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Metatheories and Better Theories: A Reply to Ruttan

Lawrence Busch

The attempt to carry out interdisciplinary research between economics and sociology has been more than problematic in that the assumptions driving the two disciplines, the methods employed and the conclusions attained can be quite different. Vernon Ruttan, in his paper "The Sociology of Development and Under Development: Are There Lessons for Economics?", attempts to point out the potential contribution of sociologists utilizing modernization theory, dependency theory and world systems theory to economists. The following paper is a response to this endeavor. It examines the article by Ruttan in two ways. It first analyzes the different metatheoretical assumptions that underlie the two approaches. Secondly, it examines issues raised by Ruttan in an effort to magnify the similarities and differences between the two. It is concluded that what is needed is not a distinction between the social sciences but the generation of a social science which encompasses human societies in all their complexities.

Introduction

When responding to a paper such as that written by Professor Ruttan, it is all too easy for a sociologist to resort to economist-bashing, an exceedingly popular indoor sport among members of the discipline. I shall attempt to refrain from this activity and, instead, to take the analysis that Ruttan presents seriously. In so doing, I wish to examine it in two ways: First, I shall examine the different metatheoretical assumptions that underlie the two approaches (of most economists and sociologists). Then, I shall examine the specific issues raised by Ruttan. My purpose here will not be to refute what he says so much as to sharpen the points of agreement and of contention between us.

Development of (Meta)theories

Ruttan speaks of a kind of economic imperialism (which he rejects) which attempts to incorporate all social processes within the framework provided by neoclassical economics. Yet, from the perspective of a sociologist, what is most remarkable about economics is its ahistorical character. Without that ahistoricity, much of what economists have to say is at best problematic.

The problem begins with the very notion of an economy. This is a modern notion, arising with the separation of the workplace and the home several centuries ago (e.g., Polanyi, 1957). Before that, it was virtually impossible to distinguish an economy from

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a society. Yet, economists are largely oblivious to this issue. In fairness to Ruttan, he rejects the idea that there is such a thing as a natural market out there waiting to be discovered or allowed to work. Yet, he is unwilling to go as far as (some) sociologists in arguing that all institutions are socially constructed.

This to us peculiar view is implicit in the suggestion that there are lessons for economics in the sociological literature. Sociology, political science, and economics arose in response to a differentiation of Western societies into three distinct domains: society, polity, and economy, respectively. Anthropology arose in order to distinguish between those in "primitive" cultures where the tripartite distinction above did not (yet, according to the conventional wisdom of a century ago) apply. In short, the division of the social sciences along conventional lines is itself an historical phenomenon linked to a particular historical epoch. As this formation begins to crumble, the categories will cease to have meaning (Busch, 1991).

Block (1990) has recently taken a very close look at the assumptions underlying key economic categories such as capital, labor, investment, and markets. He concludes that the data collection process deludes economists into thinking that the world is much the same as it once was. Thus, statistical series on such variables as GNP, capital investment, and unemployment can continue to be generated and analyzed, but have less and less relevance to the actual situation in which people find themselves. And, the weakening of the data collection system under the Reagan administration has exacerbated what was already problematic (Gleckman with Carey, 1991).

Moreover, the same holds with respect to the approach used by economists. Ruttan's paper frequently alludes to the building of better models, of the need for formalization of particular variables, of the abandonment of Parsons's perspective just when it appeared ready to deliver. This presupposes a philosophy of science that is based largely on an acritical acceptance of early twentieth century positivism (or perhaps of Popperian falsificationism). Within this philosophical approach, what is of importance is associating economics with an

(idealized) natural science mode of doing science. Thus, it is assumed (among many other things) that the world is isomorphic to mathematical processes, that the normative and positive realms are distinct, and that true knowledge emerges only from scientific inquiry.

Consider, for example, Hayami and Ruttan's implicit assertion that all or most contemporary technical change arises from the natural or social sciences. Might not non-scientific knowledge also be a source of technical or institutional change? Moreover, the conceptualization of technical change as a linear variable is problematic in that it never asks a key question: technical change for whom? While Ruttan, like most sociologists today, rejects a simplistic modernization theory in which all societies move from traditional to modern, he unwittingly slips it in through the back door by assuming that technical and institutional change are linear. Moreover, by excluding the non-scientific sources of such change from the framework of analysis, the negotiated, persuaded, and even coerced character of change is glossed over.

