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Social Preferences in Small-Scale Societies

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SOCIAL PREFERENCES IN SMALL-SCALE SOCIETIES

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A review of
FOUNDATIONS OF HUMAN SOCIALITY: ECONOMIC EXPERIMENTS AND ETHNOGRAPHIC EVIDENCE FROM FIFTEEN SMALL-SCALE SOCIETIES.

Edited by Joseph Henrich, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, and Herbert Gintis. Oxford and New York: Oxford University Press. \$98.00 (hardcover); \$24.95 (paper). xix + 451 p; ill.; index. ISBN: 0-19-926204-7 (hc); 0-19-926205-5 (pb). 2004.

This volume reports on a cross-cultural investigation of social preferences in 15 small-scale, non-Western societies. Participants from all 15 groups played the ultimatum game with members of their own culture; subjects from a subset also played dictator and voluntary contribution to public goods games. The bulk of the book (Chapters 4 through 14) consists of reports by the field workers (mostly anthropologists). Each chapter includes ethnographic information, a de-

scription of how members of the group make their living, details on the experimental protocols and results, and some discussion. Although none of the results are consistent with the predictions of the standard rational choice model (as has been true in earlier work), group average offers and rejection frequencies in these experiments display more variation than has been observed in experiments using university students from developed Westernized societies. The editors report that little of this variation can be accounted for by individual economic or demographic variables, such as gender, age, education, or wealth. On the other hand, group dummies account for quite a lot, and social preferences seem to be stronger in groups experiencing greater market integration or whose economic mode offers greater opportunities for gains from cooperative enterprise.

The book is edited by two anthropologists—Joseph Henrich and Robert Boyd—and four economists: Samuel Bowles, Herbert Gintis, Colin Camerer, and Ernst Fehr; the latter two are also well-known experimentalists. Chapter 1 provides a brief introduction to the project and discusses its origins and funding sources. Chapter 2, coauthored by all six editors and one of the chapter authors, provides an overview, synthesis, and interpretation of the results. Chapter 3, by Camerer and Fehr, reviews the basics of noncooperative game theory, describes the structured interactions (games) that have been used to measure social preferences, and discusses attempts by theorists to modify standard utility functions to take account of many of the new stylized facts.

Since possible differences among the editors are not stressed in the overview chapter, the remainder of this review focuses on tensions (some implicit) over what emphases to place on these results and how they are to be interpreted. At the end of the introductory chapter, the editors state that “no effort has been made to produce a unified theoretical interpretation. There is no party line here, and some authors suggest interpretations that are quite different from those of the editors” (p 7). And at the start of Chapter 2, they acknowledge that “[b]oth theoretically and

methodologically our results pose more questions than they answer” (p 10). Still, in the absence of minority or majority reports, one is led to wonder whether there is, after all, perhaps a party line among the editors. My conclusion is that there is not. There are some fault lines, although one has to read closely to identify where they lie.

The first, and probably most significant pinch point is the question of whether or not we can meaningfully talk about a universal human psychology and, if so, whether these types of experiments can tell us something about it. Some of the authors are sympathetic to this possibility. For example, Kim Hill and Michael Gurven speak of the possibility of an “evolved tendency to cooperate that is unique to our species” (p 382). But other authors may not be so sympathetic, and one suspects there are differences among the editors as well. As a rule, anthropologists are prone to be skeptical: their stock in trade, after all, has been the documentation of behavioral diversity among human groups.

Traditional economic theory, on the other hand, can be interpreted as sympathetic to the idea: it has tended to assume that most individuals have a similar set of underlying preferences. The rational choice approach need not be, but often it is paired with the assumption that these preferences are selfish in the sense that in most arenas we act so efficiently as to advance our material self-interest. This leads to what one can call the canonical selfish actor rational choice approach, the validity and plausibility of which has often been reinforced by a narrow interpretation of what economists believe Darwinian selection could have allowed.

In the last couple decades, a growing body of experimental research has gone beyond investigations of the cognitive underpinnings of the model to challenge its motivational (selfish actor) component. These include studies of behavior in prisoner’s dilemmas, voluntary provision of public goods experiments, ultimatum and dictator and trust games, as well as games involving opportunities for third party punishment. Collectively, these results have suggested a set of species typical predispositions at variance with those traditionally assumed by economists. These include pro-

pensities to cooperate when theory says we should not, and to punish others when theory says we should not.

