Perspectives on Choice

Review of *Choice* by Richard Harper, Dave Randall, Wes Shamrock. Polity Books, Cambridge, England.

Review by Thomas Gilovich and Lee Ross

Should I wear the navy pants or the black? Should I indulge my passion for sweets or stick to my diet? Should I persist in trying to get a job in journalism or give up and try something else? Should I begin collecting social security at 66 or wait until 70? Can we trust them to adhere to the truce or do we need to mobilize our troops?

Given the variety of decisions, big and small, that people are called on to make, it is no surprise that strategies for making decisions, and shortcomings in how decisions are often made, have been studied by scholars in a host of academic disciplines. Philosophers, statisticians, ethicists, scholars in many social science disciplines—especially economics and psychology—as well as practitioners in business, law, medicine, and other professions have offered their particular, arguably narrow, disciplinary perspectives. That is why the new book by Richard Harper, Dave Randall, and Wes Sharrock, which examines multiple perspectives, and bears the simple title *Choice,* is so welcome.

Harper and Sharrock are sociologists and Randall an ethnographer. So it is not surprising that the approaches offered in their disciplines, which are often neglected in both academic and popular books on decision making, receive special and generally very appreciative attention. But the authors also devote considerable attention to economic, psychological, evolutionary, and game-theoretic approaches to the subject. They also cover such timely topics as the ways in which the internet might be changing both decision making and decision makers. With respect to work outside their own fields of expertise, what they offer can best be described as a critical analysis, with the emphasis on *critical.* There does not seem to be much that they like about economic, experimental, or evolutionary approaches to the subject of choice. But their reservations are expressed with considerable wit and charm. Even readers whose disciplinary ox is being gored are apt to come away with a sense that they are being invited to engage in further dialogue with insightful, if skeptical, observers outside of their field.

As social psychologists, we enjoyed and felt enlightened by the authors' treatment of sociological and anthropological contributions to the study of choice, contributions about which we were relatively unfamiliar and can now better appreciate. Accompanying that enjoyment, however, was an uncomfortable sense that, as outsiders to those disciplines, we weren't sure how much credence to give the claims being made, or our evaluations of those claims. We were reminded of the experience of reading novels that offered seemingly authoritative accounts of scientific research in, say, biochemistry or physics, only to be disappointed and frustrated when the novel dealt with psychological issues about which we were truly knowledgeable—characters who not only misused jargon and mischaracterized theories but who simply got important details wrong or seemed woefully ignorant about the true state of psychological science.

Similar misgivings arose when we read the critical account Harper, Randall, and Sharrock (hereafter HRS) offer of Stanley Milgram's (1974) studies of obedience. They tell us that "in a later experiment, conducted away from Stanford University, participants were less willing to continue." As anyone reading a firsthand account of the Milgram studies would surely be aware, none of the obedience studies were ever conducted at Stanford. (Perhaps HRS are conflating those studies with Zimbardo's Stanford Prison Study, which, although often discussed alongside Milgram's research, did not examine the sort of obedience that Milgram was interested in). More important, the few studies that were conducted before research review committees essentially outlawed Milgram-style obedience experiments (for example, Rosenhan, 1979; see Blass, 1999) continued to show very high levels of obedience. And in *all* versions of the study conducted by Milgram, and by later investigators who made use of more benign versions the obedience paradigm (for example, see Burger, 2009), the rates of obedience were much, much greater than those predicted by psychiatrists, psychologists, and laypeople who were given a description of the procedures.

The message of these studies was not that the majority of people mindlessly yield to authority and "obey orders." It was that even in the absence of harsh coercion particular situations *can* lead large numbers of ordinary people to follow instructions to harm individuals to whom they bear no malice (see Ross, 1988). The fact that *other* contexts might yield less obedience does not alter the stark message that made the Milgram studies so resonant, and so memorable.

