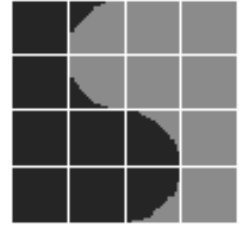




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Replications and Genetic Studies of Behavior

Michael Shanahan & Shawn Bauldry, UNC-
Chapel Hill
Ross Macmillan, University of Minnesota

The behavioral geneticist Lydon Eaves is fond of noting that Saint Augustine was among the first to make genetically-informed observations when, in arguing against the validity of horoscopes, he pointed out how very different twins could be. Fast-forward 1,600 years from the Bishop of Hippo to the publication of two papers in Science by Avshalom Caspi, Terri Moffitt, and their

colleagues in 2002 and 2003. Both papers were empirical studies that focused on genetic polymorphisms and their interactions with social experiences in the prediction of psychiatric outcomes. And both papers have put the issue of replication in the scientific spotlight.

The first such study (the “MAOA study”) reported that a genetic variant associated with low MAOA activity coupled with child maltreatment was associated with antisocial behavior in young adulthood. A 2006 meta-analysis by Kim-Cohen and her colleagues concluded that, across five extant studies, the interactive effect held. The original MAOA study (cited over 1,800 times to date) and the meta-analysis have spawned a cottage industry of discussion and attempted replications, with some scholars presently concluding that results are “mixed” (an undesirable descriptor to which we return below).

The Caspi team’s second Science paper (the “5HTT paper”) reported an interaction between the serotonin transporter linked polymorphic region (5-HTTLPR) and stressful life events in the prediction of major depression in young adulthood. If the MAOA paper spawned a cottage industry, the 5HTT paper gave rise to a veritable rust belt of heavy industry, with the original paper now cited almost 3,000 times. A highly influential meta-analysis appearing in *Journal of the American Medical Association* concluded that, across 14 reviewed studies, no such interaction was replicated (Risch et al. 2008; see also Munafo et al., 2009 for a similar meta-analysis and conclusion). Numerous deficiencies in the meta-analyses were noted (see Letters in *JAMA*, November 4, 2009, for some examples) and a qualitative assessment of empirical studies published at about the same time as Risch concluded that replication results were patterned by the measure of life-events, with semi-structured interviews being associated with rejection of the null (Uher & McGuffin, 2008). More recently, a review concludes that the interaction does indeed replicate across studies using objective measures of adversity among females, but the effect size attenuates

when self-reports are used (Uher & McGuffin, 2010; see also Caspi et al., 2010, for a recent discussion of supporting evidence with non-human primates).

So things stand at this writing. And, quite understandably, a major concern in this area of research is now replication, the reproducibility of a study’s methods and findings. Yet some basic lessons about replications have been forgotten and, with these lessons in mind, replication can proceed with a more constructive sense for building scientific knowledge bases and theories.

At the outset, it is noteworthy that replication is not a hot topic beyond gene candidate research. Turn to any issue of *AJS* or *ASR* in sociology and ask yourself how many studies constitute replications or will generate replications or will require replications to be validated. Although there are certainly areas of research that have been heavily replicated in the behavioral sciences, explicit concerns for replication in sociology are minimal. This unevenness may be thought “unfair” given the extensive attention to replication in gene-environment research, but it actually works to the disadvantage of subfields requiring little replication. In the long run, such fields cannot generate a cumulative body of knowledge.

Historians of science hold that replications were a major breakthrough for the scientific method because they required that scientists who were largely isolated from one another clearly communicate their procedures and findings. If Robert Boyle could confirm the inverse relationship between an ideal gas’s pressure and volume—and his work was then replicated across Western Europe—then something indeed had been accomplished (Daston, 1994). But herein lies the first problem with replications: the concept of the replication was originally developed in the physical sciences where essentially exact replications were necessary. Indeed, most “facts” in the physical sciences rest on hundreds of nearly identical replications, sometimes performed purposively, and sometimes performed as the

first step in an elaboration of the original research.