In contrast, recent work in the philosophy of science has toppled the positivist legacy and its successors. The well-known work of Thomas Kuhn (1970) challenged the notion that knowledge was accumulated in a linear fashion and suggested that multiple worldviews might coexist and yet be "incommensurable." Nevertheless, for Kuhn, the quest for knowledge was largely unsullied by external factors. Science was a peculiar institution that was largely divorced from the larger social world.

Feyerabend (1975) went considerably further in asserting that there was no way to justify the use of any particular method outside of a given context. Scientific knowledge was not to be given any higher status than any other kind. Thus, at the limit, anything might pass for knowledge, and the cumulative growth accepted since at least the time of Descartes is at best a questionable proposition.

More recently, there has been a collapse of the neopositivist consensus that once characterized Anglo-American philosophy of science. Fuller (1988) has argued that consensus in science may be like that in

opinion polls which forms and dissolves quickly. Thus, what appear to be areas of widespread agreement among scientists are merely areas where careful investigation has not occurred. As he notes, this becomes apparent each time that expert testimony is required; as soon as the experts must confront issues of concern to laypersons, consensus disappears.

Rouse (1987) has virtually destroyed the wall between the social and natural sciences—not by showing how to make the social sciences more like the natural sciences, but by demonstrating the impossibility of studying nature in itself. Specifically, he argues that the natural sciences must be considered in terms of the new moral and ethical problems that they pose for the larger society. Furthermore, contrary to Ruttan, who would separate knowledge from its use, Rouse (1987:20) argues that "[T]echnical control, the power to intervene in and manipulate natural events, is not the application of antecedent knowledge but the form scientific knowledge now predominantly takes."

Finally, Idhe (1991) has opened the black boxes of scientific instruments for analysis. He notes that scientific perception is always mediated by tools which at once amplify and reduce certain of the innumerable features of the world. Thus, the use of particular tools, including those of economic analysis, simultaneously amplifies certain aspects of the world while reducing others.

The sociology of science literature, too, has broken with the positivist past. No longer is the content of scientific knowledge merely a discussion of the way the world is. Instead, scientific knowledge has been shown to be a means for controlling the social world. To paraphrase Latour (1987), science is politics by other means.

Of course, the changes in the sociology of development literature chronicled by Ruttan cannot be explained away by the metatheoretical changes described above. Gouldner's (Gouldner, 1970) critique of Parsonian theory still does that task best. However, taken together, these changes in the history, philosophy and sociology of science make it at best unlikely (1) that many sociologists will participate in the project Ruttan proposes we undertake together or (2) that

such a project would succeed. Having said this, let me now turn to some specifics about development theory.

Theories of Development

There are several points on which Ruttan and I would agree. First, Coleman's questions about how institutions come to be (though perhaps not his answers) show socially constructed character of society. Many sociologists would like to see an answer to these questions, though surprisingly few work on the issue. Second, few would quibble with Ruttan's broader goals—to come to grips with pressing issues of agricultural development in a manner that is just and equitable.

Nevertheless, there are numerous problems that I have with Ruttan's perspective:

1. Ruttan is caught between the Scylla and Charybdis of technological and social determinism. He critiques Marxist historians for the tendency to see "technical change as dominating institutional and cultural change" and cites Wittfogel as an example of the alternative view. I would argue that both views are unsatisfactory. On the one hand, those who introduce a new technology never fully control its effects. Thus, while one may introduce new technology in order to accomplish certain ends, that same technology may be used to accomplish other ends as well. Moreover, those who introduce a new technology do so in order to further certain ends within the context in which they find themselves; that context is largely beyond their control. Hence, technology is neither socially nor technically determined since each is in some degree contingent upon the other. Thus, the proponents of the Green Revolution introduced the new technology in order to prevent red revolutions and without much regard to tenure relations. In so doing, they reinforced certain tenure relations that probably contributed to the very instability they sought to avoid.
2. Where do class, status, power fit into Ruttan's model? Ruttan's variables conceal these kinds

of relationships behind the apparent neutrality of "population pressure;" technical change is defined as either labor- or land- saving. While these are certainly dimensions of technical change, they impoverish the concept. One might contrast this view with that of Melman (1981) or Noble (1977).

