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Levels of Explanation in Sociobiology and Psychology: A Rejoinder to Archer

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Offers a time continuum for understanding levels of explanation ranging from distal, evolutionary perspectives through trait and social learning accounts to proximate cognitive and situational analyses. No necessary conflicts exist between these levels, but we argue, contrary to the position advocated by Archer (1988) in his critique of our 1986 study on the sociobiology of bereavement, that distal levels of explanation can at times transcend proximate levels, although each must be taken into account. In addition, we provide rebuttal of some of the specific points made by Archer by discussing (a) the role of the adaptationist program in human sociobiology, (b) the measurement of complex phenomena, and (c) whether older children are grieved for more than younger children.

There is a maxim attributable to Theodosius Dobzhansky that "in biology nothing makes sense except in the light of evolution" (1970, pp. 5-6). Our study on the sociobiology of bereavement was designed to test predictions derived from evolutionary theory to explain the variability in grief intensity of family members following the death of a child (Littlefield & Rushton, 1986). In a critique of our work, Archer (1988) reversed Dobzhansky's dictum, claiming that sociobiologically based predictions are better derived "from proximate considerations rather than genetic [ones]" (p. 272). In this article we respond to Archer's criticisms.

Littlefield and Rushton's (1986) Study

As we pointed out, a repeated criticism of human sociobiological theorizing has been that it is too often reconstructionistic; that is, imaginative stories are invented to explain post hoc the origin of behavior traits by natural selection. Our 1986 study attempted to deal directly with this criticism by deriving and testing novel a priori predictions in the context of bereavement to ascertain whether these were indeed borne out in the world of experience.

Examining distal sociobiological tenets relating to energetic investment and the potential of parents and children as future DNA replicators, we predicted that (a) mothers would grieve more intensely than fathers; (b) healthy children would be grieved for more than unhealthy children; (c) male children would be grieved for more than female children; (d) health and sex of child would interact such that the rank ordering of grief intensity would be as follows: healthy male children > healthy female children > unhealthy female children > unhealthy male children; (e) similar children would be grieved for more than dissimilar children; (f) older children would be grieved for more than younger children; (g) older parents would grieve more than younger parents; (h) parents without additional children would grieve more than parents with additional children; (i) the rank ordering of grandparental grief would be as follows: maternal grandmother > maternal grandfather = paternal grandmother > paternal grandfather; and (j) mothers' siblings would grieve more than fathers' siblings. The majority of our predictions were confirmed, and we concluded that support had been provided for the validity of the sociobiological perspective. In discussion we considered the proximal mechanisms, including differential attachments, that might have mediated the relationships.

Archer's (1988) Critique

Archer's (1988) discussion ranged from general issues in sociobiology to how to conceptualize the complexity of human emotional adjustment to highly particular points about methodology. We agree with some of the issues he raised, although they were presented as though contradicting our study. Others left us wondering about their relevance, and still others appeared to stem from a basic misunderstanding of both the main point of our study and the appropriateness of applying evolutionary theory to human behavior. On reflection, it seemed to us that the wellspring of his objections emanated from the fact that we approached human behavior from a distal and reductionistic perspective. Although Archer discussed proximate and ultimate explanations and functional and causal analyses at length, we found the upshot of this discussion to be confusion rather than clarification. Because this "levels of explanation" problem is central to our study (as well as underpinning numerous other controversies in psychology), yet is often misunder-

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stood by both friends and critics of sociobiology, we begin by presenting our own viewpoint on the issue. We then address the other points raised by Archer, following his ordering of topics.

Distal–Proximal Dimension

No necessary conflicts exist between evolutionary and genetic analyses and those from social learning and situational perspectives. Too many errors and unnecessary debates have occurred as a result of confusing distal and proximal levels of reasoning (see Figure 1). Proximate levels emphasize the environmental, cognitive, and physiological mechanisms involved; distal explanations consider the significance of phenomena from perspectives further back in time, ultimately in evolutionary terms of reproductive fitness.

When explanations move from distal to proximal, controversy does not ensue. Resistance is more likely, however, as explanations move the other way. Proximal wariness of distal explanation may be due in part to concern about extreme reductionism; for example, that learning is only secondary to genetics. Unfortunately, most researchers seem devoted to an exclusive orientation. It is not the norm, for example, for cognitive social learning theorists to be knowledgeable about behavior genetics or for trait theorists to entertain behaviorism. In our view, each of the levels provides a unique perspective for understanding behavior; however, contrary to Archer's opinion, we also argue that distal levels can sometimes transcend proximate ones. This is because they can provide a deeper and more generalized understanding of phenomena than is achieved by focusing on particulars. By modernizing Samuel Butler's famous aphorism that a chicken is only an egg's way of making another egg, that is, "the organism is only DNA's way of making more DNA" (Wilson, 1975, p. 3), interesting insights into behavior have been suggested. From the perspective of "the selfish gene" (Dawkins, 1976), if the proximate mechanisms underlying reproductive behavior in humans include the hypothalamus and limbic system as well as cultural norms and feelings of love and loyalty, this is because these are specific processes contributing to the perpetuation of DNA. Different mechanisms are involved in other species, although their ultimate effect remains the same. We now turn to the other points raised by Archer.

