

Perspectives

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SECTION OFFICERS

A CHANCE TO SOCIOBIOLOGY – AND FOR SOCIOLOGY

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The Emergence of the Maximization Principle. The "theory of evolution by natural selection" has been burdened by a major theoretical muddle. Consider the general proposition of the Modern Synthesis, a historical compromise between early Darwinism and Mendelism: "All biological organization, down to the level of molecules, has evolved as a result of natural selection acting upon genetic variation" (Dobzhansky, Ayala, Stebbins, and Valentine, Evolution, 1977). To say that X acts upon Y is to state that X is causative of Y. But natural selection is not, stricto sensu, a cause -- a primum movens in the sense in which gravity, for example, certainly is. It refers merely to a demographic statement that describes, and does not explain, a process of genetic transformation in a population of breeding organisms. Therefore, it hardly belongs at the core of the biological paradigm. For Darwin it was the "preservation of favorable variations and the rejection of injurious variations," and he had no clear conception of the preserving agent, or mechanism. Most biologists have since defined natural selection, faithfully, as "differential reproduction in a population of genetically diverse organisms" but have insisted on treating it as the mechanism of evolution.

The error has been the source of much confusion in biology, and has played a role in the alienation of sociology from evolutionary biology, whose emergence was promoted by the works of Malthus, Comte, and Spencer, among others. Biologists have chastised us for the failure to discover, or utilize a nomothetic paradigm, but we could not recognize the biological nomos and could fairly echo the ancient proverb: "It isn't for a lack of appetite that I don't eat." The nomos, however, has begun to emerge lately, and may be grasped at several levels. At one level it captures the biochemical process of cellular replication and, among sexual species, the recombination of genotypes, some of which are better adapted to the competition for survival and reproduction than others.

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The breakthrough may be credited to the alliance of disciplines known as sociobiology. I refer specifically to the so called maximization principle, an attempt to retrieve, with the help of the genetic concept of inclusive fitness, the nomothetic content of the Malthusian struggle for existence that inspired the concept of natural selection in the first place.

The progress of the theoretical upgrading, however, leaves much to be desired. That is not a fault; it is a deficiency subject to resolution. The early statements of the principle employed an overadaptationist strategy. A case in point is Barash's (Sociobiology and Behavior, 1977: 42-43) formulation: "When a behavior under study reflects some component of genotype, the animals should behave so as to maximize their inclusive fitness." In response to criticism, a reformulation has emerged that may be represented as follows: Organisms tend to behave so as to maximize their inclusive fitness. I shall return to this context.

Laws and Levels of Causation. Whatever its deficiencies, the maximization principle does provide an "ultimate" (in-the-last-analysis) metric of social behavior. Science, as we have come to understand it in the last half millennium, is not possible without laws. Without these we may have descriptions and correlations, but their reliability is ephemeral in time and place. Laws are focal points that stimulate systematic and cumulative research, thereby surrounding themselves with the ancillary propositions that round out logical structures, namely, theories.

In the absence of laws we lack: (1) a logical directive for concept formation that transcends the purely ad hoc context; hence, (2) a firm and logically adjudicated basis for intelligible and productive communication; therefore, further, (3) a basis for a theorem-like, and thus linked, unfolding of theoretical ideas and research programs; and, inter alia, (4) the means wherewith to recognize a hierarchy of causation and thereby engage in a stepwise structuring of propositions according to their varying degrees of informativeness or, conversely, specificity. The result in sociology, as Lenski (ASR, 1988: 186) has recently put it, has been a "proliferation" of theories rather than an "advancement" of theory. One of the most grievous facts of sociology is that, lacking a nomothetic metric, we have never really mined the Great Masters who opened up what remains one of the most promising frontiers of modern scholarship. Exegeses, of course, persist, typically vying for singularity of interpretation, but on balance old theories have been "crowded...out of our limited span of attention" by new theories on scientifically irrelevant grounds (Lenski, ASR 1988: 165).

To say that we need laws is to say also that: (1) we need to recognize levels of causation; (2) laws provide the ultimate, as contrasted to the proximate levels; and (3) the search for the ultimate is the first priority of science -- first the rule, then the exceptions. Herein lies the heart of "the sociological crisis" -- and perhaps of the sociological aversion to sociobiology. Many sociologists consider laws unnecessary, if not altogether irrelevant or at most secondary in importance (e.g., Giddens, The Constitution of Society, 1984). Occasionally, we are even tempted to wrongly accuse sociobiologists, most notably E. O. Wilson (Sociobiology: The New Synthesis, 1975), of holding that "we need to focus on ultimate causes and not proximate causes." Curious reasonings then follow to demonstrate that the reverse is true, and typically drag along an old gauche error: organisms behave primarily for the good of the species (e.g., Gove, in The American Sociologist, 1987: 265, 269).

