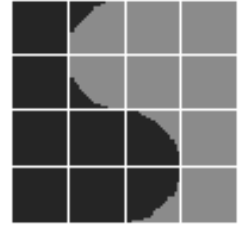




# Evolution & Sociology



Fall 2007 Newsletter of the ASA Section on Evolution and Sociology Volume 4, No. 2

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## Greetings from the Chair

Alexandra Maryanski  
University of California-Riverside

I'm writing with lots of good news and a little bad news. First, some positive: Evolution and Sociology by all accounts was a great success in New York this August. Tim Crippen's session on Sociology & Neo-Darwinism and Doug Massey's session on Sociology & Neuroscience were more than well-attended--the rooms were packed! At least 80 people came to our sessions (a few late-comers even had to stand in the back), and the informal word is that the papers delivered in our two ASA sessions were excellent. A big thank you to Tim and Doug and everyone who presented papers and delivered such a bang-up performance. Our ASA reception was also well attended, especially for a Monday evening with the convention about to end the next day.

Next year in Boston--August 1-4--our section activities will fall on the last day of the meetings, which is a mixed blessing. On the one hand, just about everyone has left (or about to) but on the other, all sections with paper presentations on the last day get a bonus--an extra session--giving us two paper sessions in Boston.

I invited Joan Huber (Ohio State University) and Richard Machalek (University of Wyoming) to each chair an open-submission paper session and they accepted: The sessions are titled:

New and Current Approaches to Evolutionary  
Thinking in Sociology #1

and

*Visit our website at  
[www2.asanet.org/sectionevol/](http://www2.asanet.org/sectionevol/)*

## New and Current Approaches to Evolutionary Thinking in Sociology #2

To be sure, these are not fancy titles, but they are intended to convey a simple message: Evolution and Sociology invites anyone whose research is grounded in modern evolutionary theory to submit a paper (anti-science types and creationists need not apply).

Finally, sad to say that our membership has fallen below 300, which gives us only one session per meeting (except when we are on during the last day of ASA). As of September, we have 243 members (6 low income members, 58 student members and 179 regular members). We lost nearly 100 members. But that's not actually too bad, as a good number of us asked (in some cases, begged) our friends and acquaintances to join just for a year to make Evolution and Sociology a permanent section (and we are very grateful). Now we have to begin building our membership up on a more solid footing by attracting scholars who are genuinely interested in bringing evolutionary theory back into sociology. But let me just say, that given our eclectic mix of members from all walks of evolutionary thinking, we will always need at least two sessions if we want to offer at least a portion of our members each year an opportunity to present papers on evolution at ASA meetings. Our paper presentations are one of the few outlets where this is possible. We lucked out this year by getting a bonus section, but next year we get only one session unless we can bring up our tally to 300 members. So, I need your help with recruitment once again. Given the great momentum we had going at our sessions this August, I think more sociologists are getting on the bandwagon (or at least curious about evolutionary thinking). But, I need you to help me by recruiting just one new member and we'll be fine one again.

All Good Wishes

Alexandra

### *NEW PUBLICATION SERIES*

Transaction Publishers of New Brunswick NJ and London England Announces the introduction of a new series ANTHROPOLOGY AND HUMAN NATURE. It will be edited by Lionel Tiger who is the Darwin Professor of Anthropology at Rutgers University.

The publishers are interested in works of social science, history, and General intellection which provide insight and contribution to the growing literature on what may be and may not be "human nature." Transaction also publishes the journal HUMAN NATURE and is receptive to works of interest to scholars and informed persons provoked by a subject matter only recently returned to active scrutiny. Even though Aristotle announced that "man is by nature a political animal," the emphasis on "political" has heretofore overwhelmed attention to "by nature." This the series hopes to remedy by publishing works widely advertised in the scholarly community and maintained in print durably and with care.

Anyone interested in proposing or contemplating a book appropriate to this adventure should contact Lionel Tiger either at ltiger@rci.rutgers.edu or at the Department of Anthropology, Rutgers University, 131 George Street, New Brunswick NJ 08901-1414.

## Bringing Biology Back In: An Intellectual Journey

(A Colloquium Presented to HDFS Faculty and Students October 30, 2006)

Robert L. Burgess  
Penn State University

Academic careers, like most others, often begin in the strangest ways. We counsel our children and our students to set goals, make plans and determine how best they can be accomplished. This sounds so reasonable, so rational, so cognitive! I am sure it works that way for some. It did not for me. Looking back, my journey to this point was often unplanned with considerable improvising along the way. As students of human behavior, it is important for us to realize that there is often a chaotic dimension to individual development but it can all work out in the end if we remain alert to opportunities when they arise. So, let me tell you a bit of my story.

Except for my senior year, my early schooling was in Catholic schools in Southern California. School came easy for me and I thoroughly enjoyed it. Once I reluctantly realized that I was not good enough as an athlete to earn a college athletic scholarship, and because the military draft was on, I decided to "Join the Navy and see the World" as the recruiting poster advertised. For an 18-year old kid, that was the start of a series of eye-opening experiences and I learned many things in fairly quick order that have served me well throughout my life. First, I was enthralled by the diverse cultures I encountered on the beautiful Islands of the South Pacific, sailing from the Marianas to the Eastern Carolines, to the Solomon Islands, to Samoa and Tahiti, as well as to Japan with its intriguing mixture of the medieval and the modern, the Philippines, Korea, that fascinating combination of ancient China and modern England, i.e., Hong Kong, and, of course, the ethnic diversity of Hawaii that was the home-port of the ship on which I was stationed. Second, I quickly learned that having special skills could lead to a more interesting and less physically-arduous life aboard ship. It also allowed for very quick advancement via Fleet-wide competitive

exams. Therefore, in less than 18 months, I advanced farther than my father did in his 6 years in the Navy and I found myself supervising sailors who had been in the Navy four-times as long as I. Third, I discovered that I had the ability to lead several lives simultaneously. I was a skilled radarman and anti-submarine warfare specialist, a petty-officer, an athlete, and a carousing sailor "on the beach" all at the same time. This chameleon-like tendency both fascinated and puzzled me and was understood only years later.

So, from my Naval experience, I learned that possessing advanced skills offered special benefits. Therefore, I realized that I needed to go to College. Because my discharge from the Navy was delayed for several months due to an international incident, I only had time to apply for admission to Long Beach City College in my hometown, and even then, just one week before classes began. This was fortunate for several reasons: enrollment was free to California residents, the instructors were excellent, and I met a vivacious 19 year-old girl with whom I quickly fell in love. I also "loved" all of my classes but I did not have the foggiest idea what I wanted as a career. While I was in the Navy, I had spent several weeks at Camp Pendleton training with the Marine Corps learning how to be a military policeman ( a "shore patrolman"), so I thought maybe that is what I should do – become a policeman. On the other hand, biology fascinated me although I was unsure how I could pursue that as a career.

Then I took courses in psychology and sociology and realized that it was possible to study human behavior scientifically just as we do plants and animals. So, I decided on a dual degree in psychology and sociology. At the very least, I thought that would be good training for a career in law enforcement. And, there was the added personal benefit that came from learning about the sociological concept of social roles and the psychological concept of personality traits and how the same trait can be can be manifested in very different ways. I was now able to understand that chameleon tendency to which I referred earlier whereby the same person could at different points in life be a good student, an altar-boy, a

seminarian, an athlete, a member of a rowdy car club, a skilled radarman, a military policeman, and a sometimes hard-drinking sailor who never backed down from a physical challenge.