The behavioral sciences simply have not proceeded in this manner (for reasons discussed by Lindsay and Ehrenberg [1993]); at best, replications are typically “incidental” in the sense that data collected for one purpose prove reasonably suitable to replicate a finding from a different dataset. As Lindsay and Ehrenberg note, this incidental quality is both good and bad. The good part: the ultimate goals of replications are (1) the validation of a finding and (2) a determination of the range of conditions in which a finding holds true. Incidental replications typically bear on the second purpose, which they view as more important than the first purpose. They are “differentiated” in their methodologies from the original study and the resulting pattern of findings establishes scope conditions to the original finding (hopefully). However, they also note that replications should ideally begin with the first purpose in mind: to validate the finding with “close” replications, ones that are very similar, methodologically (i.e., sampling, measures, research design) to the original study.

In other words, in a perfect world, a finding of interest is followed by close replications and then, if validated, by increasingly differentiated replications. The first stage of close replications is an exercise in cost-effectiveness—why bother with differentiated replications if the basic finding is very likely untrue? However, what is a close replication, particularly in gene-environment studies? Does a close replication necessarily refer to the same population but a different sample? Must it use the same measures? The issue of measurement is especially vexing because the psychometric properties of many social measures are not well understood. The MAOA study’s measure of maltreatment was based on prospectively collected data from multiple sources in New Zealand describing experiences before the age of five. Is a replication based on an American sample and retrospective self-reports of childhood abuse before age 15 more a “close” or “differentiated” replication? What about a study using a

measure of childhood maltreatment that shares no common items with the original MAOA study? And so on.

The second stage in the replication process is where life gets yet more interesting because differentiated replications help to establish scope conditions to the original finding; further validate the initial finding by establishing convergent validity; and strengthen confidence in the original finding by eliminating potential biases due to unobserved heterogeneity (Rosenbaum, 2001). Through the second stage, perhaps some replications will reject the null and some will not and, ideally, patterns among the replications will reveal the conditions under which a gene-environment effect is valid. The second stage is not without its challenges, however. Foremost, because most (virtually all) differentiated replications are incidental, they vary many methodological features of the original study simultaneously. If the null hypothesis is not rejected (i.e., a genetic finding is not replicated), it may be unclear which features of the incidental replication account for this failure to reject. Or perhaps the original finding is a false positive.

The Caspi 5HTT-Risch-Uher sequence can be read as a jumbled variant of this perfect world scenario. The 5HTT study was followed by many replications that, not ideally, fell across the continuum from close to differentiated replications but were decidedly toward the latter end of the continuum. The Risch meta-analysis revealed that across all of these studies, the interaction did not hold. However Uher showed that the interaction is replicable among females when objective measures of adversity are used. Our own review also suggests that the interaction holds among clinical samples with high levels of stressors (Shanahan and Bauldry, in press). Thus, the 5HTT interaction is no law of gravity. But it likely holds in some nontrivial circumstances. Beyond the 5HTT and MAOA studies, there is a lesson for future replications of any gene-environment study: just as no one study is decisive, no one replication is decisive. Rather it is the corpus of multiple replications that may or may not validate and establish scope conditions for an initial finding.

A second lesson also emerges, seemingly obvious but rarely discussed: reviews of replications typically ignore statistical power. As Ottenbacher (1996) notes, replications must be interpreted in terms of effect size, sample size, and Type II error rates. Although he is largely concerned with small samples that are characteristic of experiments, his point holds true for gene-environment studies where effect sizes (think Cohen's d) are thought to be small. So, assuming an association between x and y is non-zero, these factors set an a priori probability that a null will be rejected in any given study, assuming there is indeed a significant association. In fact, our recent simulation study, focused on 5HTT, and many other epidemiological studies suggest that the effect sizes of gene-environment interactions are fairly small and that studies to date have been—with few exceptions—notably underpowered. In other words, there was no reasonable expectation that a sizable proportion of extant replications of MAOA or 5HTT would have rejected the null as a matter of power alone. In this sense, many replications have been decidedly unfair—why would one expect a finding to replicate with an underpowered sample?

