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Social Norms, Discrete Choices, and False Dichotomies

Eric Schniter
Chapman University, schniter@chapman.edu

Nathaniel Wilcox
Chapman University, nwilcox@chapman.edu

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tence occupations that do not require modern human capital. People who had been patient in 1999–2000 had greater wage earnings and more modern physical assets in 2004. In a cultural context where, according to the author, there is self-selection even on pertinence to ethnic groups, the lack of attention to the potential endogeneity of the individual choices analyzed is surprising.

Norbert Ross
Department of Anthropology, Vanderbilt University, 124 Garland Hall, Nashville, Tennessee 37235, U.S.A. (norbert.o.ross@vanderbilt.edu). 25 X 11

Tucker addresses what he calls the “wealth” versus “ethnicity” debate. His data show that (1) both coping and conformity might play a role and (2) previous positions were wrongly posed as opposites. Tucker is right and wrong. Here, I would like to discuss three issues. First, while Tucker discusses the role of experiments, he leaves the issue of “mental accounting” aside. A fair amount of literature shows that money is not always treated the same way. Money received as a gift is spent differently than “regular money.” A person finding $100 on the street before entering a casino is more likely to spend that money than the $100 salary increase she just received. This very likely also applies to money to be gained in risk experiments—the risk-taking pattern with experimental money might differ from risk taking in every day life. This problem is increased if the researcher relies on one single experiment and little to no ethnographic data. Second, it is not always clear what experiments measure. The ultimatum game can be described as a way of testing individual risk taking, or it can be seen as testing social interactions (one person has to make an offer to the other player in terms of a division of a set amount, rendering any straightforward interpretation of the experiment at least problematic). Third, decisions are always made within a context. Just as risk taking in the ultimatum game is not independent of social interactions (do I want to look like an egoistic person, or do I forsake potential economic gains in order to look like a nice guy?), neither is the “wealth hypothesis.” The wealth hypothesis clearly depends on people’s aspirations and goals. Of course, in extreme poverty people might not be able to take any risk or delay small benefits for a greater benefit in the future, but that does not mean that beyond the point of extreme poverty an increase in income automatically means an increase in risk taking (or the opposite). These things depend on models, aspirations, and values (themselves embedded in relations of power, class, and inequality) that seem to be ignored by most of the relevant literature. For example, in real life a delayed benefit might be bigger than an immediate return yet very likely introduces a (perhaps small) chance of not receiving anything at all (what if the bank goes out of business, etc.). While experimentally we can hold these variables constant, the question is whether this approximates any real-life situation. Kahneman and Tversky opened up a debate of framing in decision making that should be followed by anthropologists beyond the point of framing a decision in terms of loss and gain to include a better understanding of what a specific decision is about. This leads me back to Tucker’s point, that both wealth and conformity/ethnicity play a role. For that to be the case, he needs to show that his model performs better in explaining the data collected by Kuznar, Henrich, and colleagues than their own respective models. If not, his data could be peculiar to a specific field setting, about which he does not tell us much. Part of the problem seems to be that in many of these studies the net is cast too wide and hence does not go deep. For example, Tucker correctly points out that the conformity hypothesis relies on “ethnicity” being a variable that significantly explains variation in responses. However, this could mean many things. It could mean that people conform (as assumed by the authors), or it could mean that some other variable correlates with ethnicity, driving an effect that does not link ethnicity and response variation in a causal way. Why not ask what people are doing? Why they respond the way they do? What these questions actually mean to them? Why not explore risk taking in real-life situations (Frank Cancian’s 1972 book comes to mind) and use experimental research to pinpoint more specific questions within a more confined setting? Tucker realizes the problems with experiments in that real-life decisions are often embedded in habits and wider beliefs about the world. In fact, he parallels the risk experiments to new upcoming market opportunities, yet I wonder why such market opportunities did not become the target of his study? Who takes what kind of market opportunities, and why (or why not)? This is what Cancian did, trying to understand which sector of a community would take on new agricultural strategies. Such an approach seems promising if paired with experiments as proposed by Tucker and more true to reality, which is after all what we want to explain. To conclude, Tucker is right that his data show that both conformity and wealth play a role in risk taking in his specific field setting. He is wrong in that his work cannot refute research in other parts of the world and in that his work—as did the one by previous researchers—seems to ignore many important aspects and variables that one would like to see taken into account.

Eric Schniter and Nathaniel T. Wilcox
Economic Science Institute, Chapman University, One University Drive, Orange, California 92866, U.S.A. (schniter@chapman.edu). 14 X 11

Social Norms, Discrete Choices, and False Dichotomies

We commend Tucker for a well-executed study that successfully uncovers rich relationships between contexts and be-
havior. We agree that equating ethnic and spatial effects with social learning and wealth effects with individual learning is to force a false dichotomy. These two hypotheses need not be mutually exclusive; individual and social learning may both be active at the individual level regardless of whether choices conform to norms. We would like to add further caution to these persistent and fallacious either/or debates by illustrating additional reasons why the presence and absence of social effects is tenuous evidence (at best) for a causal pathway between social learning and individuals’ discrete choice behavior.