Biologists, like other scientists, think of causality in terms of the mutual dependence of relevant factors. How could sociobiologists, for example, fail to think in systemic, or "coevolutionary," terms when evolution itself is understood as a process of speciation on the basis of adaptation, namely, the functional interdependence of organism (genotype) and environment? But systems are rarely if ever constituted by etiological symmetries, they feature overdetermining factors. The rule, thus, is that in the study of cycles of interdependence, systemic explanations and predictions given etiological priority to the overdetermining factors, sometimes referred to, for convenience sake only, as the "constants" (e.g., Pareto in A Treatise on General Sociology, 1916/1963; Lenski, ASR, 1988). How could it be otherwise since there are overdetermining precisely in the sense that, typically, the other system variables vary more in reaction to them than to one another? Thus, the constant is to an extent the thread that holds, organizes, relates the other variables together. Is the constant always biological in nature? Not necessarily. Human sociobiology has shown that cultural factors can often override biological ones (e.g. Lumsden and Wilson, Genes, Mind, and Culture, 1981; Lopreato, Human Nature and Biocultural Evolution, 1984).

Still, explanation makes no sense without laws. Imagine a physicist giving priority to the proximate factors of the following behaviors: a jogger, an airplane in flight, a meteor, a soccer ball in play. Each is influenced by "peculiar" factors, but all obey the law of gravity. And without first recourse to the law, none of them can be explained. On the other hand, a combination of ultimate and proximate factors does make explanation more complete.

Human Nature. Do organisms tend to maximize fitness at random or under the influence of broadly directive innate programs, the so-called epigenetic rules or

behavioral predispositions? Science has shown that nature abhors chaos, and studies have strongly suggested a genetic component in a variety of behaviors, including verbal ability, susceptibility to depression and suicide, conformism, alcoholism, and political orientation (e.g., Martin, Eaves, Heath, Jardins, Feingold, and Eysenck, "Transmission of Social Attitudes," Proceedings of the National Academy of Sciences, 1986:4364-4368). Details aside, therefore, the corpus of the sociobiological enterprise consists in the quest for a theory of human nature, i.e., those innate predispositions that serve the maximization tendency and guide individuals along certain paths of socialization that are more or less broad depending on the richness and permissiveness of existing traditions.

Consensus has been rising that, among the social sciences, sociology is probably the most isolated and, theoretically, among the most disoriented. Gerstein and Sciulli (Perspectives, 1987, 10:1-4), for example, wrote recently: "There is no longer a theoretical core, such as Parsons provided in the bygone postwar era." One may question whether real cores can dissolve so quickly; the statement is at any rate disturbing, especially since the authors proceed to show trends toward alliances among the other social sciences that might leave sociology out in the cold.

We Have Nothing to Lose. What should we do? What could we lose by giving sociobiology a try? Suppose that, as many argue, it is indeed a new theoretical stage in behavioral science? Anthropology and psychology, and to a lesser extent economics, linguistics, and political science have been building solid bridges to evolutionary biology via sociobiology. The developments may translate into budget influence as well as theoretical prowess. Why not us? How, in the present mood, can we cope with the scores of new evolutionist journals, the enormous research programs in child development and other epigenetic sciences, the truly path-breaking discoveries in theoretical neurobiology, the rapid mapping of the DNA molecule? How can we deny the relevance of sociobiology without first making a serious effort to demonstrate the validity of the denial? Nothing is more damaging than an unformed "No."

The bases of our resistance appear excessively flimsy. Here are some of the most common: (1) As noted, we object to what is only a fictitious determinism. (2) We fear being "absorbed" by sociobiology, thereby forgetting that the historical tendency of systems is toward differentiation; some universities have separate departments of biology, zoology, and botany! (3) Sociobiology is said to be "racist," "anti-feminist," etc. It is always best to concentrate on the logical structure of theories. Besides, how can sociobiology be racist and at the same time be accused of seeing insufficient

differences between human beings and other animals? How can sociobiology be anti-feminist when some of the few things it can say for sure about gender differences are: (1) Dimorphism has been decreasing and hardly warrants a continuation of male dominance originally based on physical power. (2) When the survival of entire breeding populations is considered, "women are to men as gold is to dirt" (Lopreato, Human Nature and Biocultural Evolution, Chapter 9). Just imagine the future of a hypothetical society of 999 men and one woman. Compare it now to one in which the proportions are reversed! And there is in vitro conception to boot.

