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MAKING LOVE OUT OF NOTHING AT ALL? NULL FINDINGS AND THE TRIVERS-WILLARD HYPOTHESIS[1]

Kanazawa (2000, 2001; Kanazawa and Still 1999) has established himself as one of the leading enthusiasts of evolutionary psychology in sociology. In the preceding comment to our article, he succinctly introduces some of the basic principles of evolutionary psychology to a sociological audience that may be largely unfamiliar with this new program. His introduction also conveys the excitement that many evolutionary psychologists feel toward the enterprise. We too are interested in the application of evolutionary reasoning to sociological problems, but we are much more agnostic on the question of how valuable Darwinian approaches will ultimately prove for sociology. Kanazawa and we agree, however, that scholars should try to move these debates to empirical grounds whenever possible.

In this spirit, our original study sought to test the application of a longstanding sociobiological hypothesis, the Trivers-Willard hypothesis (hereafter TWH), to parental investment as it has been commonly considered by sociologists (Freese and Powell 1999). In doing so, we were pursuing various recommendations that sociologists should do more to incorporate Darwinian insights into their work (e.g., van den Berghe 1990; Ellis 1996) and following Trivers and Willard's (1973) own conjecture that their hypothesis was applicable to contemporary American society. Using two national data sets and a broad range of sociological measures of parental investment, we found no evidence for TWH for investment in adolescents in contemporary American society.[2] Kanazawa, however,

[1] We thank Satoshi Kanazawa for generously making available the data extraction files from his analysis of the National Survey of Families and Households. We also thank Simon Cheng for his assistance. The research was partially supported by NIMH grant (PHS-T32 MH14588) for Freese and NSF grants (SBR-9810435 and SBR-9912267) for Powell. Direct correspondence to Jeremy Freese, Department of Sociology, 1180 Observatory Drive, University of Wisconsin, Madison, Wisconsin 53706. E-mail: jfreese@ssc.wisc.edu

[2] While these results fail to support the TWH, we disagree with the characterization of them as "throwing into question the relevance of evolutionary psychology for traditional sociological problems" (p. XX). Adjudicating this matter requires much more empirical and theoretical consideration than any single test of a single hypothesis can provide. In addition, we are reticent about referring to the TWH as an evolutionary psychological hypothesis, because it predates the specific program of evolutionary

argues that our original study is compromised by an erroneous selection of dependent variables. Using a different data set and a different measure of parental investment, Kanazawa finds results that appear to support the hypothesis, which he says provide "just another brick in the wall" of the empirical foundation of evolutionary psychology (p. 1774).

While we applaud the effort to provide further testing of the hypothesis, we warn against slapping on the mortar just yet. Our reply first addresses Kanazawa's criticism of our choice of dependent measures. Then, we question Kanazawa's own selection of the sole dependent variable he uses in his analysis. Finally, we take another look at Kanazawa's analysis and dispute that the data provide any persuasive support for the hypothesis.

DOES KANAZAWA'S CRITICISM COMPROMISE OUR STUDY?

Kanazawa's criticism of our study is confined to one point: our choice of dependent variables related to education in testing the TWH.[3] The contested variables include measures directly related to higher education, like savings for college, as well as measures like how much parents talk to their child about school. Together, they comprise seven of the twelve variables that we examine in the National Educational Longitudinal Study (NELS), and four out of the six from High School and Beyond (HSB). We dispute his criticism below, but important to note first is that even if the criticism were correct, it still leaves seven dependent variables across the two large, national data sets in our study. Tests of these variables using two different measures of status yielded only one significant effect in the predicted direction—no more than what one would expect by chance—but three significant effects in the opposite direction. Considerable sensitivity analyses, reported in the text and footnotes of the original article, failed to yield any stronger evidence for the hypothesis. Thus, one could grant Kanazawa's criticism of these variables entirely but still see

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psychology and because many of those who have done work on the hypothesis are associated more with sociobiology and Darwinian anthropology than with evolutionary psychology (see Cronk 1991; see Segerstråle [2000] for discussion of some of these distinctions among different programs).

[3] In a footnote, Kanazawa offers another possible explanation for our null findings. Kanazawa suggests that our results may be compromised by "sample selection bias" because the mean reported family income of our estimation sample using NELS is $41,600, which he considers "inordinately high" (p. XX). In comparison, he notes that the mean family income of all NSFH respondents is $29,100. The comparison is not valid: instead, to be (roughly) comparable to NELS, the NSFH sample must be restricted only to those respondents *who have an adolescent child.* The mean family income for these NSFH respondents is $34,600, and, when we adjust by the sample weights, this mean increases to $43,456.

our results weighing against the application of the TWH in contemporary society.