Suppose we failed to find effects of some spatial groupings on some choice variable. This does not imply an absence of social learning. There are at least two good reasons why social learning will not always produce evidence of social norms in spatial groupings: (1) social learning also occurs according to irregularly distributed network patterns (e.g., kin networks) that crosscut space and (2) even if and when social learning occurs in spatial groupings, choices influenced by learning can be very sensitive to the timing and sequence of sampled information.

Social learning, whether observed in birds, fish, or mammals (see Gibson and Hoglund [1992] and Pruitt-Jones [1992] for reviews), is not an indiscriminate process. Instead, these studies demonstrate that learning strategies are sensitive to cues directing organisms when to learn from others and who to learn from (Laland 2004). We expect that human foragers, who interact most frequently with affinal and consanguineal kin (e.g., see Hill et al. 2011), will differentially acquire information about things like monetary choices from familiar kin. Using logistic regressions, we analyzed unpublished survey data from Schniter’s dissertation of Tsimane nominations of experts (made by judges competent in the skill) and found that kinship is a significant predictor of individuals’ nominations even after detailed accounting for spatial relationships. With kinship and villages of nominees and judges accounted for, additional spatial variables (domicile or exact domiciles) contribute little to explained variance (R² increases by 0.0024). By contrast, with all spatial variables in the model, adding kinship explains variance by six times more. Tucker observes that in his sample individuals “intermarry freely” and “genealogies crosscut the three identities.” We suspect that by controlling for kinship Tucker might explain even more relational variance and further clarify how strategic and social variables relate to individual decisions.

Tucker suggests that some subjects did not understand all tasks. Where participants are uncertain about the task and best decisions but where they can sequentially sample others’ choices, “information cascades” (Bikhchandani, Hirshleifer, and Welch 1992, 1998) can produce “norms” that trump the influence of prior private information. Conformism under cascades is sensitive to the timing of modal choice sequences (earlier revelation causes stronger cascades). Norms produced this way are thus idiosyncratic and fragile (differentially affected by order effects). If Tucker’s data are a product of cascades, it could manifest as emergent norms in some places and times but not in others.

Three of Tucker’s capital measures (human, social, and material wealth) are reported to covary with income, and other correlations among Tucker’s explanatory variables are plausible but not reported. When subsets of explanatory variables in models are sufficiently correlated, variables in the subset can be individually insignificant while the subset variables are jointly significant. Overall model fit does not suffer, but understanding can be compromised. For example, log income has one of the largest effect sizes in Tucker’s table 4, yet it is insignificant: perhaps it and the other strategic variables would be jointly significant. We would have liked to see a correlation table for Tucker’s strategic variables and F-tests of joint significance for subsets of correlated variables to aid interpretation of the statistical results.

We close with two cautions. First, experiments reveal strong randomness of discrete risky choice (Wilcox 2008); Camerer (1989) described such choices as “distressingly close to . . . random” (81). Tucker’s scrupulous attention to the reliability of independent measures is excellent. But the low reliability of dependent measures—single-choice indicators—means that 350 observations will not sort out the effects of three dozen explanatory variables with great replicability. Second, monetary risk preferences may not share significant variance with risk preferences over other outcomes (e.g., health, status, or reproductive outcomes). The “domain generality” of risk preference is contentious throughout the social and cognitive sciences, with results both pessimistic (e.g., Berg, Dickhaut, and McCabe 2005; Hanoch, Johnson, and Wilke 2006; Hershey and Schoemaker 1985) and optimistic (e.g., Barsky et al. 1997; Dave and Saffer 2007; Schmidt 2008). While male violence and competitive risk taking predicted by Wilson and Daly (1985) may help explain risky choices (like sleeping out in a forest, going to sea, and going on cattle raids), they may covary little with monetary risk choices.

Eric Alden Smith
Department of Anthropology, University of Washington, Box 353100, Seattle, Washington 98195-3100, U.S.A. (easmith@uw.edu). 13 X 11

I commend Tucker for employing multiple currencies to measure risk and time preference and defining multiple scales of social identity to which individuals might conform in norms and practices. In addition, testing multiple alternative hypotheses with the same data is all too rare in anthropology, as he notes, and the tests here are rigorous (if a bit numbing in their thoroughness). I would have preferred a set of tests more tightly tied to deductively generated expectations, but this is certainly a sophisticated analysis by the standards of sociocultural anthropology. Tucker links his analysis to