My sociobiology students volunteer deep feelings of shame for "the anti-like" nature of human prejudice and for the "peacock complex of guys." Most understand quickly that, just as geocentrism was the obstacle of physical science prior to the 16th century, so is anthropocentrism, its sociological corollary, the fetter that links our perspective on human behavior to Creationism.

It is said that sociobiology is "immature," that it "claims too much," that it "denigrates" sociology, and so forth. Copernicus' science was backward, too. Newton was born 99 years after De Revolutionibus. Claim too much? That remains to be seen. Newton claimed far too little for Einstein's taste. Does it "denigrate" sociology? Our best response is not to denigrate, it turn, but to prosper theoretically.

The history of science shows that new disciplines develop by borrowing from established ones. But we have a great deal to teach, too. Recall the maximization principle. Its formulation in terms of a tendency is hardly heuristic. Science recommends a more demanding strategy: for example, propositions whose validity is contingent on specifiable conditions. The best statement of the principle would have the following form: Organisms behave so as to maximize their inclusive fitness, provided that conditions a.b.c...n obtain. Some of those conditions are sociological. Our work on religious behavior — for instance, ritualistic chastity — suggests a Mendelian self-deception that conditions the validity of the principle. The modern quest for "creature comforts" (e.g., freedom to compete on the market and the Kantian sense of "the beautiful and the sublime") results in fertility rates that propose a qualifier for the principle. Sociobiologists are almost entirely incapable of conceptualizing system needs ("emergences"), whereas sociologists are masters, however untidy, of the art.

To borrow a law, however soft, and thereby catch a glimpse of the possible logical structure of our knowledge is not a bad deal. To help in the upgrading of the law, among many other possible contributions, is a very good deal indeed; it may amount to a crucial contribution to one of the most fertile revolutions in the history of

science. Disciplines do not disappear when they engage in "extra-territorial" dialogues; they are swallowed up when they yield their powers and obligations to others.

In Remembrance

Members of the Theory Section regret the recent passing of our colleagues, Morris Janowitz and Nicholas Mullins.

International News

BRITISH SOCIOLOGICAL THEORY TODAY

Scott Lash
Lancaster University

What has been happening in British sociological theory? Many American theorists are aware of their British counterparts mainly through the "second brain-drain" — of the likes of Michael Mann, Steven Lukes, and Perry Anderson — of the mid-1980s, or through encounters with the increasing number of English sociologists (I would argue more a case of their increasing affluence, and of sociology's internationalization, than of labor market desperation) who turn up at conferences in America. This might well give the impression that British theory is indeed in a very unhealthy state. I am convinced that very much the opposite is the case.

What is remarkable about British Sociology is the unusually large number of high quality theorists it produced among those entering the profession from the early 1960s through the late 1970s. How many Americans are there, for example, of the caliber of Tony Giddens, Michael Mann, Steven Lukes, Gary Runciman, or Stuart Hall? British sociological theory is clearly very strong among those who now are between their middle thirties and early fifties. Among cohorts too old to have benefited from the universities' and sociology's expansion of the 1960s, or young enough to have suffered the Thatcherite cutbacks from 1979 to the present, British theory is admittedly very thin on the ground.

Sociology in Britain and in continental Europe only became institutionalized on a significant scale with the mass expansion of higher education in the 1960s. The corresponding era in the history of the US academy was the 1920s. In a phenomenon not dissimilar to the advantages of late industrialization, that is, a sort of "sociological backwardness in perspective," British sociology developed late and then with considerable force and all at once.

The first wave of this expansion, in the early 1960s, was accompanied by the rise of "conflict theory", whose central purveyors were David Lockwood, John Rex, John Goldthorpe, Tom Bottomore, and Ralph Dahrendorf. Its paradigmatic works were perhaps Lockwood's benchmark article on "System Integration and Social Integration" and Goldthorpe, et al's Affluent Worker. Conflict theorists were first and foremost anti-Parsonsians. They advocated that conflict and not consensus was the normal state of affairs in social systems. Moreover, consistent with the characteristically British historical links of sociology and the labor movement, these analysts substituted a focus on the forces of social class in place of the pluralism of their American Colleagues.

During the late 1960s and early 1970s things began to take on an altogether greater complexity. The former pristine Anglo-Saxon purity of British sociological discourse was succumbing to the seductions of ideas from continental Europe. The immediate impact of this was the meteoric rise of Marxism, first, briefly, in the guise of Critical Theory and secondly, much more lastingly, as structural Marxism. In 1973, when I began graduate studies at the London School of Economics, there were no fewer than a half-dozen student groups reading Capital. Coming from the US, and worse the Middle West, I was immediately ostracized as an "American empiricist." I hung my head for months in the student cafes, bar and refectory, until it was discovered that I could read French better than most any of the others, and hence had access to the new stuff hot off the Parisian presses before it was available in translation. My status subsequently rose quickly.