Nonetheless, we are skeptical of his criticism. Kanazawa objects to our using education variables because educational investment yields earnings and status, which he contends were only important for the reproductive success of males in our hunter-gatherer past. Consequently, he proposes that our evolved psychological mechanisms "would consider (albeit completely unconsciously) investment into children's education as parental investment only for sons, not for daughters" (p. 1767). In effect, this criticism asserts that the TWH is not directly relevant for "parental investment" as the concept primarily has been used by sociologists, who have emphasized its connection to socioeconomic outcomes (see the references we provide for the measures in our original study). Instead, as his title suggests, Kanazawa's proposal restricts the scope of contemporary applicability of the TWH to measures that reflect a parental "desire to be with and take care of their children" (p. 1769), which he suggests is psychologically independent from what motivates parents to invest in education.

If recognized as a *post hoc* conjecture about how the TWH might still be important for understanding contemporary American parental behavior despite our null findings, we certainly agree the proposal is worth testing. We object, however, to Kanazawa's claim that the failure of the education measure is what proper reasoning from an sociobiological/evolutionary psychological perspective *would have predicted all along*, and thus our including these measures in our test was a mistake (a "moment of weakness" [p. 1767]). 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 TWH. Evolutionary psychological arguments supporting this proposal are easily devised: investments in daughter's capacity to gain resources and status could be interpreted as facilitating her acquisition of the best possible mate or promoting the welfare of her offspring. Or, because modern higher education has absolutely no analogue in the Pleistocene, investment in it could have been taken as governed by the same unconscious mechanisms of parental love as other behaviors, rather than being governed by a separate mechanism dedicated to status-linked investment. Or, even if educational investments were just partly governed by a desire to "take care" of one's children, this could still be interpreted as implying the observation of the *interaction effect* that is predicted by the TWH, in addition to the *main effect* of a general sex-specific bias.

Our position here is strengthened by Kanazawa's treatment of the one variable in our study for which a significant effect in the predicted direction was observed: parental monitoring as measured in NELS. Ka-

nazawa asserts that this measure is evolutionarily biased toward daughters. Rather than dismiss it as a "sex-specific" measure, however, he states that it too is consistent with his evolutionary interpretation because we find that "poor parents monitor their daughters more closely (thereby contributing to their daughters reproductive success) than wealthy parents" (p. 1768). Even if we set aside that the observed pattern was not robust,[4] and that Kanazawa's description of it is not actually correct,[5] one could still protest that it seems as though when an effect for a sex-biased measure is consistent with the TWH, it is interpreted as supportive, but when an effect for a sex-specific measure is inconsistent with the hypothesis, it is dismissed as irrelevant.

Finally, Kanazawa's criticism implies predictions of its own regarding parental investment in education that are not supported by the data. According to his argument, instead of testing whether a Trivers-Willard effect exists for measures related to education, we should have taken an evolutionary psychological perspective to predict that these measures should be strongly biased in favor of sons (as they are "investment only for sons, not for daughters" [p. 1767]). The analyses from our original study bear directly on this prediction. If we look at the effect of child's sex for the 11 measures of education (the main effects models of tables 3 and 5; see Freese and Powell 1999, pp. 1726–1727, 1734), three effects were significant in predicted direction (bias toward sons), and three were significant in the opposite direction (bias toward daughters). Consequently, the results do not provide any more systematic support for Kanazawa's evolutionary predictions in this sample than they do for the Trivers-Willard hypothesis.

DOES KANAZAWA OFFER A BETTER DEPENDENT MEASURE?

Kanazawa presents results from the National Survey of Families and Households (NSFH) that are asserted to support the TWH. Although below we provide further analyses of these data that cause us to question the robustness of this finding, we have more fundamental misgivings about the dependent variable used in his analysis. Most important, Kanazawa's lone criticism of our study is that we use education-related items to test the TWH, but the scale that he uses as his dependent measure

[4] The result was not robust across alternative model specifications, and the pattern also was not observed for our monitoring measure from HSB.

