

From Biological Inhibitions to Cultural Prohibitions, or How *Not* to Refute Edward Westermarck

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Abstract. My aim in this paper is to take a closer look at an influential argument that purports to prove that the existence of cultural prohibitions could never be explained by biological inhibitions. The argument is two-pronged. The first prong reduces to the claim: inhibitions cannot cause prohibitions simply because inhibitions undermine the *raison d'être* of prohibitions. The second strategy consists in arguing that inhibitions cannot cause prohibitions because the two differ importantly in their contents. I try to show that both claims fail.

Key words: incest taboos, Westermarck, natural selection, sociobiology

Try to suggest that the widespread presence of a given cultural norm could be explained by a biological instinct of *Homo sapiens*, and be sure that the eyebrows will be raised even before you had the chance to give any details of your pet sociobiological theory. What is the origin of this aprioristic scepticism toward any kind of biological (or semi-biological) account of cultural phenomena? Well, to begin with, there are good inductive reasons for the mistrust: notoriously, too many of such speculations in the past proved to be not just wrong, but also extremely naive, rash and crude. However, beside this historically-inspired pessimistic induction there are as well influential philosophical arguments purporting to prove, on quite general grounds, that cultural *explananda* could never be, even in principle, elucidated by biological theories. On the basis of such “impossibility-proofs” it is often claimed that those who still hope to throw biological light on some aspects of human culture must be conceptually muddled and exceptionally unsophisticated in their methodological views.

My aim in this paper is to take a closer look at one of these arguments, and to show that it not only misses the mark completely but that, ironically, the defenders of the argument are actually the ones who are the victims of deep confusions and surprisingly crude mistakes. The argument I want to discuss was given the name “representation problem” by Bernard Williams, who also

introduced it into the philosophical literature (Williams 1983; reprinted without alterations in Williams 1995). Williams argues that any attempt to explain the emergence of cultural norms or customs as an effect of a pre-existent biologically mediated behavioral disposition faces a formidable conceptual difficulty. Once appreciated, he thinks, this difficulty should make us extremely suspicious in advance about a suggestion that in any particular instance the origin of cultural phenomena could be accounted for by a genetically based tendency of human organisms to act in a specific way under given circumstances.

I shall try to show that Williams's "difficulty" is a pseudo-problem.

1. Three desiderata

Let me start by proclaiming three desiderata to be satisfied by any worthwhile attempt to dissolve a philosophical problem; next I shall attempt to show that my enterprise fares well in all these three respects. First, if you want to demonstrate that an alleged problem is actually not a genuine problem (or not a major problem), you should be able to show that the context of the discussion in which it appears is of some general importance. Otherwise you will rightfully be accused of being concerned with a mere quibble. Second, there should be a resistance to your devaluation efforts: the problem to be dissolved should still be taken seriously, at least by some. Otherwise your whole undertaking will be regarded as a grotesque attack on an abandoned fortress. And third, in philosophy of science in particular, a problem gains in importance if it transcends the narrow frontiers of pure philosophy, i.e., if the issue is discussed in earnest within the science proper as well. Problems in philosophy of science that exclusively worry philosophers tend to have a certain air of unreality and pointlessness.

Let me now quickly run through these requirements again, and see how the "representation problem" passes this threefold test.

(1) It is perhaps easiest to show that the first desideratum (generality) is satisfied. Although Williams's discussion manifestly revolves around one highly specific issue (the origin of incest taboos), it is pretty obvious that the upshot of his argument is much broader: he wants to show that behavioral dispositions that are targeted by natural selection cannot bear the brunt of causally explaining the emergence of cultural prohibitions or norms. This is the claim that, judging by its scope, certainly deserves our attention.

(2) Concerning the second desideratum (the need to avoid attacking an abandoned fortress) providence has kindly arranged circumstances for a smooth verification. In the spring of 1995 I was teaching at the University of Notre Dame, and just as I happened to have first thoughts about writing this

paper, Bernard Williams arrived there to attend a two-day conference devoted to various aspects of his philosophy. Naturally, I grabbed the first opportunity to talk to him about the representation problem, and I soon realized that, no, I was definitely not attacking a straw man, but a very flesh-and-blood, vigorous philosopher who firmly adhered to his previously published views on the topic. So, it is not only that there is a worthy opponent, after all, inside the fortress, but to anticipate a little, you will in a moment also see Philip Kitcher standing on one of its walls, defending the place with some really strange-looking weapons.