The average taste of 20 apples simply has no bearing on the taste of an orange.

One implication is that reviews of existing studies that tally up “significant” and “non-significant” findings are naïve. The results of replications are not “yes” or “no” phenomena but rather a matter of degree. Meta-analyses reflect this fact, with their characteristic interest in the consistency of effect size across samples. Yet they typically fail to take into account the diverse methods used across the studies. Particularly when replications are largely differentiated from the original study, the probative value of a meta-analysis diminishes. The average taste of 20 apples simply has no bearing on the taste of an orange. Thus, both well done meta-analyses and qualitative assessments of extant studies

are necessary, with the latter being especially salient when incidental replications are the rule.

The foregoing suggests that statements such as “the finding didn't replicate, casting serious doubt on the original study,” “results across the studies are mixed,” “most studies fail to replicate the original study” and so on—which are fairly ubiquitous in journals and the hallways of professional meetings—are, by themselves, overly simplifying matters. Replications should not be construed in terms of a dichotomous outcome (did reject null; did not reject null) but rather in terms of their many complexities, foremost being power, effect size, Type II error rates, measures, samples and populations, designs.

If the messiness of gene-environment replications can be disheartening at times, they are also exciting opportunities to validate or not, and to learn about scope conditions. But patience is a virtue—solid conclusions require multiple replications that differ on the continuum of close to differentiated.

The upshot is, then, that (1) replications should be interpreted in terms of their methodological closeness or differentiation from the original study; (2) close replications make sense early on as necessary validations of the original findings; (3) numerous differentiated replications can establish scope conditions; ideally they systematically vary aspects of the original study; (4) however close or differentiated, replications should not be interpreted in dichotomous terms (failed to reject null; rejected null) but rather in terms of their power, effect size, and Type II error rate and measures.

Gene-environment researchers should take heart in the fact that it is not merely their subfield that often finds itself entangled in seemingly conflicting empirical studies. Consider the saga of cold fusion. Fleishman and Pons originally reported “desktop” fusion in 1989 and after untold millions of dollars and countless lab hours, a 2004 Department of Energy panel of experts was split such that two-thirds of the experts did not believe that cold fusion was an established fact, one

panelist was entirely convinced that it existed, and the remainder of experts was partially convinced. Similar stories could be told with respect to so-called endocrine disruptors such as BPA in our plastics, many prescription medications, and so on. Particularly when science matters—when findings can shape our daily lives, promote or detract from our well-being, make us more or less productive—replication is critical.

The good news is that gene-environment researchers worry about replication. Especially in many subfields of sociology, replication has not been a major concern and thus the accumulation of the knowledge base is greatly hindered. To paraphrase Nietzsche, if the replications don't kill us, they will make us stronger.

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Are Evolutionary Theory and Rational Choice Theory Compatible?

Richard Hutchinson, Kennesaw State University

(Talk presented 8/15/10 at the E,B & S Invited Session: "Evolutionary Sociology and Rational Choice: Friends or Opponents?" at the 105th ASA Annual Meeting)

As sociologists we need to be clear when we talk about evolutionary theory whether we mean biological evolutionary theory as applied to humans, or sociocultural evolutionary theory. So let me begin with the question of whether biological evolutionary theory is compatible with rational choice theory (RCT). It is central to biological evolution that individuals are self-interested. They aim to survive and reproduce. Selection takes place primarily at the level of the individual organism. Since self-interest is also the central principle of RCT, then it would seem that biological evolutionary theory and rational choice theory are compatible on a very basic level. However, as it has been developed in utilitarianism and economics, and more recently adopted by political scientists, sociologists and anthropologists, RCT has tended to make "heroic claims" (Macy 2006) about perfect information and maximizing that are clearly not compatible with an up-to-date biological understanding of the human brain. If we call this neoclassical economics variant of RCT "strong RCT," then we can focus on a radically relaxed version of RCT which has been called "weak RCT" and say that biological evolutionary theory is compatible with weak RCT.