[5] Our results did not indicate that poor parents monitor their daughters more than wealthy parents. Instead, wealthy parents monitored their daughters more than poor parents did, but the disparity is less than the difference in how much more wealthy parents monitor their sons.

includes the amount of time parents spend "helping with reading or homework" (p. 1769). We do not understand why this item is acceptably "sex-neutral," while seemingly similar items from our study are dismissed as "male-specific." In justifying why his measure is "sex-neutral," he reports that "none of the component questions in my measure of parental investment refers specifically to higher education" (p. 1769). But, if by this he means that the sticking point is whether an item specifically concerns *college*, then several more measures from our original study should not have been challenged—only two of our measures from NELS and two from HSB refer specifically to college.

In other words, if the homework item is acceptable, then his critique of our article would seem substantially weakened, as then 14 of our 18 dependent variables would not be questioned. If the homework item is not acceptable, then it compromises the only dependent variable that Kanazawa's analysis examines. We should also point out that the correlation of the homework item with the other items in the scale is as high as the correlation of these items with one another. This would suggest that help with homework does not represent some different dimension of parental investment from the other activities that included in the scale, but Kanazawa's entire critique is predicated on just such a bifurcation.

In addition, Kanazawa's analysis has the ecological inference problem of attempting to test a hypothesis about investment in *an individual child* with a dependent variable that asks parents how much time they spend interacting with *their children* in various ways. His analyses examine how this measure is affected by the presence of daughters and sons ages 10–15 in the household, but *the measure does not distinguish time spent with these children from time spent with other children in the same household.* Consequently, the measure bears only indirectly on the specific sex-by-child effect predicted by TWH. We should point out that, following other sociological work on parental investment, our study did examine two household-level measures of investment (number of educational items in the home and involvement in a parent-teacher organization). Unlike Kanazawa's study, however, we look at many other measures of investment, and our conclusions thus do not rest exclusively on a household-level measure.

DOES KANAZAWA'S ANALYSIS SUPPORT THE TRIVERS-WILLARD HYPOTHESIS?

Even if we are to accept Kanazawa's dependent variable, we are not persuaded by the results of his analysis. For his regression analysis, Kanazawa does not use the modeling strategy that we used in our original

study, in which the significance tests of the hypothesis were provided by interaction terms. Instead, his regression analyses compute separate models for respondents in the lowest and highest quartiles, with dichotomous independent variables indicating the presence of daughters and sons ages 10–15 in the household. In the regression of his investment measure on the low-income subsample, the coefficient for daughters is positive and that for sons is negative; while in the high-income subsample, the signs of these coefficients are reversed. This provides impressionistic evidence for the hypothesis, but the actual predictions of the hypothesis are about the relationship among these coefficients, *not* the relationship of any or all of these coefficients from zero. Importantly, the pattern of coefficients could be as Kanazawa observes and the comparison could not be significant; alternatively, the coefficients could be all negative or all positive and yet a significant relationship could exist among them that is consistent with the hypothesis. The reason again is that the TWH concerns the *interaction effect* of child's sex and parental status; testing only whether the four coefficients differ from zero confounds the test of the interaction with any *main effects* of sex (or, for that matter, child's age) on parental investment.

Instead, under Kanazawa's specification, if one subtracts the coefficients from the regression on the highest quartile sample from the coefficients from the lowest quartile sample, the TWH predicts that the difference for the variable "daughter age 10–15" will be greater (more positive) than the difference for the variable "son age 10–15." To test the hypothesis, we respecified the two regressions as a single model with interactions of each independent variable by status group and then conducted a Wald test of the null hypothesis $\beta_{LO,DAU} - \beta_{HI,DAU} = \beta_{LO,SON} - \beta_{HI,SON}$.[6] In reconstructing Kanazawa's analysis, we discovered a few minor errors in coding, which affect the sample size. Our table 1 presents the corrected estimates of the key effects from Kanazawa's original study for the analysis of children 0–18.[7] The Wald test is significant at the $P < .01$ level.

Thus, although Kanazawa does not explicitly provide a significance test

[6] Following Kanazawa and our earlier study, we use two-tailed tests, although of course those who prefer one-tailed tests for directional hypotheses may divide the P-values by two. We are also more strongly inclined toward two-tailed tests here because in some of our ancillary analyses on these data the results were not consistently in the predicted direction.