(3) Coming at last to the third desideratum – that the problem to be dissolved should hold some fascination and excitement even for those who do not belong to the tribe of philosophers – I think I have a special responsibility to be persuasive here. I worked on this paper while I was a National Science Foundation Research Fellow, and I am afraid that there wouldn't be much enthusiasm in the NSF for supporting the work on some highly abstract, idiosyncratic and arcane philosophical enigma, perfectly insulated from all genuinely scientific concerns. I imagine that people from my sponsor organization would give vent to their dissatisfaction with the idea of financing a purely philosophical enterprise by paraphrasing the famous words of C. D. Broad (quoted in Gellner 1963, p. 246), and by exclaiming: "How long will our impoverished foundation continue to pay the salaries of individuals whose only function, on their own showing, is to treat a disease which they catch from each other and impart to their pupils!" Fortunately for me, there is some good news. Speaking about the representation problem, the contagion did spread outside the narrow philosophical circles. Moreover, scientists infected with the disease are not some minor or peripheral figures. Among those struck by the philosophical virus are such leading contemporary authorities in the world of science as Cambridge ethologist Patrick Bateson and Stanford anthropologist Arthur Wolf, who both quote Bernard Williams's piece on the representation problem with approval (Bateson 1989, p. 290; Wolf 1993, p. 169; Wolf 1995, pp. 505–506). I conclude therefore that I am in the clear on all three counts, and that I get the green light to proceed with discussing the main issue.

In my reconstruction, Williams's argument is a two-pronged attack on the general idea that the existence of an inhibition (biological fact) could account for the emergence of a prohibition (cultural fact). To put it very concisely, the first prong reduces to the claim: inhibitions cannot cause prohibitions simply because inhibitions undermine the *raison d'être* of prohibitions. The second, much more interesting line of argument could be summarized as follows: inhibitions cannot by themselves cause prohibitions because the two differ importantly in their contents.

2. Inhibitions undermine the *raison d'être* of prohibitions

Here is the assertion that inhibitions undermine the *raison d'être* of prohibitions in Williams's own words:

The most, it seems, that a genetically acquired character could yield would be an inhibition against behaviors of a certain kind: what relation could that have to a socially sanctioned prohibition? Indeed, if the inhibition exists, what *need* could there be for such a prohibition? (Williams 1983, p. 560)

Williams is by no means a sole advocate of this kind of argument. Numerous other authors have also engaged in a similar criticism of "sociobiology" or "evolutionary psychology".² A most recent example is Philip Kitcher who, in a paper on the future of human behavioral ecology, discusses a hypothesis that incest taboos are the expression of a prior hostility toward copulations with kin. Kitcher then confronts this hypothesis with a "major worry" which in his opinion proves to be nothing less than fatal to it. The major worry is essentially the same question as the one raised by Williams: "Why should there be a taboo on behavior that people are not disposed to perform?" (Kitcher 1990, p. 104).³

To start with, let us consider a situation most favorable to the Williams-Kitcher claim: a case of inhibitions so powerful and universal that they thoroughly extinguished a given kind of behavior – to the point that it actually ceased to be an option in the human behavioral repertoire. It seems that under such circumstances it would be extremely difficult to conceive how prohibitions of that literally never occurring behavior could have developed. Well, perhaps difficult, but not impossible. To get prohibitions going it is actually not necessary that the prohibited behavior be at least sometimes encountered nor indeed, for that matter, that it be possible at all. It is entirely sufficient that the forbidden action be *thought* possible. A confirming illustration that readily springs to mind is witchcraft which was prohibited and severely punished although it did not exist, and was in fact always impossible. (By the way, in view of ever stronger attacks of deconstructionists and relativists on the "imperialism" of Western rationalism and the scientific world-view I should perhaps here apologize for such an impetuous and unabashed admission of a cultural prejudice against the authenticity of witchcraft as a magical practice.)

