

not the same as Social Darwinism. To me, one of the greatest strengths and attractions of the evolutionary approach is that it stresses the universality of the human genotype, and human nature. Race has a social, not a genetic basis, as there is more genetic diversity within the groups we call races than there is between them. We should all educate others as to this basic premise of the evolutionary approach.

How do we do this? Make your willingness to consider evolutionary ideas public. We have something of an "emperor's new clothes" problem. If no one says what they think, things will go along as they are. Do evolutionary work. Test evolutionary hypotheses. Don't forget to publicly disassociate yourself from sexism and racism. Encourage your students and others to join the evolution and sociology section. Use your bully pulpits and soap boxes if you have them.

We have one shot at this. If it fails, I know I personally won't have the energy to try again. If it succeeds, we all win, and so does sociology.

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The Incest Taboo: Biological Evolution, Cultural Evolution, or Coevolution?

Review Essay

Incest: Origins of the Taboo

by Jonathan H. Turner and
Alexandra Maryanski

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Jonathan Turner and Alexandra Maryanski's new book, *Incest: Origins of the Taboo*, is, to the best of my knowledge, the most comprehensive book ever written by a sociologist or team of sociologists on the great question of the incest taboo. It is, in my opinion, also the best and the most important. Anthropologists have written extensively on the taboo, but few sociologists have given it sustained attention. For sociologists, then, Turner and Maryanski's book is an extremely welcome contribution.

In their first two chapters, Turner and Maryanski provide an excellent discussion of the most prominent theories of the incest taboo, even going as far back as medieval and ancient thinkers. They discuss Westermarck's classic nineteenth-century Darwinian theory that close association in early childhood produces sexual inhibitions; the functionalist theories of Parsons, Kingsley Davis, and Malinowski that the incest taboo is a cultural invention designed to minimize conflict within the nuclear family; Lévi-Strauss's alliance theory; feminist theories; demographic theories; Robin Fox's synthetic biosocial theory; and recent sociobiological theories that resurrect Westermarck. Chapter 2 also reviews some of the considerable empirical evidence (e.g., marriage patterns in Israeli kibbutzim and Taiwanese sim-pua marriages) for a "Westermarck effect," as well as the large amount of evidence indicating that close inbreeding produces harmful genetic consequences.

In Chapter 3, the authors demonstrate that the extent to which the incest taboo is violated varies dramatically among the three dyads within the nuclear family. Father-daughter incest is by far the most common, and mother-son incest is rare. This appears to be the case not only in our own society, but in all societies. Brother-sister incest is somewhere in between, but, the authors claim, it occurs more frequently than we have thought and may be close to father-daughter incest in its frequency. Turner and Maryanski also demonstrate that the psychological consequences of incest vary by family dyad. Father-daughter and brother-sister incest normally have harmful effects, most commonly in the form of depression, anxiety disorders, and the like. But mother-son incest has much more serious psychological consequences, often producing full-blown psychoses. The evidence reviewed in this chapter becomes a key part of Turner and Maryanski's analysis, because they claim that a valid theory of the incest taboo must explain not only why the nuclear family incest taboo is a human universal, but why the actual occurrence of incest, and the psychological consequences of incest, vary so much by family dyad.

Chapter 4 takes us on a short detour through ape and hominid evolution. Turner and Maryanski show that, among our monkey and ape relatives, inbreeding is usually avoided by dispersal patterns. Among chimpanzees, for example, once females reach sexual maturity they leave their communities and mate with males from neighboring communities, thus avoiding breeding with siblings or parents. Toward the end of this chapter, and at the beginning of Chapter 5, the authors argue that there was pressure on our ape-like ancestors to develop a nuclear family structure out of their loosely structured community patterns, and that the evolution of the nuclear family created stronger bonds between people in all three dyads.

Out of a primitive "horde," nuclear family patterns began to emerge around the time of *Homo erectus*, and the bonds between family members, being highly adaptive under new environmental conditions, grew stronger. At the same time, old transfer patterns at sexual maturity declined and people remained in their

nuclear families longer. Fathers and daughters, brothers and sisters, and mothers and sons became increasingly attached and, as they did, the problem of incestuous behavior loomed larger. This was especially the case for fathers and daughters, because, in Turner and Maryanski's view, there was no "hard wiring" in this dyad against incest (no strong "Westermarck effect"). Turner and Maryanski accept the existence of a Westermarck effect for brothers and sisters (and, possibly – the authors are not quite sure – a more limited effect for fathers and daughters), but argue that mother-son incest avoidance was especially hard wired, being a carryover from our ape ancestry. A culturally imposed taboo was thus needed to prevent sexual relations between fathers and daughters, and sometimes between brothers and sisters, in order both to maintain solidarity within the family and to avoid the costs of inbreeding depression. The incest taboo was therefore not the exclusive result of either biological or cultural evolution, but rather resulted from the coevolution of cultural and biological forces.

