Comments on “Context, conflict, weights, and identities: Some psychological aspects of decision making” by Eldar Shafir

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My job here is to comment on Eldar Shafir’s paper, “Context, conflict, weights, and identities: Some psychological aspects of decision making”. Being an anthropologist interested in the evolution of human behavior, I have little to add to the substance. As a result, any comments I might make be things like, “great paper,” “learned a lot”, etc. I don’t think this kind of commentary would be good value for the FRBB. However, in her invitation to this conference Jane Little asked me to comment on paper, and added “We would hope that you would use this comment as an opportunity to broaden the conversation and address aspects of the topic that seem particularly interesting to you as an anthropologist.” And that’s exactly what I’m going to do.

Shafir gives us lots of evidence that that the standard model of rational choice is not an accurate description of how humans make choices. Moreover in the laboratory these differences matter—the rational choice model often doesn’t do a very good job prediction people’s choices. Of course, you could still defend the rational choice model as a useful approximate model in the domains that interest economists. Mechanical engineers don’t have to worry much about quantum mechanics, and it could be argued that economists can safely ignore cognitive psychology. In fact, this must be true is some sense, given the success of economics as a discipline.

However, I don't think it is the whole story. At the end of his paper Shafir argues that there are good reasons to think that the failures of the rational choice model detected the laboratory cause us to misunderstand how economies work, and therefore to design bad policies. I am inclined to agree. In the area I know best, the behavior of experimental subjects in the Ultimatum Game, Trust Game and so on suggests that people are motivated, at least partly, by interests in the welfare of others, even strangers, and care about the way that transactions occur, not just outcomes. Game theory suggests the social outcomes may be sensitive to such motives—even a minority of altruistic punishers can radically transform outcomes in social dilemmas. Moreover, the fact that the small scale societies regularly solve social dilemmas, especially those involved in trade and warfare, even though formal coercive institutions are completely absent. Life in the human state of nature is not solitary nor brutish (though often poor and short), yet there is no Leviathan. And, although at this writing most of the conference papers have not been posted, I take it from the titles that many conferees agree. So there is a real problem here.

Its not so clear what to do about it. I believe that one of the great strength of economics is that it is built around a unitary, mathematically formulated theory that is applied to a very wide range of questions. Having done a lot of interdisciplinary work, especially with various brands of anthropologists and psychologists, I can testify that having a shared, clearly formulated theory gives economics a greater clarity of thought compared to other social sciences. The empirical results reviewed by Shafir make it clear that this theory is wrong in important ways. Unfortunately, I don't think they don't make it so clear how to fix it.

It is tempting to react to the evidence of non-rational behavior by tinkering with the existing theory. If expected utility maximization doesn't predict how people make risky decisions, adopt a more flexible way to weight uncertainty as in prospect theory. If *Homo economicus* doesn't predict Ultimatum Game behavior, maybe *Homo altruisticus*, *Homo reciprocans,* or some other modified version of a panhuman nature will do better. While such approaches are natural, and may in the end be the best way to adjust theory response to laboratory data, there are good reasons to be cautious.

The problem with this approach is that experimental behavior is so variable, that it is hard to figure out how to map it onto real world behavior. Take the minimal group paradigm. These

experiments tells us that people can identify with arbitrary social groups. However, the experimental results are somewhat sensitive to the details. For example, in-group favoritism can disappear if there are three groups instead of two, if subjects anticipate reciprocal treatment, or even if subjects make decision in private. Results from experimental economics often suffer from the same kind of ambiguity. For example, changing the framing can cause subjects to make quite different choices in games that otherwise have the same structure.

This problem gets a *lot* more scary when experiments are done in different cultures. Both psychologists and experimental economists often discuss their results in terms like, "people are subject to an endowment effect," or "human cooperation is driven by strong reciprocity." However, it is unclear whether experiments tell us something about human nature, or about euro-american undergraduates. As Shafir notes, experiments can lead to very different results in different cultural settings. Having helped organize some cross-cultural experimental work, I can't resist the opportunity to tell you a story that illustrates the nature and magnitude of such cultural effects. The ultimatum game has yielded quite robust results. In a wide range of framings, student subjects usually make high offers, and low offers are frequently rejected. A number of us in the MacArthur Preferences Network organized a project to administer ultimatum games in 15 small scale societies. This sample included groups with very different social and cultural environments than those experienced by the usual student subjects—including hunter-gatherer groups like the Ache and the Hadza, horticulturalists living in the Amazon forests, and nomadic herders from Asia and Africa. In some groups, the results were quite similar to student subjects, but in others they were very different (Figures 1 and 2). For example, the Machiguenga, a group of very forest horticulturalists living in Eastern Peru, behaved almost like *Homo economicus*. The modal offer was 15% of the endowment, and rejections were virtually absent, even though many offers were very low. Even more telling, is the behavior of the Au and Gnau, two groups living in the Sepik River region of New Guinea. Here, subjects made offers completely across the spectrum—offers of more than half of the endowment were as common as offers of less than half. Even more surprising, the higher the offers were above half the endowment, the *more* likely they were to be rejected.

