pothesis can produce conflicting predictions depending on what particular branch of evolutionary biology is used. Thus they "predict" violence by husbands against young wives (on the basis of intermale sexual rivalry over females), whereas an evolutionary hypothesis could also predict violence against older wives—on the basis of their lower reproductive value.

Again the point is not to argue for my explanation instead of theirs. In fact, I am fairly sure that some of Daly and Wilson's explanations are correct. However, in a field as sensitive as the one they address, which many will perceive as the subject of genetic control of predispositions to homicide, sociobiologists surely have a duty to be far more circumspect in their presentation of such potentially divisive hypotheses? If they are not, understanding of human behavior will suffer as sociologists, anthropologists, psychiatrists, and psychologists ignore as irrelevant what they see as the overly simplistic theories of sociobiology.

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Response: Readers of our article will not find case-by-case pseudo-explanations of homicides, as in George's caricature. What they will find is a number of novel hypotheses about demographic and circumstantial correlates of fatal conflict within families, derived from consideration of how selection is likely to have acted upon social psychologies, as well as various analyses showing that the hypothesized correlates are indeed powerful predictors of homicide risk. Risks to stepchildren, for example, had never been assessed before an evolutionary model of parental inclinations inspired us to make the relevant comparisons. Our discovery that such children are several dozen times more likely to be slain than genetic offspring seems to us to warrant serious concern rather than dismissal as "storytelling."

George asserts that anything could be "predicted by the theory" with the implication that "the theory" is vacuous. The only general theory informing our article is that of evolution by selection, and his complaint is nothing more or less than the amply refuted old chestnut that the theory of evolution can explain everything and therefore is not science. There are many models within modern behavioral ecology, some of them mutually contradictory. Some will turn out to apply to certain species or circumstances but not others, and some will fail completely. George apparently believes that the recognition of complexities arising from multiple causal factors (such as the countervailing pressures for nepotistic discrimination and for reciprocity with nonrelatives) constitutes a sort of post hoc waffling. Unfortunately, social phenomena are complex and multiply determined. They will not be elucidated by single-factor models nor by doctrinaire hostility to the use of evolutionary reasoning to generate testable hypotheses about human psychology and action.

Of course, any of our particular hypotheses might have failed. They did not, and so we discovered several previously unidentified risk factors for homicide. George suggests sarcastically that if we had found that people typically discriminate against their own offspring, we would claim to have predicted that, too. The truth is that such a discovery would be very surprising in any animal species, but would not constitute reason to disinter Darwin from Westminster before one had made serious efforts to generate potential evolutionary explanations of the unusual case, with testable corollaries.

Finally, by the narrow equation of science with "experiment," George would banish by fiat not only all studies of homicide but virtually all of sociology, epidemiology, economics, and astronomy.

Harcourt's complaint that we "make no attempt" to test our hypotheses against alternatives is baffling. We derived and tested

detailed alternative predictions, such as (i) the distinct gender contingencies in filicides and parricides to be expected under particular psychoanalytic versus evolutionary models of parent-offspring conflict, and (ii) the patterns of kinship among participants in homicides expected under a "nepotism" model versus those expected under the only alternative in the criminological literature, namely that relationship-specific rates of violence reflect differential opportunity. Elsewhere, limited by available space, we clearly referred readers to other such tests, as for example in citing research showing that differential violence by stepparents versus genetic parents is "independent of risk attributable to low socioeconomic status, maternal youth, family size, or personality characteristics of the abusers."

It is a shame that Harcourt chose not to "spell out" the alternative "economic hypothesis" that can "explain all the trends" we describe. It is far from obvious why "small children being easier to kill than older ones" would apply to their parents but not to nonrelatives or how the "costs and benefits of the homicidal act itself and of the participants to one another" would account for stepparents being vastly more likely to kill their wards than genetic parents, or why Canadian men are more likely to kill younger than older wives when they are also more severely penalized for doing so (*1*). But the more general point is that Harcourt's contrasting of "an economic analysis" with what he calls "the evolutionary hypothesis" represents a category error: economic explanations of the acts of individuals invoke unanalyzed utilities (subjective costs and benefits), while evolutionary psychology addresses the questions of what social and other "resources" are likely to have utility at all, and why species-typical utility functions take one form rather than another.

Perhaps Harcourt's mistaken belief that good science would pit a monolithic "evolutionary hypothesis" against "alternatives" such as "economics" also explains his complaint about our exploring the implications of diverse evolutionary arguments. Why else can he not fathom the "logic" of invoking life history theory when considering life historical variables, sexual selection theory when discussing sexual rivalry, and kinship theory when discussing nepotistic discrimination? As for the possibility of generating alternative hypotheses within an evolutionary framework, we have elsewhere discussed the same example as Harcourt (*1*, p. 13)

> . . . in using selection thinking as an heuristic for generating psychological hypotheses and predictions about homicide, we do not pretend to be "testing" some monolithic "Darwinian theory." On the contrary, imaginations informed by evolutionary theory often generate *alternative* hypotheses about how selection might have shaped mind and behavior. At what age would you expect wives to be maximally at risk of homicide by their husbands? Selection thinking suggests the hypothesis that postmenopausal women will be least valued by men and therefore most at risk, but it also suggests an alternative, namely that wife-murder is the tip of the iceberg of the coercive violence that men employ to control the *most* reproductively valuable women. . . . There is nothing embarrassing or "unscientific" about the fact that imaginations informed by evolutionary theory can generate alternative scenarios. Selection thinking is not merely "a theory," but a paradigm, and it suggests fresh lines of inquiry. (To the best of our knowledge, no previous student of homicide had ever thought to inquire whether the risk of spouse-murder might be systematically related to the parties' ages.)

Our article documented major variations in family homicide rates in relation to demographic and circumstantial variables. We discovered these risk factors only because we considered the social psychology of interpersonal conflict from an evolutionary perspective, using homicides as an extreme but exceptionally valid assay of such conflict. Others may devise truly alternative explanations for some of the phenomena we have discovered and may demonstrate their superiority. That is science and we will applaud any such progress.

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1. M. Daly and M. Wilson, *Homicide* (de Gruyter, Hawthorne, NY, 1988).