The result of my discussion so far is that the Williams-Kitcher claim does not hold water even under its best-case scenario, i.e., on the assumption of very rigid and completely unyielding psychological inhibitions. Making this assumption is in fact the backbone of their argument. It is only if inhibitions *altogether* disable a behavior that the question arises: what is then the point of having prohibitions if inhibitions have already done *all* the work? But

why not move to counterattack and call in question such a crude picture of human behavioral dispositions that is here being implicitly forced upon us? The idea that inhibitions absolutely stamp out a behavioral variant which is their object flies in the face of almost everything we know about our own psychology and about evolutionary biology in general. Even the most deeply rooted and biologically vital inhibitions are occasionally overridden by sufficiently strong counteracting causes, under atypical conditions or in atypical individuals.

For basically two reasons natural selection is exactly expected to bring about such loose, tendency-like, exception-allowing dispositions, rather than inflexible, foolproof and never failing inhibitions, which are encountered only in philosophical fantasy. First, since natural selection is an ill-equipped engineer working with limited resources and opportunities it is on the whole very unlikely that it would ever manage to supply a species with a guaranteed, exceptionless inhibition, *even when such a trait would truly be an optimal adaptation*. There are constraints on perfection which make some ideal solutions simply inaccessible. Strangely enough, this by now well-absorbed piece of evolutionary wisdom appears, in this context at least, to have been lost on the author who used it with so much gusto and vehemence against others in the *Vaulting Ambition* (Kitcher 1985) in a chapter entitled "Doctor Pangloss's Last Hurrah".

Second, it is not only that strictly exceptionless inhibitions might prove to be an optimum difficult to implement and sustain in the process of natural selection; this kind of trait would most probably be selectively *disadvantageous*. In the case of incest, for instance, the main advantage of having an in-built sexual aversion toward those who happen to be one's close relatives consists in protecting the organism from inbreeding depression, i.e. the increased probability of offspring homozygotes for very harmful or lethal recessive genes. But the aversion brings with it some disadvantages as well. The animal has to go around searching for an acceptable mate, which by itself raises the danger of an attack by predators; also, under some circumstances it will be all but impossible to find a mate outside the close kin circle, and in terms of its inclusive fitness the organism would then have been better off without the aversion (because an increased chance of non-viable or defective offspring is obviously still better than a virtual certainty of no offspring). Clearly, there is a subtle trade-off between the advantages and the costs of inbreeding-avoidance propensity. In reviewing the literature, one author found that at least four costs of inbreeding, and seven costs of outbreeding had been or could be proposed (Bateson 1982). Therefore, in the case of sexual aversion between those raised together in early childhood (which Edward Westermarck first proposed as the key causal factor leading to the emergence

of the incest taboos) there is no need to picture it, unrealistically, as a mechanism that will inexorably lead an organism along the preordained rails, as it were, and without any twist or turn. Rather, as with other genetic traits it is better regarded as a “push” in a given direction, which can occasionally be counterbalanced by other causal influences, other things *not* being equal.

Once accepted that there is always bound to be some room, however small, for occasional exceptions with respect to even the most deeply entrenched behavioral inhibitions, there is nothing puzzling anymore in the *parallel* existence of similarly directed prohibitions. The meaningfulness of prohibitions is surely not dependent on their being violated *very often*. Even those acts that occur extremely seldom can produce enormous outrage. Indeed, more than that, there is a record of societies where a kind of behavior is most severely punished *and* at the same time declared to be impossible. An example of such a quasi-incoherent cultural norm which is barely intelligible (if at all) comes from the region which today also, in a different sense, defies everyone’s understanding: Bosnia. In the folk songs of Moslem Slavs in that part of former Yugoslavia the seduction of a full sister is regarded “as a crime punishable with death, *or rather as something which cannot occur*” (Westermarck 1921, vol. II, p. 97 - italics supplied).

