

Rational Choice and Sociological Theory: New Pressures on Economics as a Social Science

by

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1. Introduction

Is there a “crisis in economic theory” as suggested by a book with that title (BELL and KRISTOL [1981])? Maybe it is just a matter of interpretation whether one speaks of a crisis or of new challenges, but something is going on. In this article I would like to argue that at least part of this “crisis” is due to changes in sociology. For a long time economists could straddle the fence with regard to their being social scientists. This was possible because of the conventional division of labor between economics and sociology: the first deals with rational behavior and tidy models, the second deals with non-rational behavior and the messy social reality. This neat arrangement came to an end when the traditional sociological research program became exhausted in the sixties. Haltingly, some economists began to venture into non-market terrains, traditionally left to sociologists. As a consequence, they had to begin to deal with some of the problems traditionally shifted to “the social sciences”. The old arrangement completely collapsed when the new sociologists in the seventies had themselves adopted rational choice as a theory of action. These new sociologists now represent a new and serious source of pressure on economists to confront the issues formerly delegated to the sciences of non-rational action. Two claims frequently heard these days are clearly related to the pressure on economists to start dealing with the messy social reality: use psychological propositions and direct measurement of utility rather than unrealistic models and start explaining preferences rather than taking them as given. Is it wise to give in to these pressures? In answering this question below, I attempt to make optimal use of the experience gathered in sociology with regard to these two issues.

Since there are detailed review articles available on the development of the new sociology (OPP, [1985]) and on the impact of economics on this development (LINDENBERG, [1983a]), I will not go into any detailed description of these topics. Rather, I will attempt to flesh out the argument sketched above.

2. Economics and Traditional Sociology: A Symbiotic Relationship

Economists have never had a high opinion of sociology. Generally, they are only willing to be grouped with sociology when grant moneys make it prudent to do so. At first blush it would seem that they could easily get on without their poor cousin. Yet, since its inception as a discipline, sociology had had a very important function for economists: it allowed them the luxury of relatively cheap intellectual purity and rigor. Many potential problems in economics could be evaded by the sheer existence of sociology. The basis for this feat is the conventional view that economics differs from sociology in two distinct ways: a. it deals with rational action while sociology deals with non-rational action and b. it uses abstract models while sociology deals with the rich and messy social reality. Given this division of labor (*mutually* agreed upon), two rituals have developed in economics over the last hundred years: first, the “this should be studied by sociologists” ritual; second the “economists should incorporate more sociological richness into their models” ritual.

The first ritual had been sanctioned by Pareto in his *A Treatise on General Sociology* and it is widely used when seemingly non-rational elements (such as religion, tradition, ideology, self-sacrifice, massappeal, sense of fairness) may threaten the axioms of the economic model and/or when the simplifying model-assumptions seem to do injustice to the richness of social reality (such as social pressures, norms, social classes, complex interactions, social networks, interdependencies, power relations, reference groups, self-conceptions). For example, Schumpeter had asked for a ‘fiscal sociology’ to do justice to the intricacies of the modern tax state (SCHUMPETER, [1918]). OLSON ([1965], p. 160f) restricted his theory of collective action to groups with a high degree of rationality and to “stable, well-ordered, and apathetic societies that have seen the ‘end of ideology’”. In his view, collective action for less rational groups and societies should be studied by scholars that deal with religious motivation, mass movements, alienation, fanatic adherence to ideology and psychological disturbance.

The second ritual is used like a periodic rain dance to overcome droughts in economic theorizing. In most cases it is performed in obligatory articles in proceedings of state-of-art-conferences on economic theory (such as KRUPP, [1966]; HUPPES, [1979]; BELL and KRISTOL, [1981]). At times it deserves an entire book (for example, THUROW, [1984]).

At first glance, the two rituals seem incompatible: the first is designed to preserve the purity and rigor of economic theorizing while the second pleads for self-inflicted contamination in favor of a closer touch with (messy) social reality. Yet, precisely because purity and rigor of model building can be preserved without much trouble thanks to the first ritual, the periodic cry for the sociologizing of economic theory is generally known to be a ritualistic act, not to be taken too seriously. Sociologists have equally profited from this arrangement. They can relegate rational behavior to “the economic

realm”, thus protecting their assumption of “norm-guided” action, and they can boost their own relevance by pointing to the fact that economists themselves admit to sociology’s importance. In many ways these rituals were very beneficial. As SCHUMPETER [1954, 27] observed: “it is by no means certain that closer co-operation, so often clamored for by laymen who expect great things from ‘cross-fertilization’ ... would have been an unmixed blessing ... cross-fertilization might easily result in cross-sterilization.” Why should anything change in such a mutually agreeable arrangement?