Is Grief Adaptive?

Following the lead of Gould, Lewontin, Kitcher, and other critics of human sociobiology, Archer (1988) provided a general dismissal of what they call the "adaptationist program" (the mistaken notion that every feature of living organisms has immediate adaptive advantage, that is, contributes directly to reproductive fitness). Having once more slain this straw opponent, Archer paradoxically charged us with a failure "to consider whether grief itself is adaptive, or whether it is the by-product of some other adaptive feature" (p. 273). We find this a most odd way of arguing (Damned if you do; damned if you don't!) Our study did not address any theories of the "adaptiveness" of grief, and we mentioned it only for the sake of completion in a concluding paragraph. We are sympathetic to Archer's hypothesis that bereavement is part of a syndrome of responses

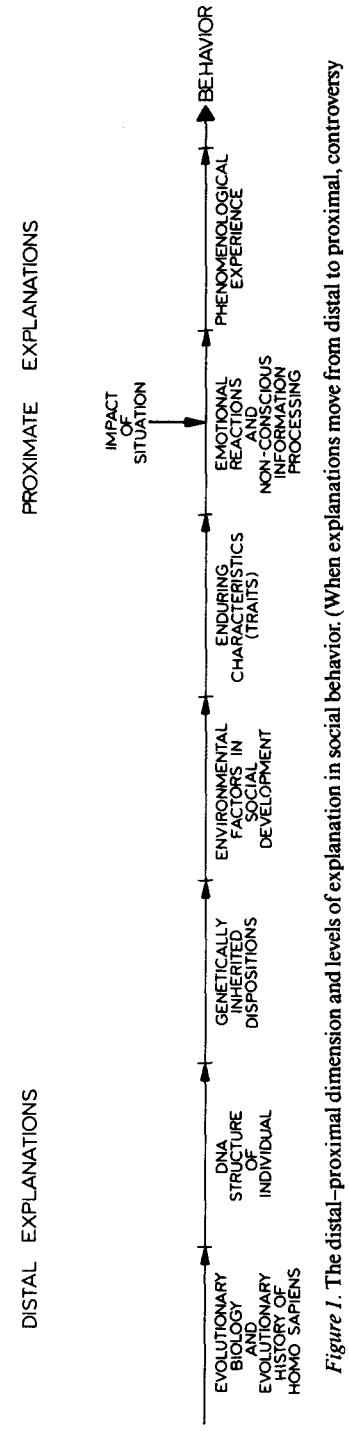

Figure 1. The distal–proximal dimension and levels of explanation in social behavior. (When explanations move from distal to proximal, controversy does not ensue, whereas the converse is less true; after Rushton, 1988.)

to separation from a loved one, which includes preoccupation, searching, and anger, and which in the majority of cases may be adaptive as it helps to reunite the individual with the lost person. We fail to see how this quite reasonable hypothesis in any way contradicts either the spirit or the findings of our article.

Dawkins (1982) and Mayr (1983) have discussed the adaptationist controversy in detail, pointing out that as a working hypothesis, adaptationism has undoubtedly been the inspiration for some outstanding discoveries. A good example would be the demonstration by von Frisch (1967) in controlled experiments of color vision in fish and honeybees. Von Frisch was driven to undertake these experiments by his refusal to believe that, for example, the colors of flowers were there for no reason or simply to delight people's eyes. Although naive functionalism may be lampooned by critics of sociobiology, approaches to adaptive function that attempt to specify the nature of the mechanisms involved are, in fact, close to being a scientific truism. Are sociobiologists really wrong to consider what the possible evolutionary functions are of the brain? Or of love and attachment? Or of the despair of loss?

Complexity of the Grief Process

Archer's (1988) next points concerned the complexity of the grief process and the ensuing inappropriateness of measuring it on a 7-point scale ranging from *no grief* (1) to *total devastation (suicide point)* (7). Of course grief is complex, and we do not disagree with Archer's componential analysis. In fact, our article reported data on a 97-item, multidimensional Grief Experience Inventory, which Archer fails to mention. As the total scores on this inventory correlated $r(260) = 0.52, p < .001$, with the respondents' rating of their own grief on the 7-point scale and provided no greater sensitivity in analysis, it seems clear that the 7-point scale is tapping valid variance. Archer's dismissal that "it is unlikely that grief can be meaningfully quantified in such terms" (p. 274) is simply mistaken. Such variegated phenomena as satisfaction judgments, mood, personality, intelligence, and aesthetics can be usefully scaled in this manner.