3. Methodological double standards

Before turning to consider the second avenue of attack on Westermarck’s hypothesis let me make a digression and try to show that in philosophy, too, practising what you preach may bring you troubles when your preaching is problematic. More concretely, Philip Kitcher’s surprisingly cavalier treatment (and dismissal) of Westermarck is in my opinion a sign of his overall theoretical consistency: in evaluating particular scientific theories he really abides by his proclaimed methodological principles, however regrettable in the end this may be. What I have in mind is a quite open and frank announcement of double standards in methodology which is assigned a manifesto-like central place in the first chapter of *Vaulting Ambition*:

Everybody ought to agree that, *given sufficient evidence* for some hypothesis about humans, we should accept that hypothesis whatever its political implications. But the question of what counts as sufficient evidence is not independent of the political consequences. If the costs of being wrong are sufficiently high, then it is reasonable and responsible to ask for more evidence than is demanded in situations where mistakes are relatively innocuous. (Kitcher 1985, p. 9)

Note that Kitcher talks about *acceptance* of theories, not just about using or applying theories, or about acting on the assumption that a given theory

is true (or false), where the expected utility approach would indeed be perfectly appropriate. His politically calibrated methodology is animated by an improper and misdirected feeling of social responsibility. First, if the politically inspired tightening of standards of criticism is going thus to increase in proportion to the degree of political hazard ascribed to scientific theories, then with respect to some hypotheses containing what some see as “very dangerous knowledge” methodological requirements for acceptance will at some point become so tough that, given the notorious fallibility of human judgment and the essentially conjectural nature of all science, these theories will, as a matter of fact, be put effectively and forever beyond our ken.

Second, if the acceptance of hypotheses is governed by the attempt to let our beliefs be formed by truth-relevant considerations then with Kitcher’s recommendation that politics should decide what is “sufficient evidence”, science as an eminently epistemic enterprise will be fundamentally compromised: the fine Humean advice that “a wise man proportions his belief to the evidence” is here being replaced with an advice that “a wise man proportions his belief (at least in part) to the envisaged political consequences of his belief”. This is essentially the same way of forming the belief as the famous reasoning known as “Pascal’s Wager”, where one is also invited to believe something (namely, that God exists) because of expected *effects* of one’s belief, and *not* because of *reasons* in favor of that belief. It has been more than once very convincingly demonstrated that the Pascalian mode of fixation of belief involves a very deep epistemic irrationality (Cargile 1966; Williams 1973, pp. 136–151; Hacking 1975, pp. 63–72; Mackie 1982, pp. 200–203; Elster 1984, pp. 47–54), so I shall not lose time to show that, *mutatis mutandis*, the criticism of Pascal’s Wager applies with full force to Kitcher’s methodological injunction. Perhaps just to drive the general point home, consider this: if I acquire a belief that *p* on the basis of what I regard as adequate reasons to believe *p*, and if I realize later that my believing *p* can have some unwanted social consequences, is it not glaringly obvious that, as long as I remain epistemically rational, I simply *cannot* stop believing *p* just *because* such a state of my mind would be more beneficial, politically?

What makes Kitcher’s advice specially extravagant is his insistence that the rationality of adopting a scientific hypothesis should depend not only on the political costs of *accepting* it, but also on the possible political costs of *failing to accept it* (Kitcher 1985, p. 9). This means that in cases where the political costs of *not accepting* a certain empirically dubious hypothesis are considered to be prohibitively high, we are actually urged to make a politically motivated effort to *accept* that theory, against our better (epistemic) judgment. One cannot help noticing here a structural similarity with Plato’s defence of the “noble lie”. Indeed, how could we ever commend the behavior of scientists

who first rejected hypothesis H because they thought it inadequately supported by evidence, but who, later, after learning that the rejection of H (or indecision about H) could have harmful social effects, promptly lowered their critical standards and obligingly accepted H?