A basic problem for the compatibility of biological evolutionary theory and RCT is the realization, based on brain research, that emotion dominates rationality in humans. The mammalian, emotional brain (the limbic system) is far larger and more important than the recently evolved human prefrontal neocortex (Massey 2005). The basic RCT

theoretical framework is cognitive, not emotional, and so it is based on what is in an important sense a pre-scientific view of the brain, cognition and rationality. Neoclassical economics (NCE), the most fully developed and influential variant of strong RCT, makes completely unrealistic assumptions about human information processing, assuming maximizing based on perfect information, that are not supported by empirical research. In recent years the field of behavioral economics has begun to carry out experiments in order to develop a more realistic understanding of human economic behavior than the NCE equations. Herbert Simon was a pioneer of this field with his theory of "satisficing" instead of maximizing in decision-making, and more generally, *bounded rationality* (Simon 1947). Prospect theory recognizes that people tend to be risk averse, and therefore value what they already have more than what they might gain in the future (cited in Macy 2006). Many trials of The Ultimatum Game show that culturally conditioned norms of fairness trump pure self-interest, leading to the failure of expected utility theory, specifically the substitution axiom (Kahneman and Tversky 1979; Henrich et al 2004). Recent Nobel Prizes in economics have been awarded to researchers doing empirical work that does not support the NCE assumption of perfect rationality (Macy 2005). On the other hand, there is substantial empirical evidence that basic self-interest is typical, lending support to weak RCT. Research on foraging peoples has found that optimal foraging theory, originally a biological theory of non-human species, also applied to humans. And research on peasant societies has found that peasants tend to make rational choices in terms of optimal yields and prices (cited in Little 1991). With the advent of urbanization, mass literacy and education, arguably the capacity for rational calculation is increasing.

The level of selection in biology also has implications for compatibility with RCT. Beyond basic individual selection, kin selection is also accepted as important in biological evolutionary theory since kin share a significant proportion of genes (Hamilton 1964, cited in Hopcroft 2009). Kin selection and kin altruism

would not seem to present any insurmountable challenge to the RCT framework, although at the margins it might seem that sacrifice on behalf of family members might run counter to pure self-interest. On the whole, though, if kin help one another, then the benefits might outweigh the costs for the individual, though the RCT theorist might predict periodic defections as self-interest trumps sacrifice for the group. But the serious problem is group selection. Since in biological evolution traits are transmitted through genes, and individuals are the level at which genes are transmitted, groups as the unit of selection is simply not seen by most biologists as being possible. On the contrary, it is the variation in a population or group, variation at the level of individuals and their genes that is the source of a species' capacity to adapt to the changing environment. Only a small minority of biologists advocate the importance of group selection (Sober and Wilson 1998).

In principle it might be different in the realm of sociocultural evolution. The main problem here in terms of compatibility with RCT is that there is no widely accepted theory of sociocultural evolution comparable to biological evolutionary theory, which has paradigmatic status in biology. One research program which is clearly compatible with weak RCT focuses on the evolution of cooperation (Hammerstein 2003). Actually, this research cuts across biological and sociocultural evolution. Much of the research takes the individual as the unit of analysis and models interaction based on norms, rules and goals. Complex modeling is made possible by computers, including the use of game theory (Axelrod 1984). This field of research includes anthropologists, economists, and psychologists. Researchers in this field have sought to develop theories of sociocultural evolution parallel to biological evolution (Richerson & Boyd 2005). Rational choice sociologists would be among the best suited to participate if they work with a realistically modified and relaxed definition of rational choice. But individual action that goes against self-interest in the form of strong group norms and strong reciprocity is part of the research program on cooperation (Fehr and Henrich 2003; Richerson, Boyd & Henrich

2003). A provocative outlier using the group as the unit of analysis is anthropologist Christopher Boehm who has argued that the long period of hominid foraging provided the basis for group selection favoring egalitarianism (Boehm 1999). So as the research deepens it might either increasingly contradict core RCT principles or force their further modification.