[7] Because of space limitations, we present results only for households with any children ages 0–18 here, rather than also including analyses of households only with children ages 5–18. This effectively provides a more liberal examination of the robustness of the findings, as in Kanazawa's original analysis it was this larger subsample that had the seemingly stronger results.

of the TWH, the results of his baseline model do appear to provide statistically significant support for the hypothesis. At the same time, it might seem odd that the sample size for the top quartile in Kanazawa's analysis is more than three times larger than the bottom quartile. This is mainly due to the listwise deletion of cases with missing values for occupational prestige. (We believe that including this control is difficult to theoretically justify anyway and that it should have been excluded.[8]) We expected that recovering these deleted observations would strengthen the statistical significance of the pattern of regression results, as increasing the sample size typically does. Instead, the second column of table 1 reveals that when the control for occupational prestige is excluded, the P-value of the test actually increases to where it is only significant at the $P < .10$ level.

This is already not a strong finding, but we might be more persuaded by it if alternative specifications yielded at least similarly marginally significant results. We believe that examining the robustness of results is necessary to determine if a finding rests on analytic decisions that are not directly germane to the hypothesis. The remaining columns of table 1 present the results of various sensitivity analyses that we conducted. Each represents an alternative way that the analysis could have defensibly been done. What if parental education had been used to measure status instead of household income, as we had also tested in our study? What if the sample would have been divided into income sextiles instead of quartiles? (If the argument is that Trivers-Willard effects are only observed at the extremes, then using sextiles should result in larger differences than quartiles). What if we had run the analyses separately for mothers and fathers? What if the key age range examined had been children 13–16 years old—corresponding to the typical age of the eighth and tenth graders in our NELS and HSB samples—instead of 10–15 years?[9] What if respondents who had any coresident daughters ages 0–18 had been compared

[8] Put briefly, if one has already divided the sample into high and low status groups by family income, what is the purpose of holding occupational prestige constant within each status group, especially if education also is included as a control in the model?

[9] The issue of child's age is especially important, and we looked in more detail at how the results of the analysis changed depending on the age group that one considered. Although our original study focused on adolescents, the Trivers-Willard hypothesis is not specifically a theory of adolescent parental investment, but instead is not age-specific (see Anderson and Crawford 1993). As we pointed out, studies by sociobiologists have looked at the TWH in the context of human behaviors toward offspring ranging from infants to adult heirs (Freese and Powell 1999, p. 1737). For example, in one model, we compared the age groups 0–4 years, 5–9, 10–15, and 16–18. When the relevant differences among coefficients were computed, two age groups yielded results in the predicted direction, and two age groups yielded results in the opposite direction, none of which were significant. Alternative groupings by age yielded similarly haphazard results.

TABLE 1

STANDARDIZED OLS COEFFICIENTS OF THE EFFECT OF CHILD'S SEX ON KANAZAWA'S MEASURE OF PARENTAL INVESTMENT, BY PARENTAL STATUS

	Kanazawa Results (Corrected)[a]	Job Prestige Removed from Model	Status as Education Instead of Income[b]	Income by Sextiles Instead of Quartiles	Maternal Respondents Only[c]	Paternal Respondents Only[c]	Children Ages 13–16 Instead of Ages 10–15	Children Ages 0–18 Instead of Ages 10–15
Lowest status group:								
Daughter in age range	.155*	.042	−.014	.016	.060	−.062	−.069	.034
Son in age range	−.008	−.019	−.028	−.019	−.027	−.025	−.069	.024
Highest status group:								
Daughter in age range	−.017	.009	−.015	.057	.061	−.048	−.096**	.094**
Son in age range	.093**	.063*	.044	.065	.046	.077	−.069*	.124***
P-value of test of hypothesis ...	.003	.072	.329	.544	.328	.540	.736	.432
N (lowest group)	291	774	694	678	694	334	774	774
N highest group)	965	1,189	714	676	681	331	1,189	1,189

SOURCE.—National Survey of Families and Households, wave 1.

NOTE.—Models include controls for parent's sex, parent's age, race (black vs. all others), and parent education. "Age range" is 10–15 years unless otherwise specified. "Status group" is measured by income quartiles except other specified.

[a] Occupational prestige included as control in model.

[b] Household income used as control in model instead of parent education.

[c] Income quartiles calculated within subsample.

* $P < .05$, two-tailed test.

** $P < .01$

*** $P < .001$.

to respondents who had any sons? In none of these cases do the results provide significant support for the hypothesis. Also, note that in none of the alternative specifications do the four coefficients differ from zero according to the pattern that was taken as providing support in Kanazawa's comment. We must make plain that this was not a matter of our "fishing around" for alternative specifications that did not work, but rather we conducted some of the more obvious sensitivity findings to see if the finding is robust.