By the time I selected my dual-major in psychology and sociology, I had married that 19 year-old girl and had gone on to Cal State, Long Beach to complete my undergraduate degree. Several months later, after the birth of our first daughter, we drove north to San Francisco to spend Thanksgiving with my aunt and uncle. My uncle was a senior scientist with the Stanford Research Institute and he gave us a tour of the Stanford campus. At one point, he drew our attention to what he identified as the Administration Building of the Graduate School. To show you how naive I was, I was not quite sure what he was talking about. So I asked him. After explaining it to us, he asked how my grades were and suggested that I explore with my professors the possibility of doing graduate work. Immediately upon our return to Southern California, I did just that. The problem was, I was unsure which discipline, psychology or sociology, interested me the most. Consequently, I applied to schools that seemed to offer both simultaneously, e.g., Harvard and its Department of Social Relations and the University of Michigan and its Social Psychology program. To my surprise, I was offered admission into these programs as well as the University of California, Berkeley, Stanford University, the University of Washington, USC, and Duke University. Yes, that required a lot of applications to fill out. That I did so, is a good measure of my lack of confidence in getting accepted into a major graduate program.

My eventual choice was Washington University in St. Louis because it had a Sociology/Anthropology Department with an option in Social Psychology. At that time, the Wash U. sociology department was ranked third in the nation behind Harvard and Cal, Berkeley. It was truly a multi-disciplinary program and, importantly, they recruited me. This was an intoxicating time for a young and naive student. Wash U. had an internationally-distinguished faculty that provided exemplary training while generating breathtaking intellectual arrogance. This must explain the

fact that, during just my third year in graduate school, I decided that I had learned all the faculty could possibly teach me and that it was time to begin my own professional career, having earned a Master's Degree in Sociology/Anthropology and since I was about to receive a Doctorate in Social Psychology. The intellectual arrogance of my professors must have been contagious.

But, what was my field of study or academic discipline? This, the 60's, was such an exciting time in the behavioral and social sciences. It was a time marked by growing optimism that we could make the world better through society-wide interventions in health, education, and welfare. It was also a period marked by intellectual ferment in all of the behavioral and social sciences. In psychology, Freudian and other personality theories were giving way to learning theory; in anthropology, culture and personality theory was beginning to give way to a recognition of the importance of evolutionary biology; in sociology, functionalism and symbolic- interaction were giving way to exchange theory. All of this fascinated me. My Master's thesis was part of a large funded research project on juvenile delinquency in the notorious inner-city housing projects of St. Louis. The tradition in the 60's, at least at Wash U., was that it was acceptable to derive your Master's thesis from the faculty's funded projects. My part of the larger study drew upon the ethnographic methods of my anthropological training as well as my experience in the Navy as a shore patrolman where I had direct contact with many forms of deviant behavior and had come to realize the critical importance of the peer groups to which one belongs.

Master's theses were one thing, doctoral dissertations, however, were another. The norm for dissertations was that they should be an original design of your own—from start to finish. In my case, it is important to understand that this was the beginning of the Golden Age of behaviorism and behavior modification and I had come under its spell. I had convinced myself that the complex subject matter of sociology and social psychology would benefit from the application of the tactics of experimental research developed and promoted by behaviorists. To test this

assumption, my doctoral dissertation was designed to compare different organizational structures and communication patterns in four-person problem-solving groups. I also designed and built the electronic circuitry to run my planned experiments. This required drawing the necessary schematics, purchasing the equipment that I would need from a Navy electronics- surplus store, and spending long hours disconnecting old wiring and then re-soldering the relays according to my design. Please do not assume that this was easy. There were many mis-steps along the way that required redesign, more soldering, many expletives, and entreaties to the electronic gods. The experiments, however, were a success and I was able to resolve previously contradictory findings in the research literature. The results were published in three successive peer-reviewed publications.

For the next ten years, while I was a faculty member at the University of Washington in Seattle moving up the academic ladder from assistant to full professor, I built upon that initial success and proceeded to experimentally analyze several other kinds of social behavior. I was in other words, a card-carrying social psychologist. Why? Because I was mainly interested in social behavior, social interaction, and group processes and it was social psychology that primarily dealt with these topics and saw them as the product of behavioral interdependence among individuals. This was a time when "small-group" studies and "group dynamics" were vibrant fields of study.

Social psychologist I may have been but, while I was pursuing my research, a dialogue had been going on in my mind that periodically thrust itself to the surface, for my early interest in biology was always lurking nearby. I felt that there was a common human nature that we needed to explain above all else, e.g., bi-parental care, long-term pair bonding, language, our lengthy childhood, deception, cooperation, competition, trust, jealousy, violence, sex-specific reproductive strategies. Yes, there are individual differences in these traits as well as in intelligence, personality, temperament, etc., but there are limits to these differences and the same differences are found all over the world and in all societies and

cultures. In taking this perspective, I was in good company. For instance, in 1871 in the *Descent of Man*, Charles Darwin wrote:

As man is a social animal, it is almost certain that he would inherit a tendency to be faithful to his comrades, and obedient to the leader of his tribe; for these qualities are common to most social animals. He would from an inherited tendency be willing to defend, in concert with others, his fellow men; and be ready to aid them in any way,

which did not too greatly interfere with his own welfare or his own strong desires. In a manner consistent with Darwin's assertion about the existence of a common human nature, social psychologists, especially after the end of WWII, had set about experimentally analyzing universal social processes such as the emergence of norms (Sherif, 1936), social conformity (Ash, 1952) and obedience to authority (Milgram, 1961) to give just a few examples. Throughout my ten years at UW, I worked within that tradition and proceeded to experimentally analyze several forms of social behavior, both in and outside of my laboratory and published studies on such topics as the factors influencing children's decisions to cooperate or compete with their peers, the structure and function of imitation in child development, the conditions under which status hierarchies emerge in small groups, and how differences in power affect the nature of exchange relations in adult dyads and a person's willingness or unwillingness to tolerate inequitable outcomes. In 1971, in recognition of this work, I was elected President of the West Coast Association for Small Group Research. My pleasure at presiding over the annual conference of the Association was enhanced because it required my return to Hawaii.

While I was carrying out these experimental studies, my internal dialogue continued as I struggled with another nagging thought. In this case, it was a lesson that I learned in my very first year as a graduate student and from the first book I was required to read. This lesson was that science consisted of more than the obsessive gathering of data. Only doing that was referred pejoratively as mere "dust bowl empiricism" leading, all too often, to outcomes equivalent to the repetitive re-discovery of the wheel. This, I was taught, is science at its

worst: overanalyzed measurement for its own sake. It is the kind of thing with which journals are stuffed, and which nobody reads. Good science, I learned, also consisted of exploring ideas and examining their logical power to explain why the data appear as they do. Indeed, the particular complexity of human behavior demands that our research be theoretically grounded. Theories are explanations of empirically established relationships and comprise an indispensable component of the scientific enterprise. The trouble with the behavioral and social sciences is not a dearth of information or data but a glut. We typically have more than we can handle. To be sure, data and theory must be in a proper balance; theoretical generalizations can be reached only from empirical evidence, but it is theory that gives value and interest to data.