Macro-level theories of sociocultural evolution are more contested, and more difficult to test empirically to the extent that they theorize an N of 1: human social structure at the level of the entire species. It is less clear how any of them might be synthesized with RCT. The evolution of the entire social structure, as in the theories of Spencer, Durkheim, Lenski, Harris, Turner and Sanderson, may be compatible with individual self-interest on the ultimate level of an individual's greater likelihood to survive if the society survives (Sanderson 2007). Beyond that, some theorists see sociocultural evolution as increasingly in conflict with biologically evolved human nature (Turner & Maryanski 2008), while others see sociocultural evolution as an expression of the underlying biologically evolved human nature (Sanderson 2001).

In conclusion, while work continues on establishing the precise mechanisms of sociocultural evolution, biological evolutionary theory and weak RCT seem to be compatible and should be combined to the extent possible in understanding the patterns of human social interaction.

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Why Rational Choice Theory and Sociobiology* Are Natural Allies

Stephen K. Sanderson
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Talk for ASA, Atlanta 2010

(This talk was scheduled for presentation in the section's invited session, but was not delivered because the session was early in the morning and the author's alarm clock malfunctioned.)

Rational choice theory's basic principles (as stated by Debra Friedman and Michael Hechter) can be summarized as follows:

1. Social behavior is the result of actors who are acting purposively in accordance with a hierarchy of preferences. Actors are striving to realize these preferences with a minimum of cost. Actors are rational calculators of benefits and costs.
2. The rational calculations made by actors are subject to at least two kinds of constraints. Individuals confront opportunity costs, or costs associated with foregoing certain courses of action. They also confront institutional constraints, which act as positive or negative sanctions on the net benefit of any given course of action.
3. Actors are in possession of a certain amount of information regarding what choices will best realize their preferences. How much information people have will affect the choices they make and the outcomes of those choices.
4. Individuals act rationally in accordance with their own subjective sense of what is in their interests.
5. Choices made by individuals concern the means they use to achieve their goals, not the goals themselves.

Rational choice theory is not popular in sociology and is often severely criticized. However, much of the criticism is based on several misunderstandings. One type of misunderstanding stems from the very name of the theory. "Rational" connotes to critics that people always know what they are doing and that they achieve the results they seek. But this is often not the case: In making decisions, people possess a certain amount of information, and obviously there are circumstances in which this information is insufficient to produce a positive outcome (see point #3 above). Moreover, critics tend to assume that "rational" means substantively rational, i.e., rational in terms of the goals sought. But rational choice theorists are talking about instrumental rationality, i.e., rationality with respect to the means chosen. They make no assumptions about whether the goals are "rational," i.e., "good" goals for which to strive (see point #5 above).

Another basis for misunderstanding involves the word "choice." Critics of all types think this means that individuals are always making deliberate calculations, often very complex calculations, and that such calculations are the basis of all social behavior. It is true that "choice" often implies deliberate calculation, but it is not true that some sort of choice is always involved, and it is seldom true that individual behavior is driven by highly complex calculations. The human brain is simply not built to do this; it is built to simplify matters and use "workarounds," as stressed by recent cognitive psychologists.

Another problem with the word "choice" is that much social behavior is driven by emotions that lie below the level of conscious awareness, and thus cannot really involve deliberate choosing. So the name of the theory is problematic. A better name, I think, is cost-benefit analysis (another possibility is interest theory). People are self-interested creatures who seek various kinds of rewards and who wish to minimize the costs of obtaining them. Before he married, Charles Darwin made a list of "reasons to marry" (benefits of marriage) accompanied by "reasons not to marry" (costs of marriage). He eventually decided that the

first list was more compelling than the second, so he married. Most people are not so deliberate, of course, but their minds may still work in such a manner.

Understood in this way, I believe that rational choice theory has much to contribute; still, by itself it is not enough. Its most critical deficiency is the problem of preferences, a deficiency freely acknowledged by rational choice theorists themselves. Rational choice theorists have in mind various goals that people pursue, but these are simply assumed, rarely theorized. However, understanding what these preferences are and where they come from is critical to sociological explanation, and rational choice theorists' failure to address this is a serious lacuna in their work.

