



# Evolution & Sociology

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## **A note from the Chair What are we about?**

Alexandra Maryanski  
UC-Riverside

Let me update you on our progress as a section-in-formation. First, although we are not eligible for a regular Evolution and Sociology session or even an ASA-sponsored reception until we meet the 300 membership requirement, we did manage to secure a place in the regular ASA program with a one hour panel session (see Tim Crippen in this newsletter for details) and we are going to have an Evolution and Sociology reception on Saturday evening, August 13th at 6:30 pm in one of the convention hotels. A thank you to Janet Astner and Mike Murphy of ASA for working with us to make this possible. Please mark your calendars for both events and bring along your friends (and enemies, we're not picky) to be recruited for membership. At our ASA reception, a \$500 check (thanks to a generous member) will be presented to the graduate student who wrote the best paper on evolution and sociology.

Second, while we are off to a promising start with 117 paying members, we face a running, uphill battle to get 300 paying members. If we are to reach that magic number, I need to call upon everyone to help with recruitment. One tack is to urge your colleagues to become members or at least personally invite them to our panel session and reception. Another is to adopt some ASA-member graduate students and pay their section dues (it only costs \$5.00 each).

**If each of us gets just *two* more  
ASA members to join the section,  
the section will be permanent!**

In recruiting, be prepared to answer some challenging questions already put to me that include: Why is evolutionary theory relevant for sociology? Why do we need a special section focused on evolution? And what is a section devoted to evolution and sociology about anyway?

The first question is easy to answer since evolutionary processes affect all life forms, and the Modern Synthesis is already the key unifying paradigm for disciplines as diverse as archaeology, primatology, systematics, biology, ecology and genetics; and in the social sciences, evolutionary thinking is flourishing in anthropology, economics and psychology. It is time for sociologists to take a more objective attitude towards biological arguments. Richard Udry, a strong proponent of a biologically informed sociology, has long pointed out that if human bodies are the result of evolution, human behavior to some degree must also be the product of selection forces. Yet, as he has emphasized, "sociologists do not need to become biologists or to learn any biological laboratory or field techniques in order to begin to recognize what the biological sciences can contribute to the improvement of their sociology." Answering the second question is also easy; for, although an increasing number of sociologists now believe that modern evolutionary theory has much to offer sociology, it remains a bone of contention for many others. A section highlighting evolutionary reasoning is necessary now because it will hopefully provide a stepping stone for integrating biological variables into mainstream sociology and traditional sociological theory. Someday, assuming we are successful, an Evolution and Sociology section may not be needed because evolutionary theory will have become an essential part of mainstream sociology.

Finally, although we are a diverse group we must all pull together to make this section work. Since evolutionary sociology is not really a clearly defined specialized area of sociology (as other sections are), our membership is bound to be eclectic. Yet, despite our lack of a collective identity, we do share the common goal of using evolutionary logic to address traditional sociological problems. Thus, you can tell interested sociologists (and interested

others) that membership in our section requires very little necessary background. Indeed, any science-oriented scholar who is skeptical but none-the-less curious about modern evolutionary theory and its application in sociology is very welcome indeed.

Alexandra

P.S. To sign up an existing ASA member by paying his/her section dues or if someone asks you how to become a member:

1. Send a check with our section name and the name of the new member to:

The American Sociological Association  
1307 New York Avenue, NW  
Suite 700  
Washington, DC 20005

2. Pay by credit card online by going to:

<http://www.asanet.org/members/membership.html>

Once there you have three choices: to add a section membership to an existing membership; to renew ASA and/or sections for 2005; or to simply update contact information. (Courtesy of Mike Murphy)

**The next meeting of the  
Human Behavior and Evolution  
Society will be  
June 1 - 5, 2005 at the  
University of Texas, Austin  
See [www.hbes.com](http://www.hbes.com)  
for more details**

## Becoming an Evolutionist

Gerhard Lenski,  
Professor Emeritus  
University of North Carolina at Chapel Hill

Many years ago, when I was a novice graduate student in sociology, one of the first lessons I learned was that evolutionism was passé and that functionalism was the coming thing. All of the evolutionists of the past had been mere armchair theorists whose speculations lacked the kind of solid empirical foundation science demands. As Crane Brinton contemptuously put it, "Who reads Spencer today?"

We students, I'm afraid, were a gullible lot. We saw what we were told we should see, and like the people in the old story, enthusiastically praised the emperor's new clothes. It was only a decade later, after many semesters of teaching the introductory course and finding myself frustrated year after year by the task of making sense of the discipline of sociology to yet another group of students, that I began to see merit in the work of the older evolutionists whom I had earlier rejected. Several experiences along the way still stick in my memory.

