Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques; author replies must be concise as well. Comments are expected to address specific substantive errors or flaws in articles published in *AJS*. They are subject to editorial board approval and peer review. Only succinct and substantive commentary will be considered; longer or less focused papers should be submitted as articles in their own right. *AJS* does not publish rebuttals to author replies.

VERSTEHEN FOR SOCIOLOGY: COMMENT ON WATTS[1]

INTRODUCTION

Duncan Watts, in "Common Sense and Sociological Explanations" (*AJS* 120 [2014]: 313–51) has done our field a great service by raising a question at the heart of the sociological enterprise: *What makes for valuable sociology, and when does it improve upon commonsense explanations?* Watts makes three key observations that we believe are quite productive to contemplate. First, one may distinguish between two distinct modes of sociological inquiry: a *verstehen* (Weber 1968; Watts, p. 318) mode that resembles commonsense explanations in that it aims to establish an empathetic link between the readers or users of such accounts and the actors who populate them; and a "causal" mode, which seeks to identify "generalizable causal mechanisms" (Watts, p. 327; cf. Hedström 2005) that can, in principle, generate testable predictions and, thus, be vetted for their causal validity. Second, sociological theories must have greater causal validity than commonsense explanations if sociology is to make good on its promise of being more valuable than mere common sense. Third, in conducting their research, sociologists often focus on the verstehen mode without engaging in the causal mode.

For Watts, the upshot of these three observations is that sociological explanations are typically indistinguishable from, and therefore no more valuable than, common sense and that sociological practice must undergo a fun-

[1] Direct correspondence to Catherine Turco or Ezra Zuckerman, Sloan School, MIT, 50 Memorial Drive, Cambridge, Massachusetts 02142. E-mail: cturco@mit.edu or ewzucker@mit.edu

0002-9602/2017/12204-0007$10.00

damental transformation.[2] In particular, Watts advocates that the field shift to the causal mode of inquiry—marked by the use of experiments, large-*N* data sets, and verification via predictive utility more generally. In his view, the verstehen mode of inquiry has been "satisfying" to sociologists in the past largely because it conforms to our own commonsense intuition and because the accounts it produces can be "extremely rich and detailed . . . limited only by the analyst's imagination" (p. 343). However, he contends that verstehen is "in tension" with "scientific validity" (p. 343). As the field begins to appreciate this, he argues, "sociologists will increasingly have to choose between unsatisfying scientific explanations and satisfying but unscientific stories" (p. 313). Watts makes a pitch for the former and says that the payoff will be theories of greater explanatory power.

All of this raises the question, however, of what—if any—role verstehen might play in the development of causally valid explanations that are superior to common sense. Watts stops just short of saying that verstehen has no useful role to play. At the end of his essay, he concedes that it could "be viewed as a source of plausible hypotheses." Nevertheless, he immediately goes on to say that "in the absence of future testing, [such hypotheses] are no more likely to be causal than numerous other potential mechanisms, both plausible and implausible, that might have quite different implications for prediction and intervention" (p. 343). This is hardly a ringing endorsement for the pursuit of verstehen. What is more, Watts does not provide a single example where the two modes of inquiry complement one another, nor does he provide any guidance on how they might ever do that. As noted, he instead asserts that the two modes are "in tension" (p. 343) and argues that "explanations that are invoked to make sense of observed behavior in terms of actors' intentions, habits, beliefs, opportunities, circumstances, and so on"—that is, explanations developed in the verstehen mode—"cannot in general be expected to satisfy standards of causal explanation." Accordingly, he directs sociologists to "place less emphasis on understandability (i.e., sense making)" going forward (p. 315). The strong implication is that the pursuit of verstehen is a *diversion*—a waste of sociological time and energy.

This implication is what motivates our comment. We do not challenge Watts's three observations noted above. We seek only to demonstrate that his observations do not imply that the pursuit of verstehen is a diversion for the field. To the contrary, we argue that *theories that account for actors' situated intentions, beliefs, and opportunities aid in the identification of generalizable causal mechanisms*.

We organize our essay as follows. First, we note two logical flaws in Watts's argument about the relationship between verstehen and causal va-

[2] Watts focuses on sociology but at various points suggests that sociological issues are generic to the social sciences. Similarly, we focus on sociology and suppose that the issues apply, mutatis mutandis, to other social sciences.

