

each of these observations (in reverse order), and we conclude by proposing a different approach to comparative research.

1. Population variability. The existence of significant population variability is convincingly documented by the authors, who find it as soon as they look beyond the ridiculously narrow samples on which claims of universality have typically been based. Although in agreement with the finding, we have a problem with the authors' underlying assumption. This is the idea that it is possible to neatly sort variable features of human behavior and psychology from universal ones. As anthropologists, we have no problem in accepting that cultural, historical, and environmental contexts affect all the features discussed in the article, but this observation has no bearing on the question of whether such features are "universal" or "variable." It is the variable/universal dichotomy itself (and the questions it generates) that is misleading. This is because human beings are affected simultaneously by processes of a different nature, among them phylogeny, history in its social and cultural instantiations, and ontogeny. But none of these processes is ever active in isolation, making it impossible to track its universal or variable effects. Searching in any human phenomenon for the clear signature of one of these processes in isolation is a wild goose chase.

2. WEIRD as outliers. The authors use three broad population contrasts in order to zoom in on the weirdness of the subject population used to generalize about human nature. The point is well taken, as is the call for more research among non-WEIRD populations. In their eagerness to condemn the reliance on WEIRD subjects, however, the authors end up presenting and conceptualizing population variability in terms of extremely dubious categories. Curiously, while they feel the need to clarify what they mean by the term "Western" and to acknowledge its limitations, they offer no apology for using "small-scale societies" as if the term referred to a unified, meaningful whole (a similar point could be made for "non-Westerner" or "East Asian"). This uncritical lumping together of a variety of disparate societies is particularly odd in a paper that denounces unsound generalizations. As clearly demonstrated by the results of the economic games, some "small-scale societies" can vary just as much among themselves as they do from the WEIRD population – a fact that should not be surprising given that "small-scale societies" are as caught up in the flow of human history as any other. One could argue that the extreme weirdness of the WEIRD population is partly the result of having lumped together other populations under too simplistic and under-theorized labels.

3. Our proposal. As anthropologists committed to the study of human nature (see Bloch 2005), we welcome Henrich et al.'s critical appraisal of the behavioral sciences' comparative database. We feel, nonetheless, that the authors have not sufficiently taken to heart the fundamental implications of their analysis. One obvious conclusion they might have drawn is that behavioral scientists should pay more attention to the work of cultural/social anthropologists, since these are the scientists who have made human variability their main focus. It is striking, however, how little reference Henrich et al. make to anthropological research. This, of course, is no accident. It has to do with the kind of data that anthropologists have produced, which in turn has to do with the history of their discipline.

At the start, anthropologists went to the field with ready-made questions that were generated by a simplistic, yet highly influential, evolutionary theory, which is still the basis of popular understandings of the difference between "civilized" and "primitive" societies (the latter sometimes euphemistically called "small-scale"). But such outmoded theory had to be abandoned because, it was soon realized, human history does not proceed along a progressive and unilinear path. Because of the human capacity for culture, each human society is the unique product of a unique, albeit not isolated, history.

Ever since the recognition of this fact, anthropologists have faced a methodological difficulty: Questions formulated from within one historical context produce misleading answers when

transposed elsewhere, as they appear weird, uninterpretable, or mean something else (arguably, this is what generates the weirdness of the WEIRD population, since what distinguishes it from all the others is that it is the one that generates the questions). The way anthropologists have tried to overcome this challenge has been to abandon, initially at least, all questions formulated outside the context under their investigation. Rather, through participant observation, they have allowed themselves to discover, from the inside, the terms and values of the people they study. This strategy is not without difficulties, as it generates a kind of data that appears impressionistic and anecdotal and which, crucially, precludes comparison and generalization – which is why such data is so often ignored by other behavioral scientists, such as the authors of the target article. We recognize that this is a very serious limitation, but we insist that behavioral scientists must acknowledge and never underestimate the equally serious and unavoidable problem that led anthropologists down this methodological route in the first place.

Therefore, the solution cannot be, as suggested by Henrich et al., to administer studies upon studies to the billions of (poor) people around the world who remain untapped by the behavioral sciences. The solution is far more complicated and costly. It requires an often uncomfortable compromise between internal validity and generality, and a lot more detailed ethnographic work than many seem to be willing to accept (see, e.g., Astuti & Harris 2008; Astuti et al. 2004). Only in this way will data from non-WEIRD populations become a meaningful and indispensable ingredient of any general theory about our species.

Weird people, yes, but also weird experiments

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Abstract: While we agree that the cultural imbalance in the recruitment of participants in psychology experiments is highly detrimental, we emphasize the need to complement this criticism with a warning about the "weirdness" of some cross-cultural studies showing seemingly deep cultural differences. We take the example of economic games and suggest that the variety of results observed in these games may not be due to deep psychological differences per se, but rather due to different interpretations of the situation.

Henrich et al.'s article fleshes out in a very useful and timely manner comments often heard but rarely published about the extraordinary cultural imbalance in the recruitment of participants in psychology experiments and the doubt this casts on generalization of findings from these "weird" samples to humans in general. The authors mention that one of the concerns they have met in defending their views has been of a methodological nature: "the observed variation across populations may be due to various methodological artifacts that arise from translating experiments across contexts" (sect. 7.2, para. 1). Here we want to express a less sweeping methodological concern. While accepting the general conclusions and recommendations of the article, we believe they should be complemented with a warning about the "weirdness" of some experimental designs that have been used across cultures and seem to show deep cultural differences. In fact, they may just show quite different interpretations of the experimental situation by the participants. This is not to deny, of course, that these differences in interpretations are themselves

both psychological and cultural and are worth studying in their own right. In fact, unless one pays attention to them, it unclear what the experimental evidence is really about.

