle, Ullica. 2000. *Defenders of the Truth: The Battle for Science in the Sociobiology Debate and Beyond*. Oxford: Oxford University Press.

Serpell, James. 1986. *In the Company of Animals: A Study of Human-Animal Relationships*. New York: Blackwell.

Shostak, Sara. 2003. “Locating gene-environment interaction: at the intersections of genetics and public health.” *Social Science & Medicine* 56: 2327-2342.

Smuts, Barbara. 1995. “The Evolutionary Origins of Patriarchy.” *Human Nature* 6:1-32.

Symons, Donald. 1995. "Beauty is in the Adaptations of the Beholder: The Evolutionary Psychology of Human Female Sexual Attractiveness." Pp. 80-118 in *Sexual*

Nature/Sexual Culture, edited by P. R. Abramson and S. D. Pinkerton. Chicago: University of Chicago Press.

Thornhill, Randy and Craig T. Palmer. 2000a. *A Natural History of Rape: Biological Bases of Sexual Coercion*. Cambridge, MA: The MIT Press.

Tooby, John and Irven DeVore. 1987. “The Reconstruction of Hominid Behavioral Evolution Through Strategic Modeling.” in *The Evolution of Human Behavior: Primate Models*, edited by W. G. Kinzey. Albany, NY: SUNY Press.

Tooby, John and Leda Cosmides. 1989. "Evolutionary Psychology and the Generation of Culture: Part I. Theoretical Considerations." *Ethology and Sociobiology* 10:29-49.

Tooby, John and Leda Cosmides. 1990. "The Past Explains the Present: Emotional Adaptations and the Structure of Ancestral Environments." *Ethology and Sociobiology* 11:375-424.

Tooby, John and Leda Cosmides. 1992. "The Psychological Foundations of Culture." Pp. 19-136 in *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, edited by J. H. Barkow, Leda Cosmides, and J. Tooby. Oxford: Oxford University Press.

Tooby, John and Leda Cosmides. 2000. Unpublished letter to *The New Republic*.

Trivers, Robert L. 1974. *Parent-offspring conflict.* American Zoologist, 14, 249-264.

Trivers, Robert L. 1985. *Social Evolution*. Menlo Park, CA: Benjamin/Cummings.

Trivers, Robert L. and Dan E. Willard. 1973. "Natural Selection for the Parental Ability to Vary the Sex Ratio of Offspring." *Science* 179:90-92.

Turkheimer, Eric. 1998. "Heritability and Biological Explanation." *Psychological Review* 105:782-791.

Wade, Michael J., Stephen M. Shuster, and Jeffery P. Demuth. 2003. "Sexual selection favors female-biased sex ratios: the balance between the opposing forces of sex-ratio selection and sexual selection." *The American Naturalist* 162:403-414.

Wahlsten, Douglas. 1990. "Insensitivity of the analysis of variance to heredity-environment interactions." *Behavioral and Brain Sciences* 13:109-161.

Wilson, Edward O. 1975. *Sociobiology: The New Synthesis*. Cambridge, MA: Harvard University Press.

Wilson, Edward O. 1978. *On Human Nature*. Cambridge, MA: Harvard University Press.

Wilson, Edward O. 1984. *Biophilia*. Cambridge, MA: Harvard University Press.

Winterhalder, Bruce and Eric Alden Smith. 2000. "Analyzing adaptive strategies: Human behavioral ecology at twenty-five." *Evolutionary Anthropology* 9:51-72.

Wright, Robert. 1994. *The Moral Animal: Evolutionary Psychology and Everyday Life*. New York: Vintage Books.