For example, I remember one student in particular. In a very large class of 200 or more, he stood out because of the perceptive questions he would ask and the insightful comments he would make at the end of class sessions. At the end of the final lecture, I remember that he waited patiently at the edge of a group of students that had gathered to ask questions about the final exam. When the last of them left, he stepped forward to say how much he had enjoyed the course. But he said there was just one question he had not been able to answer. As I preened myself and looked forward to putting the capstone on his wonderful experience, his question totally deflated me, "What is sociology?"

This question was especially unsettling because I had already picked up signals in previous semesters that this was a problem that bothered a number of students. As a result, I had introduced a new lecture at the beginning of the course in which I carefully

explained what sociology was all about. But clearly that had not done the trick. The diffuse and disjointed nature of the materials we discussed, and the absence of a meaningful theoretical framework to integrate them, left far too many students puzzled about the discipline itself and wondering whether there even was a discipline underlying all the diverse materials.

About this same time, I remember another incident that occurred in a coffee shop where a young history instructor and I were commiserating with one another over the hard lives we led. Each of us was sure his own responsibilities were the more onerous. At one point, however, after I had made what I thought was a singularly telling point, my friend said, "But the trouble with teaching introductory history courses is that they are all simply the study of one damn thing after another."

Like St. Paul on the road to Damascus, I was struck by this statement; it hit me like a blinding light, a revelation. For the first time, I understood the problem that troubled my students. Sociology, taught from a functionalist perspective, was simply the study of one damn thing after another: some things about family, some things about communities, some things about ethnicity, some things about conflict, some things about cooperation, some things about this, and some things about that. Truly a thing of shreds and patches. But in Yeats' immortal words, the center would not hold. In fact, there was no center except for the unhelpful assertion that every social process and every social arrangement serve some function for somebody.

About this tie, I discovered Walter Goldschmidt's slender little classic *Man's Way: A Preface to the Understanding of Human Society*. It provided a fresh and exciting approach to the study of human societies—an openly evolutionary approach that integrated an amazing range of materials into a coherent and meaningful whole. In short, it had the center that functionalism lacked.

In those days Goldschmidt was a voice crying in the wilderness. Most social scientists shared Brinton's contempt not only for Spencer but for evolutionists as a whole. But Goldschmidt's primer made sense in a way that the popular theories of the day did not.

Also about this time, I encountered V. Gordon Childe's volume *Man Makes Himself*. It provided an exciting historical dimension that was largely implicit in Goldschmidt's work. As I began incorporating these new materials into my thinking and teaching, I began finding all kinds of things falling into meaningful patterns and relationships.

During these same years, I was regularly leading a graduate seminar in social stratification, or social inequality. Here too, I found the greatest problem was that of establishing some kind of theoretical integration of the wealth of diffuse materials.

In those days, the two alternatives appeared to be functionalism and Marxian theory. But both left much to be desired. In an effort to develop a more solid foundation for the study of inequality, I began reading historical and ethnographic monographs as widely as possible. In time, this led to the publication of *Power and Privilege* in 1966, a volume that was explicitly evolutionary in its approach.

From there, it was a short step to the publication of the first edition of *Human Societies* in 1970. This volume was an attempt to formulate a modern evolutionary theory and use it to provide what I hoped would be a more intellectually stimulating introduction to the discipline of sociology than the standard textbooks of the day, with their eclectic and largely descriptive surveys of contemporary American society. I also hoped that *Human Societies* would help students to see how materials from seemingly disparate fields as sociology, anthropology, archaeology, history, political science, economics and even biology could all be brought together into a single theory capable of explaining the incredible transformation of human life during the last 10-12,000 years and of providing new and useful insights into the process of change in our own day.

Over the years, the evolutionary approach to sociology has proved so successful in the classrooms in which it has been tried that I find myself wondering why it is that more sociologists have not adopted it, and why the ASA even needs a special section on evolution. To update Crane Brinton, "Why are we not all evolutionists today?"

Are most of our colleagues so well satisfied with the largely atheoretical and eclectic approach to our discipline that is reflected in most of our standard textbooks, and are they satisfied with the supermarket approach that most theory courses currently provide? Or is it that the problem lies with deficiencies in our graduate programs—deficiencies that allow, and even encourage, neglect of the sociologically relevant and important aspects of biology, history, anthropology, and other sister disciplines?

Whatever the cause, I can only hope that, for the sake of the future well-being of the discipline of sociology, more of our colleagues will soon begin to recognize the impossibility of creating a meaningful or useful science of human societies on any foundation other than an evolutionary one. Creating such an awareness can, and should, be the primary mission of our new section, Evolution and Sociology.

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## **Why do we need a new section on evolution and sociology?**

Rosemary L. Hopcroft  
University of North Carolina at Charlotte

I thought I would take some space in our newsletter and say a few words about who I am, and why I thought it necessary to establish a new section on evolution and sociology.