Such variation makes it hard to amend economic theory empirically, "from the bottom up." It could be that groups differ in basic predispositions. Perhaps the Machiguenga are less

altruistic than other groups. But it just as easily could be that the experiment elicited specific behavioral rules. Maybe, the Machiguenga would have made fair offers if the stakes had been meat, which is more commonly shared, instead of money, which is not. In the end, empirical work will have to do the job. But the task would be much easier if we had some way to constrain the space of possibilities.

Experience in biology suggests that adaptationist thinking is one very useful source of such constraints. As Shafir notes in his paper, brains (human and otherwise) are not a general purpose problem solvers. A tiny desert ant with a brain the size of the head of a pin can solve navigation problems that would stump many humans with their quart and a half of brains. Biologists have found that trying to figure out what kinds of problems evolved to solve tells you a lot about what kinds of data the organism should collect, and what kinds of computations it should do on these data. This is for sure true in the case of animal navigation. For example, the desert ant, *Cataglyphsis*, leaves its nest and wanders about searching for food. When it finds some, it has to finds its way back to the nest, a vast distance (for an ant) over hostile, searing terrain. Because we understand navigation as a problem, we know that there are a menu of possible solutions, and we can test these against the ants behavior experimentally. It turns out that *Cataglyphis* is (among other things) doing dead reckoning, approximating a vector integral along its out bound path so that when it finds food it knows the direction and distance to its home. Other species face different problems. Migrating birds, like the indigo bunting, have to be able to find their way to places that they have never been before during seasonal migrations. Dead reckoning won't work now, the birds need some absolute referent. It turns out that like human navigators they use the stars, and come with a built in star-chart-template in their heads.

I believe that that same approach can be helpful in understanding human cognition. Humans evolved in small scale foraging societies, and their brains are designed to solve the ecological and social problems that arise in such settings. I think that evolutionary psychologists (like our Sunday evening speaker, Steve Pinker) are right that thinking about the kinds of problems that human brains are designed to solve can help understand the structure of human minds. For example, human foragers make use of a very wide variety of plants and animals. Thus we should expect that human brains should be equipped with machinery to make accurate inferences about the properties of plants and animals species. And, there is fairly good evidence

that this is the case. For example, people seem to come equipped with the innate, default assumption that animal species have a large number of hidden, "essential" properties, and that these are transmitted from parents to offspring. Even small children seem to understand ecological categories like predator and prey, and readily make appropriate inferences about organisms in different categories. Of course, many theories survive the adaptationist's sieve: there is much we don't know about the challenges facing Pleistocene foragers, and even if we had such people available to study, there are often many possible solutions to any particular evolutionary problem. Nonetheless, I think that economists and psychologists could get quite good mileage out of thinking in evolutionary/adaptationist terms.

I say this, even though I am known for arguing that culture makes it much harder to apply adaptationist reasoning to humans than to all the other non-cultural organisms. When you see a complex adaptive structure, or a complex adaptive behavior in other animals, you can be pretty confident that it is the product of natural selection. With humans this is not the case, because human beings can acquire culturally evolved adaptations, complex, adaptive behavior patterns for which they have no specific, innate, evolved psychology. Again navigation provides good examples. While the European peasant of the middle ages could not find his or her way from town to town, the medieval sailors who plied the treacherous coastal waters of Northern Europe had an elaborate, effective culturally evolved navigational system based on dead reckoning. Half a world away, contemporary Micronesians who sailed great distances across the tropical Pacific a quite different, culturally evolved system based on absolute stellar navigation. Such culturally evolved differences in spatial cognition are not limited to complex-society specialists like sailors. Aborigines living in the vast open spaces of Australia have an acute sense of cardinal direction that permeates their cognition. By contrast forest peoples of central Africa, like the medieval peasants, almost lack the concept.