I am greatly impressed with Turner and Maryanski's knowledge of a wide body of literature in sociology, anthropology, primatology, and clinical psychology, and with their heroic effort to integrate ideas from this literature into an original synthetic theory. However, I do not agree with their main coevolutionary conclusion. Contrary to them, I submit that Westermarck's old theory is largely sufficient to explain all of the things that Turner and Maryanski say need explaining. Let me first examine what I see as the difficulties of their argument, after which I will propose my own Darwinian theory of the incest taboo and violations of it.

Where Turner and Maryanski get into the most trouble is by claiming that the growing intensity of emotional bonds between nuclear family members in hominid evolution would have led to greater sexual arousal. This is problematic in that the authors have conflated two completely different kinds of bonds: familial bonds and sexual bonds. The one does not imply the other; in fact, evidence strongly suggests they are inversely related. This evidence comes from Mark Erickson (1989), a psychiatrist who has treated hundreds of

victims of incest. He found in his clinical practice that almost every incestuous relationship emerged from some sort of family dysfunctionality. This led him to the hypothesis, which is really just an extension of Westermarck, that familial bonding undermines or reempts sexual bonding. When the dyadic units within the nuclear family form strong familial bonds, somehow the individuals involved become unable to form, or at least highly unlikely to form, sexual bonds, i.e., to become sexually attracted to and interested in each other. Natural selection has produced this effect – whose precise mechanisms of action we certainly do not yet understand – because inbreeding produces many harmful genetic consequences and this is fitness reducing.

The authors argue that among ancient hunter-gatherer bands it was likely that daughters would have remained in their natal units until well past puberty. Not necessarily. We know that most recent hunter-gatherers are either patrilocal or neolocal, and this would have been likely for prehistoric hunter-gatherers as well. Female puberty in bands and tribes normally occurs around the ages of 14-16, and marriage usually occurs shortly after puberty. Thus, in many instances girls would be leaving their natal homes right after sexual maturity. Turner and Maryanski's argument certainly does apply to modern industrial societies, where puberty comes much earlier (usually around 11 or 12, sometimes even earlier) and where girls are unlikely to leave their natal households until the age of 17 or 18, and often several years later. However, Turner and Maryanski's theory assumes the evolution of an incest taboo as early as two million years ago or earlier. They make the point that, with rising levels of family dysfunctionality in recent decades, the frequency of incest has increased, and this is undoubtedly the case. The fact that sexually mature females are now coresident with their brothers and fathers for perhaps as long as 8 or 10 years would certainly increase the temptation to incest, and thus its actual frequency of occurrence. However, all of this is just a tick of a second in the span of the last two million years of human prehistory and has no bearing at all on the question of cultural selection for an incest taboo.

As I was reading the early chapters of *Incest*, I had at the ready the point that the incest taboo is only rarely a highly conscious and explicit social norm that is directly transmitted to each new generation. However, the authors anticipate this point by noting that “rarely does a parent take a child and instruct him about the dangers of incest and of the importance of not violating the taboo . . . [I]n fact, it is probably rarely mentioned. Yet somehow just about everyone in modern societies comes to sense that [my emphasis] certain actions are taboo. . . . In many societies, the taboo is not even part of the legal system because everyone ‘knows’ (but how?) that incest is not done.” And they continue by saying that the incest taboo “was never spoken or written down. Indeed, this is just how the taboo works today; and in fact, sometimes acts are so tabooed that no one speaks of them but, somehow, everyone knows [my emphasis again] about the prohibition.”

Informal surveys of college students that I conducted years ago provide strong evidence that, in our society at least, Turner and Maryanski are exactly right: The incest taboo is seldom discussed and seldom transmitted by socialization, and thus is overwhelmingly implicit rather than explicit. Only a tiny fraction of students in my surveys reported even the barest hint of socialization into the taboo, and my own parents never discussed it for a single second. Yet somehow we all “knew” that this was just not done. There is every reason to suspect that my own experiences and those of my students are replicated throughout our society, other industrial societies, and most preindustrial societies. So, if the incest taboo is even partially a cultural product, it is a most curious fact that people are not speaking of it and socializing the next generation into this norm.

The authors claim that Darwinian evolutionary arguments cannot explain the differential rates of incest occurrence in the three nuclear family dyads, nor can it explain why mother-son incest leads to much more serious psychopathologies than the other two types of incest. I disagree, and in this respect would like to develop three main points. First, Erickson's argument and evidence to the effect that familial bonding undermines sexual

bonding tells us that – and, in a general sense, how – father-daughter incest avoidance occurs in the absence of a cultural norm. Moreover, in Darwinian terms, since all nuclear family dyads involve people who are related by 50 percent of their genes, there has to be equal selective pressure against incest in all three dyads because the genetic consequences of inbreeding depression will be the same for each. It is implausible to argue, as Turner and Maryanski do, that the father-daughter aversion is not hard wired just as the brother-sister and mother-son aversions are; the father-daughter aversion, from a natural selectionist point of view, should also be hard wired.

