

6 *From Genes to Incest Taboos*

THE CRUCIAL STEP

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Not in Our Genes?

Today the idea that an evolutionary approach may be fruitful for research in the social sciences is being passionately defended by some and no less passionately contested by others. The resistance to Darwinism comes mainly in two distinct varieties. The first type of criticism is based on empirical or methodological objections against the current attempts to use evolutionary considerations to throw some light on social science explananda. The other line of opposition, however, is much harder to pin down and discuss because it is fueled more by rhetoric than by argument. It defines itself, rather vaguely, as a fight against “biological reductionism” and “genetic determinism” and is often accompanied by slight (or not so slight) ideological overtones. In this chapter, I will deal only with the former (methodological) kind of criticism. But since I don’t want to leave the latter, hazily antireductionist source of opposition to biology without comments, and since I don’t know how to approach it in a serious way, let me wiggle out by presenting to you a rhymed parody, “Gene-mania,” that captures some of the more ideological criticism’s characteristic flavor:

GENE-MANIA

Who today is not sick and tired
Of all those guys so gene-inspired?
They find a gene for every this or that:
For being gay, smart, alcoholic, fat . . .
We have to stop that madness. Take no offense,
But this approach doesn’t make much sense.
True, some fools thought after Watson-Crick
That genes could really do the trick.
What they sought they did not find—
Those ill-fated biologists of the mind.

Loonies still insisting on double helix
 Must be cured of that *idée fixe*.
 Now we know better, we've been instructed:
 Human phenotypes are socially constructed.
 "Darwinism, yes," we exclaim with glee,
 Adding a proviso: "Only to some degree!"
 It is quite all right for a fly or bird,
 There the gene-talk is not at all absurd.
 But, wait, don't rush to generalize
 From these creatures of smaller size.
 Looking at *Drosophila melanogaster*,
 Please, don't read too much into it, buster.
 Well, they rub their genitals, no doubt,
 But that's nothing to get hot about;
 You must be completely off your tracks
 If you think that what they have is—sex.
 Your anthropomorphism and sex obsession
 Deserve of course our full compassion;
 Yet for the prejudices so amazing
 You badly need some consciousness-raising.
 Besides, to tell you frankly, there's another thing,
 It all too much smacks of—hmm, the right wing.
 Hence in Boston, Stanford or Minneapolis
 We might well need the thought police.
 Why? Because all this genetic chitter-chatter
 Is certainly not a laughing matter.
 So be responsible, mind what you say.
 Danger! You are talking DNA!

Incest as a Test Case

To what extent is evolutionary biology relevant for social science? The battle over this question is being fought on many fronts, and the discussion encompasses a number of specific topics. The issue of incest taboos stands out though. It is typically regarded as the critical case for evaluating theoretical aspirations of sociobiology and evolutionary psychology. For example, both in his book *Consilience* and in several subsequent interviews, Edward O. Wilson illustrates the success of sociobiology with the example of Westermarck's biological account of incest prohibitions.¹ Richard Dawkins also confirms the special place that this theme occupies in contemporary debates about biology and culture: "Although I usually resist the temptation to indulge in simple 'selfish gene' explanations of the social behavior of humans and other domestic animals, inbreeding avoidance is the one case for which I feel reasonably confident. 'The social science orthodoxy'

has always seemed to me *particularly* daft in this area.”² John Maynard Smith echoes Dawkins’s thought: “To me, the most interesting question is how far evolutionary biology can contribute to the human sciences. As I have explained, I am a doubter. But I have been wrong on this issue before. Ten years ago I regarded incest avoidance as an entirely cultural phenomenon; only a bigot could hold this view today.”³

Three Claims About the Aversion

In an earlier publication I suggested that Westermarck’s theory about incest is best broken into three component claims:

1. A sexual aversion tends to develop between those raised together in early childhood.
2. This aversion is an evolutionary adaptation (serving as a barrier to inbreeding depression).
3. The aversion causes (expresses itself as) the incest prohibition.⁴