Unfortunately, we lack a widely accepted theory of how cultural adaptation occurs. Nonetheless, cross cultural work, both ethnographic and experimental, can provide useful for interpreting variation in experimental results. For example, in the MacArthur study we were able to account for variation in Ultimatum Game behavior. Mean offers were strongly positively related to two factors: how much cooperation played a role in every day subsistence, and market contact/social complexity. Ethnographic data also help understand some of the details. The

mysterious behavior of the Au and Gnau becomes a lot less mysterious when you learn that they are societies in which competitive gift giving plays a big role.

Conclusion

Obviously there is a lot of work to do. This may not be such good news for those of you in the efficiently-manage-the-economy business, but it is very good news for those of us in the basic research business. I'd like to conclude with a bit of sermonizing about how we ought to go about doing this work.

One of the things I like about Shafir's paper is that it takes for granted that where economics and psychology meet, they should tell the same story. This may seem like obvious common sense, there is only one world out there, and if economics and psychology disagree, at least one of them is wrong. The weird thing is that this view does not predominate in the social sciences which go on as if such interdisciplinary disagreements were not a problem at all.

You can see this most clearly if you compare the way that biology and the social sciences are taught to beginning undergraduates. Although universities typically have a number of departments in the life sciences—molecular biology, physiology, genetics, ecology, and so forth—biology is taught as an integrated subject up through the first course in college. Good instructors take care to present the unifying themes of biology—genetics, basic metabolic principles, and evolution. They do not do this out because they value a general education. Rather all of these levels of organization are linked together in a causal web; none can be understood fully in isolation. By contrast psychology, sociology, economics, anthropology, history, and political science all teach proprietary first-year introductory courses. In their 10 o'clock econ 1 class students are told that people maximize expected utility. Then they trot over to Psych 1 at 11 where they learn that people don't have preferences. Then at noon their Anthro 1 lecturer tells them that it is impossible to understand society in terms of individual psychology.

It seems to me that this is a scandalous state of affairs. All of us in the social sciences need to pay more attention to what is going on in other disciplines. We also need to resist the temptations of disciplinary ethnocentrism. Conferences like this one are a good start.