Further misgivings about HRS's familiarity with the research under discussion arise from the credence they give to Orne and Holland's (1968) assertion that the reason so many of Milgram's participants were willing to administer dangerous levels of electric shock to another person is that they knew the shocks weren't real. The authors do not mention Milgram's effective rebuttal to that claim (Milgram, 1972), or the details gleaned from the extensive records Milgram kept that are now accessible to interested scholars. Nor do they discuss the films Milgram made of a great many of his sessions. Anyone who has watches those films will see clear evidence of physiological stress on the part of research participants that they would not have experienced if they thought they were engaging in innocent role-play or some type of hoax. (It is noteworthy that the more accepted critique of Milgram's studies was that they were unethical precisely because they put the participants under excessive stress—a criticism directly at odds with the one HRS embrace.)

Telling omissions and mischaracterizations are also apparent in HRS's critique of another set of findings that we personally know well—the influential work by Daniel Kahneman and Amos Tversky on heuristics and biases in judgment under uncertainty. One of Tversky and Kahneman's most striking demonstrations involves the “conjunction fallacy,” or the willingness to assert that the conjunction of two features is more likely than the simple occurrence of one of them. In their famous “Linda problem,” for example, participants who read about a politically left-of-center person named Linda judge her as more likely to become a “bank teller who is active in the feminist movement” than simply a “banker teller”—even though the former is logically a mere subset of the latter.

HRS dismiss this finding by claiming that participants interpreted “bank teller” to mean “bank teller who is *not* active in the feminist movement.” Again, true familiarity with the relevant research literature precludes such a claim. Not only did Kahneman and Tversky directly address this potential alternative explanation, they conducted several studies that rule it out (Tversky & Kahneman, 1983). In one study, some participants were asked to rate, not rank, the likelihood of several events, including that Linda ending up a bank teller, and *other participants* were asked to rate the likelihood of her ending up a “bank teller who is active in the feminist movement.”

In this “between-subjects” design, of course, there is no basis for participants to assume that by “bank teller,” Tversky and Kahneman must have meant “non-feminist” bank teller. The fact that participants in this study nonetheless evidenced the conjunction fallacy is telling. It suggests an inclination for participants to focus on “representative” or prototype-matching exemplars of the category of bank-teller, and not to consider “non-representative” exemplars of that category.
This same influence of the representative heuristic is seen when one group offers estimates of the number of homicides in Detroit each year that exceed those of another group estimating the number of homicides in the whole state of Michigan.

The larger point that HRS seem not to appreciate highlights their outside perspective on the work in question—and their concomitant unfamiliarity with critical details. The point, as Tversky and Kahneman were careful to point out, is not that people typically display the conjunction fallacy when making estimates of likelihood or category size. It is that the influence of the heuristic in question, availability or representativeness, is potent enough to prompt not just ill-advised judgments, but even violations of the requirements of basic logic. In most everyday contexts, the representiveness heuristic may only rarely lead to such marked departures from normative principles. But it can nonetheless lead people to a host of socially important misconceptions and errors in judgment, such as being slow to consider the frequency of non-representative instances of many categories—police-shootings of white suspects, scientists from working-class families or humble academic institutions, or mammals that fly or that live exclusively in the sea.

Notwithstanding some troubling omissions, mischaracterizations, and lack of full appreciation of seminal research in social psychology and behavioral economics, there is a part of HRS’s critique that we think is important and in some respects quite accurate. They draw upon Brannigan’s (2004) contention that psychological experiments are sometimes more like theater than science to complain that research in these fields often leaves one without a clue about how consistent, predictable, or robust many reported effects might be. As they put it:

> "…too often behavioral economics…offers very little evidence that these effects are consistent and systematic—even across the experimental contexts provided, let alone in the world at large. We would need to know, as well, that the same or similar degree of bias is present to the same degree in a range of different…contexts; only then would we say that the bias is something more than a caprice and chance, that it is systematic." (p. 50).

There is often more than meets the eye—especially the untutored and untrained eye—in the demonstration experiments that researchers in social psychology and behavioral economics point to in their lectures and popular writings. There is undeniably a great deal of art in designing, pre-testing, and polishing such studies—in choosing the right examples, in setting the right context, and even in matching particular procedures to particular types of participants. Some research does explore behavior in familiar, everyday contexts and examines responses to challenges people commonly face as they go about their lives. But other research—particularly the kinds most likely to be oversimplified in the popular media—has a different objective. It seeks to capture not what *does* happen in everyday experience but what *can* happen, because of the power of particular psychological principles, pressures, and constraints in combinations and contexts rarely if ever encountered in daily life (Mook, 1983). Too often, we concede, investigators themselves fail to make clear exactly what their studies do and do not show, and where their findings are likely or unlikely to apply in everyday circumstances.

