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Discussion on Social Capital

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At a time when economists have been accused of being imperialistic for attempting to infiltrate other social sciences, we should not be too surprised by attempts to incorporate other social sciences into economics. The Lynne paper is clearly an effort in this direction. The other two attempt to reform or broaden the mainstream neoclassical model.

The organizers of this session presumably knew what they were doing in inviting me to discuss these papers, but I am dubious. I doubt that it was because I have a reputation for discovering major flaws in the neoclassical model and thus would readily accept the arguments presented in these papers. My Chicago training and published work would suggest that hypothesis to be a little far-fetched. On the other hand, I am loath to admit that I am incapable of understanding or valuing anything "new" or "modern." A final hypothesis is that I might be expected to enliven the meeting, and if that is the reason I am here I will try not to disappoint.

What is primarily at issue here is a squabble over methodology, an old pastime for economists. The crucial question is: are the models in these papers, particularly Robison-Hansen (R-H) and Schmid-Robison (S-R), meant to be an extension to the neoclassical paradigm, or are they a revolution in the Kuhnian sense of one paradigm attempting to replace another? The Lynne paper can hardly be considered anything other than a revolution.

A few years ago a California colleague and I were invited by the editor of *Rural Sociology* to give our impressions of sociology as a social science (Gardner and Nuckton 1984). We looked at a few of the books that sociologists considered to be mainstream. We then used Thomas Kuhn's model of scientific revolutions to inform our appraisal (Kuhn 1970). Briefly, Kuhn's view is that a science is considered mature when it settles on a single paradigm that transforms researchers into a profession. The synthesis achieved by reaching a consensus on a paradigm defines the field and creates the mechanism by which scientific progress can be made. Participant researchers perform what Kuhn calls "mop-up" work--articulating the paradigm and extending it to other areas of experience. It is not part of the research agenda of a mature science for members to challenge the established paradigm, for it is the paradigm that determines the scope of the view of the world, identifies the crucial problems, and sets the bounds for prescribing a better condition for the world. We argued that the neoclassical paradigm that has ruled economics qualified the discipline as a mature science. Sure, the Marxists, the Austrians, and the institutionalists have at various times proposed alternatives to and modifications in the basic paradigm, but their followers mostly have been barking at the edges of the discipline campfire, although I am sure that many would dispute that conclusion.

By contrast, sociology even prides itself as having several competing paradigms such as models of order, conflict, and interactionism. The title of a popular book by Ritzer (1975) refers to sociology as a multiple-paradigm science; thus, if true, qualifying sociology as an immature science in Kuhn's structure.

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Now, does the broadening of the neoclassical model, as proposed by these papers, constitute an alternative and competing paradigm, and thus lead us from a mature to an immature science, or does it simply enrich the basic neoclassical paradigm to make it more robust? If the former I would strongly resist this broadening, and if the latter, I would give mild support providing that the analytical power of the paradigm is not lost.

The reason for my limited support is an empirical observation that paradigm modifications have not been very successful in the past. It was primarily this same issue which arose when Robinson's imperfect competition, Simons' satisficing, Leibenstein's X-inefficiency, and Williamson's bounded rationality were introduced to the profession. They all had the appeal of rejecting certain assumptions of the competitive neoclassical model that appeared to be inconsistent with some behaviors of real-world actors. The problem was that, for the most part, all were generally incapable of generating falsifiable hypotheses about behavior, and therefore, have lost ground scientifically. Would the same be true of meta-preferences, we-utility functions, and social capital? Would they have a short period of at least partial acceptance by many in the profession, and maybe even find their way into some textbooks, only to be later abandoned as analytically sterile? In my view no one knows for sure.

The recognized essential core of the neoclassical model is optimization utilizing the assumptions of rational decision makers with stable preferences who are active maximizers of something such as utility, wealth, profit, or revenue. Schmid-Robison's (S-R's) view of social capital as relationships among people being an extension of human capital in my mind, but maybe not in theirs, places it squarely in the neoclassical framework. The reason for any ambiguity is that their position is not always clear. The assumptions of the neoclassical model appear to be explicitly maintained in their rational model, and almost throughout the R-H paper, and the positive methodological task of developing and testing hypotheses appears to be utilized. This is not quite so clear, however, when the discussion turns to firms, but to the extent that their model employs optimizing and rationality, the scientific nature of the methodology is in the core of economics rather than sociology. Therefore, I would have been happier if this had been granted up front in the paper and the relevant literature cited to link up with human capital theory. Instead, there is this tendency to lash out at a neoclassical strawman while, at the same time, to preserve and utilize its essential core.