Therefore, during this same period, drawing upon my Master's thesis and behavioral principles such as The Matching Law, another young assistant professor at UW, Ron Akers, and I published theoretical articles explaining delinquent, criminal and drug-abusive behavior. Fifteen years later, and to my great surprise, one of these articles, A Differential Association-Reinforcement Theory of Criminal Behavior, resulted in an award from the American Society of Criminology for being one of the ten most cited articles in the field at that time. As I mentioned earlier, the die had been cast since graduate school and, from that point on, theoretical issues played a significant role in my thinking and had led, first of all, to the co-authoring of a book in 1969, Behavioral Sociology, with a former fellow graduate student, Don Bushell, examining the importance of the theoretical principles and research techniques of operant conditioning for understanding social behavior. Theories, of course, serve many functions. They allow us to make sense of empirical observations; they can lead to recognition of unexpected connections between seemingly unrelated phenomena; and, they can lead to previously unanticipated hypotheses resulting in new knowledge. Less appreciated is the fact that theories can also lead us to question anew what we think we know by encouraging us to look at data through different lens. But, I am

getting ahead of myself and will return to this idea later.

Toward the end of my tenure at UW, I became increasingly interested in studying behavior in non-laboratory settings. I had grown a bit weary of trying to explain the relevance of my lab experiments to social scientists from other traditions. Frankly, I had come to the realization that social psychology experiments were often merely demonstrations of what was already known. Although, if we were honest with ourselves, we would have to admit that this criticism applies even today to much social science research. In any case, in 1974, as Director of the Center for Studies in Social Psychology at UW, I received a request for proposals (an RFP) from NIMH indicating that they were interested in encouraging researchers from outside family studies to investigate a problem that was increasingly on the front pages of our nation's newspapers, i.e., child abuse. Given my long-held interest in aggressive and violent behavior, I jumped at the opportunity and designed a study drawing upon my earlier work on communication patterns, the observational methods of anthropology and primatology, and the theoretical principles of behavioral psychology. I was fortunate to have my application funded even beyond the years I requested. But before I could get the study underway, Penn State's College of Human Development invited me to give a lecture where I described my still-developing and exploratory ideas about the importance of evolutionary biology for understanding human behavior. A few months after my visit, I was asked to apply for a position in the Department now known as HDFS. Given that I was in certain respects a man without a fixed disciplinary identity, the multi-disciplinarity of the Department was and remains very appealing to me and, thus, here I have been for the past 31 years.

Upon arriving, I often felt as if I had returned to graduate school and that I had much to learn in a very short period of time. I found myself with outstanding colleagues who had been trained differently than I; colleagues who read professional journals I had never read; colleagues who went to conferences that I had never attended. It was challenging and exciting and I was permanently changed by the

experience. I often felt as if I was hanging on by my finger tips and could fall at any time. I never felt “caught up” nor do I still. It was a very humbling experience. So at the same time that I was beginning my examination of abusive and neglectful families, trying to understand how parents could seriously and sometimes fatally harm their own children, I was also trying to understand the nature of individual development, individual differences, and my niche within HDFS. As part of this effort, Ted Huston, another social psychologist in the Department and I, organized a conference entitled Social Exchange in Developing Relationships. A guiding assumption of the Conference was that relationships have developmental trajectories just as individuals do. The Conference was a success and led to a 1979 book by the same title. The Conference and the book convinced me that I really did fit into this multi-disciplinary Department and College.

That aside, I still had a large research project to get under way where we were attempting something that had never been tried before, namely the observation of abusive and neglectful families in their own homes. I do mean families because we recorded verbal and physical interactions between and among all family members. That, too, was unique. We were able to do so by using the focal sampling techniques developed by primatologists. Each family member would periodically and randomly be the “focal subject” and we would record any and all contacts between that person and other family members. Little did I know as a graduate student how valuable my training in anthropology would eventually be.

It is sometimes said that all scientists stand on the shoulders of giants. There is much truth to that; we all are influenced by the training we receive and, hopefully, by the trials, errors, and successes of our own scientific experiences. Certainly that was the case for me. For example, I apparently still possessed some of the intellectual arrogance of my professors at Wash U. Why else would I, who had never even had a course in the family, think I could possibly contribute some original and significant ideas to a topic as important and perplexing as child abuse and neglect? More importantly, the approach I took to this difficult

task was in part a product of my training in experimental social psychology. This is seen most notably in my assumption that we need to distinguish between words and deeds more than we do. I felt then that far too much of behavioral and social science research was based upon the use of questionnaires and interviews and too little upon the actual observation of behavior (i.e., deeds). Similarly, my work in behavioral analysis led to my assumption that critical “process” variables (as opposed to “marker” variables like poverty, social class, or personality) would be found in interpersonal contingencies of reinforcement and punishment operating within families. I also assumed that these contingencies would be evident in day-to-day social interactions that transpired between parents and children and, given my anthropological training, I assumed further that these should be observed in the natural ecology of the families’ homes.

It was these patterns of interaction among all family members that were the focus of this study, which I called Project Interact, and that were examined in abusive, neglectful, and control families. The “control” families were matched on all relevant criteria to the abusive and neglectful families. These criteria were marker variables that had been found to be correlated with maltreatment in previous studies. An observational code was carefully designed and used to record who interacted with whom (verbally and physically); who was the initiator and who was the target; the emotional affect of the behavior; whether the interaction included a command and, if so, whether the command was followed by compliance or refusal. The intent was to test the hypothesis that there were patterns of day-to-day interaction that distinguished abusive and neglectful families from other families that were similar in all other respects but where neither abuse nor neglect had occurred.

Given these various assumptions and my principal hypothesis, it was necessary to determine the number of observational sessions that would be required to obtain a reliable and valid account of a family’s typical way of interacting with each other. To accomplish this, I constructed a computer program wherein simulated families were assigned different interaction profiles. These

simulations were used to determine the minimal number of times each family member should be designated the "focal subject" and, therefore, how many observational sessions there should be. Finally, it was decided that there should always be at least two observers independently recording family interactions so that we could continually assess observer reliability. These observations were recorded with special equipment designed by the Primate Lab at UW.

Apart from the issue of reliability, these observer teams were randomly rotated because of the possibility of "observer drift". To address this issue of observer accuracy, we videotaped several model families and developed a transcript of their interactions. Thereafter, project observers would at unexpected times every few weeks be required to score interactions from variable parts of the videotapes to test for observer drift. Whenever such "drift" was found, an observer would be given a refresher course in the code and re-tested.

Well, after all of this, what did we find? The results from these studies are found in various publications over the next several years. Briefly, when we compared family interactions in abusive and neglectful families with non-maltreating families living under similar circumstances, we found that there was a kind of basic training for mutually coercive exchanges within the maltreating families. These parents and their children were found to reciprocate each other's negative behavior more than their positive behavior, leading to increasingly aversive behavior, escalating counterattacks, and domestic guerilla warfare. This, in turn, contributed to the parent perceiving the child as troublesome and as a costly investment and eventually rejecting the child. This was found to be true for the neglectful families as well as the abusive families. In fact, the largest difference between the maltreating and non-maltreating families was that the former were less contingently positive to one another and they interacted with one another less often. To be honest this was somewhat surprising and I did not fully understand it until later. Overall, however, similar findings were reported by John Reid in 1984 in Oregon and, in Spain by M. Angeles

Cerezo in 1997. Fundamentally, then, the principal hypothesis of Project Interact was confirmed and replicated in other studies.

Following these initial results, an intervention program was designed, implemented and assessed. In general the program was promising in the sense that the relative frequency of contingently positive parental behavior could be increased and coercive behavior decreased. Nevertheless, the gains were hard-earned, modest, and difficult to sustain over time despite the fact that the intervention program also addressed the multiple environmental stresses that emerge in these families and that exacerbated conflict and disaffection between family members. Unfortunately, intervention programs targeting maltreating families continue to this day to have only modest effects.