Economists typically make an exception to the assumption of universal selfishness when modeling behavior within the family. The range of allowance for altruistic behavior within the field has roughly coincided with that explicable within biology as the consequence of Hamiltonian kin selection. But the experimental results involve behavior among anonymous subjects who are not close relatives and, thus, present challenges to the model that range far beyond the phenomena of parents sacrificing for children. The experimental interactions are, moreover, typically one shot, in principle precluding appeal to repetition or concern with reputation in explaining why people behave the way they do.

Work in this volume extends this experimental research beyond the typical subject pool of university students in developed countries. The editors take pains to emphasize that it remains the case that none of the results are consistent with the predictions of the rational choice approach. They list this point first in their summary of results (p 10) and repeat it several times thereafter.

But the runner-up emphasis is on the variability of results among groups. The message here is that culture matters; the implication is that scholars may make big mistakes if they generalize from experimental data on Western university students in forming predictions of behavior in other social groupings.

There is an important tension between these two emphases. The main statistical conclusion emerging from consideration of the experimental results as a whole is the low explanatory power of individual-level economic or demographic variables as opposed to group dummies in explaining variation in individual behavior. After appropriately ruling out differences in group genetic averages as the explanation for this finding, the editors, and most of the authors of the field reports, interpret these differences as cultural, reflecting group-level differences in learned beliefs and norms.

Before considering the implications of this interpretation, let us go back for a moment

to the original body of experimental work on social preferences. There continues to be a real struggle to gain acceptance for this research, one that involves educating skeptics about the aims and successes of experimental methods. The power of the approach is that it offers a means of controlling for the embeddedness of most observational data in repeated interactions where issues of reputation matter. In spite of this, it is common to hear behavior in ultimatum games, or one shot PD experiments, dismissed on the grounds that subjects do not fully grasp that they are in a one shot situation. As a result, it is claimed, they import into the experimental context heuristics and behavioral rules that have served them well in nonexperimental contexts. Stated alternately, the criticism is that the intended controls for repetition and embeddedness do not really work.

This style of dismissal remains common in spite of the fact that experiments by Fehr and Fischbacher (see page 79 of the volume under review for description) have clearly refuted the hypothesis that subjects cannot or will not recognize when they are in a one shot situation and adjust their behavior accordingly. If the hypothesis is left standing, however, one is led to conclude that this type of experimental research tells us little about innate species typical behavioral predispositions. A corollary is that the corpus of work no longer poses a challenge to the canonical model, whose motivational assumptions can continue to be "confirmed" by armchair intuition and a limited reading of Darwin.

Return now to the emphasis on group differences in this volume. To say that variation in behavior is a cultural phenomenon is to say that members of different groups bring to experimental situations different heuristics or rules of behavior that have served them well in nonexperimental circumstances within their society. For example, to make hyperfair offers because one lives in a society of aggressive gift giving where the object is not altruistically to transfer wealth but selfishly to put others in your debt, or to reject all offers because of concerns that it may create obligations to others, must mean that you, as an experimental subject, do not fully understand that you are in a one shot anonymous inter-

action, or understand but are unwilling or unable to alter your behavior, or are “mindlessly” applying heuristics from life outside the experiment.

Note that the explanations offered for the group differences are very similar to the arguments used by skeptics to dismiss the original body of experimental work. The evidence is pretty clear that the skeptics are mistaken in their critique as it applies to university student subjects. But perhaps the claim is right for these new studies. What would that mean? It would mean that the experimental methods had been unsuccessful in one way or another in controlling for reputation and repeated interaction. Some of the larger group variance would reflect noise that might be reduced with retests and refinements of experimental protocols.

These experiments are in some respects curious instruments with which to document cultural differences, since if we accept that there are no significant genetic differences among (as opposed to within) human groups, then persisting group differences in behavior in these games reflect partial failure of our methods. If one wants to study cultural differences, why use tools that were originally intended, at least in part, to abstract from them?

The interpretation of the variance in group averages as cultural poses something of a problem for behavioral economists. If the variation is a purely cultural phenomenon, what is to stop us from concluding that the average level of behavior across all subject populations—what one might take as an estimate of species typical predispositions—is also an entirely cultural phenomenon? But if that is so, then the power of the large body of experimental literature referenced above to challenge the selfish actor part of the standard economic model largely disappears. We are back to arguing that we are innately selfish in all arenas save those in which kin selection operates, and it is only a thin veneer of culture and civilization that keeps us away from each other’s throats.

One is led to ask, therefore, whether this research provides marginal enhancements to an existing body of experimental research, or a significant challenge to it. If one emphasizes

the primary conclusion, that none of the results are consistent with the predictions of the canonical model, one gravitates toward the first conclusion. But if one focuses on the second emphasis, that of differences in group averages explicable as the consequence of cultural variation, one may be inclined toward the second.