S-R's social capital (love, caring, sense of community, sympathy, guilt, and hatred) resulting from investment is not alien to the Stigler-Becker so-called new home or family economics, where the production function of the optimizing family includes outputs consisting of marketable and nonmarketable commodities, and where inputs consist of all scarce resources available to the family, including time and productive services as traditionally defined. However, social capital as employed in the S-R paper is quite explicit in specifying the kinds of relationship that are alleged to be important, and therefore, is a useful extension of human capital theory as traditionally understood.

The social capital models in both S-R and R-H are not unrelated to Becker's altruism model so long as they retain the assumption of rational maximizing actors with stable preferences. It is when S-R stray over into the area of what they call a-rational behavior that they depart significantly from neoclassical assumptions. They assert that custom and habit may not be rational activities. However, Stigler and Becker (1977) accounted for this nearly two decades ago by arguing that custom and habit are usefully modeled as rational phenomena. They are a means of economizing on resources devoted to repeated search and knowledge acquisition. The appeal of regarding them this way is that they clearly come under the domain of economics rather than psychology or sociology.

I don't think it has to be demonstrated that social capital as defined by S-R and R-H exists or has a role in decisions. Perhaps our colleagues felt they had to demonstrate as much in order to prove the legitimacy and need of their paradigm extension. The question for me is how important is social capital empirically, i.e., how much of actual behavior can be explained by its use that cannot be accounted for without it? Thought experiments in used cars, catastrophic risks, and bank loans, rather than actual trades where prices and opportunity costs can be assessed are not very convincing. There may be social capital investment by students and loan officers in giving professors and community residents what they want to hear, as long as the tradeoff is hypothetical and thus costs them nothing in sacrificing real wealth. It reminds one of the Jordan-Tweeten (1987) finding that most people across a wide spectrum of demographic, economic, social, and political characteristics are highly approving of government programs supporting family farms as long as they don't raise the price of food.

It is plausible that an owner of a used car might benefit by investing in social capital by selling the car to someone he/she likes, or why landlords who are altruistic toward certain tenants might benefit by accepting less than maximum rent. But why should they prefer a share lease to a cash lease to make this transfer? Is it somehow less costly or more acceptable socially to accept a lower share of an uncertain income stream than granting a cash rent rebate? Surely a former farm operator, now a landlord, could offer specialized knowledge of the farm to benefit a tenant under a cash lease just as well as under a share-lease arrangement. If the tenant is in a position to degrade the land, and it is costly to monitor his practices, the landlord may not want to give him an incentive to maximize yield which may be more likely with a cash lease than a share lease. But this is simply the traditional explanation for share leases, and I can't see clearly the link with social capital.

Problems are faced by firms in a competitive environment that may constrain their preferences for relationships that do not exist with consumers. If the banker is more likely to grant a loan to someone he knows well socially, it may be because he likes the person because of investment in relationships (social capital), or it may be because his knowledge of the borrower's character is helpful in determining the likelihood of default. I gather the former result supports the social capital thesis, but the latter does not. It is asserted that the goal of bank advertising was equally to let people know about bank services and that the bank cares about them. But what does "caring" really mean? "Care" might mean that the bank wants them as customers to increase profits. Or it might "care" because it has invested in social capital with customers and is willing to transfer real resources to them in the loan terms or in providing community services. Are both investments in social capital? The first "caring" is entirely consistent with normal profit-maximizing theory, but the second appears to be inconsistent with it. In any case, if social capital is the second type primarily, then S-R must explain how the bank can survive in a competitive environment in the long run? They would have to allege imperfect capital markets, monopoly rents for bankers, scale economies, or some other reason why high-cost firms can remain competitive with lower-cost firms.

There seems to be an presumption in all these papers that utility maximization and self-interest or selfishness are equivalents in the neoclassical model, and since real people are not always selfish the model is flawed and must be reformed. But Gary Becker, everybody's quintessential neoclassical imperialist, argues in his book *Treatise on the Family* (1981), as well as in other papers, that interpersonal relationships such as family love and altruism, and other opposites of selfishness, are arguments in the maximized utility functions of individual decision makers who benefit from making trades. His "rotten kid theorem" is a vivid example of a malevolent person who benefits from the altruism of his parents. Are there economists these days who would deny that these types of relationships matter in important trading decisions? But there is nothing here that suggests that the assumption of utility-maximizing behavior should be abandoned analytically.

R-H also claim that the neoclassical model can be improved by more carefully defining the utility function. Quite possibly! They argue that in practice the maximands are wealth, revenue, or profit rather than utility. Indeed, this is because these variables, often used as proxies for utility, are observable and quantifiable magnitudes that conduce to unambiguous analysis and prediction. I welcome the effort of R-H to more rigorously specify and enlarge the utility function to be maximized by adding social capital, providing they can monetize and quantify its contribution so as to permit scientific hypothesis testing.