Bill Durham has usefully dubbed these three claims (1) the aversion hypothesis, (2) the adaptation hypothesis, and (3) the expression hypothesis.⁵ Each of these claims says something about the sexual aversion: (1) The adaptation hypothesis says that the sexual aversion exists; (2) the adaptation hypothesis says that the cause of the sexual aversion is natural selection; and (3) the expression hypothesis says that the effect of the sexual aversion is the social prohibition of incest. I agree with Durham that the aversion hypothesis has the strongest empirical support and that the expression hypothesis is the weakest link in Westermarck’s line of argument. One reason for problems with the expression hypothesis is that a lot of preliminary analysis and clarification has to be done before one can embark on the straightforward task of empirical evaluation or hypothesis testing. The complexity of these anthropological issues naturally attracts the visitors whose professed job is precisely to untangle difficult conceptual and methodological puzzles: philosophers. But scientists do not always warmly welcome philosophers to join the discussion. And, as it happens, this inhospitableness is not wholly unjustified.

Steven Weinberg once wrote, “The insights of philosophers have occasionally benefited physicists, but generally in a negative fashion—by protecting them from the preconceptions of other philosophers.”⁶ In the debate about incest as well, it seems to me that, unfortunately, some philosophical contributions have clouded the issues instead of elucidating them. I hope, however, that the analysis undertaken in the next section will be recognized as containing more than just this purely negative result (the criticism of other philosophers’ views).

Inhibition and Prohibition: A Difference in Content?

The crucial idea in Westermarck's account of incest taboos is that it is the biological inhibition that gives rise to a cultural prohibition. It is quite clear that this hypothesis is fraught with difficulties and that many questions have to be answered before the argument that biological inhibition leads to cultural prohibition becomes persuasive. But some thinkers want to go much further than just pointing to that explanatory gap; they claim that a very general reason blocks in advance any transition from biological inhibition to cultural prohibition and that we should acknowledge that even without looking into empirical issues. Bernard Williams refers to this transcendental obstacle as the "representation problem."

Not only does extra conceptual content have to be introduced to characterize the human prohibition, but also the introduction of that content stands in conflict with the proposed biological explanation of it. . . . There are no sanctions against marrying those that one is brought up with (as such); the sanction is against marriages which would constitute close in-breeding. The conceptual content of the prohibition is thus different from the content that occurs in the description of the inhibition. It indeed relates to the suggested *function* of that inhibition, but that fact will not explain how the prohibition which is explicitly against in-breeding will have arisen. It certainly does not represent a mere "raising to consciousness" of the inhibition.⁷

Although Williams's idea reduces to a simple conceptual point, it still promises to settle an empirical issue; it is offered as a reason why we are entitled to dismiss a scenario in which cultural prohibition is seen as a mere manifestation of biological inhibition. Despite being quite influential and even making an impact on some scholars conducting empirical research on incest, this piece of a priori anthropology is fundamentally flawed. Since I have criticized it extensively elsewhere, let me here explain my reason for disagreeing with Williams more briefly and in a slightly different form.

Westermarck's basic idea is that for evolutionary reasons (i.e., the danger of inbreeding) a sexual aversion tends to develop between those who happened to be close associates in the crucial period of early childhood. Why does an aversion between childhood associates exist? Well, the hypothesis is that during the long time that natural selection has molded human psychology, close childhood associates were in fact almost always siblings. One obvious reason why it would be difficult for the aversion to be focused directly on siblings is that the concept of sibling is a fairly complicated social category—apparently an unlikely kind of object to which a genetically produced aversion might be immediately attached. Therefore, if there is a co-extensiveness (or very strong correlation) between these two properties, childhood associates (CA) and siblings (S), then natural selection might have found it easier, although no less effective as an instrument against in-