In summary, the behaviorally-oriented research carried out by Project Interact clearly did add to our understanding of child maltreatment. However, a number of questions remained unanswered. For example, why is it that coercive interaction and parental rejection develop so easily in some families? Why is it so difficult to intervene successfully with maltreating parents? Given that the correlates of child maltreatment are multi-dimensional, involving both marker and process variables, how do we link them all together? Are these various correlates of equivalent explanatory power or are some more important? Is there a theory that can help us answer questions like these? I suggested that there is but it requires that we look at child maltreatment in a new and more comprehensive way. We must temporarily set aside the wide-angle lens of anthropology and sociology and the micro-lens of psychology and behavior-genetics and take up the telephoto-lens of evolutionary biology that has a depth of field that allows us to examine the significance of our evolutionary past for understanding the nature of human nature. Doing so, directs us to a branch of evolutionary biology termed behavioral ecology that examines linkages between ecological conditions and adaptive behavior, including parental investment. A key mid-level theory of behavioral ecology is termed life-history theory. In several publications, I have employed a modified version of life-history theory to explain

how personal, social, and ecological factors, as well as interpersonal contingencies of reinforcement and punishment, combine to produce the family dynamics culminating in child maltreatment.

I cannot go into all of the details of this theoretical model other than to draw your attention to several of its key assumptions and characteristics. (1) It draws upon research from several disciplines. (2) Because of this, it recognizes that, cross-culturally, the maltreatment of children has a long and inglorious past. (3) It acknowledges that while parental investment is biologically influenced and culturally universal, historically it has also been variable and contingent on a variety of factors. (4) It stipulates that the level of parental investment, high or low, is a function of the ratio of perceived benefits and costs. It is important to note that as costs increase, benefits do not necessarily decrease. The relationship is more complex and takes the shape of a sine curve. This can be seen in Figure 1.

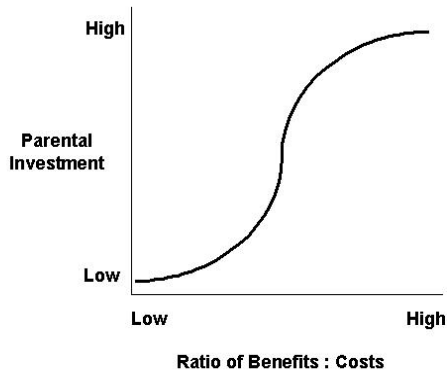


Figure 1. Ratio of Benefits-to-Costs and Parental Investment

(5) Because costs are likely to fluctuate and to do so in short spans of time, perceived costs are assumed to be more influential than perceived benefits in influencing levels of parental investment. This can be seen in Figure 2.

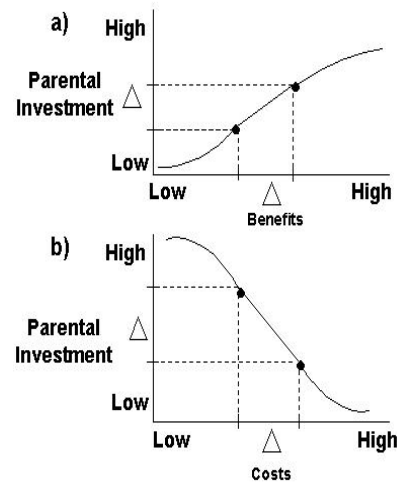


Figure 2. Parental Investment as a function of (a) benefits when costs are held constant and (b) costs when benefits are held constant. The conclusion to be drawn from (a) and (b) is that equivalent changes in the predictor (i.e.,  $\Delta B = \Delta C$ ) does not result in equivalent changes in the outcome (i.e.,  $\Delta PI_a \neq \Delta PI_b$ ). In fact, the change in Parental Investment given a change in benefits is less than the change in Parental Investment given an equivalent change in costs (i.e.,  $\Delta PI_a < \Delta PI_b$ .)

(6) It explains low-investment parenting and child maltreatment as products of a combination of contextual factors, individual and ecological, that impact parents' perceptions of the benefits and costs of parental investment. (7) It concludes that low investment parenting and child maltreatment are not invariantly linked because proximate antecedents, such as coercive interaction and poor family management practices, mediate the relationship between parental investment and child maltreatment. This can be seen in Figure 3.

I realize that all of this looks rather complicated. Human behavior is complicated. This does not mean, however, that everything is connected to everything else—a view that is hopelessly vacuous. It does mean that the complexities of human behavior can be made more tractable when viewed through the lens of empirically-derived theoretical principles. Evolutionary theory and the mid-level theories associated with it such as life history theory are singularly well-placed to accomplish this task. Why? Because it is the most general theory we have in the life sciences and, therefore, has the greatest potential to unify the behavioral and social sciences. Theories are important because science, as I noted earlier, is concerned not only with establishing

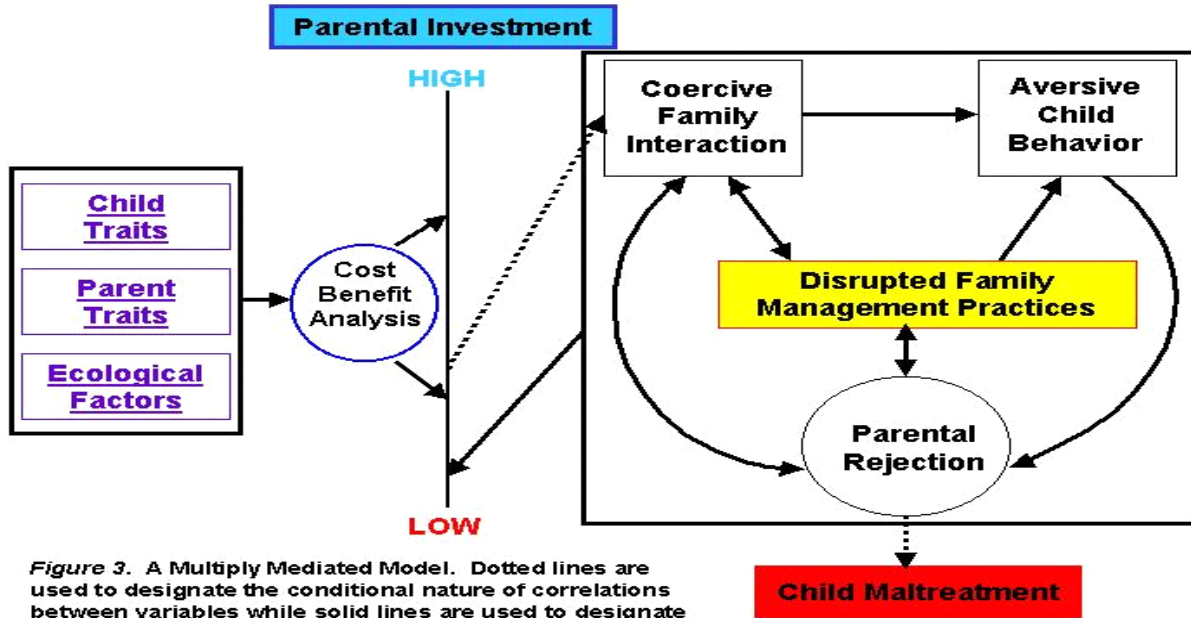


Figure 3. A Multiply Mediated Model. Dotted lines are used to designate the conditional nature of correlations between variables while solid lines are used to designate causal relationships