The fact that Kitcher's standards for acceptance (and rejection) are so pervasively corrupted by political considerations helps us explain what would otherwise be an extremely odd *faux pas* and misjudgement in such an astute philosopher. In accordance with his announced methodological approach Kitcher uses rigorously high and extraordinarily demanding criteria in the evaluation of sociobiological theories (because of their probably explosive political content), but when taking up objections to these theories he tends to accept them uncritically and only after superficial consideration – in a way that, if practiced by so-called “hereditarians” or “genetic determinists”, would be the object of much of his ridicule. A good example is precisely his hasty and poorly argued rejection of Westermarck's hypothesis. The irony is that, apparently, Westermarck's hypothesis itself does not have anything that would make it politically sensitive or dangerous. Its only sin, as far as I can see, consists in the fact that it is standardly cited as the most successful attempt to date to show the relevance of biology for social science. The mere existence of this one particular sociobiological enterprise with flying colors is annoying to some socially concerned and high-strung philosophers because they think that, if not resisted, it could encourage similar theoretical advances in those politically more sensitive areas where they would find them most unwelcome.

4. The mismatch of contents

Picking up again the threads of the main argument left over by interjecting the digression on methodological double standards, let me move now to Williams's second, very general objection to the sociobiological account of incest taboos. Williams starts by arguing that the biological inhibition postulated by Westermarck crucially differs in content from the cultural prohibition. By making this purely conceptual point (and admitting that he is altogether side-stepping the “factual merits of these explanations”), he thinks that he can nevertheless conclude therefrom: first, that the biological element (the inhibition) cannot by itself explain the prohibition, and second, even more strongly, that the inhibition does no explanatory work at all (Williams 1983, p. 561). Before attacking the *nervus probandi*, the incongruity of content thesis, I would like to point out that here again, curiously, there is no indication that Williams is aware of the prehistory of the discussion. Namely, exactly as in the case of his first line of attack on Westermarck a look into

the literature discloses that, once more, Williams's criticism is actually a repetition of an objection put to Westermarck by James Frazer (Frazer 1910, pp. 96–97) and later reiterated with a proper acknowledgement by Sigmund Freud (Freud 1952 [first published in 1913], pp. 122–123). Also, Westermarck (Westermarck 1921, vol. II, pp. 204–207) has again replied in detail, showing (and moreover explicitly saying) that he considered the objection very important.

Let us, however, return from history to philosophy, and take a closer look at the objection itself. Williams is right that the Westermarckian inhibition (if it exists) works by taking as the object of sexual aversion those with whom one has been raised in early childhood. He is also right that the prohibition against incest says *nothing* about this group! “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” (*loc. cit.*). So far there is no ground for complaint. Indeed, Westermarck's scenario precisely suggests that evolutionary forces take advantage of the approximate coextensiveness of two properties (i.e., “being siblings” and “being raised together”), and that when the first and truly aimed property proves too difficult, or impossible, to target the same purpose is actually achieved by simply directing the inhibition toward the second, merely accompanying property which happens to be more easily accessible to natural selection, that ingenious but ill-equipped tinkerer.

Where I part ways with Williams is when he goes on to claim that “[t]he conceptual content of the prohibition is thus different from the content that occurs in the description of the inhibition” (*loc. cit.*). In my opinion, Williams here conflates two different questions: (a) which property is *causally* responsible for the inhibition, and (b) which property figures in the *content* of the inhibition. Once the two questions are distinguished it will be recognized that a property which, objectively speaking, typically produces an aversion may not actually appear in the subjectively given representation of that aversion at all. In other words, the content of a mental attitude, seen from the inside, is not necessarily a good indicator of what, seen from the outside, typically causes mental states with that content (and vice versa). For instance, I may dislike a certain news magazine, thinking that this is because there is something irritating about its visual appearance or journalistic style, whereas in reality the true explanation of my antipathy may well be that the magazine is for me simply associated with unpleasant feelings because, say, I used to read it only in the dentist's waiting room.

In the case of Westermarckian sexual aversion, too, the mere “external” fact that the aversion happens to be triggered by co-habitation in a crucial period of early childhood by no means settles the issue about what will be

the *internal* representation of that aversion. Therefore, what Williams has established is solely that there is a mismatch between the property figuring in the incest prohibition (“Don’t have sex with *close relatives*”) and the property which is *causally* operative in producing the inhibition (“*being raised together*”). The question of what is the *subjective* content of the inhibition is not so much as touched upon, although this is precisely what has to be worked out for his argument to go through.