The fragility of the results becomes even clearer when we re-examine the graph of mean differences that follows Kanazawa's regression analysis (his fig. 1). The crossing pattern of lines may again provide impressionistic support, but no actual significance test is conducted. Our own calculations indicate that the relationship presented in this graph is at best marginally significant ($P = .127$; $P = .122$ for the other panel in Kanazawa's fig. 1),[10] but even this weak relationship is exaggerated by connecting the lines between the highest and lowest quartile means, which may suggest a linear relationship between income and investment. Panel a of our figure 1 reproduces Kanazawa's graph for households with children ages 5–18, while panel b provides a more fine-grained look at the relationship by dividing the data into deciles and calculating the means of the investment measure within each decile. This graph shows that the relationship between income, child's sex, and the dependent variable is far from straightforward. Yet this still exaggerates the extent to which the data conforms to the predicted pattern of the TWH, because women are disproportionately represented among lower-income respondents (86% of the lowest quartile respondents are female, compared to only 59% of the highest quartile respondents) and because fathers and mothers both, on average, report higher levels of parent-child activities when they have a same-sex child ages 10–15. Panels c and d present separate graphs for mothers and fathers, which even impressionistically do not lend support for the hypothesis. Conducting the same test of conditional means as before but taking mothers and fathers separately, we also find that in neither case do the differences between lowest and highest quartiles provide significant support for the hypothesis ($P = .51$ for mothers; $P = .49$ for fathers). This further corroborates our concern about the robustness of the claimed support for the TWH.

Kanazawa acknowledges that the results reported in his article are "quite weak" (p. 1774), but our sensitivity analyses suggest that they are so weak that their statistical significance is highly contingent on the re-

[10] We tested the difference in conditional means by specifying a regression model with terms for the interactions of child's sex and status (and no controls) and then testing the null hypothesis that $\beta_{LO,DAU} - \beta_{HI,DAU} = \beta_{LO,SON} - \beta_{HI,SON}$.

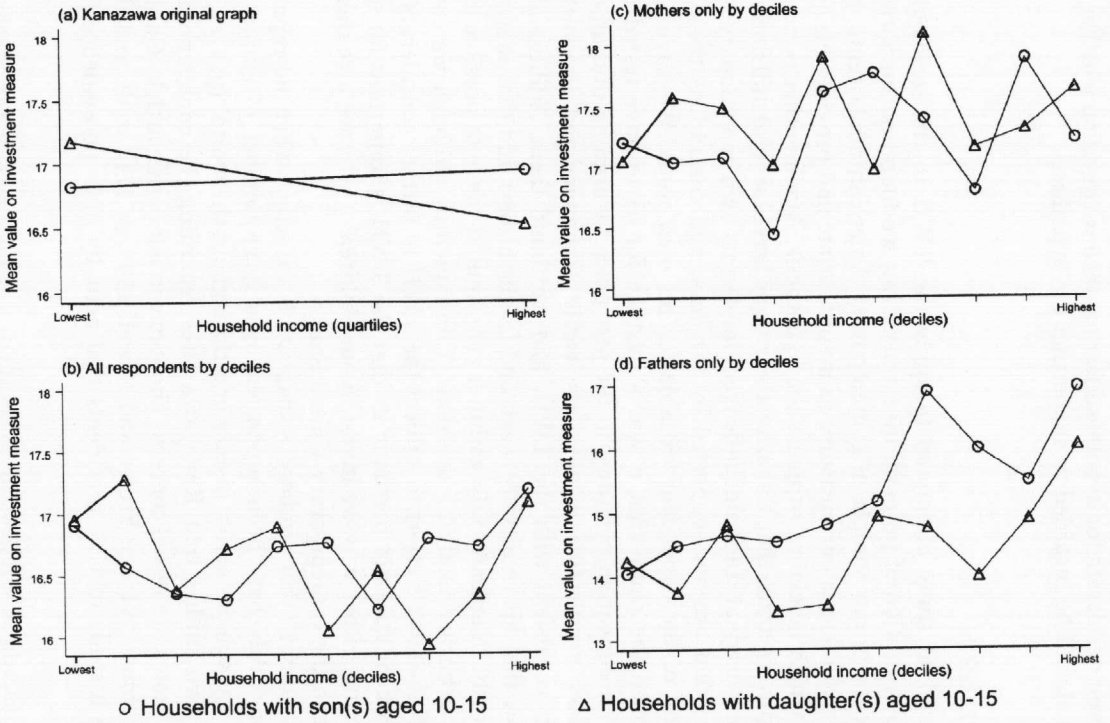

FIG. 1.—Graphs of parental investment by household income and sex of child, for households with children ages 10–15 years (National Survey of Families and Households).

searcher making a particular set of analytic decisions that are not directly related to the hypothesis (i.e., what age range of respondents to consider, how to divide sample into upper- and lower-status groups). Consequently, if one regards statistical significance and the robustness of results as being important desiderata for hypothesis testing, then we question whether the results should be regarded as supporting the hypothesis.