lidity: (a) he notes problems with verstehen-based sociology but does not show that the road is any easier when one puts verstehen aside, and (b) he rejects the proposition that verstehen is *sufficient* to achieve causal validity but leaves open the more reasonable proposition that the pursuit of verstehen can, and often does, improve causal validity. Second, we demonstrate that experimental research by sociologists often relies on verstehen to develop predictions involving generalizable causal mechanisms. This undermines Watts's suggestion that experimental research is an alternative to verstehen (pp. 335–36); and the studies we discuss provide existence proof for the general proposition that a focus on verstehen can improve causal validity. Third, we provide an overview of the logic of ethnographic explanation. Watts does not mention ethnography, perhaps assuming that ethnographers' tendency to embrace interpretive methods and to avoid explicit causal theorizing is consistent with his thesis that a focus on verstehen comes at the expense of causal validity. In contrast, we discuss how great ethnographic work improves upon common sense even while relying on verstehen and how it can be fertile ground for the identification of generalizable causal mechanisms. Finally, we argue that research that focuses solely on causal validity risks making less accurate predictions than research that also pursues verstehen. Here, we focus on two examples from Watts and his colleagues' own research on small worlds and on the social construction of musical preferences. Each line of work has made important contributions, but each has limitations precisely because little attempt was made to empathize with the agents involved. We conclude by reviewing the good news that sociologists' focus on verstehen is appropriate and productive, although we add the sobering news that verstehen-based sociology will always look less "scientific" than might be wished by those who use the natural sciences as their standard.

LOGICAL HOLES IN WATTS'S ARGUMENT

The linchpin of Watts's argument is that there is a tension between two sociological modes of inquiry and associated epistemological standards for evaluating theories. What we refer to as the verstehen mode is alternatively referred to by Watts and others as "verstehen," "empathy," "mental simulation," or "rationalization." In this mode, good explanations are those that meet the standard of "understandability," which involves isolating the intentions, beliefs, and opportunities that motivate action. The causal mode involves the discovery of "generalizable causal mechanisms" (p. 327) that can, in principle, be manipulated to produce testable predictions. In this mode, good explanations are those that have been shown to be predictive.[3]

[3] We agree with Watts (see pp. 335–42) that once one has (usefully) defined prediction to encompass (a) nondeterministic (as well as deterministic) prediction, (b) out-of-sample

As noted, Watts's treatment suggests that an unintended consequence of sociologists' focus on satisfying the standard of understandability is that they may be diverted from meeting the standard of predictive utility and, thus, fail to generate causally valid explanations. By the end of his essay, Watts predicts that as more "scientifically valid" methods—that is, experimental tests, "big data" analyses, and explanations that have been vetted for their predictive utility—come to dominate the social sciences in the next several years, we will discover that much of received sociological wisdom—born as it has been from the mode of verstehen—actually has little explanatory power.

There can be no debating Watts's premise that the two modes of inquiry and associated standards are distinct. As he points out, the physical sciences operate purely in the causal mode. Physical scientists do not find it productive to imagine what it would be like to be an electron or cell in order to explain its behavior. Yet while there is no place for verstehen in the physical sciences, Watts observes that the social sciences are different. Sociologists, in particular, often work in the verstehen mode and invoke the standard of understandability much like commonsense explanations: we try to explain people's behavior by putting ourselves in their shoes and understanding their intentions, beliefs, and opportunities.[4] The question is whether working in the verstehen mode and trying to satisfy the standard of understandability tends to divert sociologists from working in the causal mode and satisfying the standard of predictive utility.

Watts's treatment provides much reason to think the answer is yes. His pessimism is rooted in his observation—which he derives from Hempel and Oppenheim (1948)—that "understanding of the empathetic variety [is] neither necessary nor sufficient for scientific validity" (p. 318). This observation has two implications: (a) that some "empathetic explanations" (i.e., the sort of explanations generated by the verstehen mode of inquiry; p. 318) do not have predictive utility; and (b) that there exist some predictively useful non-

---

testing in data about the past (as well as the prediction of future events), and (c) the prediction of stylized facts (i.e., repeated social patterns) rather than specific facts, then satisfying the standard of predictive utility is necessary to establish causal validity. In addition, Watts correctly notes (p. 337) that it is quite possible—and often productive—to make predictions based purely on analyzing relationships among variables in data about the past but without attention to causal structures. Such exercises are not Watts's focus, nor are they ours.

[4] It is worth noting that commonsense explanations of the physical world generally take the same form as scientific explanations in that domain as well. That is, we generally do not ascribe motives to chairs or earthquakes either. One could argue that this is a result of the achievements of modern science, and that premodern societies indeed saw motivated action (of the gods) where we now believe there was none. Watts seems to be arguing that something similar should occur in the social sciences—that "scientific" work should not concern itself with motives and reasons. We argue in this essay that he is mistaken.

empathetic explanations of social phenomena (i.e., explanations that do not rely upon verstehen). Each of these implications seems reasonable. As Watts notes, commonsense explanations seem to be empathetic but often have limited causal validity. And Watts's references to experimental evidence on unconscious stimuli provide existence proof for the fact that nonempathetic explanations may be causally valid. Also, as discussed below, Watts and his colleagues have made progress by furnishing us with theory that is causally valid, and such work has not explicitly relied on verstehen.

But nowhere in Watts's essay does he consider whether the two modes of sociological inquiry might be complementary. A minimal version of such a proposition of complementarity might read thus: satisfying the standard of understandability *can improve* the causal validity of a social theory. Watts provides no reason for sociologists to lose their confidence in such a minimal proposition, nor even in a stronger proposition that meeting the standard of understandability *generally improves* the causal validity of a social theory.