Psychometric Issues

In addition to his theoretical argument against measurement, Archer (1988) claimed that our particular operations were flawed. For example, he asserted that interobserver reliabilities of 0.5 "be regarded as unacceptably low" (p. 275), that aggregating across different estimates while reducing error variance "will not turn an effect of low magnitude into a larger one" (p. 275), and that, in any case, we should have considered the magnitude of effect sizes rather than "overrelying" on probability tests.

The case for aggregation effects has been made elsewhere (e.g., Rushton, Brainerd, & Pressley, 1983). Contrary to Archer's (1988) reading, the literature shows that multiple assessments not only reduce error variance but also increase magnitude estimates. This general methodological point also applies to effect sizes. For example, in a recent reanalysis of sex differences in empathy, for 7 studies of reflexive crying in infants, a correlation of .69 ($p < .05$) was found between the size of the effect favoring girls and the total time possible to be measured in the dependent variable; and for 21 studies using questionnaires, a correlation of .73 ($p < .001$) occurred between the size of the effect favoring girls and total number of items in the questionnaire (Rushton, 1988). Thus, the greater the aggregated variability in the dependent variable, the greater the effect size. Finally, we cannot resist querying how, if so much error variance swamped our measures, we were able to partition the variance in such predictable ways. Error, of course, or at least unsystematic error of the kind being discussed, has the effect of *reducing* the significance of effects. In other words, if we had been able to use even more reliable measures, our predictions would have been even more strongly confirmed.

Specific Hypotheses

Archer (1988) elaborated on each of our hypotheses, sometimes providing alternative rationales from sociobiological theorizing and other times showing how cultural factors can influence outcomes. It is not clear to us what Archer's intent was, other than to show that alternate and sometimes more complex rationales can be generated. We encourage the interested reader to align and compare the principles underlying each of our hypotheses with those outlined by Archer. We believe that whereas there is a systematic a priori justification for ours, those generated by Archer are uneconomic. Consider one example for which new data are at hand. In Hypothesis 6, we predicted that older children would be grieved for more intensely than younger children, because they represent a greater loss of energetic investment. We attributed our inability to confirm this prediction to a "restriction on range in the ratings" (Littlefield & Rushton, 1986, p. 802). Archer claimed we did not find the relation because this was an unsound prediction to begin with, based on a "logical flaw" he calls the *Concorde fallacy* (p. 276). A more extensive rating study deriving predictions from Fisher's (1930) theory of reproductive value has, in fact, confirmed the prediction, showing that the expected grief intensity for an offspring increases to a peak when the offspring is a young adult and then declines as the offspring gains in years to the point of no longer being reproductively viable (Crawford, Salter, & Jang, in press).

Conclusion

We concur with Archer's (1988) final paragraph that sociobiological theorizing aims not to replace studies of proximate mechanisms, but rather to inform them. As clearly shown in Figure 1, there is no necessary conflict between different levels of explanation. We do not, however, accept the limiting position advocated by Archer that it is inappropriate to argue directly from distal positions to human behavior, for we believe not only that each level can be useful, but also that particularly novel insights into behavior can be derived from distal perspectives.

References

Archer, J. (1988). The sociobiology of bereavement: A reply to Littlefield and Rushton. *Journal of Personality and Social Psychology, 55,* 272–278.

Crawford, C. B., Salter, B. E., & Jang, K. L. (in press). Human grief: Is its intensity related to the reproductive value of the deceased? *Ethology and Sociobiology.*

Dawkins, R. (1976). *The selfish gene.* Oxford, England: Oxford University Press.

Dawkins, R. (1982). *The extended phenotype: The gene as the unit of selection.* Oxford, England: W. H. Freeman.

Dobzhansky, T. (1970). *Genetics of the evolutionary process.* New York: Columbia University Press.

Fisher, R. A. (1930). *The genetical theory of natural selection.* Oxford, England: Clarendon Press.

Littlefield, C. H., & Rushton, J. P. (1986). When a child dies: The sociobiology of bereavement. *Journal of Personality and Social Psychology, 51,* 797–802.

Mayr, E. (1983). How to carry out the adaptationist program? *American Naturalist, 121,* 324–334.

Rushton, J. P. (1988). Epigenetic rules in moral development: Distal–proximal approaches to altruism and aggression. *Aggressive Behavior, 14,* 35–50.

Rushton, J. P., Brainerd, C. J., & Pressley, M. (1983). Behavioral development and construct validity: The principle of aggregation. *Psychological Bulletin, 94,* 18–38.

Von Frisch, K. (1967). *A biologist remembers.* Oxford, England: Pergamon.

Wilson, E. O. (1975). *Sociobiology: The new synthesis.* Cambridge, MA: Harvard University Press.

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