CONCLUSION

Our original study attempted to apply the TWH to the sociological conceptualization of parental investment, but we found no support for the theory in our sample of contemporary American adolescents. Kanazawa challenges our measures as inappropriate and provides his own study, which he claims supports the hypothesis. Among other points in this reply, we argue that Kanazawa's critique leaves several null findings from our original study unchallenged, implies its own evolutionary predictions that are not supported by the data, and contradicts the construction of the dependent variable for his own study. We have also questioned the claim that it was a mistake for us to have tested the TWH for education measures in the first place by arguing that an evolutionary perspective would have easily accommodated supportive findings consistent with the TWH, had we found them. Additionally, we show that his results are weak and fragile at best and that they are statistically significant only within a very limited range of possible and largely arbitrary analyst's decisions. Consequently, although we commend Kanazawa's effort to subject the TWH to further consideration, his comment does not persuade us that the TWH is important for understanding either why we invest in our children or why we love them, at least in the contemporary United States.

What about other studies? Kanazawa is incorrect when he reports that the TWH has "been supported by a large number of empirical studies on societies across history and throughout the world" (p. 1766).[11] Of the two studies that Kanazawa cites regarding the contemporary United States, we raised concerns about one in our earlier article (Gaulin and Robbins 1991; see Freese and Powell 1999, p. 1713 n. 6), and the other is a study of U.S. presidents that actually finds no evidence for

[11] The "comprehensive review" by Cronk (1991) that Kanazawa cites is actually a discussion of cases of female-biased parental investment, not the TWH per se. Only 13 selective groups are summarized (pp. 410–11), and of these only eight have patterns that are claimed to possibly fit the Trivers-Willard model. The only contemporary developed society among these eight is the United States (based on Gaulin and Robbins [1991]).

the hypothesis in its more contemporary sample (Betzig and Weber 1995). By the same token, recent analyses of other large data sets also do not provide support for the TWH among younger children. Using the Child Development Supplement to the Panel Study of Income Dynamics, Keller, Nesse, and Hofferth (in press) find no support for the TWH in tests that include four dependent variables that would seem to reflect closely on parental love and that have nothing to do with education: two measures of the warmth of the parent-child relationship (parent report and interviewer assessment) and two measures of the amount of time parents spend with their child. We have also examined the applicability of the Trivers-Willard hypothesis to the investment measures in the recently available Early Childhood Longitudinal Study—some of which are related to education, and some not—and we also find no support for the hypothesis for these families of kindergarten-age children (Freese and Powell 2001). Taken together, these studies increase the number of null findings that weigh against the American application of the hypothesis.

In trying to explain why weak effects might be observed in the contemporary United States, Kanazawa suggests that the material wealth of the United States has reduced the "necessity to choose one sex or another," which he identifies as "an important scope condition of the [TWH]" (p. 1774). We are skeptical of this specific conjecture, given that investment in potential offspring, especially in contemporary societies, is balanced against a host of other drains on adult resources (perhaps of greatest sociobiological interest, investment in other kin, status seeking, and efforts to secure additional mating opportunities). In any event, many of the tested measures concern uses of *time*, which contemporary parents do not obviously possess abundantly more of than their Pleistocene ancestors.

Nevertheless, it is perfectly possible that the evolved psychological mechanisms suggested by TWH do exist but that the predicted behavioral patterns are not observed because of some aspect of the novelty of contemporary, developed societies. Before we put too much stock in this, however, we would like to see more systematic cross-cultural evidence that favors the TWH. As things stand now, we are concerned by the seeming *presumption* that mechanisms shaped by a Trivers-Willard dynamic are "part of our innate human nature" (p. 14), and that null findings are merely elaborating the exceptional scope conditions of this default state of mind. Such a presumption needs to be closely grounded in evidence or else it only increases the impression that some

sociobiologists, not unlike some advocates of other theoretical programs, assume the truth of propositions that they should be more judiciously testing.

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