Let us illustrate our point with the case of economic games (discussed in sections 3.2, 4.1, and 6.1 of the target article). In these experiments, people are given a sum of money for free (which never happens in the real life) and have to share it with someone about whom they have no information (which also never happens in real life). Many researchers, including one of the article's authors (see Henrich et al. 2005), have pointed out that cultural variations in economic games may have more to do with methodological problems than with actual cultural differences (Ensminger 2002; Heintz 2005; Lesorogol 2007). In particular, participants in these games have no information about the rights of each player over the stake and are asked to make a "blind" decision. But who owns the money? Is the money a gift? Is the money a payment in exchange for my participation? Who is the other participant? Is he or she someone I know? Does he or she have rights over the money? And so on.

This leaves open the possibility that behavioural differences observed in economic games are not due to deep psychological differences per se, but rather due to different *interpretations* of the situation (for a similar point, see Hagen & Hammerstein 2006; Heintz 2005). For example, Henrich et al.'s (2005) study in 15 small-scale societies reveals a striking difference between the Lamalera, who make very generous offers in the Ultimatum Game, and the Tsimane and the Machigenga, who make very low offers in the very same game. But the game is likely to be construed very differently within these societies. The Lamalera, being collective hunters, may indeed see the money as jointly owned by the proposer and the recipient. By contrast, the Tsimane and the Machigenga, who are solitary horticulturalists, may see the money as their own property and therefore feel entitled to keep it. In the same way, Westerners may appear as outliers not because they have a different moral psychology, but rather because, living in very large, democratic and capitalist societies, they make different assumptions in economic games (e.g., that, not knowing the other participant – a situation of anonymity that is common in large-scale urban societies – they have no particular duty to share the stake with her).

In line with this idea, economic games framed within a more detailed context tend to show that people's decisions are based on property rights (Oxoby & Spraggon 2008), past contributions to collective actions (Cappelen et al. 2007; Frohlich et al. 2004), or a personal link of solidarity (Cronk 2007). One possible interpretation is that participants try to be fair with others when they distribute the money: If the other player has produced the money, she has more right over it; if she has been more productive or has invested more money, she deserves a bigger part of it; if both players are friends, they have special duties toward each other; and so forth. Such a "sense of fairness" combined with contextual differences might well explain the variety of results observed around the world. When confronted with cultural differences in experimental result, we should therefore ask: Are they the product of deep differences in the psychological dispositions and processes these experiments are intended to illuminate, or do they reflect differences in the interpretation of the experimental situation? One way to help answer this question would be, for instance, to present the Lamalera and the Machigenga with, as much as possible, the same rich context (e.g., clarifying the source of the money and the relationships between the participants) and assess whether they use the parameters at stake (i.e., rights, past contributions, social links) in the same way.

The importance of the way participants interpret a task – which may differ from the way the experimenter intended them to interpret it – has been often stressed in experimental psychology (e.g., Sperber et al. 1995). The more the experiment is artificial and devoid of "ecological validity" – in other terms, the weirder it is – the greater the risk of misinterpreting the differences between societies. When it comes to cross-cultural comparisons, ignoring

this pragmatic dimension of participants' performance may cause one to exaggerate or to miss genuine psychological differences.

Weirdness is in the eye of the beholder

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Abstract: Henrich et al.'s critical review demonstrating that psychology research is over-reliant on WEIRD samples is an important contribution to the field. Their stronger claim that "WEIRD subjects are particularly unusual" is less convincing, however. We argue that WEIRD people's apparent distinct weirdness is a methodological side-effect of psychology's over-reliance on WEIRD populations for developing its methods and theoretical constructs.

In their important article, Henrich et al. offer both weak and strong versions of an argument against the widespread use of research with WEIRD (Western, Educated, Industrialized, Rich, and Democratic) people as a means to learn about general human psychology. The weak version critiques the over-reliance on such samples and reviews an extensive body of literature across domains to establish that widespread cross-cultural differences exist for many of the psychological findings researchers have assumed were species universal. We are one hundred percent convinced of the weak argument and strongly endorse its attendant plea for moving beyond WEIRD samples. Their review is a major contribution to the literature, and we thank the authors for it.

The strong version of the argument makes the additional point that WEIRD people are literally weird, atypical of humankind at large. On this account, it is the field's ironic misfortune that of all samples to study, psychology should have picked this one. This strong argument is intriguing, and Henrich et al. present extensive evidence suggesting that this narrow slice of humanity indeed is a cultural outlier. For reasons that form the basis of our commentary, however, we remain skeptical with respect to this strong argument.

The evidence for the distinctness of WEIRD samples comes from studies that generally take the following form: Findings originally conducted on the WEIRD population are assessed with a different population, and a different pattern of results emerges. When a broader range of groups is considered, the WEIRD population tends to be at the extreme in its responses. For example, Henrich et al. cite Segall and colleagues' replication of the well-known Müller-Lyer illusion (Segall et al. 1996). Segall et al. find not only a wide distribution of the magnitude of the illusion across cultures, but also that the U.S. sample is the most extreme in magnitude (see their Fig. 1). Other phenomena they review demonstrate a similar trend.

Base rates provide one clue that there might be something amiss with the argument that the group with which we are most intimately familiar is also the most distinctive. If there are a thousand potential samples, then the probability that the first selected is the most deviant is one out of a thousand.

We think the apparent extremity of WEIRD populations can best be explained by two factors contributing to what we have called "the home-field disadvantage" – that is, the tendency for research developed in one's "home-culture" and subsequently co-opted for cross-cultural comparison to result in one-sided conclusions about the nature of cross-cultural differences (Medin et al., under review).

The first factor is the similarity between researcher and researched. Variations across cultures may reflect both adaptations to particular environments (e.g., Nisbett & Cohen 1996) and "niche