Since economists have generally been skeptical both about the potential influence of culture on behavior (and, more generally, its utility as an explanatory variable) and about the possibility that humans might have some biologically altruistic predispositions beyond those expressed within the family, either conclusion leaves a challenge on the table for the canonical approach. But the nature of the challenge in the two cases differs. In the first instance, it requires acknowledging the role of culture in resolving the problem of social order, and in sometimes fashioning different solutions to it. In the second, it requires relaxing somewhat the selfish actor assumption, and allowing for a set of innate prosocial predispositions upon which culture builds. In the first case, prosociality inheres entirely in information acquired after birth stored in brains. In the latter, it inheres in part in a set of species typical genetically mediated behavioral and cognitive predispositions. The cognitive component may involve differential preparedness to learn in certain directions, as is the case in the acquisition of language, helping to account for some of the universal features of human culture.

The possibility that we have these inclinations is, of course, related to the continuing debates about the role of higher-level (group) selection in molding human psychology. Most readers of the *QRB* are familiar with the history of this contentious issue: the widespread acceptance of the empirical importance of group selection through the first half of the 1960s, the attack by George Williams (1966) on Wynne-Edward’s work (1962), the emerging consensus over the next two decades that group selection, if not theoretically impossible, required demographic conditions so unlikely as to render the possibility that it had any significant behavioral or morphological legacy close to nil and, finally, the efforts by David Sloan Wilson and others

to reintroduce the topic into respectable conversation (Wilson and Sober 1994). Wilson's development of models more resistant to the standard critiques than were those of Sewall Wright (1945) has made considerable headway in the last decade, causing dismissals of the topic to be more circumspect. But it is an understatement to say that group selection is still controversial: it remains the third rail of biological discourse.

What does this have to do with this research? One reason many economists and others have remained skeptical of the experimental results is that they believe that natural selection would have precluded any favoring of dispositions that might, upon initial appearance at low frequency, have been biologically altruistic within particular groups (see Field 2001 for discussion). Wilson's success in providing more robust models of how such traits could have survived, and thus suggesting a more elaborate range of mechanisms through which natural selection might have operated, forces practitioners of the canonical approach to reconsider this buttress for their skepticism about the experiments.

Wilson's progress is now, however, being subtly undermined by the propagation of what one might call the currently fashionable position. Adherents to this set of views (see, e.g., Richerson and Boyd 2004) argue that although biological group selection is theoretically possible, it has left no measurable behavioral or cognitive legacy on humans. They go on to say that, in contrast, cultural group selection has been a powerful force molding human behavior, benefiting from conformity norms and transmission through means other than from parent to child.

Cultural group selection, although it may use mechanisms (selection, mutation, drift) analogous to those used in models of biological selection, is not biological group selection, which results in changes in gene frequencies. Cultural group selection cannot have influenced the spread of prosocial genetic predisposers unless coevolutionary forces were sufficiently powerful to allow such change. Gene culture coevolution can plausibly account for the evolution of lactose tolerance in pastoral societies and the spread of genes in tropical areas that in the homozy-

gous form cause sickle cell anemia, but in the heterozygous form provide protection against malaria. But neither of these oft-cited examples involves genetic predisposers to prosocial behavior, and Richerson and Boyd (2004:244) come close to denying that it was possible for coevolutionary forces to favor such inclinations.

Aside from these difficulties, there is the question of what happened prior to 500,000 years ago. We have had the capabilities of developing and transmitting culture in ways that differentiate us from other animals only for perhaps a half a million years. Cultural group selection cannot have been a force before then.

By continuing to emphasize (cultural) group selection, the Boyd and Richerson position seems to acknowledge the inroads that Wilson has made in reintroducing group selection to the behavioral sciences. But, in reality, it drastically reduces the legacy of biological group selection, and thus has the potential to return the debate to where we were after the publication of Williams (1966). This in spite of the fact that Williams has backtracked considerably from his earlier positions, and now acknowledges a potentially significant role for biological group selection (see Williams 1992; Field 2006).

Among the editors of this volume, Boyd has been the most active in promulgating this set of views, but Fehr and Henrich here and in other works sign on to essentially the same position (Henrich was Boyd's student). Gintis and Bowles have been more ecumenical in allowing for the possibility of a legacy of biological selection, and Camerer's position on this issue is less clear.

Although there may not be a party line about the possible behavioral legacy of biological group selection, adherents to the currently fashionable position form at least a majority among the editors. This issue is critical in understanding the tension between the two main themes in the editors' interpretation of the results. Let us grant that the group differences are cultural. If we combine this with the position that there has been no empirically significant legacy of biological group selection, then we are inexorably led to the conclusion that the species typical tendencies

reflected in the average behavior of all experimental subjects is also a purely cultural phenomenon. We are thus almost back to thin veneer explanations of human sociality, and a denial that prosocial predispositions might have a genetic/biological substrate.