3. The Exhaustion of the Traditional Sociological Research Program

The symbiotic relationship between economics and traditional sociology depended on the conventional division of labor between these two disciplines. But this division of labor, in turn, depended on the viability of the research programs it would permit. For sociology, the viability of its research program (in the sense LAKATOS [1970] used the term) had little to do with its positive function for economists. With the aid of hindsight, it is now possible to see clearly that what made traditional sociology a success was the abundance of biological misperceptions, prejudices, and naive “individualistic” preconceptions of the nineteenth century. Sociology had one distinct message: human cognition, motivation and action is socially determined. Every theory and every research project drove this message home, no matter what the manifest intentions of their authors were. In this way, sociologists attacked views about the determinative influence of biological characteristics (e.g. race and ethnic origin) on criminal behavior, on intelligence, on ways of thinking, on achievement motivation, on social organization and inequality. They attacked each and every explanation of social phenomena by instinct, by biologically given sentiment or by biologically determined state of mind (such as the explanation of the incest taboo by instinct, the explanation of the institution of the family by the ubiquitous sentiment of jealousy, the explanation of suicide by mental illness, or the explanation of religion by “primitive mind”). In a similar vein, they took on all explanations of social phenomena that failed to acknowledge social determination explicitly (such as historical explanations based on “great men”, all explanations based on human preferences that failed to acknowledge the social influences on human goals and values, all explanations of social inequality based on individual characteristics and motivation, as for instance talent and laziness). Finally, they locked horns with generally held views that failed to take the complexity of social determination into account. Thus, they would show that conflict is not all bad since it also produces solidarity, that crime is not all bad because it reinforces common values, that informal networks would form around formal organizational structures, that many social phenomena are unintended consequences of human action and thus much more complex than most people thought they are.

Seen in this light, traditional sociology was a *debunking* science and it was very successful. In virtually every confrontation with a sociologically "naive" view, sociology would come away the winner. This success was largely due to the models of man used by sociologists (see LINDENBERG, [1983 a]): SRSM (Socialized, Roleplaying, socially Sanctioned Man) and its watered-down version OSAM (Opinionated, socially Sensitive, Acting Man). The first sort of man is playing roles into which he or she has been socialized, and inner (conscience) and outer sanctions see to it that behavior does not deviate from the role expectations. The second sort of man had been invented to make role-playing man fit for large survey research: he or she has an opinion (or attitude) about everything and acts accordingly, but due to the social sensitivity of man, his/her opinions are the product of social influences. Thus both models are designed to optimally focus on the social determination of human cognition, motivation and action.

The success of traditional sociology was not without irony. In the sixties, Western culture was thoroughly sociologized. Economics, with its model of rational man and its taking preferences as given could never have achieved this feat. But sociology was so successful that it had made itself superfluous. No more scientific debunking was possible. The general sociological message had reached everybody and everybody repeated it to his neighbor. In fact, the research program was exhausted and in order to survive as a discipline, sociology had to change. Yet, given the sociological models of man, the options were quite restricted. One option was to leave theory and research and carry debunking into the political arena: people only pay lip service to the sociological message because otherwise they could not still act as if blacks and women were innately inferior; they could not still treat criminals as if they were personally responsible for their crimes; they could not still treat poverty as the result of individual choice; they could not still act as if power and success were due to individual talent and effort. Science itself is a social product and thus serves social purposes, etc. The counter culture of the sixties and seventies was like a huge communal celebration of the sociological message. Another option was to stop debunking and start working on detail: don't just show that something is socially determined but also how it is so determined. Due to the models of man, there were only two ways to do this: either refined studies of socialization or refined studies of social influences on attitudes and opinions. In the sixties and seventies, we witness an enormous increase in these kinds of studies together with the development of very sophisticated research methodologies.

Because the sociological models of man are ideal for debunking but very bad as theories of action, it was not possible to explain in any detail how human *behavior* was socially determined. Time and again it could be shown that attitudes (and values) are bad predictors of behavior. This suggested a third option: to change the model of man. George HOMANS [1961] had done just that by replacing the sociological models of man with behavioral

propositions based on learning theory (see OPP, [1985] for a more detailed account). Although this was a radical change of the sociological research program, its main impact was restricted to studies of small groups. Why this was so will be discussed in section 5 below. Suffice it to say that Homans' efforts, important as they were, are comparable to a coup in one of the provinces rather than the beginning of a protracted revolution.