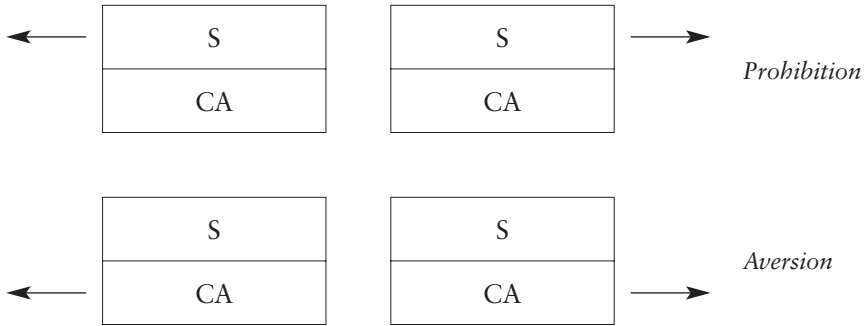


FIGURE 6.1. Aversion and Prohibition: Mismatch

breeding, to instill a sexual aversion between childhood associates (and not between siblings as such). Of course, the assumed strong correlation between CA and S would mean that as a consequence there would be a sexual aversion between siblings as well, but still, *sensu stricto*, it would only be correct to say that, in causal terms, what was producing the aversion was the characteristic CA, not S! This is readily confirmed in a pair of counterfactual or presumably rare exceptional situations in which the correlation between CA and S is violated: (1) As long as the CA-relation obtains, the aversion would still be there, even without S; (2) despite the presence of S, the aversion would not be there if the S in question failed to be CA.

On the basis of this (correct) description of the situation, Williams mounts an attack on the suggestion that the aversion could produce the incest prohibition. In Figure 6.1 the contrast between the aversion and the prohibition comes to the fore. Typically, two individuals of the opposite sex are *both* childhood associates (CA) *and* siblings (S), but the reasons they are kept apart are different in the two cases (aversion and prohibition).

Although as a rule it is the same individuals that are sexually kept away from one another by the aversion and by the prohibition, Williams wants us to notice that the forces of separation in these two cases are essentially different. The aversion is produced by a force opposing a CA-relationship, whereas what the prohibition condemns is just an S-relationship. Ergo, the argument goes, the prohibition cannot be a mere expression of the aversion because the two are directed to two entirely different aspects. The fact that CA-relationships are usually, or perhaps always, also S-relationships (and vice versa) is neither here nor there. Surely even after we concede that the two characteristics are correlated, it remains quite unclear by what kind of transformation the content of the prohibition that speaks only about S could be suddenly obtained from the mere aversion toward CA.

Williams's argument looks persuasive because it trades on a crucial am-

biguity. The statement that the aversion is directed toward CA (childhood associates) can actually mean two things. First, in the *causal* sense, it can mean that the characteristic that is actually producing the aversion is CA. Second, in the *subjective* sense, it can mean that people who have the aversion experience it as being directed toward CA. I would like to point out that there is no necessary connection between the two senses; they are distinct and separable. In particular, if the aversion is directed toward CA in the causal sense, it by no means follows that the subjects would also experience it as being directed toward CA. They may well believe (wrongly) that the aversion is, say, generated by S.

The causal sense is relevant for the natural selection scenario. And yes, Westermarck's theory does contain a claim (the adaptation hypothesis) that the sexual aversion is objectively produced by being triggered by CA-aspect. The expression hypothesis, on the other hand, says that the prohibition is a manifestation of the inhibition. But here, in the context of the expression hypothesis, the content of the inhibition (that gives rise to the prohibition) is determined by the *subjective* sense of "being directed to," and *not* the *causal* sense! The fact that in the causal sense the aversion is indeed directed to CA (in contrast to the prohibition, which is directed to S) does not imply that there is a "mismatch" between the two contents, simply because the causal sense of the aversion's "being directed to" does not speak about content at all. It only speaks about which property is *objectively* causing the aversion. The content emerges only at a later stage when we ask how the people having the aversion experience it *subjectively*, that is, in terms of which property they conceptualize their own aversion. So it seems that Figure 6.1 was an oversimplification. It should be replaced by Figure 6.2.