On the few occasions in which rational choice theorists have attempted to deal with this problem, the results have not been very impressive. Michael Hechter, for example, calls attention to the so-called typical value assumption of rational choice theory, which holds that actors are motivated to attain such instrumental goods as wealth, power, and prestige, goods that can be exchanged for other goods that are valued in and of themselves. This assumption is highly realistic, in my view, but it is unfortunately a theoretically ungrounded assumption. No reason is given as to why people should value these things, nor can such goods be considered simply instrumental. They can be exchanged for other valuable things, but they are also valuable in and of themselves. Be all this as it may, Hechter then goes on to propose the idea of a hierarchy of nested values:

At the most fundamental level, biological determinants produce values that are common to or, perhaps, constitutive of all human beings. This source of values produces no variation to be explained. [This is definitely not true and shows that Hechter has an overly simple understanding of sociobiology – AU.] Ecological determinants of values indirectly influence the establishment of a set of social institutions that, in turn, highlight

certain values at the expense of others. The Nuer, a tribe in East Africa, offer a good example. Given the nature of the environment that the Nuer occupied, pastoralism was the most viable mode of production. To the degree that the social institutions of all pastoral societies take the same form [and this is certainly a big oversimplification; it can be only partly true – AU], the members of such societies will have a set of common values – in addition to those that they share as members of the same species.

Next come institutional determinants. To the degree that environmental conditions allow for the establishment of different kinds of social institutions, we would expect to find members of these respective societies to have systematically different values. Clearly there is a great scope for institutional differentiation within the same ecological parameters. Advanced technology certainly loosens the coupling between social institutions and the environment. Hence, in advanced societies, we would expect that more variation in values would be due to social institutions than to ecological variables per se.

The penultimate cause of variation in personal values lies in idiosyncrasies of personal biography, some of which can be explained by individual patterns of group affiliation. Membership in each group may foster particular values. For example, we might expect to see (with a positive probability) certain kinds of common values held by Catholics as against Protestants, by members of the Chamber of Commerce as against union members, and by sociologists as against economists.

There is much in this statement that is sensible, and it is certainly an improvement on rational choice theory's usual silence. Unfortunately, Hechter's formulation does not take us much beyond traditional sociology, nor does he explain why it is that biology and ecology should be determinants of values.

So enter sociobiology. It can take us quite a long way in establishing human preferences, especially those that are found in all societies. Here is a list:

1. People value kin over non-kin and close kin over more distant kin.
2. People have genetic interests, that is, an interest in maximizing their reproductive success, although they are not necessarily aware, or at least fully aware, of such interests.
3. People highly value status and wealth. This is because these are the main avenues to reproductive success.
4. Some people like power. This, like status and wealth, promotes reproductive success. However, like the quest for status and wealth, the quest for power can become partially detached from the quest for reproductive success. People can strive for status and wealth in their own right. The same is true for power – some people simply like dominating and controlling other people.
5. People like sex and will expend a great deal of effort to get it. This seems to be especially true for males.
6. People everywhere – not all people, but most of them – have needs that are difficult to meet through mundane means. They turn to supernatural agents to help them meet these needs. Religion is about a number of things, but one of the most important is relief from anxiety, insecurity, and uncertainty.
- 7.

Let's take some examples. There is a famous sociobiological hypothesis known as the Trivers-Willard hypothesis (TW), which is assumed to apply to both animals and humans. It contends that mothers in good condition will produce more sons and mothers in poor condition will produce more daughters. The hypothesis derives from the fact that it takes more energy and effort to produce sons. In the human case, Trivers and Willard assumed that social status could be used as a proxy for condition, and thus that high-status mothers will bear more sons and low-status mothers will bear more daughters.