Before describing the revolutionary changes in present-day sociology, I want to sum up the main points so far. Economics could develop as it did because of the existence of traditional sociology. This sociology, in turn, could develop as it did because of rampant racial and ethnic prejudices and naive "individualistic" views about society. With great cunning and considerable effort, sociologists have been so successful in eliminating these prejudices and naive views that their message has become trivial. In 1981, Irving Kristol, a renowned professor of social thought, observed that "there simply does not exist a body of knowledge that permits sociologists to talk with more authority about society than someone who may never have received sociological training." (KRISTOL, [1981], p. 202). In fact, the body of knowledge has been absorbed by Western culture.

Given this development, both rituals in economics described above become intellectually ludicrous when they refer (as they do) to traditional sociology. Threats to purity and rigor of economic models cannot be averted by invoking an exhausted research program. It is no coincidence that the new political economy and other non-market applications of microeconomic theory began to develop with considerable force at the same time that traditional sociology ran out of steam. But, as we saw with Mancur Olson above, the temptation to invoke some science of non-rational behavior in order to preserve the purity and rigor of one's own model is still strong. What happens in the new sociology today will block this safety valve and will eventually force a new division of labor between economics and sociology.

4. The New Sociology: New Pressures On Economics

In the mid-seventies several developments had converged to start a protracted revolution in sociology. First of all, the exhausted research program had made various scholars very receptive to alternative options. Second, Popper's philosophy of science, with its emphasis on depth in explanation, had been brought to bear on the social sciences by the pioneering works of Hans Albert (cf. for instance ALBERT, [1971]). Third, Homans had shown that a change in the model of man may be a viable option at least in some areas of sociology. Fourth, the "radical" years of sociology in the sixties had drawn new attention to the work of Marx; and although this particular version of sociology did not seem as fruitful as some had expected, it did reintroduce a social science

in which sociology and economics were not yet divided. When the bicentennial celebration of Adam Smith's *Wealth of Nations* focused renewed attention on this classical author, many were ready to see him as an example of an integrated social science. Fifth, this Smith reception was greatly aided by the developments in the new political economy (broadly conceived). For sociologists, the works of Downs, of Buchanan and Tullock, of Olson and of North were particularly influential. Sixth, due to its use in the new political economy, game theory finally became relevant to sociologists, especially its treatment of social dilemmas. The combined effect of these six developments was that a growing number of sociologists in various countries adopted a rational choice model of man. The immediate result was a change in the climate of discussions in conferences because rational choice attacked the core assumption of traditional sociology. The truly revolutionary impact of this change was its broadside attack on the conventional division of labor between sociology and economics. Not surprisingly, there was no handy way of communication in this new situation, either between traditional and new sociologists or between the latter and economists. The language that had developed on the basis of the old division of labor was hopelessly inadequate. Terms like 'individualistic' versus 'collective', 'rational' versus 'non-rational', 'moral' versus 'instrumental', 'values and sanctions' versus 'preferences and constraints' were so laden with past confusions that a fruitful substantive discussion seemed very difficult. The revolution in sociology makes it impossible that these questions are solved in the traditional way. The package that bundles "collective, non-rational, moral, values and sanctions" has fallen apart and the pressure on economics to pick up the pieces has already begun. For example, OPP [1985] urges economists not to take preferences as given and to start using psychological theories for the explanation of preferences and for the direct measurement of utility (see also ALBERT, [1984]). These kinds of suggestions are not new but they appear in an entire new light since economists cannot treat them any longer as part of a longstanding ritual. The new sociologists have become a viable source of pressure. Yet, among them there are disagreements about what exactly should be required and why. Are not some of the requests apt to weaken the economic program rather than improve it? In the following section, I will present my own views on the matter.

5. Suggestions of a (New) Sociologist for Economists

In the following, I will concentrate on two suggestions that seem to me most important because they are quite different from the opinion of many other (new) sociologists: model-building versus psychological propositions and constraints versus preferences. Other suggestions can be found in LINDENBERG [1983a, 1983b].

5.1. The Difference Between Individual 1 and Individual 2

First, there is considerable controversy about the place of psychology in economics. Interestingly enough, the pressure to use psychological propositions instead of a “logic of choice” comes from many new sociologists because they are used to working with hypotheses rather than models and because they rightly assume that in order to start dealing with (messy) social reality, economists cannot afford to ignore the research done in (social) psychology. What should be done about it?