Research on what can happen and what does happen is often complementary. For example, while laboratory investigations of the fallibility of eyewitness testimony identified some shocking examples of just how mistaken people can be when offering confidently-held testimony, that work would not have prompted the legal reforms that have since been undertaken if researchers had not gone on to systematically explore how common such mistakes are under circumstances like those in which witnesses to actual crimes identify perpetrators. Research on overconfidence in judgment has likewise gone to great lengths to investigate how pervasive overconfidence is, the circumstances in which it is most likely to be observed, and the price it can exact.

But the value of mere "demonstrations" of what can happen under particular circumstances should not be dismissed as unimportant or unrevealing. As Mook (1983) notes, if a scientist were to establish that a single chimpanzee *can* learn language, our thoughts about language would be expanded, no matter how exceptional or non-representative the chimp in question might be. Learning that even in the absence of great rewards or threats of dire punishment, and in the absence of any malice, nearly two-thirds of Milgram's participants were willing to administer what they thought was (or might be) potentially harmful levels of electric shock merits our sober reflection. The lesson is revealing about human susceptibility to specific types of malign influence in particular contexts—and would remain worthy of continuing reflection even if the level of destructive obedience were to drop off substantially under somewhat different circumstances.

The point, again, is that investigators should be more careful to distinguish the practical from the purely scientific implications of their work. Otherwise, they leave themselves open, as HRS point out, to staking claims that go way beyond the data they've collected. However, we find it puzzling that HRS would level this critique at Tversky and Kahneman's research (although they are hardly alone in doing so) because those two much-honored psychologists were so careful in this regard. Anticipating how controversial their claims about errors and biases would be, they made a concerted effort to clarify the status of their claims and the meaning of their demonstration experiments. In their 1983 article on the conjunction fallacy, for example, they explicitly noted that they were not claiming that people generally or often judge the conjunction of two events to be more likely than one of its constituents. Such a pattern of judgments, they explained, is only likely to be observed when the conjunction seems more representative of an applicable category than one of the individual elements. The more general demonstration of the role that representativeness (and other heuristics and biases) can play when people make quick and unreflective judgments about frequency or likelihood is the most important message for real world practitioners (Kahneman & Frederick, 2002: see also Kahneman 2011).

A strategy employed by some of our field's most important contributors has been to show that particular theory-based interventions are sufficiently strong to rise above the noise and countervailing forces inherent in the contexts of great applied significance. But HRS seem unimpressed by this type of research as well. They are especially scornful of efforts inspired by Richard Thaler and Cass Sunstein to "nudge" decision-making in directions that nearly everyone would deem constructive. In critiquing such efforts, and in harkening back to limitations they see Asch's seminal studies of group influence, they completely ignore the field studies that show the value *in vivo* of well-conceived nudges. Interventions employing behavioral insights from psychology have proven effective in getting people to save more for retirement (Beshears, Choi, Laibson, & Madrian, 2008), to conserve energy (Schultz, Nolan, Cialdini, Goldstein, & Griskevicius, 2007), to show up at the polls on election day (Nickerson & Rogers, 2010), to eat a healthier diet (Wansink, 2006), and to stay in college rather than drop out (Cohen, Garcia, Apfel, & Master, 2006; Yeager & Walton, 2011).

Alas, despite the commendable effort to consider the full range of approaches to the subject of decision making, and despite the fluency and wit of HRS's prose, we believe the discerning reader must look beyond *Choice* for a balanced, informed, and informative evaluation of research and findings in social psychology and behavioral economics. We suspect that the authors' more informed discussion of work closer to their fields of expertise, which we personally found engaging and enlightening, will fare better in the estimation of scholars with an insider's perspective on those approaches.

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