There is a good deal of empirical support for the hypothesis, especially in preindustrial societies. But one can derive a related hypothesis from TW, which is that high-status parents will invest more in sons, low-status parents more in daughters. There is considerable support for this too. Studies of eighteenth- and nineteenth-century China and India carried out by Mildred Dickemann have shown that the daughters of lower-status parents had better marital prospects than sons because of hypergyny, i.e., the tendency of women to marry upward in the status hierarchy. Lower-status parents therefore favored daughters over sons. But in higher-status groups sons had better marital prospects, and thus sons were favored over daughters. In fact, in some higher-status groups sons were so strongly favored and females so strongly disfavored that rates of female infanticide were often exceedingly high.

Another example. The Mukogodo of Kenya studied by Lee Cronk say they favor sons but their actual behavior shows that they clearly favor daughters. Their daughters have better marital prospects than their sons because the Mukogodo can marry off their daughters to the Masai, who are a higher-status group linked with the Mukogodo in a marital exchange system. (I suspect the reason the Mukogodo say they favor sons is that they are imitating the high-status Masai, who not only declare son favoritism but actually practice it.)

Were members of the groups in question acting rationally? Yes, specifically in terms of their reproductive interests. But if we used rational choice theory alone, we might fail to see the kind of preference that was driving the parents' behavior. Why one group favored sons and the other daughters would likely remain a puzzle. (Most sociologists would probably say, "It's just because of their culture," a useless culture-culture explanation à la George Homans.)

Using rational choice theory and sociobiology together makes good sense because they share at least two important assumptions: Individuals are the basis of society, which is built from the ground up; and

individuals are trying to maximize something, or at least receive a satisfying level of it. In essence, sociobiology allows us to take rational choice theory to a deeper level and establish a kind of metaphysics, or a set of first principles for social action. I have been trying to convince some rational choice theorists of this, but so far I have been met with resistance. However, not all of them resist, and perhaps fewer will in the future.

*I use the term sociobiology rather than evolutionary psychology because I think it more clearly expresses how sociologists use (or should use) Darwinian evolutionism. The name evolutionary psychology was apparently concocted for two reasons. One was to get rid of the political baggage associated with the term sociobiology; the other was that the new term was coined by psychologists who had become Darwinians. The principles of evolutionary psychology are almost the same as those of sociobiology, and evolutionary psychology as practiced today has become increasingly narrow in its choice of topics (e.g., the ratio of the second to the fourth finger in males vs. females and the relationship of the ratio to testosterone levels). Sociologists have a much broader range of topics to explore – I certainly do – and therefore I choose the term sociobiology.

New Publications of Section Members

Abrutyn, Seth and Kirk Lawrence. 2010. "From Chiefdoms to States: Toward an Integrative Theory of the Evolution of Polity." *Sociological Perspectives* 53:419-442.

Blute, Marion. "Reflections on Trees of Knowledge." *Spontaneous Generations: A Journal for the History and Philosophy of Science*, 3(1) 223-225, 2009.

_____. "Review of Myra J. Hird's *The Origins of Sociable Life: Evolution After Science Studies*." New York: Palgrave Macmillan, *Canadian Journal of Sociology* 34(4) 1161-1164, 2009.

_____. "On Dupré and O'Malley's *Varieties of Living Things: Life at the Intersection of Lineages and Metabolism*." Blog Post, Jan. 16, 2010. *Philosophy & Theory in Biology*.

<http://philosophyandtheoryinbiology.org/>

_____. With Paul Armstrong, "Reports of the Death of the Sociology of Science Have Been Greatly Exaggerated." Forthcoming, *Canadian Review of Sociology*.

_____. "Review of Martin Carrier, Don Howard and Janet Kourany (Eds.). *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*." Pittsburgh: The University of Pittsburgh Press, 2008. Forthcoming, *History and Philosophy of the Life Sciences*.

_____. "Review of W. G. Runciman, *The Theory of Cultural and Social Selection*." Cambridge: Cambridge University Press, 2009. Forthcoming, *Canadian Journal of Sociology*.

_____. With Paul Armstrong, "The Reinvention of Grand Theories of Science." Forthcoming, *Perspectives on Science*.

Hopcroft, Rosemary L. and Joseph M. Whitmeyer. Hopcroft. 2010. "A Choice Model of Occupational Status and Fertility." *Journal of Mathematical Sociology* 34: 4, 283 — 300.