relationships between phenomena (empirical research) but also explaining why these relationships obtain. This is the primary function of general theories in science: to explain empirically established relationships. They add simplicity and parsimony to the understanding of how our complex world works. Simplicity and simplistic, however, are not synonymous. Even though general theories usually consist of a few general and simple principles, the derivation or deduction of complex phenomena from these general principles is seldom a simple matter. I realize that all of this looks rather complicated. Human behavior is complicated. This does not mean, however, that everything is connected to everything else—a view that is hopelessly vacuous. It does mean that the complexities of human behavior can be made more tractable when viewed through the lens of empirically-derived theoretical principles. Evolutionary theory and the mid-level theories associated with it such as life history theory are singularly well-placed to accomplish this task. Why? Because it is the most general theory we have in the life sciences and, therefore, has the greatest potential to unify the behavioral and social sciences. Theories are important because science, as I noted earlier, is concerned not only with establishing relationships between phenomena (empirical research) but also explaining why these relationships obtain. This is the primary

function of general theories in science: to explain empirically established relationships. They add simplicity and parsimony to the understanding of how our complex world works. Simplicity and simplistic, however, are not synonymous. Even though general theories usually consist of a few general and simple principles, the derivation or deduction of complex phenomena from these general principles is seldom a simple matter.

And, apart from its generality, there are other reasons why evolutionary theory can no longer be ignored by behavioral and social scientists. First, empirical support for the theory has been increasing at an ever-increasing rate. This empirical support has been accelerating ever since the breaking of the genetic code. Second, as a result of that breakthrough, we now possess what is essentially a molecular time-clock that allows us to accomplish such tasks as estimating the elapsed time since species split off from a common ancestor. Third, our genome is a sort of autobiography of our species recording when important events happened. It even permits reconstructing the history of human migrations in the last several thousand years. Fourth, it is the only scientific theory that has successfully explained pan-specific traits, i.e., those traits that are shared by all normal members of a species. In our case, these traits describe our common “human nature”.

A core assumption of my approach is recognition of the importance of different levels

of analysis—different levels of generality. This was most elegantly explained by the Nobel Laureate, Niko Tinbergen. As he pointed out in 1963: In the life-sciences, explanation always occurs on four complementary levels of analysis. I have recently written that these different levels reflect the fact that the various behavioral disciplines are divided less by the theories they employ than by the problems they address. These four levels include: (1) the evolutionary history of a trait; (2), its adaptive function, i.e., how the trait affects survivorship and reproductive success; (3) the development of the trait in an individual's life-span; and (4), the specific proximate mechanisms that cause a trait to be expressed at a particular time and place. At the level of theory, evolutionary history and adaptiveness are more general than developmental and proximate antecedents, yet a common thread runs through each of these analytical levels. The reason for this is that in each case, a genetic process must be involved. The development of a behavior must involve genetic action in some way and the potential or capacity to exhibit a behavior must have been adaptive at some point in historical time. It is important that we understand, then, that we are not dealing with a continuum from nature to nurture: both are always involved. For example, the environment affects development by switching genes on and off that allow for our remarkable plasticity and our ability to learn from experience. Therefore, it follows logically that developmental and proximate mechanisms can be deduced from (i.e., explained by) the first two and more general levels under what philosophers of science call empirically-specified "given conditions".

Recognizing evolutionary theory as the most general theory in the life-sciences does not lessen the significance of the allied disciplines of anthropology, economics, history, psychology, or sociology, nor their "middle-range" theories such as attachment theory, learning theory, exchange theory, or rational-choice theory, to name a few. The behavioral and social sciences have made many empirical discoveries, but the central intellectual problems of these fields are not analytic, i.e., discovering new and general theories. Rather, their problems are synthetic: showing how

genes and environments, in accordance with evolutionary principles, combine to produce our common human nature and the diversity of ways in which that nature is manifested. Each of the behavioral and social sciences and their middle-range theories contribute a piece of the puzzle in our attempt to understand the nature of human nature.

There is nothing too surprising here. The concept of the phenotype, as a product of genotypes, communicates the flexible and variable ways in which individuals respond to differing environmental circumstances and developmental experiences. The ability to adapt to different environments and to learn different things is a product of natural selection; hence, learning, development, and phenotypes depend on evolutionary history and principles. Tinbergen was right: There are four recurring and complementary levels of analysis. And, while it is certainly acceptable to restrict one's work to only one or the other of those levels, at some point, and I believe this to be an important point, Humpty Dumpty has to be put back together again. This I have tried to do in the courses that I have taught, and in a variety of articles and chapters, most extensively in my 2005 book with Kevin MacDonald of Cal State, Long Beach. This book is entitled, *Evolutionary Perspectives on Human Development*.

To conclude, as developmentalists there is much that we can learn from biographies and case studies. In this brief intellectual autobiography, I have tried to describe as best as I can how I got interested in the subjects I have worked on and what influenced me. I am quite aware that the picture I have painted has little in common with what the picture of an ideal scientist says it should be. This journey I have taken was often intensely personal and at times a matter of chance. As I mentioned earlier, I was profoundly influenced by the very first book I had read as a graduate student. The book was *Social Behavior: Its Elementary Forms*. The author was the Harvard sociologist, and former President of the American Sociological Association, George C. Homans. I had the privilege of George writing the Prologue to my first book. He opened our conference here at Penn State on Social Exchange in Developing Relationships and he

was the author of the Forward to the book that followed. I also had the privilege of being invited to his retirement ceremony at Harvard and contributing a chapter to a book in his honor. He was my intellectual mentor although I am confident that were he still alive, he would be dismayed at what I learned from him, which I am also quite sure is not exactly what he would have wanted me to get from him.

Whatever the case may be, I have in this account come full circle. I began this autobiographical sketch in Long Beach, California; I described my early and enduring interest in biology; my commitment to a multi-disciplinary approach to explaining human behavior and its development; and, why theory is so important in a science as complex as ours.

Finally, let me leave you HDFS faculty and students with your commitment to understanding life-span development, with the following thoughts. In the beginning, each of us arrived here because of a chance encounter between a particular sperm and a particular egg. Had it been a different sperm or a different egg, the result would have been different. This is pure biology. And as an antidote to the intellectual arrogance to which I referred earlier, it is important to remember the melancholy fact that, in the end, all victories are temporary. This, too, is biology.

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## **Book Review**

### **Materialist and Ecological Evolutionism: Some Thoughts on Sanderson's Evolutionism and Lenski's Ecological-Evolutionary Theory**

Melvin Barber  
Florida A & M University  
Florida Community College at Jacksonville  
(MBarber265@aol.com)

Reading Sanderson's book, *Evolutionism* (2007), was a real treat. I really liked his treatment of previous evolutionary theories; he provides pithy discussions of the works and contributions of previous theoretical attempts at evolutionary theory in the social sciences (primarily sociology and anthropology). After a very brief but informative discussion of the concept of evolutionism, Sanderson clarifies his conception of evolution and the aims of his book. He limits discussion to theories providing a general mechanism accounting for societal change and further limits his discussion by focusing of the theories and ignoring their historical contexts. He discusses the early theorists like Spencer, Morgan, and Tyler and uses them as the foundation for his discussion of evolution throughout his book. However, I was disappointed when he discussed Spencer and did not really emphasize how he was the formulator of the idea of the struggle for existence and the survival of the fittest (together composing the theory of natural selection). He also misses the point that Spencer made by demonstrating that natural selection was the primary mechanism in the social world when he emphasized the importance of laissez-faire. Experimentally, laissez-faire allows for the empirical observation of natural selection. Nevertheless, as Sanderson observes, the early theorists had a fatal flaw in their theories, which led to the eventual abandonment of their approach; they saw evolution as a progressive process. I believe the early theorists (especially Spencer) correctly identified a key

component in the development of evolutionary theory; they identified the dynamic process of natural selection.