To be precise, there are several logical problems with Watts's attempt to persuade sociologists that the pursuit of verstehen is in tension with the pursuit of causal validity. For starters, Watts does not actually focus on rejecting either of the above propositions. He focuses instead on rejecting the proposition that verstehen is sufficient for achieving causal validity. This is evident in his treatment of what he calls "the indeterminacy problem." Here, Watts attacks explanations of collective behavior that rely solely on rationalizing such behavior in terms of "representative agents"—in particular, individuals who display a group's average characteristics. Watts alleges that sociologists generally have not absorbed "one of the signature accomplishments of mathematical social science" (p. 331), which is that collective behavior is emergent. As exemplified by Granovetter's (1978; see also Schelling 1978) "threshold" model, the lesson is that, when individual preferences or beliefs are interdependent, slight differences in otherwise identical distributions of such preferences or beliefs—which imply very similar representatives if computed as averages—will generate starkly different collective outcomes. And yet it is straightforward to demonstrate how verstehen plays an important role in advancing causal validity in models of collective emergence—in other words, how verstehen is complementary to, not a diversion from, the pursuit of causal validity.

Consider the following question: How does one decide whether to apply Granovetter's threshold model or Lieberson's (2000) "taste for popularity" (TFP) model? (See also Arthur 1994; Lieberson and Lynn 2003; Obukhova et al. 2014.) These two models of collective behavior are very similar. Each assumes that an individual's interest in a practice is a function of how many of her peers have adopted (or are expected to adopt) that practice. Each assumes that individuals vary in their "threshold" for adoption—for example, some people may be willing to join a protest if at least 20 other people join,

whereas others will join only when 40 others have. And each demonstrates that different collective dynamics will emerge depending on slight differences in how those thresholds are distributed in a population. At the same time, there is one key difference between these models. In the TFP model, individuals are assumed to have a cap as well as a threshold for adoption, with collective outcomes becoming unstable when the practice becomes too popular. In the threshold model, however, practices never become less attractive due to their popularity. The question then is how one decides which model to use in order to predict collective behavior.

This question becomes tractable as soon as we apply verstehen and place ourselves in the position of the individuals whose collective behavior we are trying to model. The threshold model should be applied when the decision at stake is whether individuals will contribute to a public good; this is because individuals tend to understand, however unconsciously, that the benefits from such goods do not decrease as the number of contributors increases (but see Centola 2013). By contrast, the TFP model is appropriate for cultural expression; practices can be "too popular" in contexts where individuals seek to distinguish themselves from others (see Zuckerman 2016). In short, whereas Watts thinks that emergent collective behavior demonstrates the futility of verstehen, this is true only for a straw-man version in which one rationalizes collective behavior via representative agents.[5] To use these models to make predictions, verstehen is crucial.

Besides the "indeterminacy" problem, Watts spends much of his essay laying out the significant obstacles that prevent empathetic explanations from having causal validity. But even if we concede the significance of these obstacles—in particular, the "frame problem" (pp. 328–31), the "outcome problem" (pp. 332–34), and the "overfitting" problem (pp. 339, 343) —this hardly implies that the pursuit of verstehen is a fruitless diversion. To be sure, the first two problems are thorny epistemological issues, and the third represents a difficult ethical challenge. But Watts has not shown that these are problems that specifically apply to empathetic explanation. Even if we were to conclude that these problems were so damning as to doom empathetic explanations to futility, this would not imply that nonempathetic explanations had escaped the same fate. At least with respect to overfitting, the problem is arguably worse for nonempathetic explanations because our lack of intuition for the mechanisms means that the sole basis for acceptance of such research lies in the results that are presented. Empathetic theories do not rely solely on empirical validation but also on how plausible it is that reasonable individuals would act in the manner supposed by the theory; this sets a higher bar for acceptance of the theory independent of empirical results. Regard-

[5] It is notable that Watts supplies no examples of contemporary sociologists who have not absorbed the lesson from Granovetter's work, which is highly cited in the field.

less, the point is simply this: if one wants to demonstrate that one approach is superior to another, it is insufficient to demonstrate crippling limitations in the latter; one must also demonstrate that these limitations do not apply to the former, and Watts has not done this.

Verstehen and Experimental Work

The main point of the previous section was that contrary to what Watts's treatment suggests, the verstehen mode of sociological inquiry can be complementary with the causal mode of inquiry. We furnished initial evidence for such complementarity by showing how attention to verstehen plays an important role in the causal validity of models of collective behavior. In this section, we provide further evidence in the context of experimental research—a method that Watts presents as an antidote to a reliance on verstehen.