Put somewhat differently, Williams is arguing that if incest prohibitions are a mere expression (or fairly direct by-product) of antecedent inhibitions, the contents of the two must be similar. For if the contents did not match, how could social prohibitions of behavior Q ever grow out of individually felt inhibitions which were directed to something quite different (i.e., behavior P). Note that it would be quite wrong here to invoke the coextensiveness of properties Q and P in an attempt to explain prohibitions by inhibitions. It would verge on absurdity to suggest that a prohibition of Q emerged because there was an aversion to P, and because, coincidentally, Q was coextensive with P. Namely, this would leave the crucial question unanswered: to wit, why was the inhibition not expressed in the cultural domain simply as itself, that is, as a prohibition of P.

From this alleged essential disparity of contents Williams concludes that with Westermarckian inhibitions as a starting point there is a lack of necessary conceptual resources to reach incest prohibitions, and that, therefore, there is a kind of *a priori* reason why one can never get from here to there. The final claim derived from all this is: since incest prohibitions are articulated in terms which are different from, and indeed inexpressible in, the content of inhibitions, and since the genesis of these prohibitions can only be explained by mental attitudes already containing the requisite culturally-loaded categories, it follows that the explanation of incest taboos will have to be cultural *and* that biological inhibitions will fall out from the story as an explanatory idle wheel.

As I intimated earlier, there are serious problems with the mismatch of content thesis. True, the Westermarckian scenario does assert that what is causally operative in producing the sexual aversion is the property “being raised together in early childhood”, and *not* the property “being close relatives”. But upon granting this, it still remains completely open how this aversion will be presented to the minds of its possessors. Obviously, there is no guarantee that it will be *correctly* presented. As in the example with my dislike for a certain news magazine, it is here also quite possible that the real source of aversion will be obscured and that the subjects will be confused about the true etiology of their own mental attitudes. Schematically: (i) if there is something about things with property P that actually produces aversion A,

(ii) if property P is regularly conjoined with property Q, and (iii) if subjects have no clear idea how aversion A is caused in them, there should be nothing surprising if they happened to think, wrongly, that there is something about things with property Q that produces their aversion.

In the case of incest such a misconception is not only possible but in a certain way to be expected. The key to recognize this is a beautiful objection that Havelock Ellis raised against Westermarck, the objection that, in a curious twist, can be turned on its head and used to defend Westermarck. Ellis accused Westermarck of having assumed the existence of an innate tendency which is “as awkward and artificial an instinct as would be, let us say, an instinct to avoid eating the apples that grew in one’s own orchard” (quoted in Westermarck 1921, vol. III, p. 197).

Indeed, the analogy is very pertinent. There is really something rather awkward and artificial in the Westermarckian aversion. For heaven’s sake, what could there be common and important in those persons with whom one merely happened to have spent one’s early childhood that could produce a sexual aversion exactly and only against this so heterogeneous and ill-defined group of people? What connects these persons is such an external and ridiculously irrelevant property, just as is the case with the apples that were united only by “having grown in one’s own orchard”.

Agreed. But why not turn the tables against Ellis and claim that the undeniable strangeness of Westermarckian property should not make us suspicious about its causal efficacy, but should, rather, make us expect that, because of its very queerness, its influence will tend *not* to be perceived for what it is? The more artificial and awkward a property, the more likely its causal power will be misconstrued and attributed, wrongly, to a “more natural”, regularly accompanying property, if there is one to be found. And in our case such a companion property is indeed conveniently available, for “being raised together” does as a rule closely coincide with the sibling relationship. Therefore, for all we know, it *could* be that the Westermarckian aversion is indeed activated toward (and only toward) those with whom one has been intimately associated in the critical period of early childhood (kin or no-kin), but it *could* still turn out that, because the aversion typically takes as its object close relatives, it comes to be conceptualized as being *essentially* directed at close relatives. To give a concrete illustration, if through the operation of Westermarckian mechanism a woman were to find the thought of having sex with her own brother deeply repugnant, she could think that she feels so because this is her *brother*, and not merely because of the fact that this particular male just happened to be much around when she was a kid. She would be mistaken about this, of course, but the whole situation neatly explains how a culturally loaded and content-rich prohibition could after all be an immediate

reflection of a biological inhibition, if only the subjects are confused enough about the nature of their own aversion.⁴