The discussion of Marx and Marxism is also excellent. I disagree in the classification of Marx as an evolutionary theorist, but he was important in bringing forward the early theoretical tradition, especially in identifying the contributions of Morgan. Together with Engels, Marx sketched out a theory of the structure of society and found a novel way to apply the tradition of dynamics from the early theories. There is one important issue I have with that presentation; Sanderson fails to identify the primary contribution Marx made to theory. Marx provides the best theory of structure that social science has to offer. And while most sociologists emphasize Marx's contribution to the theory of social change, Marx's dynamics are really not as good as his theory of society. Marx emphasizes the importance of exploitation and when you observe peoples where exploitation is practiced, Marx's theory provides vivid descriptions of what occurs. I do not believe Marx's and Engels' conception of the development of society is especially useful for theory. However, I do agree that Marx's conception of the different social formations present a challenge to evolutionary theory. Sanderson is quite thorough in his discussion of the various confluent streams of evolutionary thought. Only a few can be mentioned here and the bulk of the review will concentrate on some of the most relevant recent contributions to evolutionary thought. The discussion of classical evolutionary thought demonstrates the importance of the early theorists, especially Spencer. The research efforts of Sumner and Keller reflect the normal scientific approach to Spencerian theory and Westermarck extends the work of Morgan and Tyler. While Sanderson sees the continuity of the work of Sumner and Keller with the later work of Murdock, he apparently does not realize that Spencer also developed the first Ethnographic Atlas. Sanderson provides excellent discussions of Leslie White and Julian Stewart. For many sociologists, consideration of their work is very important. For completeness, Sanderson even considers the work of Parsons as an evolutionary sidebar. I will discuss the rest of the book in light of a discussion of

Lenski, which provides the backdrop for many present-day evolutionists and for Sanderson.

I read Sanderson's discussions of theories with great anticipation because I saw so many similarities between his view of theories and my own. Sanderson emphasizes what he refers to as theoretical research strategies, an idea about theories derived from the works of Imre Lakatos (1970) and Marvin Harris (1979), both of whom have good understandings of the use of theory in science. The disappointment I experienced was profound when I realized that despite this common background, Sanderson has a radically different view of scientific theory than I do. I kept looking for the identification of the fundamental problems of the science and the formulation of the fundamental concepts and principles, which solve those problems, to no avail. I also sought the identification of some simple mechanism that would explain the evolution Sanderson asserts is existent in the social world.

In my view, if you come up with more statements attempting to solve the problem than the statements required to state the problem, the attempt at theory has failed. Sanderson discusses societies, and it is apparent that societies are an important part of his evolutionary theory, but after reading *Evolutionism*, I haven't the faintest clue about what societies are, unless they are identified with what we all call nation-states. And if they are identified with nation-states, how are those groups we also acknowledge as existing prior to and without states, related to those nation-states? In addition, Sanderson relies heavily on Lenski's (2005) typology of societies as a demonstration of evolution, but it seems to me, that Lenski's typology is really a typology of peoples and cultures rather than societies. His focus is cultural development, not a theory of society. And it is really difficult to identify the natural things and forces in Lenski's theory. Lenski emphasizes the importance of technology, environment, and population without demonstrating how these things determine the type of society. He also throws in energy consumption, which is inert and passive with respect to his theory; it is not used as a causal factor in the development of culture. What he does is identify specific technologies with specific societal types, but he

fails to demonstrate that those traits are clearly the results of identifiable forces. At most, the types of societies serve as ideal types to help us observe the differences among peoples.

It is interesting that Lenski goes through his entire book on ecological-evolutionary theory without drawing on the extensive work of ecologists. His model of ecological change provides little to solve problems of the theory as it might apply in the social world. In fact, he fails to utilize the model of natural selection that has almost a hundred years of research behind it. Instead he substitutes an equation [economic surplus = (resources)(technology)(capital)/population] that fails to capture the essential details of the relationship among population size, population density, technology, etc. What do all these terms mean? How are they related to one another? He sees population as a determinant of change, when, in ecology, population is seen as the dependent variable. The function  $dN/dt = rN(1-N/K)$  provides a better description of the process of evolutionary change. What this describes is the instantaneous growth in population ( $dN/dt$ ), which is directly seen as a function of  $r$ ,  $N$ , and  $K$ .  $K$  could be a measure of technological development (the carrying capacity of the land).  $N$  is population size, and  $r$  is rate of population growth. It can be demonstrated mathematically that the carrying capacity of the land is sensitive to the level of development of technology. The greater is the development of technology, the higher the carrying capacity of the land, and the greater the population. As technology increases the carrying capacity of the land increases. The result over time is an increase in the carrying capacity of the land and a larger population. This equation can be used to examine all the developments Lenski discusses in his book. It is incomprehensible why ecological theory has not been used. There is much more that can be said in this respect.

If we use the ecological principle, population growth is more of a result of technological development than the cause of that development. The issue then becomes, what is the cause of technological development if it is NOT population? At the end of the book, Lenski (2005) sets out criteria for the evaluation of his theory and

concludes that his ecological-evolutionary theory is an effective theory. The list of criteria is woefully incomplete; they are:

1. The theory provides a comprehensive and coherent account of human societies and their development.
2. The theory is testable.
3. The theory is correctible.
4. The theory generates research and explains anomalies from previous theories.

Lenski claims that his theory meets the set of criteria and that all theories must meet them. He indicates that in his formulation of the theory of evolutionary development of society and in the case studies he has presented with it he has provided sufficient proof of the adequacy of his theory. However, there are several criteria that scientific theories must meet that are not met by his theory, which may account for the shortcomings of Lenski's work.

A key criterion overlooked by Lenski is whether or not the theory identifies the causal mechanism for the science and can make empirical predictions using that mechanism. It is in this way that a scientific theory is testable. The formulations of the second and fourth rules are insufficient. This is not the place for detailed examination of the criteria for the evaluation of theories, but it is important to demonstrate the problem in Lenski's approach to theory. The theories at their most basic and fundamental level should survive systematic testing and application over an extensive period and be applied to an extensive range of phenomena. The theories must describe and connect some mechanism or mechanisms that connect facts that have been discovered in the world of experience. This criterion is separate from but closely aligned with Lenski's second criterion of testability and his fourth criterion of research fertility. If the theory is indeed revolutionary, it will spawn the research. In this respect, while numerous theorists and researchers use Lenski's theory in most cases the researchers are not testing his theory, but rather assume it is correct. This is important because no new propositions and experiments are being pursued using his theory, instead the theory is being used in precisely the same way Lenski used it and that is to "see" phenomena, not explain it.

Sanderson along with a host of others, Cf. Turner (2000), Maryanski and Turner (1992), etc. seems to follow in Lenski's path. I agree with Sanderson in his criticism that Lenski does not provide a compelling theory of technological change, but of course, he is only obligated to demonstrate that technological change is occurring in relation to other variables. The core problem is how the changes occur in the entire system. That is, what is the mechanism is causing these changes? Somehow, in Lenski's theory, all of these things are connected. It is the task of theory to demonstrate HOW they are connected, but that often involves identifying what is fundamental in the natural world being described.