We agree with Watts that experimental research has validated certain "behavioral" mechanisms that cannot be understood as rational responses to situational conditions (or even boundedly rational responses to situational conditions). However, Watts misses the fact that "rationalizing" individual behavior is key to much experimental work by sociologists and is often what distinguishes it from experimental work by psychologists. Examples of sociological work in this vein include (a) Kollock's (1994) analysis of how dyadic trust emerges from attempts to manage uncertainty in product quality; (b) Willer and colleagues' analysis of why people enforce norms they do not endorse (Willer, Kuwabara, and Macy 2009); (c) Kuwabara's (2011) reconciliation of the Molm-Lawler debate about whether cohesion is promoted by negotiated or reciprocal exchange; (d) Ridgeway and Correll's (2006) demonstration of how status hierarchies depend on social validation; and (e) Hahl and Zuckerman's (2014) analysis of why and when high-status actors are viewed as cold and inauthentic. In each of these studies and many others, the experiments test a theory that was developed to make better predictions by rationalizing a puzzling regularity in social life (i.e., "stylized facts"; Watts, p. 339). In short, these works we point to are aimed at satisfying standards of both understandability and causal validity; and in our view they achieve that aim.

To illustrate how this works in practice, consider the set of articles that Toshio Yamagishi and colleagues have published since 2000 that have effectively dismantled the line of research known as "social identity theory" in psychological social psychology (Yamagishi and Kiyonari 2000; Yamagishi and Mifune 2008; see also Rabbie, Schot, and Visser 1989).[6] This line

[6] Note that Yamagishi and colleagues' work has been critiqued by Simpson (2006). We believe that Yamagishi and Mifune's (2008) response and follow-on work (e.g., Platow et al. 2012) have addressed this critique more than adequately.

of work is founded on a canonical experiment, conducted innumerable times since the late 1960s, called the "minimal group" experiment (and the related "mere difference" experiment). The remarkable finding from such experiments is that merely by assigning individuals to one or another arbitrary social category (e.g., those who prefer the artist Klee vs. those who prefer the artist Kandinsky), individuals come to prefer, and are more likely to cooperate with, members of their "in-group" as compared with members of their "out-group."[7] These results have long seemed to derive solely from behavioral mechanisms that are resistant to rationalization.

Yamagishi and colleagues do not challenge these results. Rather, they develop an alternative interpretation that can account for these results as well as make predictions that are inconsistent with the standard interpretation. Specifically, they argue that the subjects' behavior in these experiments is quite reasonable given the situation in which the experimenter has placed them—a situation in which essentially the only information that is common among the individuals is knowledge of the two social categories and who is in which category. Under those conditions, category boundaries become a "focal point" for cooperation (Schelling 1960). When there are gains from cooperation with others in such minimal group experiments, it is reasonable for participants to guess (that all others will guess . . . and so forth) that these boundaries will be used to mark an in-group and an out-group and also reasonable for participants to worry that their access to important resources will be determined by their reputation for cooperating with group members (Yamagishi and Mifune 2008).[8] Accordingly, Yamagishi and colleagues predict and show that classic in-group biases disappear when each individual knows the group to which she has been assigned but such membership is not common knowledge.

This line of work clearly meets the standard of predictive utility, and it does so precisely by dint of its success at meeting the standard of understandability. Indeed, its success is notable for the way that it demonstrates the pitfalls of a line of research that failed to rationalize the fictive situations it had created. Social identity theorists thought that they had merely provided information about social difference; they failed to recognize that they had also created a situation in which it was quite rational to cooperate with one "group" rather than another.[9]

[7] For more on these experiments, see Tajfel (1978) and Ridgeway and Correll (2006) for review.

[8] These experimental situations are akin to those real-world ones in which an authoritarian regime has a monopoly on the public sphere and it uses it to transmit common information about a division in society (e.g., Walder 2009; cf. Swidler 2001, chap. 8).

[9] This error is ironic since it was made by psychologists, the scholars who are responsible for teaching us about the "fundamental attribution error"—which is to assume that actions reflect behavioral dispositions and miss that they might be (rational) responses to

THE DISTINCTIVE LOGIC AND CONTRIBUTION OF ETHNOGRAPHY

Having demonstrated how verstehen plays a crucial role in experimental sociology, we now turn to a very different research tradition that is ignored by Watts: ethnography. Watts makes no mention of ethnography, implying that it has no place in his brave new world. This should not surprise us since ethnography is the method most focused on achieving understandability and, so, by his reasoning, the method least likely to produce generalizable causal mechanisms that can be vetted for their predictive utility. Yet we believe it is a mistake to overlook ethnography in this discussion.

In this section, we aim to demonstrate two things. First, whereas Watts seems to suggest that there is no appreciable difference between the commonsense feel of sociological insight born of verstehen and actors' own commonsense understandings of their world, we lay out the logic for why these are actually quite distinct. When done well, ethnography in pursuit of verstehen can, and often does, transcend mere commonsense logic. Second, we demonstrate that the empathetic explanations emerging from ethnography can reveal generalizable causal mechanisms, which can be the basis for testable predictions.