Powell, Brian, Catherine Bolzendahl, Claudia Geist, and Lala Carr Steelman. 2010. *COUNTED OUT: Same-Sex Relations and Americans' Definitions of Family*. New York: Russell Sage Foundation (American Sociological Association Rose Series in

Sociology). NOTE: Chapter 5 (“Accounting for Sexuality: God, Genes and Gays”) should be of particular interest to section members. It discusses how Americans have becoming increasingly supportive of genetic explanations of sexuality. The chapter discusses how genetic explanations can sometimes be used as a tool not to perpetuate the status quo (as critics often claim) but rather to promote social equality.

Announcement

Double Special Issue “Micro-Macro Links and Micro-Foundations” *Journal of Mathematical Sociology* 35(1/2) 2011

Special Issue Editors:

Marcel van Assen, Vincent Buskens, and Werner Raub

A double special issue on “Micro-Macro Links and Micro-Foundations” of the *Journal of Mathematical Sociology* will appear in early 2011.

The contributions to the special issue focus on two essential issues: (i) how macro-conditions affect actor behavior at the micro-level and how actor behavior affects macro-outcomes (micro-macro links), and (ii) how different micro-models affect macro-outcomes (micro-foundations). The special issue comprises eight papers:

Contents

1. Werner Raub, Vincent Buskens, and Marcel A. L. M. van Assen. “Micro-Macro Links and Micro-Foundations in Sociology.”
2. Simon Gächter and Christian Thöni. “Micromotives, Microstructure and Macrobehavior: The Case of Voluntary Cooperation.”
3. Kazuo Yamaguchi. “Population Heterogeneity and Between-Group

Substitutability and Complementarity of Social Actions.”

4. Arnout van de Rijt. “The Micro-Macro Link for the Theory of Structural Balance.”
5. Mark Fossett. “Generative Models of Segregation: Investigating Model-Generated Patterns of Residential Segregation by Ethnicity and Socioeconomic Status.”
6. Andreas Flache and Michael W. Macy. “Small Worlds and Cultural Polarization.”
7. Dirk Helbing, Wenjian Yu, and Heiko Rauhut. “Self-Organization and Emergence in Social Systems. Modeling the Coevolution of Social Environments and Cooperative Behavior.”
8. Karl-Dieter Opp. “Modeling Micro-Macro Relationships: Problems and Solutions.”

The contributions in the special issue reflect key features of micro-macro modeling in sociology as well as recent progress in this field. The papers address important topics such as core features of explanations of social phenomena using micro-macro models, the problem of cooperation, heterogeneity of actors, structural balance, opinion formation, segregation, and problems of micro-macro models that are based on rational choice assumptions. Moreover, the contributions show how different research methods can be applied fruitfully, such as laboratory experiments, equilibrium analysis, and agent-based modeling.

For further information, see:

<http://www.informaworld.com/openurl?genre=journal&issn=0022-250X>

Presentations of Section Members

Barber, Mel. Spencer, Durkheim, and Marx and the Quest for an Evolutionary Science of the Social World. The paper was presented at the XVII ISA World Congress of Sociology entitled Sociology on the Move, Gothenburg, Sweden on 17 July, 2010.

J. Scott Lewis presents the faculty award to Allan V. Horwitz at the Section Reception.

CONGRATULATIONS TO THE FIRST WINNERS OF THE EVOLUTION, BIOLOGY AND SOCIOLOGY SECTION AWARDS

Faculty Award:

Allan Horwitz of Rutgers University, for his book: Allan V. Horwitz, Jerome C. Wakefield, and Robert L. Spitzer (2007). *The Loss of Sadness: How Psychiatry Transformed Normal Sorrow into Depressive Disorder*.

Student Award

David Peterson of Northwestern University for his paper "The Ivy and the Trellis: Agency, Biology, and Socialization"



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*Neurosociology: the nexus between
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David D. Franks
Springer Press

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