Now we see that the fact that the aversion objectively picks out childhood associates as its object does not necessarily signify that its *content* is discrepant from the content of the prohibition (which forbids sex with siblings). For the content of the aversion is not fixed by such objective matters as the question about what is causing what. On the contrary, it essentially depends on how the aversion looks "from the inside" to those people who have it. And for all that matters, there is no guarantee here at all that there will be an accord between the objective and the subjective. In fact, given that natural selection found it easier to achieve its end (decreasing the probability of sex between siblings) by directing the aversion toward the more accessible and conveniently correlated proxy (childhood associates), it is perfectly possible that people, too, picking out environmental cues to make sense to themselves about their own aversion, also choose the line of least resistance and simply reach for the more meaningful, social category like *brother* or *sister*, rather than a seemingly irrelevant and queer charac-

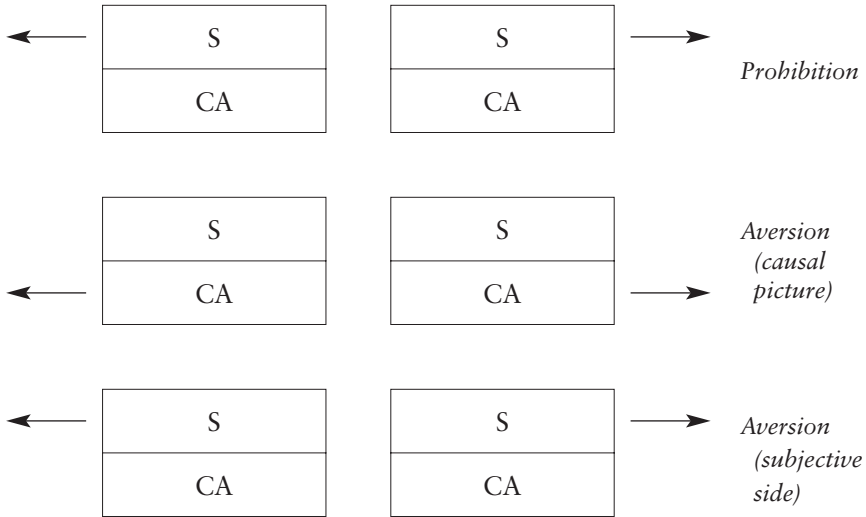


FIGURE 6.2. Aversion and Prohibition: No Mismatch

teristic like *someone with whom I spent the first years of my childhood*. Notice that I say that this is possible, not true. My concern here is not the truth, but the criticism of Williams’s “impossibility proof.” I tried to show that the transcendental obstacle he placed in the path of the Westermarckian account can be removed.

Williams hoped to demonstrate on very general grounds that the biological aversion couldn’t produce the cultural prohibition. But his argument is based on the unjustified assumption that the content of the inhibition has to be presented to the subjects in terms of that property that is in fact causally operative in producing that inhibition. Once we realize that this is not necessary at all, the argument breaks down. The inhibition may well be subjectively presented differently (in a way that does not reflect its actual causal origin) and then, distorted in the “right” way, it may well match in content the corresponding cultural prohibition.

Westermarck’s Argument as a Zero-Sum Game

There is another, again very general argument that is proposed as undermining the theory that the aversion gives rise to the prohibition (the expression hypothesis).