To start, it is important to acknowledge that ethnography is, of course, more of an interpretive and empathetic endeavor than a predictive one. It is a craft devoted to the pursuit of verstehen, to adopting the emic stance, to discovering actors' meanings in situ—in the terms of this discussion, to meeting the highest standard of understandability. Few have articulated this objective better than Clifford Geertz (1973, p. 3) in his writings on "thick description." According to Geertz, the ethnographer's job is to explain social action with reference to the meaning it has for the actors involved. Drawing on the work of Gilbert Ryle, Geertz tells us that ethnography is about "sorting the winks from the twitches" (Geertz 1973, p. 16)—not just noting the rapid contraction of an individual's right eyelid, but knowing the difference between a mere blink and an affectionate wink, between an affectionate wink and a sly one intended to trick someone into believing there it a conspiracy afoot.

Furthermore, few would debate that ethnographers are more the diagnosticians of our field than its prognosticators. They step into social worlds different than their own and try, in the words of Geertz, to figure out "what the devil is going on" or "what [the actors] are up to" in the situation at hand (1973, pp. 27, 15). Their diagnostic tools are a keen eye, an empathetic

---

contextual conditions (see also Zuckerman 2016). One way of summarizing our critique of Watts is that we worry that a sociology that does not work hard at verstehen is one that is at risk of falling prey to the fundamental attribution error.

stance, and theoretical intuition. What is of importance to our discussion, however, is that skillful use of those tools often allows ethnographers to see more deeply into the logic of a situation than the actors themselves, and this ultimately distinguishes ethnographic explanation from actors' commonsense understandings of their situation. Good ethnography does not just capture actors' ex post rationalizations; it captures their behavior as it is unfolding along with their interpretations and explanations of it, before, during, and after (see Jerolmack and Khan 2014). Moreover, the thorough ethnographer does not simply capture the native point of view. There are multiple, often conflicting points of view in any situation, and a full accounting demands an empathetic stance toward them all—something actors in the situation can neither easily access nor obtain. Finally, an ethnographer armed with theory will be directed to features of the situation that may be obscured from those living it.

In short, when done well, ethnography lets us see things that we otherwise would not and that the actors themselves cannot. We see, for instance, that the Balinese cockfight is not actually about betting (which is all one gets from a cursory glance and which is all the actors participating in it might tell you if asked), but rather a dramatization of men's status concerns (Geertz 1973). We see, too, that the sneaky thrill of shoplifting may explain such acts better than background and demographic characteristics (Katz 1988). Were we to set verstehen aside and focus solely on the causal mode's standard of predictive utility, we would forsake such knowledge. We would be left with big data sets of twitches and systematic predictions of when men are more or less likely to stand around and gamble over their cocks.

At the same time, it is a mistake to think that ethnography can offer only a rich understanding of the situation at hand. As Michael Burawoy writes of the ethnographic project, "We are interested not only in learning *about* a social situation . . . but also in learning *from* that situation" (Burawoy 1991, p. 5; original emphasis). And if this statement represents a goal of good ethnography—and if ethnography is the most case-driven method of our discipline—then it is not clear who Watts is addressing when he alleges that sociologists are uncomfortable generalizing beyond the case at hand (Watts, p. 341). The fact of the matter is through rich interpretive accounts great ethnographies draw out general lessons that have relevance beyond the local setting. And therein lies the path to prediction and causal validity.

To be sure, few ethnographers codify explicit predictions at the end of their work, and some might be uncomfortable embracing this as an objective. (Even this comment's coauthors disagree about the importance of doing such explicit codification in the context of ethnography.) Nevertheless, a hallmark of all great ethnography is that, one way or another, it leaves us with a more refined understanding of social action and a more refined in-

tuition for what to expect in other situations. It leaves us, in particular, with generalizable causal mechanisms that can be vetted for their predictive utility.

To demonstrate this, we next profile two exceptional ethnographies, each of which we believe leads us to more refined understandings of social action. Whereas Watts suggests that "rationalizations" of social action "are no more likely to be causal" than "implausible" explanations, our examples illustrate how the pursuit of verstehen via ethnography can lead to the identification of novel, generalizable causal mechanisms. We acknowledge that deriving predictions from work that has not offered them up explicitly can feel awkward or forced, but the point is simply this: whether you get your kicks from codifying predictions implicit in ethnographic work or you prefer to devour a good ethnography like a good novel, you do so because, when done well, ethnography offers generalizable lessons about the human condition that you will carry into your work and life.

First, consider *Opposing Ambitions,* Sherryl Kleinman's 1996 organizational ethnography of the alternative health cooperative she calls Renewal. Upon entering Renewal, Kleinman encountered a puzzle: even as members were committed to ideals of equality and opposed to capitalist motives, they reproduced conventional inequalities in the organization and obsessed over money in their discussions. With an incisive ethnographic eye, Kleinman came to see that members were so invested in their alternative identity that they denied they also cared about the organization's conventional legitimacy. In the process, they failed to see that their attempts to meet both ambitions (the one they acknowledged, which was to be alternative, and the one they denied, which was to be conventionally legitimate) led them to contradict their own ideals and blindly enact conventional inequalities and logics.

