The Empirics of Social Capital: Some Skeptical Thoughts

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I. Introduction

This discussion presents some arguments concerning the use of social capital as an organizing principle in understanding socioeconomic processes. My views on this subject are somewhat idiosyncratic in that while I am in the category of economists who are extremely sympathetic to the enrichment of economic analyses with sociological ideas, I am quite skeptical of many of the claims that have been made about social capital. One reason for this skepticism is that in its conventional usage, the concept of social capital suffers from significant conceptual vagueness. Another reason is that efforts to establish the empirical salience of social capital (once an empirical version of social capital has been defined) have generally suffered from serious identification problems. Hence, while at the level of descriptive theory, social capital ideas seem very appealing, my own view is that empirical efforts to demonstrate the importance of social capital have largely been failures.

I will first address some of the sources of my skepticism and then make some suggestions for future work.

II. Two problems

My reading of the literature suggests that from the perspective of theory, the concept of social capital is very ill defined. A typical definition of social capital is that used by Ostrom (2000):

"Social capital is the shared knowledge, understanding, norms, rules, and expectations about patterns of interactions that groups of individuals bring to a recurrent activity…When they face social dilemmas or collective-action situations…participants must find ways of creating mutually reenforcing expectations and trust to overcome the perverse short-run temptations they face." (pg. 176)

As argued by a number of authors (e.g. Dasgupta (2000), Durlauf (2002), Portes (1998)), using concepts such as trust and cooperation to define social capital is problematic for several reasons. One problem is that this can lead to functional and hence nonfalsifiable claims concerning social capital: whenever cooperative behavior or benign attitudes are observed, social capital must be the explanation. (Functional explanations also impose the false assumption that social capital is always benign. I have no doubt that social capital, as conventionally understood, was high among members Ku Klux Klan.) Another problem is breadth; so many disparate phenomena can be linked to trust and cooperation that little discipline is imposed in using social capital in a given context. Yet another problem is that trust and cooperation can arise for reasons that are very different from the civic community ideas associated with social capital. For example, trust can follow from trustworthiness, which can stem from ethical values that have no social component. Alternatively, are draconian legal penalties a form of social capital if they deter noncooperative behavior?

These definitional problems do not disappear when one moves from descriptive theory to statistical analysis. Statistical analyses, of course, require precise definitions of variables that correspond to social capital, so one might think that the ambiguity I have referred to must disappear, but this is not the case in the sense that serious identification problems exist with social capital models.

How is the influence of social capital typically assessed? Standard practice in economics and sociology is to run regressions of some outcome of interest against a set of controls and some asserted empirical proxies for social capital. These regressions are often justified by an informal argument that the empirical proxy act as instrumental variables for the unobserved "true" social capital measure. At one extreme, one finds analyses such as Furstenburg and Hughes (1995) in which the probability that an individual drops out of school is related to variables such as the presence of a father in the household or the educational aspirations of the person's friends. At another, analyses such as Knack and Keefer (1997) attempt to explain cross-country growth differences using survey measures of trust.

In Durlauf (2002), I provide a detailed discussion of the econometrics of such papers. The basic message of this evaluation can be reduced to some basic criticisms. These criticisms represent generalizations and I can well imagine individual authors claiming they do not apply to their own work, but the claims seem fair given my understanding of the literature.

First, social capital studies fail to distinguish between social capital effects and any other influence of group characteristics or behaviors on individuals. There is no shortage of reasons why group memberships influence individuals. For example, in recent models of income inequality, primary emphasis has been given to peer group and role model effects as influencing educational outcomes for youths. The problem is when one claims that there is a social capital effect and uses a group level variable to measure it, this claim will not be credible unless one is able to argue that the group-level variable is measuring social capital versus some other group level effect. This is one of the places where the definitional vagueness of social capital has empirical consequences.

To see how this matters, consider the use by Furstenberg and Hughes of friends' educational aspirations as a measure of social capital. One can at least equally plausibly argue that this variable measures a psychological proclivity to imitate one's peers. So, ambitious friends lead an individual to put greater value on behaviors that facilitate success. Alternatively, if aspirations are influenced by differences in available educational opportunities, then the variable could be proxying for heterogeneity in opportunities and therefore reveal nothing about group influences per se. In other words, there is ambiguity with respect to the meaning of the empirical proxy.

It should not be thought that the problem of distinguishing different types of group effects is resolved once one has added additional regressors to a statistical model of social capital in order to proxy for the alternative sources of group influences. As shown in Durlauf (2002), using ideas developed originally in Brock and Durlauf (2001) and Manski (1993), there exist potential identification problems in regressions that incorporate a full range of alternative group influences. Intuitively, the problem is that the various group influences that are endogenously determined will be correlated with each other and with those group influences that are predetermined. While identification is possible, it requires prior information on what variables influence the various individual choices under study.

Second, social capital proxies are typically endogenous and the use of instrumental variables to account for this is based on ad hoc exogeneity assumptions. In some cases, this is obvious; when one talks about membership in organizations, it is obvious one must account for the fact that these memberships are choice variables. In other cases, the endogeneity problem is more subtle. For example, Narayan and Pritchett (2000), using village level data, argue that measures of village level trust can instrument for memberships. Yet, there is no reason that such a variable is a valid instrument. Trust is presumably related to trustworthiness, and I see no reason why trustworthy behavior is any different than membership in an organization in terms of whether it is a choice variable. And without a theory of what determines trustworthy behavior, there is little hope of constructing credible instrumental variables for it in these types of regressions.