A second difficulty Sanderson finds with Lenski I do not share. Sanderson argues that Lenski claims that both social evolution and biological evolution are very similar in that they both involve increasing information. I feel that Lenski does put forward this idea, but he fails to exploit the idea. The really innovative part of Lenski's theory is seeing cultural information as an important mechanism. It is easy to see how various cultural systems are selected by natural selection. From Lenski's discussion, it is actually easier to see those developments than it is to see the "progress" of social development. It must be remembered that any "decisions" that we humans make with respect to the world around us are subject to the forces of natural selection. And of course, all organisms are subject to that same constraint. Sanderson's criticism that social information is different from biological information is neutered by the observation that peoples are differentiated from one another based on the informational systems they employ. Therefore, the developments seen in what is called societies are due to changes in information systems. I agree with Sanderson's assessment of Lenski's view of long-term social evolution and other factors such as organization and ideology. I think the biggest problem with Lenski is his fusion of culture and society. His categorization of "societies" is a categorization of "peoples and cultures" not societies. He IS describing what he is able to observe, but what is needed is an explanation of those observable facts. At least Lenski

realizes that what he is doing is developing ideal types that can be used to describe observable things; however, Lenski fails to see that his ideal types hamper his ability to explain what he is observing. The unfortunate side effect of this is that accepting Lenski's categorization of phenomena gives one the sense of having explained them. It is most important to realize that description is NOT explanation. So, Lenski's categorizing societies is not an explanation of evolutionary development; it is a description of peoples and populations in terms of their technological development (culture).

Sanderson adopts Lenski's theory of societal development with all of its imperfections. In Sanderson's theory it is unclear what is being explained. In his initial description of the theoretical strategy he says that there are three great evolutionary transformations: 1) Neolithic, 2) Agrarian, and 3) Capitalism. In my view, this approach tends to fuse social and cultural evolution. It also tends to fuse description with explanation. It is true that the transformation brought about by the use of stone tools was revolutionary. It is also true that the appearance of agriculture was revolutionary. And finally, it is true that the transformations brought about in Europe is due to the emergence of a new form of capitalism, but it is yet to be demonstrated that the changes brought about by these revolutions are evolutionary and it is equally yet to be demonstrated that these are social transformations. I find in these great transformations, at most, an evolution of culture, but social life does not appear to evolve. At least there is no demonstration in Sanderson's work of how social life evolves. This is an approach reminiscent of the culturological approach advocated by Leslie White and Marvin Harris. The chief limitation of the culturological approach is that no mechanism is described that accurately accounts for the observed phenomena. Evolutionary thinkers from Spencer to Carneiro share elements of this approach. We can describe the development of culture phenomenologically, but we have to resort to different kinds of thinking to explain those developments.

I am not convinced by Evolutionism that social life or society has developed or evolved. I can observe changes in the social milieu within which societies reproduce and die. I also observe other forms resembling societies which I cannot demonstrate are societies. Some of these forms are larger and more complex than societies, but societies change the milieu in which they grow. Societies appear to combine and form different structures, but I find no convincing demonstration of how they do combine. What we observe as nation-states are not necessarily societies. As we can see via Lenski, we describe observable things, but what we need is a non-observable thing like a gene or an atom. So, I am not convinced that we observe the evolution and development of societies when we observe people utilizing hunting and gathering to post-industrial (information technological) nation-states. Perhaps what is happening is that there are new species of peoples or cultures developing. Very clearly, there are some species of people that are predatory or parasitic, there are a huge number of new and emerging organizations, there are also all sorts of pre-societal phenomena like social movements, crowds, new religions, gangs, etc. All of these phenomena appear to be explainable by the principles of natural selection, but the evolution of society is not demonstrated.

I have another issue. Just because selection works at the level of the individual organism in biology does not mean that selection in society does. The major point in the social theory is that society is the causal mechanism. As sociologists we must start with that fundamental outlook (that is, the theory that society is the causal mechanism for all phenomena we attribute a social character). It may be that that fundamental theory is in error, but we must pursue that theory until it is clear that there truly is a better way to explain the phenomena we observe.

We aren't observing animal populations. We are observing the products of the behavior of humans (culture). We are exploring the theory that human behavior and its products (culture) are caused by social forces (society). Therefore, the theory that human behaviors are best explained at the individual level is not only

counter productive, it is antithetical to the fundamental sociological theoretical outlook. In my view, a natural thing does not change its nature, but through its interactions with other things, the way in which its nature is expressed may appear to be different from the nature and traits identified with it. In reality, the nature of the natural thing remains the same.

For example, when we observe material things in nature, after intensive observation we have concluded that material things on earth can exist in three states: solid, liquid, and gases. Water is especially instructive in this regard. When we see water in the frozen state it is a solid. Water has not changed its nature, but the condition under which it exists has changed. When we see water in a liquid state we usually assume that that is its natural state, but that is its natural state within specific conditions (those conditions usually found in most places on earth). Water is still water in either its state of being ice or its state of being a liquid. When water is heated to a specific temperature and evaporates, or, under certain other conditions, it becomes molecules of gas (water vapor), but it is still water. It has not been separated into its component elements. When the conditions exist, it will become liquid again.

To assert that society is a natural thing implies that its nature always remains the same. The key thing for scientific investigation is to describe that nature and determine how that nature is reflected in the things it interacts with and to observe the responses of that natural thing to those interactions.

Following Darwin's astute observations, the argument of biological evolution is that organisms have certain traits. These organisms, observed as things in nature, remain unchanged, but different organisms having slightly different traits combine with them in some way to produce new organisms. This problem was the problem Darwin sought to solve with his theory of the origins of species. The organisms that are combined do not change (the principle of natural selection), but because the new organisms have traits that are better adapted to the environment they inhabit, the new organisms (with the new traits) more rapidly reproduce than the organisms without the traits. Over time, the new

organisms replace the contributing organisms and the older contributing organisms frequently become extinct. It is important to see that this is not necessarily assuming gradualism as many sociologists see and as Sanderson also observes, on the contrary, it is a process that can also take place rapidly.

It has been discovered in biology that the organisms may not change, but they are composed of genes, which do mix with other genes in predictable ways. Genetics have been demonstrated to be the organizing force in organisms. We also find that in the process of reproduction genes do change or mutate. At the present time we are unable to determine why genes change or what is responsible for those changes (we now assume they are random changes), but we are able to utilize our knowledge of the nature of genes to make genetic changes.

The key thing is that evolution in biology uses two fundamental mechanisms: natural selection (which Spencer and Darwin both described well and Darwin demonstrated how natural selection IS responsible for the origins of species) and genetics, which existed as a dormant mechanism (black box) for almost 100 years. Mendel demonstrated the importance of genetics but it was still primarily a black box about which little was known until Watson and Crick opened that box and demonstrated how genes actually work. When that occurred a huge number of problems were solved. It is important to take that same approach to the social world. After Darwin biology was content with taxonomy and the discovery of new species. Watson and Crick showed the way to understanding the actual relationship among all the different taxa around the world.