First, my doctorate is from the University of Washington, where my dissertation was supervised by Pierre van den Berghe. For those of you who don't know him, Pierre was a forceful, erudite and articulate proponent of sociobiology from the mid 1970s on. It won him few friends in sociology. As a graduate student, I found nothing offensive in evolutionary thinking, but was not so naïve that I thought I could get away with doing evolutionary research for my dissertation. So, my dissertation ended up being comparative and historical, and focused on institutions and economic change.

The 1990s brought the emergence of evolutionary psychology, which I personally

found to be a more appealing approach than straight sociobiology. It helped explain for me all the human behavior I saw around me that did not appear to be “fitness-maximizing.” There was also a change in sociology, reflecting change all over the academy, towards a greater willingness to consider evolutionary ideas as relevant to social science. My former office-mate from the University of Washington, Satoshi Kanazawa, bravely set forth publicly promoting evolutionary psychology within sociology.

This came at a time when I became tenured, and where I was taking stock of my own research career such as it was. I was hesitant to venture into evolutionary work as it would pose costs for me: both intellectual and personal. I had seen how Pierre van den Berghe’s work had been received within sociology. However, it seemed unreasonable to me that we couldn’t even test hypotheses drawn from evolutionary theory without sociologists taking offense. Why is biology a dirty word in sociology? Are we not humans – an evolved species like any other?

Also, it seems to me that evolutionary ideas are an endorsement of the sociological approach. We evolved to be social by nature, and that means that we evolved to be influenced by what others in our group do – hence the impact of social norms, roles, institutions, and all the social factors that sociologists discuss. Yet, like other organisms we also evolved to be self serving and look out for our own wellbeing and that of our genetic relatives. We also evolved to be not necessarily conscious of our own motivations, whether self serving or prosocial.

Initially I just planned to do evolutionary work, and started to do quantitative research in the area. Some of this has now been published (e.g. see Hopcroft 2002, 2005). However, given the initial reception to this work by my colleagues, I began to see the need for a section. First, it is difficult to test evolutionary hypotheses when sociologists will not even acknowledge that it is legitimate to do so. There needed to be a body of people who would take the approach seriously, and who had enough knowledge of evolutionary theory that they could constructively critique the work.

Others who I approached initially said that it would be better if evolutionists did not just “talk to each other,” but that they find a professional home in the various substantive section areas. I agree that, in a perfect world, this would be ideal and eventually I hope that will be the case. But to get evolutionary ideas on the table, I thought it was necessary to have the support of the institutional base created by the section. This is particularly important for young scholars who want to work in the area, but worry (and rightly so) about how it will affect their careers.

What do I think the section should do? First and foremost, it should serve to support evolutionary research in all areas. Broadly, it should promote research that examines biological effects, as well as others, on social behavior, as well as the reverse (social effects on biology). In particular, it should support young scholars in the area who are seeking to make careers in sociology, but do not yet have the security of tenure.

To do all this we need to become a permanent section. We have just the rest of this year and next to get the 300 members we need to achieve this. We need to convince our fellow sociologists that evolutionary theory will help them, that it is not something that necessarily proves “sociological” theories wrong. Rather, it can buttress and fortify the whole discipline and reinvigorate the sociological approach.

We need to convince our colleagues that it does not promote sexism. No one is trying to say that females are biologically inferior to males. Different, yes, in ways that are not yet fully understood., but certainly not inferior. I quote Olivia Judson, from the New York Times Op ed section on 1/23/05: “Beliefs that men are intrinsically better at this or that have repeatedly led to discrimination and prejudice, and then they been proved to be nonsense. Women were thought not to be world class musicians. But when American symphony orchestras introduced blind auditions in the 1970s – the musician plays behind a screen so that his or her gender is invisible to those listening – the number of women offered jobs in professional orchestras increased.”

We also need to convince them it does not promote racism, and that evolutionary theory is

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.

### References

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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

(Paradigm Publishers, forthcoming June 2005;  
260 pp., \$65.00 hardbound, ISBN 1-59451-116-0;  
\$26.95 paper, ISBN 1-59451-117-9).

**Stephen K. Sanderson**

**Indiana University of Pennsylvania**

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.<sup>1</sup>

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.

#### References

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## **Sociology and Evolutionary Science: A natural alliance**

**A panel of distinguished scholars to speak at this year's annual meeting in Philadelphia**

Timothy Crippen  
University of Mary Washington

Curious to hear what Jon Turner thinks about linkages between sociology and evolutionary biology? Doug Massey? Richard Machalek? Rosemary Hopcroft? If so, have we got a panel for you!