Third, social capital regressions rely on untenable comparability assumptions about observations. Consider the regressions employed in Helliwell and Putnam (2000) to show the effects of social capital on economic growth. These authors regress regional output growth in Italy against initial output and measures of civic community, institutional performance, and citizen satisfaction. They find these three measures explain persistent differences in regional growth rates and conclude this supports social capital explanations of economic performance. Among the many questionable assumptions that underlie such a conclusion is the assumption that the regression they employ is using comparable objects as observations. In other words, the analysis assumes that each observation is generated by a common growth process. What must be assumed about the growth process in different regions when one includes Northern and Southern Italian regions in a regression? One answer to this question is that one must assume that given the variables included in the regression, the errors for the observations of different regions cannot be distinguished, at least from the perspective of their distributions. Put differently, one must assume that the regression is such that there is no reason to expect that the error from a particular region has a nonzero expected value, for example. But how can a regression of this crudity make such a breathtaking claim? The historical and social science literatures give any number of reasons why this assumption is false in contexts such as Italian regimes. But if the assumption is false then one cannot defend the interpretation provided by Helliwell and Putnam for their regression results.

As argued by Brock and Durlauf (2001), the appropriate notion of comparability of observations can be interpreted as a requirement of exchangeability of the residuals in regression errors. One reason why an exchangeability violation can occur is from omitted variables; another source of a violation can be neglected nonlinearity. Nothing in Helliwell and Putnam (2000) allows one to credibly believe that their particular growth regressions are immune from these sorts of violations. Their analysis gives very little scope to omitted heterogeneity in the regional growth process. Similar criticisms may be made of Knack and Keefer (1997), see Durlauf (2002). In my opinion, every study I have seen of social capital using aggregates such as states, regions, or nations suffers from this criticism.

III. Suggestions

In light of these criticisms, my conclusion is that empirical work on social capital will require a considerable restructuring if it is to have lasting value.

First, empirical analyses need to step back from grandiose approaches to social capital and focus on the more mundane but potentially far more fruitful task of analyzing specific sociological components to individual behavior. This does not require abandonment of social capital as a general organizing idea, but rather means that evidence in favor of social capital should be derivative from specific claims about social influences on individuals. For example, despite my criticism of Ostrom's vague definition of social capital, her empirical studies of how different societies have coped with various collective action problems are very valuable in terms of describing and analyzing very well defined environments.

A useful contrast may be made between the Helliwell and Putnam (2000) paper and a recent study by Glaeser, Laibson, Scheinkman and Soutter (2000). Rather than run regressions that make incredible assumptions about the exchangeability of regional growth rates, Glaeser et al construct well crafted experiments to see how attitudes and background characteristics influence the choice of strategies in various economic experiments. In the context of these experiments, notions such as trust are quite well defined since it amounts to expectations about the play of other agents in the game. This very clean environment provides much more compelling evidence of how trust influences behavior than can be obtained from ad hoc regressions. To be clear, economic experiments are not a panacea for the limits of inference with observational data. One problem is generalizability; it is far from clear how behavior in economic experiments maps into behavior in the larger economy and society, although Glaeser et al make an important advance by attempting to correlate behavior in experiments with behavior in the "real world" by participants. Further, as discussed by Manski (2001) in an important recent paper, there are identification problems in experiments as it is often difficult to distinguish behavior that is driven by altruistic preferences from behavior driven by selfish preferences but with expectations of trustworthy behavior by others. Nevertheless, Glaeser et al is an important advance in the social capital literature.

If nothing else, moving the discussion of social capital away from generalities to specific mechanisms in the way I suggest will allow one to deal with issues of endogeneity and exchangeability much more effectively, since it will facilitate far more precise and comprehensive modeling of causal mechanisms than one finds in the social capital literature.

Second, I believe that there needs to be greater recognition of the limits to statistical analysis in contexts such as the evaluation of social capital. This is partly a restatement of the first suggestion in that I do not believe that anyone in social science has developed anything close to a persuasive way to evaluate broad claims of the sort one finds in the social capital literature. In contrast, I believe economists need to be much more receptive to the sorts of evidence found in other disciplines. For example, sustained descriptive histories can teach us much about the ways that social structures influence individual conduct even if they are not constructed in the form of claims about F-statistics and the like. At the other extreme, there is a wealth of information in the social psychology literature that addresses in precise ways the inchoate ideas about individual behavior that underlie the social capital literature. This suggestion requires greater openmindedness on the part of economists to qualitative sources of information.

In fact, I would be tempted to argue that the question of how social capital has facilitated socioeconomic or political development should be treated in the same spirit as questions such as how has religion influenced development. These are not meaningless questions; but it is necessary to accept limits as to the precision with which such questions can be answered and what it means to say the question has been answered. Perhaps the problem with much of the social capital literature is the effort to make (nearly) monocausal claims about vast complicated phenomena rather than nuanced claims about its contributory role among many factors.

IV. Conclusions

In these notes I have tried to describe why the empirical social capital literature seems so unpersuasive. The failings of this empirical literature are especially unfortunate, since the work in social capital is an active front in which the "undersocialized conception of man" for which economics has been criticized (Granovetter (1985)) is being addressed. However, attempts to provide social richness of economic analysis will only succeed if the empirical work that accompanies this effort is subjected to the same rigorous standards that are required of other empirical analyses in economics. In contrast, the extravagant claims so often found in this literature (an outstanding example of which is Putnam (2000) who appears capable of attributing every conceivable societal virtue to social capital) have no prospect of having lasting social science value. What is needed is more matter and less art.

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