Evidence that there are strong biological predispositions to incest avoidance in all three nuclear family dyads involves a personal story. My mother was a very beautiful woman, an attendant to the homecoming queen at the University of Missouri in 1942. Despite a great deal of physical closeness, and coresidence for some 9 or 10 years after I reached puberty, the last emotion on earth I had was a sexual desire for her. I seriously doubt whether I ever thought about it, but if I did the thought would have been extremely repugnant. There is no doubt that my mother and I experienced a very strong Westermarckian sexual aversion to each other. Moreover, I have a sister who is three years younger. We were coresident for nearly 19 years, well past puberty for both of us. She was also extremely attractive and widely sought after by young men her age. She even walked around the house in her underwear from time to time. But I felt absolutely no sexual attraction whatsoever. Never did, never have, never will. We were negatively sexually imprinted against each other. However, these experiences are not anomalous for Turner and Maryanski's theory, since they accept the existence of a Westermarck effect for the mother-son and brother-sister dyads.

But then, later in life, I had a daughter, now grown. Just like her aunt and her grandmother, she is also extremely attractive, but I have never felt the slightest twinge of sexual desire even though I have hugged and kissed her frequently since she was a tiny child. So my daughter and I are negatively sexually imprinted against each other, just as my

mother and I were and my sister and I were and are. I bonded strongly with my mother and sister, and also with my daughter (very strongly with my daughter). Familial bonding undermines sexual bonding. The only question is, are my own experiences typical of most people in our own society, other industrial societies, and in all types of preindustrial societies? I submit that they are indeed exactly that. There is no reason to think otherwise.¹

Second, explaining the different frequencies of incestuous behavior in different family dyads in Darwinian terms is not difficult. Father-daughter incest is most common because there is a universal male preference for younger females (evidence reviewed in Sanderson, 2001). The strong attraction of older men to much younger women – including women a full generation or more younger – leads to many more temptations under circumstances in which a natural incest aversion may not have “taken.” Mother-son incest will be the rarest because males are seldom attracted to older women – especially those a full generation older – and women are seldom attracted to much younger men. There are many fewer temptations to contend with here. And brothers and sisters are in between these extremes, so brother-sister incest should have an intermediate rate of occurrence, which apparently it does.

Third, a straightforward Darwinian explanation is also easily available for the differential rates of psychopathology in the three nuclear family dyads. The evidence shows that everywhere mothers are far more nurturant than fathers and take most of the responsibility for infant and child care (reviewed at length in Sanderson, 2001). In contemporary hunter-gatherers – the closest thing we know of to the ancestral human environment, the environment in which the incest taboo would have evolved – mothers commonly breast feed children for as long as 5 or 6 years, and thus there is extremely close and long-term physical contact between mothers and very young children. Therefore, the emotional bond that develops between mother and child is much stronger than the emotional bond that develops between father and child. Fathers have much less contact with their children and are much more likely to be

emotionally distant, especially in highly patriarchal societies. Hence, mother-son incest would breach the familial bond in a very extreme way, much more than father-daughter incest or brother-sister incest, thus leading to much more severe psychopathology when the bond is breached.

And so it is my contention that all of the phenomena that Turner and Maryanski correctly argue need to be explained can in fact be explained with the Westermarck theory, Erickson's extension of it, and a few basic principles of sociobiology or evolutionary psychology. Cultural selection is not really involved and need not be invoked, except in some cases where people recognize the deleterious genetic effects of certain kinds of cousin marriage and thus act to forbid those marriages (cf. Durham, 1991). My explanation is somewhat speculative, but so is Turner and Maryanski's, as they fully concede. And mine has the advantage of being more consistent with the range of known evidence, as well as being more parsimonious since it invokes only one form of evolution rather than two. I wish to stress that my disagreement with Turner and Maryanski is disagreement within a common framework or paradigm. All three of us strongly believe in a scientific and a biosocial sociology. The main difference between us is that they still cling to some older sociological ways of thinking, whereas I have largely abandoned such thinking, no longer seeing it as useful or necessary. Who is right? No one can say for sure. In time, with more research and serious thinking, we may well find out. Right now, both theories are "underdetermined" by the evidence, and thus debate and discussion will continue.

My grand conclusion is that, despite my disagreement with Turner and Maryanski's main argument and my Darwinian alternative to it, it is difficult to say enough good things about this book. It is a marvelous contribution to the literature and should be read by all sociologists and anthropologists who take human biology seriously (as well as by those who don't in hopes that they still might!). The authors have an exceptional command of a wide range of theoretical and empirical literature, and the book reveals a great deal of sophisticated reasoning from basic premises to an original

conclusion. It should serve as an exemplar for the very best way to do sociology – or, more accurately, interdisciplinary social science.

Note

1. This review is based on my reading of Turner and Maryanski's original manuscript and the authors had a chance to read the review before publication. As a result of my argument and evidence that father-daughter incest avoidance is also subject to a Westermarck-like effect (an Erickson effect might be a better name for it), the authors have modified their argument slightly in the final published book. They now accept that there might be a biologically based father-daughter incest aversion, but they stress that if such a predisposition does exist it is weaker and thus not as hard wired as the aversions for the other two family dyads.

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