By carefully tracking patterns of interaction and probing actors' in situ understandings, Kleinman makes some fascinating observations. For example, despite the organization's rhetoric of gender and class equality, the male professionals were afforded higher status and given more leeway when breaking valued alternative norms compared to women and nonprofessionals. True or not, everyone assumed the white male professionals on staff could have pursued a more lucrative conventional path than working at a quirky alternative health center with a sliding pay scale. With this "choice" came local power and status: other members saw these men as having willingly sacrificed their privileges to work at Renewal and that sacrifice as worthy of deference.

Kleinman also observed that members constantly declared their hatred for talking about money, but then spent 90% of their meetings talking about just that and in a more animated and engaged way than when they spoke of any other topic. Over time, Kleinman came to see that all this talk of money served an important function for the group because it bolstered both their

alternative and conventional identities. Hours spent agonizing over the moral acceptability of fee raises validated their sense of being alternative, while simultaneously affirming that they were steering a fiscally responsible, legitimate enterprise.

In each of these cases, Kleinman was highly attuned to the actors' own understanding of their situation, but, in each case, she ultimately saw deeper into the logic of that situation than they could. She saw, in short, that their unacknowledged commitment to conventional legitimacy drove them to reproduce conventional inequalities and practices. Each case also lends itself to formalizing a prediction that one might take into another setting or use to advise the leaders of an organization with similarly alternative ambitions. One prediction is that attempts to eradicate gender hierarchy will be undermined to the extent that high-status actors are seen as choosing to give up their status. Another is that attempts to distance an alternative organization from capitalist logic will be undermined to the extent that an organization's survival depends on the financial viability of its business model. (As testament to the predictive utility of Kleinman's study, the intuition embedded in the second prediction guided an inquiry into another alternative organization, the findings of which led to further theoretical refinement and development of several explicit predictions [Turco 2012].)

For a second ethnography, consider *Waiting for José,* Harel Shapira's 2013 study of the Minutemen Civil Defense Corp. Between 2005 and 2008, Shapira camped out with the Minutemen, a group of Americans (almost all men) who left their homes for months at a time and drove days to the U.S.-Mexico border to conduct voluntary patrols for illegal immigrants—patrols that were not always welcome by the U.S. Border Patrol or local residents, and patrols that rarely resulted in spotting an illegal immigrant. In the book's introduction, Shapira takes us back to the first patrol he observed and the puzzling realization he had at the time: "I've started to understand something," he said to himself that night, "We aren't here to catch Mexicans" (Shapira 2013, p. 7). The ethnography that follows is a moving, empathetic exploration of what the devil the Minutemen were up to instead.

Like Kleinman, Shapira makes a series of fascinating observations. For one, when he asked these men why they had joined the Minutemen and came to the border, they talked in terms of immigration and terrorism, and yet their behavior could not be explained solely as the outcome of right-wing ideology. Their ideas about immigration were more nuanced and contradictory than one would expect of a motivating ideology, and they were not without sympathy for hard-working migrants crossing the border. Also, when they spoke of America, it was in terms more similar to progressive notions about the decline of civic participation and the destructive effects of big business than to racist ideology. Not to mention that when the movement's leaders spoke of immigration policy, the rank and file mostly tuned out.

Shapira gained perspective into why these men went to the border by taking account of experiences they had had earlier in their lives and what those experiences meant to them today. Most of the Minutemen were veterans who now felt America had lost its way, and they, their place in it. These men came to the border to enact the masculine soldier ethos they missed and to recreate a sense of themselves as men who protected and served their country. They organized themselves into a strict military-like chain of command, called one another by their handles, spoke of "musters" and "recon work," wore fatigues and salivated over military-grade surveillance equipment, and through it all (re)constructed a sense of self that held meaning and value for them (2013, p. 69).

Shapira's study leaves us with a deeper understanding of the Minutemen than one could get from either the Minutemen's own accounts or prior scholarship and media portrayals of them. And yet we do not just learn about the Minutemen from his study. As Shapira notes in the appendix to his book, his study speaks to a key question for social movement scholars: What motivates people to participate in movements? The Minutemen suggest that beliefs are a necessary but not sufficient answer, he argues. The movement's speeches and press packets were not particularly illuminating as to why these men kept returning to the border. What shed more light on their behavior was a deep understanding of these men's biographies and in the day-to-day practices that let them be the type of person they wanted to be. Accordingly, the study directs us to the importance of biography and practices in social movement recruitment. If individuals construct a sense of self and identity through practices that call forth earlier, meaningful experiences, then social movements wishing to recruit certain types of people would be wise to think about the identities those people most cherish and the sort of practices the movement might let them perform to enact those identities. One simple prediction we might draw from Shapira's study is that social movements wishing to recruit former veterans will be more successful to the extent that participants perform practices that recreate some aspects of military life. But, of course, the prediction need not be about veterans or even social movements. Indeed, one can imagine that company's like Google and Facebook have embraced a similar insight in their attempts to recruit young, talented engineers by recreating a college-campus-like atmosphere in the workplace.