In order to answer this question, we have to look at the context of theories (see WIPPLER and LINDENBERG, [1985]). In economics and sociology, the main task is to analyze social systems. In other words, the *analytical* primacy is focused on social systems. In order to explain social systems and related social phenomena, both disciplines have to make use of theories of action; i.e. the *theoretical* (or explanatory) primacy is focused on the individual. In psychology, the situation is different. Here both the analytical and the theoretical primacy focus on the individual. Given that the analytical primacy in both cases is different, different requirements are placed on the “individual” theories in psychology on the one hand and in economics and sociology on the other. Let us distinguish between individual 1 and individual 2 according to the following scheme:

	individual 1	individual 2
analytical primacy	individual	social system
theoretical primacy	individual	individual

To require psychological theories to be used as behavioral propositions is identical to the claim that individual 1 theories are perfectly adequate for individual 2 purposes. Yet clearly this is not the case. While individual 1 theories should be able to influence individual 2 theories, they should not replace them. This is exactly the difference between *methodological* individualism and psychologism. In order to qualify as “individual 2” a theory must satisfy at least the following requirements:

- (1) It must not require much information about each individual to whom it is applied;
- (2) It must allow us to model institutional and social structural conditions as defining intermediate goals and constraints of action;
- (3) It must allow psychological (including physiological) theories to influence its assumptions. For example, the information processing capacities of individuals must not be fixed by axiom;

(4) It must allow us to express our degree of ignorance explicitly. Thus it must allow us to introduce simplifying assumptions in such a way that they can be replaced with more complex assumptions as our knowledge increases (method of decreasing abstraction). Via this capability, requirement (3) has an explicit means of being satisfied;

(5) It must be well corroborated as a theory that explains behavior of human beings in the aggregate, inclusive of resourceful behavior.

Let me briefly elaborate each point. The first point seems obvious but its implication is often not considered. Theories of clinical psychology are meant to deal with concrete individuals and require much information about each case. Learning theories require ideally the entire learning history of an individual. More importantly (point two), a theory of action for economics and sociology must allow the simultaneous attention to both the analytical primacy of social systems and the theoretical primacy of the individual. For example, profit maximization must not be seen as a motive (individual 1) but as an intermediate goal created by certain institutions (the right to the residual) given resourceful human beings (individual 2). Since psychological theories serve a different purpose, they make it very difficult, if not impossible, to combine institutional and individual aspects on the same level. Third, since individual 2 theories are still theories about human nature, advances in individual 1 theories must be able to have an impact on individual 2 theories. This, however, is only possible thanks to the fourth requirement which allows the replacement of simplifying assumptions by more realistic ones.

The fourth requirement is also essential for dealing with requirements one and two and its importance can hardly be overrated. Social conditions can be very complex and without the ability to simplify, we would be in a vicious circle: we would have to know what we are still trying to find out. And without the ability to make our assumptions more realistic we would be trapped in the methodology of Max Weber's ideal types, i.e. we could not improve our theories. In other words, *model building is absolutely essential for the social sciences and it is not incompatible with the incorporation of psychological theories.*

Fifth, the action theory has to capture human nature to such a degree that it actually works for predictions and explanations *on the aggregate level*. As an individual 2 theory, it does not have to model cognitive processes, motives and perceptions (this is a task for individual 1 theories) and *a fortiori* it does not have to allow or encourage direct measurements of these things. A working *as if* theory that meets all five requirements is perfectly adequate.

A last item of requirement five is "resourceful behavior". As MECKLING [1976] has pointed out, this aspect is at times forgotten in economics. But traditional sociology had absolutely no use for this kind of behavior and was therefore greatly handicapped in saying much about institutions. Many institutions only exist because human beings are resourceful agents. For exam-

ple, institutions dealing with problems of control are resourceful solutions to problems that were created by resourceful behavior (cf. DEMSETZ, [1974], NORTH, [1981]).

It can now be easily seen why Homans' theories were restricted to small groups. His propositions were individual 1 and are thus misplaced when used *directly* as a theory of action in the social sciences, rather than working indirectly via requirement three. Even the best psychological theories today violate requirements one, two and four. Similarly, it can be seen that neglect of requirements two, three and five would cause much concern to authors who rightly insist on economics as an empirical science. There are versions of the *homo oeconomicus* which seem to focus excessively on requirements one and four to the detriment of all others. They represent the "pure logic of choice" that has been attacked time and again by Hans Albert (cf. ALBERT, [1984]). Still, the only theory to date that can be made to meet all five requirements is the theory of rational choice in its various forms of elaboration. For this very reason, the shift from behavioral sociology to rational choice is so much more important than some authors make it out to be (OPP, [1985], p. 229 maintains that "on the theoretical level at least there are only differences in terminology").