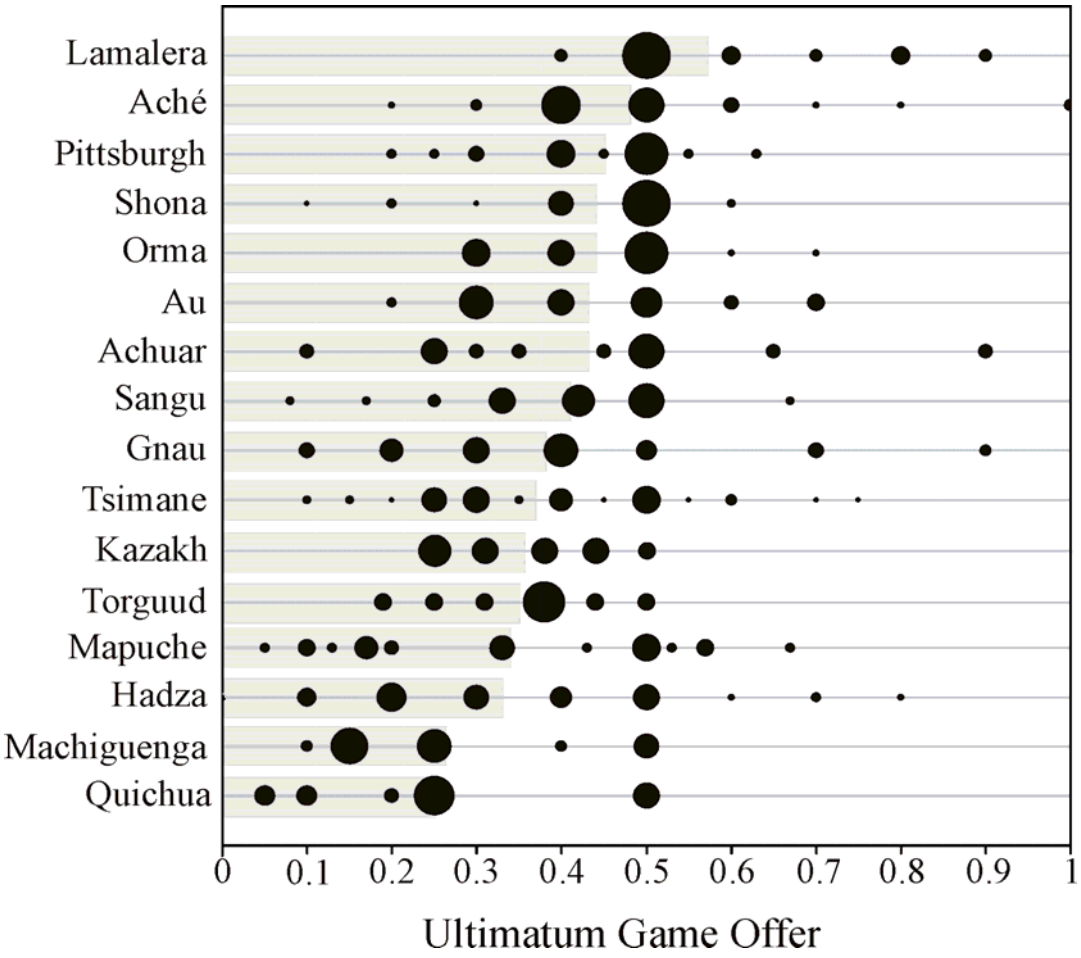

Figure 1. A Bubble Plot showing the distribution of Ultimatum Game offers for different cultural groups in the MacArthur Preferences Group study. The diameter of the bubble at each location along each row represents the proportion of the sample that made a particular offer. The right edge of the lightly shaded horizontal gray bar is the mean offer for that group. Looking across the Machiguenga row, for example, the mode is 0.15, the secondary mode is 0.25, and the mean is 0.26.