Ruttan and other proponents of induced innovation theory, like Parsons, tacitly assume a democratic society that is relatively free of conflict. However, unlike Parsons, they assume that unbiased technical change comes from the efficient aggregation of individual preferences. Yet, in many if not most Third World nations, open debate and discourse hardly exists; even if it did, Ruttan's individualist approach may be challenged as an inadequate rendering of technical change. Recent studies of Korea by Steinberg et al. (1981) and especially Burmeister (1988) note the importance that force and conflict play in technical change. Nor should we view democracy as something that merely comes about. Democracy itself is hardly a universal trend but something that must be fought for and won.

3. Ruttan sees the dependency and world systems approaches to political economy as "bad economics" and wonders how sociologists could embrace it. (His explanation for this phenomenon is curiously sociological in character.) Yet, while it has been abandoned by mainstream American economists, it is still alive and well elsewhere. Moreover, both Marx and Smith saw political economy as at the center of the debate over production. The mainstream of economics has abandoned or redefined political economy only by avoiding the central questions it posed—not by subsuming it into some broader paradigm.
4. Ruttan repeatedly chides sociologists for being too willing to engage in policy debates instead of building the theories their discipline so dearly needs. Meta-theory, in particular, he feels has been abandoned. Two points need to be noted here: First, the founders of both disciplines were

committed to issues of development and policy; their theories were built in response to those issues. Hence, theorists as diverse as Marx, Smith, Malthus, Weber, and Durkheim were both theorists and actively involved in politics. Second, Ruttan's stressing of theory belies his own policy concerns. Theory for Ruttan is, rightly, a way to get to policy.

5. Ruttan is certainly correct in stressing the importance of the micro-macro issue. This issue has plagued sociologists and economists for years. Yet, to some degree the issue is an artifact of the theoretical frameworks we have developed. On one level, as Boltanski and Thévenot (1990) have noted, Durkheimian sociology and neoclassical economics presuppose each other. Neoclassical economics assumes that individuals share preferences so as to create demand. In contrast, Durkheimian sociology assumes that its collectivities consist of individuals. On another level, as Rasmussen (1973) and Knorr-Cetina and Cicourel (1981) have argued, the human condition is such that we are between autonomy and sociality, neither fully socialized nor fully individuated. Thus, the micro-macro issue is in many respects more a matter of the way theories treat people (i.e., the simplifications they make) than it is a problem in the nature of the world. This brings me to the last issue.
6. Ruttan laments that questions of metatheory have been replaced by those of policy since the demise of Parsonian theory. Yet, what about the work of Giddens, Bourdieu, Touraine, Habermas, Coleman, and even Alexander? One can hardly call this neglect. What has disappeared is the artificial consensus that reigned in the early years of Parsonian sociology; sociology is clearly better off for that.

(NO) Conclusions

In short, I must agree with Ruttan's assertion that

the sociology of development is in disarray. Its disarray reflects the disarray that the world itself is in today. Yet, I would argue that the solution to theoretical problems we face lies not in the development of bridges to economics. Instead, it lies in the removal of the distinctions among the social sciences—perhaps the human sciences [Geisteswissenschaften] is a better term—so a new philosophically and historically grounded social science might emerge that encompasses human societies in all their complexity. Such a science will surely make some use of mathematical models. It will surely subsume much of the literature of the existing social sciences. But as surely as oxygen is not phlogiston, such a new social science will not be either economics or sociology warmed over.

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RESUMEN

Metateorías y Mejores Teorías. Una replica a Ruttan

El intento interdisciplinario de investigación llevado a cabo entre la Economía y la Sociología ha sido más bien problemático, en la suposición de que las dos disciplinas, el empleo de métodos y las conclusiones logradas pueden llegar a ser bastante diferentes. Vernon Ruttan, en su artículo "La sociología del desarrollo y del subdesarrollo: ¿Tiene lecciones para los economistas?", realiza un intento por puntualizar cuáles son las potenciales contribuciones de los sociólogos que utilizan la teoría de la modernización, la teoría de la dependencia y la teoría del sistema mundial, para los economistas. El siguiente artículo es una respuesta a ese esfuerzo. Se examina el artículo de Ruttan en dos sentidos: en primer lugar se consideran las diferentes metateorías, subrayando los supuestos de los dos tipos de aproximación. En segundo lugar, se examina el punto en disputa levantado por Ruttan como un esfuerzo por magnificar las similitudes y diferencias entre los dos. Se concluye, que esta distinción no es necesaria entre las ciencias sociales para la generación de una ciencia social que abarque a las sociedades humanas en toda su complejidad.

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