To make matters worse, there is little or no direct support for Westermarck’s “moral disapproval” step. First, of the three studies already cited as supporting

Westermarck's argument about intimacy and aversion, not one shows evidence for a moral disapproval expressive of that emotion. . . . Any one of these cases might be taken as exceptional, and therefore dismissed from concern. But it is hard to accept that argument for all three, especially since the most familiar of all potential partners are not prohibited. In effect, the available evidence says, "Aversion, yes—moral disapproval, no."⁸

This is not a conceptual or transcendental argument. Durham's objection is based on probability. As he correctly notes, the existence of the Westermarckian sexual aversion between close childhood associates gets empirical support basically from three studies conducted in three different environments (Israel, Taiwan, and Lebanon). In all these cases there were clear signs of the lack (or decrease) of sexual attraction between childhood associates, although sexual contacts between the individuals in question were not prohibited but even encouraged. Durham then stresses one thing. These three studies are the best available evidence for the existence of the aversion. But in *none* of the three cases did the aversion lead to the prohibition. Why is that? He suggests that if the prohibition failed to emerge in only one of those situations, then it could still be reasonable to cling to the expression hypothesis and say that something exceptional occurred in that single instance that disrupted the usual tendency of the aversion to produce the prohibition. But if the same thing happened in *all* the three cases (as it did), he argues that this should make us strongly doubt, on purely inductive grounds, that there is any causal connection between the aversion and the prohibition.

I disagree. In my opinion, even the three repeated instances of "aversion, yes—moral disapproval, no!" do not justify skepticism with respect to the expression hypothesis. Contrary to Durham, I do not think that the three-fold presence of aversion-sans-disapproval points toward the conclusion that the former does not produce the latter. True, the fact that in those three quite vital cases for the evaluation of Westermarck's theory the aversion is *not* accompanied by moral disapproval cannot be dismissed as a mere coincidence (or "a few exceptions that prove the rule"). The absence of moral disapproval needs to be explained. One reason why the aversion is there, but moral disapproval is not, may indeed be that there is no intrinsic connection between the two and that in those other cases when they happen to be found together they are actually the result of different and largely independent causal processes. This is Durham's tack: if A is not followed by B in the three key cases, this undercuts (at least to some extent) the hypothesis that A produces B. But there is another way to understand why the aversion appears without moral disapproval in those three test cases. Maybe the absence of moral disapproval does not indicate that there is no general causal connection (aversion → taboo); perhaps it's just a

sign that we are here dealing with a particular kind of situation in which, by a very special logic of hypothesis testing, the aversion could not have produced the taboo. Let me explain.

Westermarck's aversion hypothesis asserts that there is an inborn sexual aversion that spontaneously develops between siblings, qua close childhood associates. The hypothesis faces an obvious difficulty. Namely, since as a matter of fact the aversion is usually correlated with the corresponding cultural taboo against sexual contacts between siblings, how do we know that the aversion is not simply a consequence of the taboo (rather than being an inborn, biologically mediated inhibition)? Well, fortunately for the aversion hypothesis, there are three extensively researched cases in which there is no taboo but the aversion still seems to be there.⁹ These cases constitute critical empirical support for the aversion hypothesis. But, somewhat mischievously, these same cases undermine the expression hypothesis. To spell it out, on one hand, if the correlation between the aversion and taboo is occasionally broken, this is a welcome result for the aversion hypothesis because the aversion occurring without the taboo shows that the aversion stands by itself and is not just a side effect of the taboo. But on the other hand, the aversion occurring without the taboo is bad news for the expression hypothesis; it is weakening the hypothesis because what the hypothesis basically says is that, other things being equal, the aversion leads to the taboo. So, Westermarck's global theory about incest has two components, the aversion hypothesis and the expression hypothesis, that pull empirical evidence in the opposite directions. For this reason, his global theory is in a strange epistemological predicament in that, under the circumstances, it just cannot receive full empirical confirmation. It is a zero-sum game; what the theory gains by collecting evidence in favor of the aversion hypothesis it automatically loses on the other front because the very same empirical data chip away at the expression hypothesis.

Because of the zero-sum nature of Westermarck's argument, we should not be much impressed by the fact that in every single case that supports the existence of the biological aversion the taboo is missing. This is dictated by the logic of the situation.