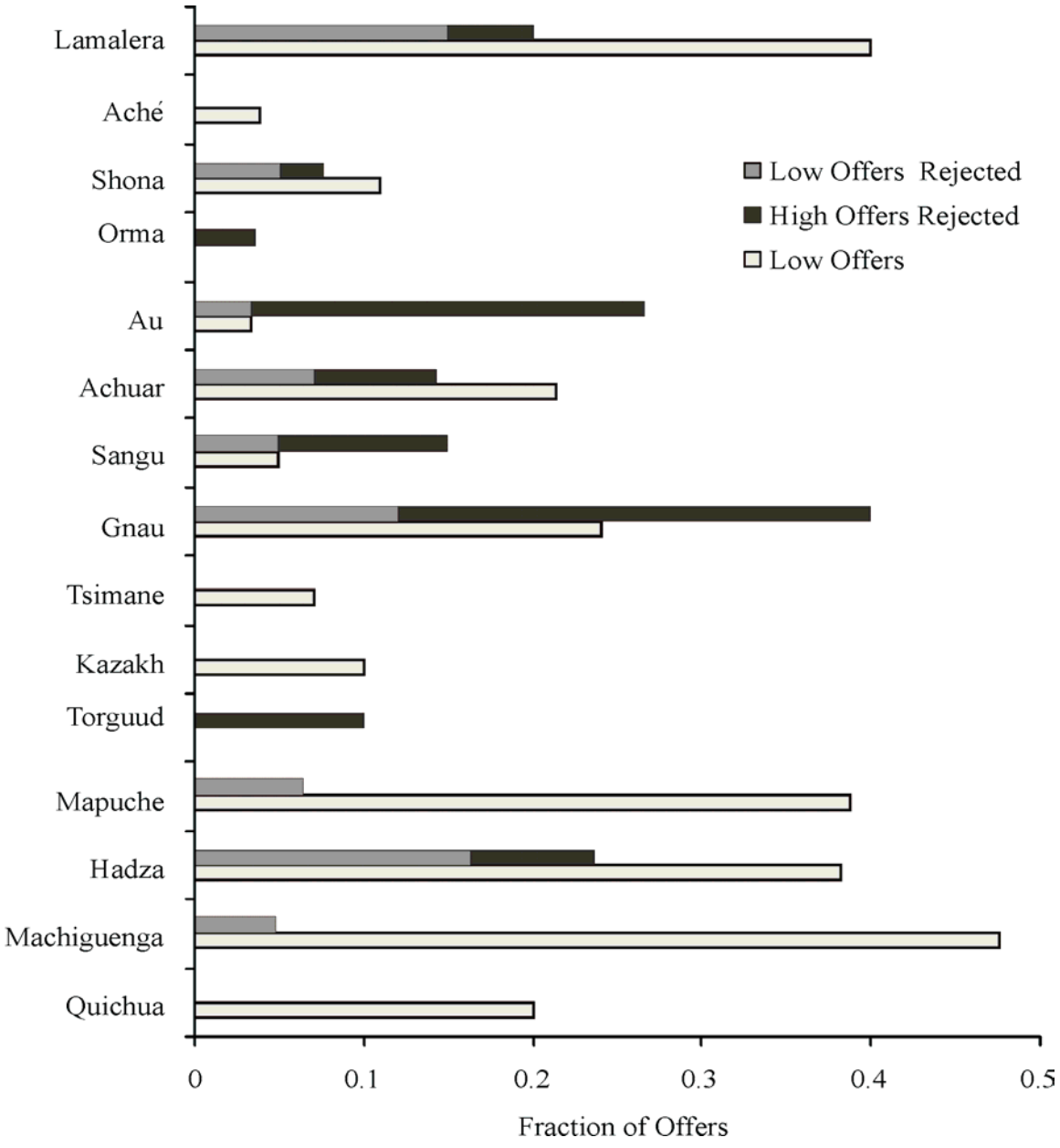

Figure 2. Ultimatum Game Responder's Behavior in MacArthur study. The lightly shaded bar gives the fraction of offers that were less than 20% of the pie. The length of the darker shaded bar gives the fraction of all Ultimatum Game offers that were rejected. The length of gray part of the darker shaded bar gives the number of these low offers that were rejected as a fraction of all offers, while the black section of this bar gives the number of high offers rejected as a fraction of all offers. The low offers plotted for the Lamalera were sham offers created by the investigator.

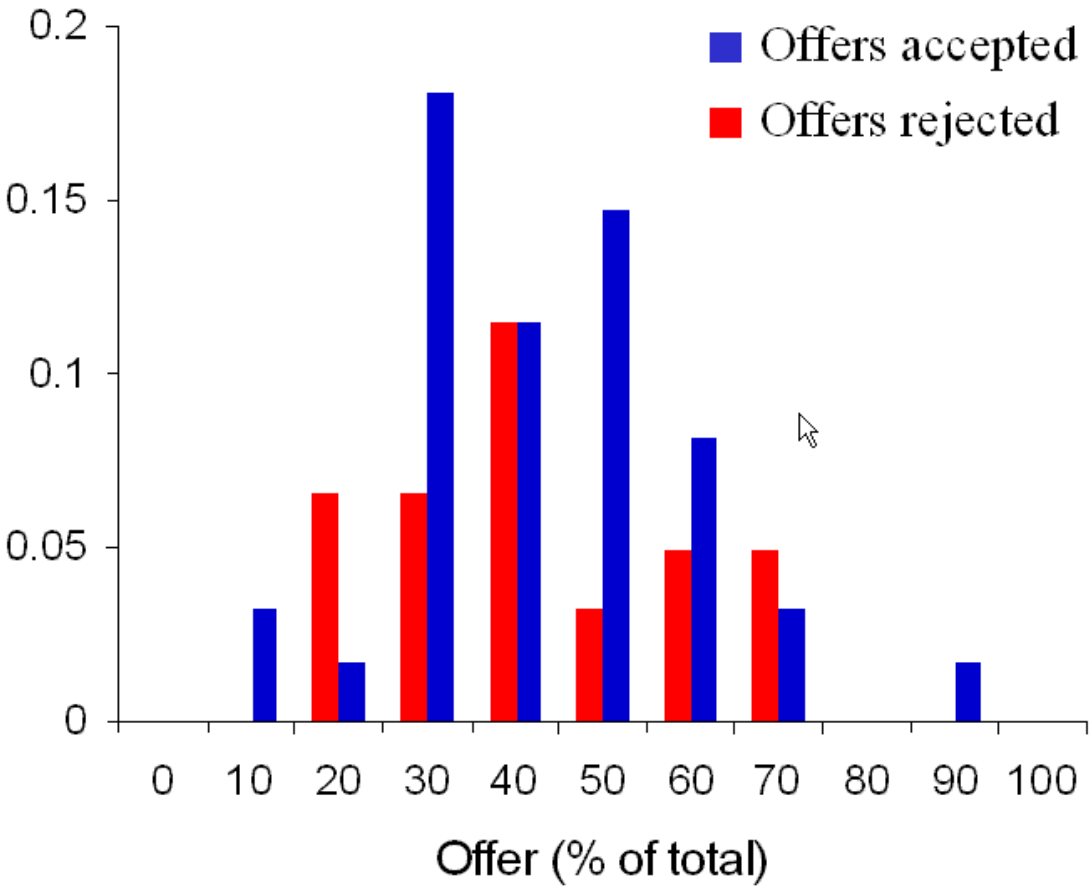

Figure 3. Ultimatum game offers (n = 61) for the Au and the Gnau. Blue bars are offers that were accepted, while red bars are offers that were rejected. Notice that a substantial fraction of high offers were rejected.