This position requires us to accept that our ancestors a half million years ago had no prosocial predispositions (since culture could not have been operative before then). This would have included the absence of any restraints on intraspecific harm, which most mammals exhibit. A half million years ago we were, according to this story, a savage set of beasts inclined not at but *before* the slightest provocation to tear each other to shreds. It was the fortuitous invention of culture that saved the day, particularly after we discovered how to make weapons from tools (see Lorenz 1966).

I do not find this position defensible, a conclusion I suspect is shared by the economist editors of this volume—Gintis and Bowles, if not Fehr and Cameron. Explicit discussion of the problem is, however, studiously avoided throughout the volume. The closest Henrich comes to acknowledging some noncultural substrate is to ask rhetorically whether there might not be “innate social grammars . . . for acquiring contextually specific cues about fairness, cooperation, and punishment” (pp 164–165). But there is no serious treatment of the mechanisms of natural selection that could have allowed such grammars to be favored.

These studies, and their interpretation, leave unresolved the question raised near the start of this review. Do we accept that there is a universal human psychology, a set of species typical behavioral and cognitive predispositions that are genetically and biologically mediated, upon which cultural norms build in ways that have some commonalities, but can also generate systematic differences in the average behavior of members of different groups? Or do we throw out the notion of universal human psychology, returning to a blank slate view in which the entire enterprise is driven by culture? This tension is reflected in the introduction where the editors describe their priors before Henrich ran the initial experiments on the Machiguenga: “Since

all of the previous experiments had been done using students from urban, literate, market-based societies, there was no way of knowing whether the social preferences at work were a part of human nature, or a consequence of the particular cultures from which subjects were drawn. It seemed likely to most of us that the social preferences were universal, but until somebody did real cross-cultural experiments, we wouldn’t know for sure” (p 3).

So, do we now know for sure? And do we know for sure that social preferences are *not* part of human nature? The logic of attributing differences in group averages to cultural differences will lead some to the conclusion that the population averages are also a cultural phenomenon, and thus that the original experiments on university students do not reveal basics of a universal human psychology, and do not pose a challenge to the motivational assumptions of the standard economic model. I infer that the economist editors, who come from the behavioral side of economics, were (on balance) unwilling to sign on to this position. Thus, the primary conclusion is that, like the original research, none of these experimental results are consistent with the predictions of the standard economic model.

But if we put this emphasis front and center, then the experimental results reported here begin to look more and more like second order footnotes to the original work, perhaps reflecting difficulties in administering experimental protocols in the field. I infer that the anthropologist editors and most of the chapter authors were not enchanted with this interpretation.

What can be done to move the discussion forward? I do not think that doubling the number of small-scale societies studied would add much. Thus, I strongly agree with the opinion voiced by Hill and Gurven: “At this point, we think more investigations into the effects of changed experimental conditions will teach us more about cross-cultural variability than we can learn by simply increasing the sample size of different cultures tested” (p 409).

For example, the argument that ultimatum game behavior reflects an inability to distinguish between and modify behavior in the

light of the fact that one is in a one shot, anonymous interaction, is remarkably resilient. One sees it popping up in John Patton's discussion of the Conambo (p 98), or Jean Ensminger's suggestion that "we might find behavior in one shot games consistent with behavior more appropriate to repeated games" (p 358). As noted, this claim has been tested and rejected for university student subject pools. But perhaps, for one reason or another, it is applicable under these field conditions. One way to resolve the issue would be to replicate the Fehr and Fischbacher (2003) experiments using these nonuniversity student subjects. If it turns out that subjects are not adequately differentiating between one shot and repeated interactions, this will be a tip-off that they do not fully understand the games in which they are involved or that there are other defects in the protocols that need to be remedied.

At the end of the day, one needs to step back and ask which is the greater threat to progress in developing an empirically-based behavioral science consistent with our understanding of evolutionary theory. Is it that theorists will mistakenly interpret the results of experimental work done on Westernized subjects as having more universal applicability than is justified? Or that the selfish actor rational choice paradigm will persist, unmodified by the wide range of experimental work inconsistent with its predictions that preceded the work reported in this volume? I am more concerned with the latter possibility. In the long run, I think we will conclude that this research offers some enhancements to the original body of experimental research, but should not derail the continued effort to flesh out the components of a species typical human psychology, to which the original effort has made such important contributions.

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