Referring to Figure 6.3 and the two circles, the area of intersection represents the presence of both the aversion and the taboo. These cases are consistent with the expression hypothesis, but they cannot provide evidence for the aversion hypothesis. If all the data were in that area, the aversion hypothesis would be considerably weakened because then it would make sense to hypothesize that the aversion is produced by the taboo and is not an independent psychological phenomenon. However, the area where aversion occurs without the taboo sends an opposite epistemological message. The data points located in that section support the theory that the

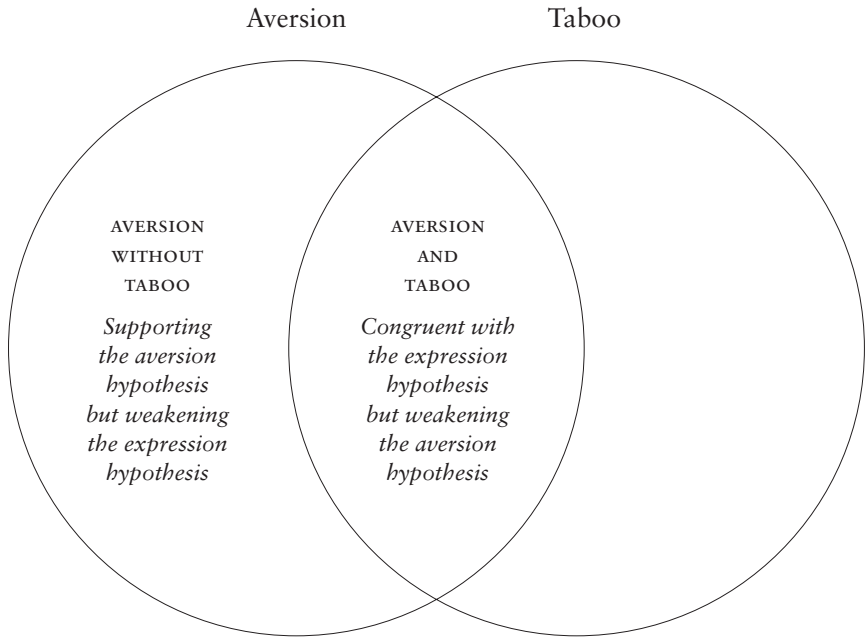


FIGURE 6.3. The Expression Hypothesis and the Aversion Hypothesis: An Epistemological Tension

aversion is an autonomous event and not a mere offshoot of the taboo. But at the same time this break of connection between the aversion and the taboo creates a problem for the expression hypothesis, which states that the aversion regularly brings about the taboo.

So, there is nothing puzzling about the fact that all three cases supporting the aversion hypothesis speak against the expression hypothesis. Instead of being regarded as a consilience of independent cases strongly pointing toward the probable falsity of the expression hypothesis, this fact is better seen as just reflecting the logical peculiarity of Westermarck's theory. Although the two components of his theory, the aversion hypothesis and the expression hypothesis, are perfectly compatible and mutually consistent, there is an epistemological tension between them in that, at least at the present stage of theory testing, empirical evidence cannot support the aversion hypothesis without raising some doubts about the expression hypothesis.

For the purpose of illustration, let me give another example that comes from an entirely different context but which, analogously, exhibits the same kind of epistemological tension. Take the following hypothesis: "Many peo-

ple who approach the state of clinical death have the same strange experience: it appears to them that they perceive the world from a location outside their own bodies.” Let’s call this hypothesis NDE (near-death experience). Take now another hypothesis: “Usually, a near-death experience is immediately followed by death.” Let’s call this latter hypothesis DSA (death-soon-afterward).