In this just-so-story, there is a truly dialectical process. In the first stage, natural selection tries to establish a barrier to sexual relations between close kin, but she finds it easier to achieve her end in a roundabout way, by instilling a sexual aversion between those who as a matter of fact grew together (and who almost always *are* close kin). In the second stage, the subjects follow the line of the least conceptual resistance, and make sense of their own aversion to themselves in terms of the most “natural” category available (close kin relationship), and “feel” that what is repellent to them is sex with *close relatives*. Finally, in the third stage, since the content of the aversion (inhibition) has been subjectively distorted it is now prepared, as it were, to serve as a basis for the emergence of a fully-fledged and explicit prohibition: “Don’t have sex with *close relatives*!”. Through a sort of “double negation”, or a process where two consecutive mistakes cancel out, prohibitions end up by forbidding exactly what, in its own clumsy way, natural selection was also fighting against in the first place.⁵

5. Conclusion

Let me at last clearly state what I did *not* intend to say (or imply) in this paper. I certainly did not want to urge you to accept the Westermarck hypothesis on the origin of incest taboos. Although this theory has risen from the dead in the fifties and although, after a long period of having been universally but undeservedly rejected and ridiculed, it is presently shining with a new radiance, it is still far from being conclusively established. (For some difficulties see Leavitt 1990). In only two respects it seems that something of a consensus is today emerging that Westermarck was right: (1) that there really is an inborn sexual aversion between those raised together in early childhood, and (2) that this aversion is, as Westermarck suggested, an evolutionary adaptation (serving as a barrier to the inbreeding depression). What remains highly debatable, however, is a third point: (3) whether the existing inhibition is actually *causing* the incest prohibition. Westermarck’s hypothesis is here facing one formidable rival, the claim that the central causal factor in explaining the genesis of incest taboos is human awareness of the harmful effects of close inbreeding. According to this theory (see Durham 1991), the norm against incest was introduced precisely because the behavior was widely (and correctly) perceived as detrimental. In contrast to Westermarck’s strongly biological account where the whole causal chain largely by-passes human consciousness, i.e., where the danger of inbreeding depression first produces the inhibition which then directly produces (or expresses itself as)

the prohibition, in the alternative scenario cultural factors loom large, in that the emergence of incest taboos essentially depends on the prior existence of social beliefs about the deleterious effects of incestuous relationships. Since in this theoretical conflict the jury is still out I surely do not want to short-circuit the controversy and proclaim the winner. Rather, I only tried to make sure that Westermarck's hypothesis not be rejected for the *wrong* reasons.

If I were asked to summarize the conclusion of the paper in one sentence I would probably, like most of my philosophical brethren, respond with an outcry of indignation that telegram is not an appropriate medium for profound and intricate philosophical messages. But if for whatever reason I just had to make that abominable compression I would in the end come up with a one-liner, describing a principle of methodological parsimony for the discussion of sociobiological theories. In a slight rewording of Occam's razor, and in an attempt to make the condensed claim at least more resounding, I would render the slogan in Latin as follows: *objectiones non sunt multiplicandae praeter necessitatem*.

Notes

¹ I worked on this paper while I was a NSF Research Fellow at the University of Minnesota during the academic year 1995-96. I have benefited from discussions after presenting the paper at the regular Friday Colloquium of the Minnesota Center for Philosophy of Science, and at the Department of Philosophy, University of Leeds.

² "For if the 'taboo' is indeed genetically prescribed, what need is there for mere social legislation to enforce it?" (Rose et al. 1984, p. 137).