Why structural Marxism? Perhaps because (despite the characteristic refusal of the Athusserians to engage in reasoned argument) of its emphasis on "rigor" and science that British graduate students found compatible with their own previous schooling in analytic philosophy and Popperian philosophy of science. Some of the work of the British structural Marxists, for example contributors to New Left Review and Hindess and Hirst's Pre-Capitalist Modes of Production was quite high quality. A lot of it wasn't.

Structural Marxism ~~was~~ faded quite quickly in the late 1970s. A number of its practitioners shifted their interest from social theory to empirical political economy, which is thriving today as much in Britain as it is in North America. At about the same time, considerable interest developed in studies of culture — largely sparked by the work and personality of Stuart Hall and his Birmingham based Centre for Contemporary Culture Studies. Its original Marxist and Gramscian impetus has been largely displaced today by theoretical inputs from semiotics and postmodernism, but its shift in focus from

base to superstructure surely was part of the beginning of the end of British sociological Marxism.

Finally, the remarkable rise of Tony Giddens can be dated from about the same point in time. Giddens, as well as Lukes, Mann, and Runciman, have their roots in the older conflict theory. They have, however, creatively situated the latter in the context of newer European ideas. Britain has in fact functioned from the late 1960s, and for this we can credit Perry Anderson and New Left Review, as the effective "entrepot" for Continental European theory. These same figures have also brought to sociology the argumentative skills of analytical philosophy.

The most recent generation of sociological theorists of the 1980s are also centered at these conceptual crossroads -- European ideas, conflict theory, neo-Marxism, analytic philosophy -- and are producing at quite a fruitful pace. Along with the decline of the labor movement and the seemingly eternal reign of Thatcherism, there is little Marxism of any kind any more in the thriving British Sociological Association Theory Group. There is instead interest in Foucault, Habermas, feminism and postmodernism as well as a bit of a renaissance in Weberian thought, all of which is reflected in Theory, Culture, and Society, the notably successful British journal of the past half decade or so.

Despite the brain drain and Thatcherist public spending cuts, sociology, and theory, continue to thrive in Britain. Access to the public sphere -- registered in the sheer quantity of sociologists contributing to New Statesman and Society, The Guardian, The Times Higher Education and Literary Supplements, Marxism Today as well as in television appearances -- is unmatched in America. Salaries are lower (say for entry assistant professor level some 25% lower) than at the big, prestigious U.S. universities. But I have heard of American full professors with incomes as low as \$37,000 per year. In Britain the very minimum is some \$45,000.

The real weakness and danger to British sociological theory lies in the near impossibility of movement for academics from one university to another within the country, and in the climate of pessimism bequeathed to departments by Thatcherite depletion of resources. Worst of all is the virtual absence of junior level job openings in recent years. In one month of 1988 there were over 100 junior positions advertised in the ASA Employment Bulletin. The median number of tenure track appointments per year in Britain in the 1980s has been about two to four. It is this, perhaps more than anything else, which is responsible for the masses of Britishers whom one sees these days around conferences and senior common rooms in the United States.

Conference News

British Sociological Association Theory Group

The British Sociological Association Theory Group held a conference on Marxism and Social Change at Newcastle Polytechnic January 5-6, 1989. Scheduled speakers included Mihailo Markovic (Belgrade), David Lockwood (Essex), Godfried van Benthem van den Bergh (The Hague), John Urry (Lancaster), Edmund Mokrzycki (Warsaw), Timothy O'Hagan (East Anglia), Christopher Bryant (Salford), and Nicos Mouzelis (London). For details contact Dr. Roy Boyne, Department of Applied Social Science, Newcastle Polytechnic.

International Sociological Association Research Committee for Sociolinguistics

A conference on Conversation, Discourse, and Conflict will be held March 21-23, 1989, at Trinity College, Dublin. The conference will address both substantive and methodological issues in the analysis of discourse and conversation, ideological representations in text and talk, and relations between knowledge and power. Ethnic identity, gender division, medical discourse, political discourse, rhetorical structures, conversational strategies, and conversational analysis will be among the topics discussed. For more information contact the Department of Sociology, Trinity College, Dublin 2, IRELAND.