On Saturday, August 13th, at the annual meetings of the ASA, our section-in-formation will sponsor a one-hour panel session on "Sociology & Evolutionary Science: A Natural Alliance." Jon, Doug, Richard, and Rosemary will deliver brief and informal remarks on this general theme. Their comments likely will reflect at least some of the diverse views of our membership. And, we hope that sufficient time will remain for our panelists to entertain comments and questions from those in attendance.

When the final program becomes available, please be sure to note the time and location of our Saturday session. So, mark your calendars and join us for what promises to be an informative and lively session. And, don't forget to tell your friends and colleagues. After all, time is short for us to achieve a total of 300 subscribed members, the number necessary to establish our section-in-formation as a permanent presence in the ASA.

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## **Biosocial Interaction Sessions at the Annual Meetings of the American Sociological Association**

### **I. Applications of evolutionary theory to social behavior**

1. Why sociology and biology should be more than just friends: Evolutionary theory as a unifying paradigm for sociology by Rosemary Hopcroft (UNC Charlotte)
2. The social exchange heuristic: Managing errors in social exchange by Toshio Yamagishi (Hokkaido University), Shigeru Terai (Hokkaido University), Toko Kiyonari (McMaster University), Nobuhiro Mifune (Hokkaido University), Satoshi Kanazawa (London School of Economics and Political Science)
3. Family, Kinship and the Origins of the Incest Taboo by Alexandra Maryanski (University of California-Riverside)
4. The Evolution of Emotions and their Role as an Exaptive Social Force by J. Scott Lewis (Bowling Green State University)

### **II. The Body Sociological: Connecting biophysical and social processes**

1. Biological Constraints on Gender? Feminists Wrestle with Testosterone by Barbara Risman (North Carolina State University), Shannon Davis (Carolina Population Center), and Cathy Zimmer (University of North Carolina)
2. Unified Stratification Theory: Structure, Genome and Status across Human Societies by Daniel Adkins, Guang Guo (Univ of North Carolina-Chapel Hill)
3. Integrating Biological and Social Data in the Study of Health: Benefits and Challenges by Jenna Mahay, Erin York, Stacy Lindau (University of Chicago)
4. Racial Disparities and Multiple Births by Seung-Eun Song (University of Texas at Austin)

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## Recent Publications by Section Members

Thomas D. Hall. 2005. "Borders, Borderland, and Frontiers, Global." Pp 238-242 in *New Dictionary of the History of Ideas*, Vol. 1 edited by Maryanne Cline Horowitz. Detroit: Charles Scribner's Sons.

Thomas D. Hall and James V. Fenelon. 2004. "The Futures of Indigenous Peoples: 9-11 and the Trajectory of Indigenous Survival and Resistance." *Journal of World-Systems Research* 10:1(Winter):153-197. on-line: <http://jwsr.ucr.edu/index.php>

Thomas D. Hall "Mongoly v mir-sistemnoi istorii (Mongols in World-System History). 2004. Pp. 136-166 in *Mongol'skaia Imperiia i Kochevoi Mir (The Mongol Empire and the Nomadic World)*, edited by B.V. Bazarov, N.N. Kradin, and T.D. Skrynnikova. Ulan-Ude, Russia: Izdatel'stvo Buriatskogo nauchnogo tsentra SO RAN (Published by the Buriat Scientific Center of the Russian Academy of Sciences).

Thomas D. Hall. "Ethnic Conflict as a Global Social Problem." 2004. Pp. 139-155 in *Handbook of Social Problems: A Comparative International Perspective*, edited by George Ritzer. Thousand Oaks, CA: Sage Publications.

Thomas D. Hall. "Genizaros." 2004. P. 356 in *Encyclopedia of the Great Plains*, edited by David Wishart. Lincoln: University of Nebraska Press.

Jonathan Turner and Alexandra Maryanski. 2005. *Incest: The Origin of the Taboo*. Paradigm Press. (due out April 2005).

### The Evolution & Sociology Section-in-Formation of the ASA

invites submissions for the

### Best Paper Award for Student Members of the Evolution & Sociology Section-in-Formation

The paper should be article length. It may be based on a master's or doctoral thesis, course paper, or a paper submitted to a journal or conference. It can be published or unpublished. Co-authored papers are accepted if all authors are students, but the award must be shared.

Author(s) must be student members of the Evolution & Sociology Section-in-Formation at the time of submission to qualify for the award.

The award is \$500.

Please send an electronic version of the paper (as a word or pdf file)

by **May 1st, 2005** to Rosemary Hopcroft at [rlhopcro@email.uncc.edu](mailto:rlhopcro@email.uncc.edu).

***If you would like to contribute something to this newsletter (essay, information, etc.) please contact the newsletter editor, Rosemary L. Hopcroft, [rlhopcro@email.uncc.edu](mailto:rlhopcro@email.uncc.edu).***