To summarize, this section has made two simple points. First, good ethnography is committed to meeting the standard of understandability, yet it does more than simply reproduce actors' own commonsense understandings of their behavior. Second, good ethnography is committed to meeting the standard of understandability, yet it is also fertile ground for revealing generalizable causal mechanisms that meet the standard of predictive utility. Indeed, its achievement of understandability is what makes the predic-

tions we draw from ethnographies so often compelling. It is hard to see how one could arrive at the predictions we have derived from Kleinman and Shapira by some method other than ethnography, and they seem unobtainable if one avoids the verstehen mode entirely. As in our discussion of experimental work, we have demonstrated a degree of compatibility between the two modes of sociological inquiry that provides a strong counterpoint to Watts's suggestion that the two modes are in fundamental tension.

To be fair, just as Watts acknowledges that his sociological future will be less satisfying for those seeking understandability, we acknowledge that the sort of mechanisms and testable predictions emerging from ethnography may not always be explicitly articulated in the way that Watts and mathematical sociologists would like. That does not mean they are not there, however. Ethnography can be interpretive and predictive, and, as such, it helps us better understand the social world. We agree with Burawoy when he says that ethnography "brings together the perspective of the participant who calls for understanding and the perspective of the observer who seeks causal explanation. It necessarily combines both hermeneutic and scientific moments" (Burawoy 1991, p. 3).[10] If that is true of ethnography, our field's most inductive and interpretive method, then there seems no reason it cannot be true for all sociology.

THE LIMITS OF PREDICTION WITHOUT UNDERSTANDING

Our discussion to this point, ranging as it has from experimental work to ethnography, has provided support for the idea that the verstehen mode of sociological inquiry can be quite complementary with the causal mode of inquiry. In this final section, we argue this same point from a different angle—namely, that research focusing solely on causal validity without attention to verstehen actually risks generating inferior explanations as measured against the standard of predictive utility (i.e., Watts's own stated standard for causal validity). Two cases in point come from two of Watts's justly celebrated contributions—his work on the small world and on the MusicLab. We argue that, in both cases, Watts's predictions are improved by exercises in verstehen.

First, consider Watts and Strogatz's (1998) model of the small world. The main insight of this model is that network structures are often both highly clustered into multiple distinct subgroups and highly connected through short path links. Watts and Strogatz demonstrate that such highly clus-

[10] Indeed, we might do well to heed Burawoy's use of the term "moments" instead of "modes." Rather than see the verstehen and causal approaches as distinct modes of inquiry, perhaps it is more productive to think of them as simply two moments that every good sociological endeavor should encompass.

tered/highly connected social structures are produced whenever there are a few "shortcuts" that bridge otherwise distant clusters. Moreover, insofar as information, ideas, and practices travel along such shortcuts, it is possible for a social system to have high levels of community cohesion (within subgroups) at the same time as it has rapid transmission of information and ideas (between subgroups). All that is required is the "rewiring" of a few shortcuts to produce dramatic gains in the efficiency of such transmissions.

But how accurate is the prediction that small world networks facilitate the spread of ideas, information, and practices? One reason to doubt the model's predictive utility derives from the fact that it makes strong assumptions about how and why actors transmit and receive valuable resources. For instance, Reagans and Zuckerman (2009) note that the model implicitly assumes that the actors are altruistic, and this is problematic because shortcuts necessarily create powerful middlemen who are in position to extract a high price for facilitating transmissions (cf. Burt 1992; Reagans and Zuckerman 2008). This is not an issue in physical networks (e.g., transmission of disease) where the nodes are "dumb." However, to use the small world model to make accurate predictions in human interaction and outcomes, one must understand whether middlemen will attempt to profit from their situation or not; if they do, valuable information and resources cannot be expected to flow smoothly unless they are compensated in some way, thus adding friction to the system.

Furthermore, Centola and Macy (2007) note that the prediction that small worlds will efficiently transmit ideas, information, and practices also depends on a second assumption—that individuals are as likely to accept a transmission when they hear it from one person (i.e., someone on either end of a shortcut between clusters) or from two (i.e., within one's cluster). And yet it requires just a bit of verstehen to recognize that there are some ideas—labeled "complex contagions" by Centola and Macy—that require social validation to be credible or legitimate (cf. Carroll and Hannan 1989; Ridgeway and Correll 2006). In another experimental contribution rooted in verstehen, Centola (2010) demonstrates that his and Macy's model has greater predictive utility than does the small world model; small world networks do not, in fact, facilitate the transmission of complex contagions.