The point is that there is a single process of evolution. There is not one evolution for biology, another evolution for sociology, and still another evolution for physics and chemistry. It must be realized that there are two essential components of evolutionary theory in biology: natural selection and genetics. It appears that natural selection is operative in the social world and genetics are important to biologists. However, there is another class of facts that we have discovered in the social world that are not directly biological. These facts have been postulated

to be the results of something above and beyond the organisms we are familiar with in biology; they are made by groups of organisms. If evolution applies to the social world, then we must demonstrate how natural selection operates in the social (super-organic) world. It appears to be self evident that natural selection (the dynamic principle in evolutionary theory) is operative in the social world. However, a different structural mechanism above the organism level is responsible for the facts observed in the social world. If social evolution is occurring, sociologists have the burden of demonstrating precisely how that evolution has occurred. The key things to be demonstrated are as follows: What is evolving? What is responsible for the traits we observe? And what is responsible for any new traits that appear to have come about because of the emergence of the new organisms? For the scientific examination of the social world, culture, in a very restricted sense of observable facts about what people do, is the primary focus of research and theory. A description of the culture of a people provides a description of the traits we observe. However, the traits we observe are present because of a different mechanism from the biological mechanism. Culture traits are not due to the natural selection of genes or of organisms, but rather they appear to be due to some super-organic mechanism. Sociology has formulated various theories of the super-organic mechanisms (theories of society) but the science has not been able to resolve the problems within and among the theories. Evolutionary theory provides a basic strategy for solving these problems.

The changes occurring in human organisms appear to be changes that are super-organic rather than organic changes. From anthropological studies it has been concluded that *Homo sapiens* has not changed in biologically significant ways for the last 100,000 to 200,000 years.

We have also observed that there have been significant changes in the use of objects we assume are super-organic in nature. This use of super-organic objects has a longer history than *Homo sapiens* has, so it is safe to assume that the development of the use of these objects by humans is subject to the

pressures of evolutionary forces. How that all takes place is perhaps the object of social evolutionary theory. Anthropologists have been systematically recording the evolutionary record of developments in this area from the time of the use of the first stone tools up to the present use of "sophisticated" science-based technology. The key thing to be observed is that technology is a natural phenomenon that has been heavily selected by natural selection. What we find in evolutionary theory for biology is that Darwin describes the fundamental dynamics of living things without a full comprehension of their fundamental structure. Mendel provides the basis for describing the fundamental structure, but that structure was not fully described until Watson and Crick unraveled the structure of DNA; we are still responding to the implications of that theory. The fundamental dynamics that Darwin expressed for living things appear to be the same in the case of social life, but the fundamental structure is different.

As a result, what we have in sociology is still the same fundamental problem of describing the fundamental structure of social life. The description of that fundamental structure should provide a basis for an authentic (genuine) evolutionary theory. It has been the thesis of sociology that the fundamental structure is society. From a theoretical point of view it is important to describe society in terms of which we know it needs to be; we must describe its nature. The exciting thing about the evolutionary approach to sociology as I see it is that it provides two clear directions for the expansion of sociological theory. In the area of dynamics, it is necessary to demonstrate the effectiveness of the theory of natural selection to describe and explain the changes in culture that we can observe. This project takes the form of normal science in Kuhn's (1970) view. In the area of statics it opens up the problem of describing how the facts we observe (culture in a very restricted sense) are the results of the fundamental structure of society (revolutionary theory in Kuhn's view). What this means is that culture is a result and not a determining or causal factor (this includes technology. This broad layout of the research strategy provides a basis for generating cumulative theory at

both the normal science level as well as the revolutionary science level. There are several areas theory that have already been heavily developed as ecological theory; the Lotka-Volterra formulation of the dynamics of populations, the Pearl-Verhulst formulation of population growth, and the abundant work on species development in theoretical biology. All of these areas of work have direct applicability to sociology. However, there is a clear need for the development of a scientific theory of society. This is the area where classical sociological theory has contributed much. The development of this area of theory in line with the application of mathematical ecology demonstrates a horizon where sociological theory can significantly contribute to the expansive project of the growth of science. Sanderson's book opens up the possibility

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## New Publications of Section Members

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## People

**Paul Kamolnick** (East Tennessee State University is currently serving on the newly-created ASA Task Force on Academic Freedom and Scientific Integrity which is scheduled to continue until a report is presented to Council summer 2008. This TF is charged with developing data gathering strategies for ASA to consider in its efforts to determine whether academic freedom is currently being threatened in sociology. It is also charged with gathering data from academic sociologists on their perceptions of academic freedom and potential experiences with its violation. Paul Kamolnick also applied for and received a generous grant from the Pioneer Fund in 2007 to underwrite full-time writing of two books on race during the summers of 2007 and 2008. The books--Race Beyond Taboo: toward a new national compact, and, The New Scarlet Letter: how the politics of cultural determinism corrupts the science of race in academe--are presently in progress with a goal of publishing Race Beyond Taboo during the centennial year of the founding of the NAACP (1909-2009), and the New Scarlet Letter sometime thereafter.

**Murray Milner** (University of Virginia) is currently on a Fulbright Fellowship in India.

**Kent Schwirian's** (The Ohio State University) research on Somali refugees was recently featured in a three-part TV report on refugee assimilation produced by NBC-TV 4, Columbus, Indonesian-TV, and Ohio University.

*The Biosociology of Dominance and Deference*

Rowman and Littlefield will send free exam copies of the book by Allan Mazur, *The Biosociology of Dominance and Deference*, to everyone who requests one for possible class use.

Requests for exam copies (for professors considering adopting the book) go to Renee Legatt in Rowman & Littlefield's college marketing department. Her email address is rlegatt@rowman.com.

*Social Evolutionism and its Critics: Deconstructing and Reconstructing an Evolutionary Interpretation of Human Society*

Section members may qualify for a free exam copy of Steve Sanderson's new book, *Social Evolutionism and its Critics: Deconstructing and Reconstructing an Evolutionary Interpretation of Human Society* by sending relevant course title, expected number of students, and semester/quarter to be taught to Patriciag@paradigmpublishers.com

*Criminology: An Interdisciplinary Approach*

Section members who are teaching criminology and who want to emphasize a biosocial approach, including evolutionary arguments about crime causation, may request a review copy of Anthony Walsh & Lee Ellis (2007). *Criminology: An Interdisciplinary Approach*. Email: Jennifer.Reed@sagepub.com

### **CONSTRUCTAL THEORY OF SOCIAL DYNAMICS**

A. Bejan and G. W. Merx, editors

This book brings together for the first time social scientists and engineers who present predictive theory of social organization, as a conglomerate of mating flows that morph in time to flow more easily (people, goods, money, energy, information). These flows have objectives (e.g., minimization of effort, travel time, cost), and the objectives clash with global constraints (space, time, resources). The result is organization (flow architecture) derived from one principle of configuration evolution in time: for a flow system to persist in time, its configuration must morph such that it provides easier access to its streams.

This book is based on two NSF-sponsored international conferences held at Duke University in April 2006 and 2007, and organized in collaboration with three Duke professors of sociology: Edward Tiryakian, Kenneth Land and Gilbert Merx. The objective is to develop a community of scholars around the idea that the evolution of design can be put on the basis of a physics principle, which applies across the board, from geophysics to biology and human social dynamics.

Publisher: Springer-Verlag, 2007.

### **SEX DIFFERENCES Summarizing More than a Century of Scientific Research**

Lee Ellis, Scott Hershberger, Evelyn Field, Scott Wersinger, Sergio Pellis, David Geary, Craig Palmer, Katharine Hoyenga, Amir Hetsroni, and Kazmer Karadi

This book is the first to aim at summarizing all of the scientific literature published so far regarding male-female differences (and similarities).

No exclusions were made in terms of subject areas, cultures, time periods, or even species.

Results from over 22,000 studies are summarized within approximately 3,000 tables, with each table pertaining to a specific possible sex difference. The book's length is 1,019 (+14) pages plus a CD insert containing a 650 page file of references to all of the studies cited throughout the book.

Publisher: Lawrence Erlbaum Associates (Taylor and Francis), 2007.

For pre-publication orders:

<http://www.psypress.com/9780805859591>