German-American Theory Conference

The German-American Theory Conference, sponsored by the Volkswagen-Foundation) on Culture, Effects and Consequences will be held in Bremen June 23-25, 1988. Participants will include Neil Smelser, Michael Schmid, Robert Wuthnow, Mike Featherstone, Johannes Weiss, Shmuel Eisenstadt, Eugene Rochberg-Halton, Guy Swanson, Jeffrey Alexander, Stephen Kalberg, Klaus Eder, Michele Lamont, Renate Mayntz, and Richard Münch. This promises to be an important conference.

Getting Theory Papers Published

John Wilson
Duke University

A roundtable at the Atlanta ASA meetings provide some lively discussion about the problems of getting theory papers published and, the assiduous note-taker, some helpful hints. Some interesting insights from the editorial chair into the process of reviewing theory

manuscripts were provided by those invited to speak-- Charles Lemert, Charles Smith, Stephen Warner and Norbert Wiley. All agreed that there is no substitute for creativity and that nothing condemns a manuscript more certainly than its being simply a review essay. However, there was also agreement that many authors of theory papers seem to underestimate their creative faculties and fail to push their ideas to their limits. To overcome this particular problem each of the editors spoke encouragingly of their willingness to work with authors through successive drafts of their papers. In this respect, some at the roundtable believed, getting theory papers published was somewhat different from getting more empirical material accepted, the process being more negotiable and interactive in the case of the former. Few papers, it was noted, were rejected outright, and in most cases positive recommendations for improvement and revision could be made.

There was a mixture of opinion about a number of issues discussed. Some felt that theory papers fared better if they had clear empirical applications while others did not consider this an advantage at all. Some argued that papers which consist chiefly of conceptual refinement were not theoretically creative, while others considered this a legitimate contribution to social theory. Some were convinced that trying to publish theory papers presented peculiar difficulties while others were equally sure that they were no different from those encountered by more empirically-oriented writers.

The editors reminded their interlocutors of some "recipe knowledge" about theory publishing. First, be well-read in the area in which you choose to write, and make sure you include in your reading the journal to which you submit your essay. Second, carefully follow the guidelines published by the journal and submit a "clean" manuscript with the requisite number of copies. Submit as if you assume your manuscript will be given serious attention. Third, keep abreast of what is being published where and when. Avoid being the nth submission on a tired subject and avoid submitting a highly specialized essay to an interdisciplinary journal. Know what the leading thinkers are saying right now, and keep abreast of things. Fourth, show your manuscript to colleagues before submitting it for formal consideration. Have a friendly critic tell you whether or not you really have anything to say and whether or not you make your essential points clear in the paper. Fifth, if your paper is not accepted but you are invited to revise and resubmit it, try to resist the temptation to send it off immediately to another journal unchanged. Not only is this discourteous to the editor and reviewers, who have spent many hours working on your paper for your benefit, it is also impolitic because the world of sociological theory is small and manuscripts have a tendency to "do the rounds" among a fairly limited group of specialists in a given field.

Writing theory papers is, as one contributor put it, an awesome responsibility, because we are asking others to take our ideas seriously. We cannot simply assume the originality of our data will guarantee us a place in the table of contents. We must, to some degree, change people's minds, shift their often firmly fixed perspective. This is a daunting prospect to those who aspire to publish theory in refereed journals. On the other hand, the Atlanta roundtable provided comforting reassurances that those who must make the crucial decisions on our papers are "on our side", prepared, as often as not, to give us the benefit of the doubt. They urge us to be ambitious and take risks.

Work in Progress

Toward a Sociology of Change in the Legal Profession

Giovanni Busino
Universite de Lausanne

We are currently engaged in a research project on the professions. The main focus is on lawyers and change occurring in their professional organizations. This paper starts out on a quasi-ethnographic basis with a study of lawyers in the French speaking canton of Vaud, in Switzerland. This first step will ultimately lead us to the more general consideration of lawyer's activities, viewed as an important part of the intricate state machinery. The structure of the profession is being transformed by a number of factors including new types of clients, new activities, internationalization of business, and the growing importance of lawyer/client conferences prior to the establishment of new ventures (especially enterprises of an international character). Hence the profession has to deal with growing diversification and tends to spread outside the traditional boundaries of the law. This transformation necessitates the establishment of a sociology of change for the legal profession.

Book Announcements

Margaret Archer, Culture and Agency, Cambridge University Press, 1988.

Cynthia Fuchs Epstein, Deceptive Distinctions, Yale University Press, 1988.

Miriam Johnson, Strong Mothers, Weak Wives, University of California Press, 1988.

(Notify Perspectives of new publications)

Reminder: Nominations for the 1989 Theory Prize are due February 14, 1989. If you have a paper to nominate, send five copies to Samuel W. Kaplan, Department of Sociology, Bryn Mawr College, Chair of the Theory Prize Committee.