³ Just as an interesting aside, Williams and Kitcher seem to be unaware that the very point they are making has been actually raised a long time ago by James Frazer (1910, pp. 97-98), and subsequently by Sigmund Freud (1922, p. 177; 1952, p. 123), in their criticism of Edward Westermarck, and moreover that Westermarck tried to come to grips with the objection in the fifth, extensively rewritten edition of his *History of Human Marriage*.

⁴ Harry Lewis suggested in conversation that the subjects need not reach for the concept "sibling" or "close relative", for they could well conceptualize their aversion in terms of another "natural" and readily available property: "those persons with whom one has been *familiar*". But the problem with that suggestion is that familiarity as such does not breed aversion. There is a critical period for the development of the sexual aversion (the first two or three years of life); after that age the regular cohabitation and familiarity remain without effect. Therefore, it seems that after all there is no easy way, from the inside, to make sense of the Westermarckian inhibition (other than via the concept *kin*).

⁵ Note that those who claim that the prohibition is caused by the inhibition face an additional obligation of which they are not always aware. Namely, not every inhibition or aversion gives rise to the corresponding prohibition. In many societies people are almost universally disgusted at the idea of, say, eating insects alive, but still hardly anyone would take trouble to try to prohibit such a practice when it occurs. This shows that in order to make their case the defenders of Westermarck have also to explain the following: why the aversion postulated as the cause of the incest taboos is of such a kind that it not merely inhibits subjects from having sex with close relatives but tends, in addition, to generate the condemnation and suppression of these acts in others.

References

- Bateson, P.: 1982, "Optimal Outbreeding", in P. Bateson (ed.), *Mate Choice*, Cambridge University Press, Cambridge.
- Bateson, P.: 1989, "Does Evolutionary Biology Contribute to Ethics", *Biology and Philosophy* **4**, 287–301.
- Cargile, J.: 1966, "Pascal's Wager", *Philosophy* **35**, 250–257.
- Durham, W.H.: 1991, *Coevolution: Genes, Culture, and Human Diversity*, Stanford University Press, Stanford.
- Elster, J.: 1984, *Ulysses and the Sirens*, Cambridge University Press, Cambridge.
- Frazer, J.: 1910, *Totemism and Exogamy*, vol. IV, Macmillan, London.
- Freud, S.: 1922, *Introductory Lectures on Psychoanalysis*, Allen & Unwin, London.
- Freud, S.: 1952, *Totem and Taboo*, W. W. Norton, New York.
- Gellner, E.: 1963, *Words and Things*, Victor Gollancz, London.
- Hacking, I.: 1975, *The Emergence of Probability*, Cambridge University Press, Cambridge.
- Kitcher, P.: 1985, *Vaulting Ambition: Sociobiology and the Quest for Human Nature*, MIT Press, Cambridge, Mass.
- Kitcher, P.: 1990, "Developmental Decomposition and the Future of Human Behavioral Ecology", *Philosophy of Science* **57**, 96–117.
- Leavitt, G.C.: 1990, "Sociobiological Explanations of Incest Avoidance: A Critical Review of Evidential Claims", *American Anthropologist* **92**, 971–993.
- Mackie, J.L.: 1982, *The Miracle of Theism*, Clarendon Press, Oxford.
- Rose S., Lewontin, R.C. and Kamin, L.J.: 1984, *Not In Our Genes: Biology, Ideology and Human Nature*, Penguin, Harmondsworth.
- Westermarck, E.: 1921, *History of Human Marriage* (5th edition), Macmillan, London.
- Williams, B.: 1973, *Problems of the Self*, Cambridge University Press, Cambridge.
- Williams, B.: 1983, 'Evolution, Ethics, and the Representation Problem', in D.S. Bendall (ed.), *Evolution from Molecules to Men*. Cambridge University Press, Cambridge.
- Williams, B.: 1995, *Making Sense of Humanity and Other Philosophical Papers 1982–1993*, Cambridge University Press, Cambridge.
- Wolf, A.P.: 1993, "Westermarck Redivivus", *Annual Review of Anthropology* **22**, 157–175.
- Wolf, A.P.: 1995, *Sexual Attraction and Childhood Association: A Chinese Brief for Edward Westermarck*, Stanford University Press, Stanford.