5.2. Constraint-Centered Heuristic

"An explanation of preferences ... is necessary if social processes are to be explained. All social sciences aim to do this." (OPP, [1985] p. 236) This claim is heard frequently from sociologists and at times also from economists (cf. VON WEIZSÄCKER, [1984]). What seems more reasonable than to ask that *both* preferences and constraints be explained? Yet, I think an important warning can be gleaned from the history of traditional sociology. But first let us dispense with an obvious point. If we mean by 'explaining preferences' the same as 'explaining choice', then the claim is trivial and not worth talking about. Thus if every time a person chooses A rather than B we say that he prefers A to B then that preference has to be explained. If, however, we mean by 'preference' something that explains choice (such as the exponent in a Cobb-Douglas utility function), then the matter is not trivial. For example, VON WEIZSÄCKER [1984] wants to explain a "true" change in taste for clean and natural environment by the effect of cognitive dissonance on people who claim to have more taste for this environment than they really do. In order to reduce cognitive dissonance, they adapt their taste to their public statements. From then on, these people will choose differently (in terms of the marginal utility calculus) and that change is explained by a change in preference.

My argument against using "genuine" preference changes in order to explain choice is the following (see LINDENBERG [1984a] for a more detailed account). Even if there was something like genuine preference change, the social sciences

are and will be too inexact to determine the exact proportion of preference change and changes in constraints. It thus becomes a matter of heuristics: either you are looking for changes in constraints or you are looking for changes in preferences. In either case, you never know whether you have searched far enough to explain whatever you want to explain. If you mix the two, you broke off the search in either direction. If you invoke one of the two, you neglected to search (or continue to search) for the other. In von Weizsäcker's case it is clear that he has chosen not to pursue the search for changes in constraints that would explain the changes in choice. I now argue that a preference-centered heuristic will drive out detailed attention to constraints, such as price and income changes. Worse yet, a preference-centered heuristic will draw attention away from choice because constraints (i.e. alternatives) play virtually no explanatory role. We know from traditional sociology, in which behavior has been explained by values, that the concepts of choice, scarcity, constraints have had no place. Still worse, a preference-centered heuristic will draw increasingly more attention to situations that influence values held by people, i.e. to socialization episodes. This had been practiced *ad nauseam* by traditional sociologists without much success in explaining behavior. In fact, a preference-centered heuristic will end up explaining behavior by the influences on values held, i.e. by role expectations.

In sociology, we have had a hundred years of experience with a preference-centered heuristic. This experience is not encouraging in terms of explanatory power and it should be a warning to economists who are tempted to go in that direction. On the other hand, the change in sociology makes it more difficult for an economist to straddle the fence with regard to this issue. What we are really interested in as social scientists is social structure and institutions, both prime examples of complex constraints. It seems to me that a clear constraint-centered heuristic increases our explanatory standards and advances the social sciences. This means that what goes for a utility function today, should be decomposed further into a higher order utility function and one or more production functions, as we would do in a Stigler-Becker world of *de gustibus non est disputandum* (STIGLER and BECKER, [1977]). In other words, we would have to explain what aspects of the social and institutional structure make a particular commodity able to confer utility, i.e. what makes it a means to some ultimate ends. Adam Smith has suggested some ultimate ends towards which all other commodities are only means *given* the social-structural and institutional constraints: physical well-being and social approval (see LINDENBERG, [1984b]). There may be more such goods but this seems to me a good beginning.

6. Conclusion

Developments in sociology forced economics to become a social science. No doubt this change will be accompanied by signs of reduced consensus, unsolved

problems and new uncertainties. No doubt, over the years to come, a new division of labor between economics and sociology will have to evolve with confusion in language, methods and goals. But in the process, social science stands to gain. This development could be hampered (not halted) by unnecessary confusion and this article is meant as a contribution to reduce avoidable misunderstandings. Since the cry for more integration between economics and sociology was a ritual part and parcel of the old arrangement, some sociologists and economists today revive this cry because it fits nicely into the general mood: to start dealing with messy social reality. They forget or are unaware of the fact that traditional sociology is exhausted. There is no use asking for an integration with an exhausted research program. The new sociology will not deliver the expected goods because it has moved into rational choice theory and model building. Rather than forming two halves that should be fitted together, economics and the new sociology are now confronted with very similar problems. In dealing with these problems, traditional sociology can still be useful: we all have accepted its general message and we can learn from its mistakes.

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