It is quite clear that, from a logical point of view, NDE and DSA are fully consistent with one another. But here again, as in the case of Westermarck’s two hypotheses, there is an epistemological strain. For, if the evidence in favor of DSA becomes too strong, then as a direct consequence of that, NDE will lose much of its empirical support. Namely, if the regularity postulated by DSA were perfect and exceptionless, and if, accordingly, all cases of NDE were immediately followed by death, there would be simply no one left to tell us about the existence of NDE! On the other hand, and this is the key point, if there happened to be some cases where, contrary to the general tendency expressed in DSA, the subjects of NDE survived and were able to report later about their experiences, it would be *wrong* to use this to attack DSA, along the lines reminiscent of Durham’s argument in connection with Westermarck. That is, it would be wrong to say, “Of all the cases supporting NDE, not one shows evidence for DSA. Any one of these cases might be taken as exceptional, and therefore dismissed from concern. But it is hard to accept that argument for all of them.”

True, all the cases supporting NDE undermine DSA, or at least weaken it to some extent. But this, in itself, should not be interpreted as a confluence of several independent instances, pointing to the probable falsity of DSA. Rather, the fact that all the cases corroborating NDE do clash with DSA is better regarded as just trivially following from the zero-sum logic of confirmation involving these two hypotheses. To put it differently, if from the outset it were quite open whether the data supporting NDE will happen to be in accord with DSA or not, and if it then turned out that all of them were aligning themselves in the opposition to DSA, this could indeed be reasonably taken as accumulation of important negative evidence against DSA. But this is not how things are. Actually, we know well in advance that any confirmation of NDE (i.e., an *ex post facto* first-person report about NDE) will inevitably be a counterexample to the regularity asserted in DSA (“NDE is immediately followed by death”). For this reason it is misconceived to ask, “Why is it that not just one or two cases confirming NDE undermine DSA, but all of them do?” The answer is, It could be no other way. The data speak here with one voice because of the epistemological peculiarity of the situation. This is not a probabilistic indication of anything.

Conclusion

I tried to show that Westermarck's theory about incest is not threatened by the two very general methodological objections directed against it. But I hope that amid the critical tones, which dominate this chapter, a more positive message will be recognized too. Namely, the epistemological scrutiny of Westermarck's views that is here undertaken reveals a theoretical edifice of great complexity and conceptual sophistication. Although today the main efforts seem to be focused on the attempts to evaluate empirical evidence that will ultimately decide the conflict between the biological account of incest taboos and its rivals, at those moments when we are forced to stand back and inspect the logical structure of Westermarck's theory, we realize that some of its implications, interconnections, and "forking paths" have yet to be fully explored.

NOTES

1. Edward O. Wilson, *Consilience* (New York: Knopf, 1998); Edward O. Wilson, "Resuming the Enlightenment Quest," *Wilson Quarterly* (1998).
2. Richard Dawkins, "Opportunity Costs of Inbreeding," *Behavioral and Brain Sciences*, vol. 6 (1983): pp. 105–6.
3. John Maynard Smith, "Constraints on Human Behavior," *Nature*, vol. 276 (1978): p. 121.
4. Neven Sesardic, "From Biological Inhibitions to Cultural Prohibitions: How *Not* to Refute Edward Westermarck," *Biology and Philosophy*, vol. 13 (1998): p. 224.
5. See William H. Durham, Chapter 7 in this volume.
6. Steven Weinberg, "Against Philosophy," in *Dreams of a Final Theory* (London: Hutchinson, 1993), p. 132.
7. Bernard Williams, "Evolution, Ethics, and the Representation Problem," in *Making Sense of Humanity and Other Philosophical Papers, 1982–1993* (Cambridge: Cambridge University Press, 1995), pp. 105–6.
8. William H. Durham, *Coevolution* (Stanford, Calif.: Stanford University Press, 1990), p. 323.
9. However, for serious methodological doubts about one of these cases (the alleged evidence from Israeli kibbutzim), see Jonathan Hartung, "Review of *Incest: A Biosocial View*," *American Journal of Physical Anthropology*, vol. 67, no. 2 (1985): pp. 169–71.