But what can we really learn from the experiment with students dividing hypothetical time to joint and individual study projects where team members are friends, strangers, or cheats? It would be difficult to conceal from these students that the purpose of the experiment was to determine the value of friendship. What person would say that they do not prefer to work with friends, especially if it didn't cost them anything. Why not do a real experiment where real resources must be sacrificed? Experimental economists have been doing this for a long time. Regardless, it is not clear to me that because people prefer friends to cheats that the neoclassical strawman is somehow flawed?

A question for R-H? They quote Calonius on the importance of family businesses, at the end of which he points out that most don't survive into the second generation. If social capital is so important and contributes to a successful business, why don't families invest to assure that the family business succeeds in perpetuity? Could it be that investment in social capital diminishes profits and therefore forces exit from the industry.

Let me now make a few comments on the Lynne paper specifically, since it most explicitly rejects the neoclassical synthesis. Gary asserts that "attitude" in social psychology is a close kin to "utility" in economics. Perhaps Gary can tell us what it would mean, even in principle, to maximize attitude? He tells us that social norms matter. But who would dispute that? The question is how they matter and what is their analytical importance in explaining individual behavior? One can try to incorporate them in the utility function of optimizing and rational individuals in order to predict their behavior, or one can attempt to predict behavior in some other way. This dilemma may be illustrated by Lynne's example of whether to adopt a soil conservation practice. Hirschman's (and presumably Lynne's) way is to postulate alternative preference functions, one reflecting only profit and the other Hirschman's meta-preference function. The decision on adoption will depend on which one wins out, to use Lynne's words. But is this a falsifiable hypothesis? How can it be without specifying quantitatively which function yields the greatest utility? or income? or profit? or something? About the best one can do in Lynne's framework is to argue, as he does, that if the decision is adoption the evidence supports the meta-preference function and is against the profit function. But unless it is clear that the adoption is "profitable" and is then rejected, there is no test of a profit hypothesis.

The alternative, it seems to me, is to try to incorporate both profit and other "benefits" into a single framework where benefits and costs are aggregated in good old-fashioned neoclassical ways. Then the hypothesis that adoption will occur if the benefits exceed the costs is unambiguous and clearly capable of falsification. If a "beneficial" project is then rejected by the decision maker, then it may prove useful to begin a search for missing benefits or costs and test again.

Lynne's parable of the strawberry grower who is a "conservationist at heart" but chooses the old technology because it is profit-maximizing is illuminating. What is meant by a "conservationist at heart" and how do we know he is? Because he said so? Lynne argues that the meta-preference was not sufficient to override the profit preference. But the farmer may have realized that he had no alternative in a competitive world--he maximizes profits and competes while claiming he is a conservationist, or he really becomes one and exits the industry. Economists have known about this class of problem ever since the famous Machlup-Lester controversy over whether businessmen really use marginal analysis in their decisions. Lester (1946) argued that they didn't even know how to define marginal cost, so how could they use it? But Machlup (1946) countered that they act "as if" they know, and that is sufficient. The assumption that they would equate things at the margin was therefore a useful analytical simplification. To really know if farmers care about conservation, do you simply ask them? They probably know what it is, but it costs them nothing to say "yes, I care" especially if they can cultivate good will by giving the politically-correct answer. But predicting their behavior is another matter. And it is far from clear to me that even multiple regressions containing mostly hypothetical attitude questions reveal anything much about actual behavior where tradeoffs must be made.

In summary, we have before us an old problem in the methodology of economics: increased complexity leading to scientific sterility. When decision makers do unprofitable things, it is easy to allege model error and assert that there are meta-preferences, or pleasure and moral utilities, or different values that drive people, and end the inquiry. You might even show that WE preferences matter statistically. But this doesn't get us very far in testing hypotheses and making predictions. This is why Stigler and Becker (1977) argued in a classic article that "tastes are not in dispute." If we find actors doing uneconomic things, the scientifically fruitful approach for economics is to look harder for relevant arguments in a single utility function that is assumed to be maximized. Time and non-market activities with their relevant "shadow prices" usually produce the missing links. The fruitful extension of the neoclassical economic model to analyze problems thought to be the domain of the other social sciences is proof of its scientific robustness.

I close by again complimenting our colleagues for producing three provocative papers that have clarified for me some important issues, such as the importance of investing in social relationships. Will the effort produce lasting good? Maybe it is disciplinary inertia, maybe it is too costly to move out of the rut of the familiar paradigm. I guess my prediction is that for most of us in the discipline, the effort will be evaluated as a methodological detour that would only lead us down a path toward scientific obscurity.

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