Similar verstehen-based limitations also pertain to the MusicLab study of Salganik, Dodds, and Watts (2006; Salganik and Watts 2008). This creative experimental study made a landmark contribution to our understanding of two stylized facts about cultural tastes in contemporary Western societies—that such tastes tend to be (a) concentrated, with winner-take-all outcomes, where the most popular cultural products achieve a level of popularity that belies the small objective differences between them and less popular products; and (b) unpredictable, so that even industry insiders can rarely anticipate which products will do well and which will not.

Remarkably, the MusicLab experiments were able to reproduce these effects. To do so, the authors had anonymous participants in an online community of alternative rock aficionados sample and rate a set of songs from unknown bands. Some participants were randomly sent to "social" conditions in which the popularity of the songs (based on prior downloads) was visible, whereas others were sent to an asocial condition where song popularity was not visible. The main result was quite clear: (a) concentration was significantly higher in the social conditions and (b) the winners and losers were different in each social condition, such that knowing the outcome in the asocial condition was of limited value in predicting success in the social conditions. Moreover, the logic behind these results is quite straightforward. As Watts (2014, p. 331) hints by citing Bikchandani, Hirshleifer, and Welch's (1992) classic "information cascade" model, participants in the MusicLab were using visible popularity information to make inferences about the quality of the songs. Bikchandani et al. showed that, theoretically, such inferences can lead to cumulative advantages whereby early, random leads in popularity build on themselves to generate outcomes that are substantially decoupled from underlying quality differences (see also Zhang 2010; Gould 2002).

But how predictively useful is the MusicLab experiment? As reviewed by Zuckerman (2012, pp. 227–30), there is at least one additional "stylized fact" that defines cultural markets in the West but that Watts and his colleagues do not mention—namely, rapid change in what is popular (see Lieberson 2000; Lieberson and Lynn 2003). Above, we discussed a key reason why: individuals in modern liberal societies tend to avoid overly popular cultural practices and to seek some degree of differentiation from others; accordingly, the very popularity of a cultural practice eventually undermines itself. But in the MusicLab, early advantages cumulate forever. The reason is an unacknowledged limitation of the MusicLab experiment: participants were anonymous, so there was no reason for them to seek differentiation and, thus, no basis for fashion cycles to emerge as they do in the real world. Moreover, a second limitation derives from the anonymity and the independence of consumption that follows from it: Salganik and Watts (2008) show that cumulative advantage processes are effectively broken when popularity negatively correlates with quality, but this is a problematic prediction if we apply it to contexts where there are strong incentives to conform with prevailing practices lest one be considered incompetent or disloyal (e.g., in an authoritarian regime [Obukhova et al. 2014; cf. Zuckerman 2012, pp. 233–34]). The MusicLab thus demonstrates a generalizable causal mechanism—that is, the tendency to infer quality from popularity—but this mechanism is of limited utility for making predictions in contexts where consumers are not anonymous and where other mechanisms (the tendency to avoid practices that are highly popular; the tendency to avoid practices that are regarded as deviant) are thereby operative.

As with the small world model, we view the MusicLab experiment as a significant contribution. At the same time, we note that something is missed in both of these studies because the authors did not engage in verstehen and recognize the particular social situations that were simulated or created. Watts and colleagues have certainly identified generalizable causal mechanisms of importance, but we can do a better job using them to predict social outcomes if we think carefully about the situation in which the actors find themselves and make adjustments to take into account those actors' intentions, beliefs, and opportunities.

CONCLUSION

As we stated at the outset, we are grateful to Watts for having pushed sociologists to clarify the method to our madness. There is a great deal of room under the big tent that is contemporary sociology, and that includes nonempathetic explanations of social phenomena. We believe, however, that we have made the case for why verstehen-based sociology continues to be at the center of the sociological enterprise. It is not simply because such explanations are "satisfying." It is because they are useful for revealing generalizable causal mechanisms (that can be vetted for their predictive utility). Moreover, we have made the case that in sociological research ranging from experiments to ethnography, efforts to meet the standard of understandability often, and perhaps always, improve predictive utility as well. If the natural conclusion from Watts's essay is that verstehen is a diversion from the causal mode, we have tried to show that it is, instead, essential. As Pierre Bourdieu (2008, p. 95) once put it:

> While [the sociologist] declines to give credit to the consciousness that the subjects have of their situation and to take literally the explanation they give of it, he takes that consciousness sufficiently seriously to try to discover its real foundation, and can only be satisfied when he manages to bring together in the unity of an understanding the truth immediately given to lived consciousness and the truth laboriously acquired through scientific reflection. Sociology would not be worth an hour of effort if its sole aim were to discover the strings that move the actors it observes, if it were to forget that it is dealing with people, even when those very people, like puppets, play a game of which they do not know the rules, in short, if it did not assign itself the task of restoring to those people the meaning of their actions.

We close with a caveat, however. Although we believe that verstehen-based sociology can be a fertile ground for the generation of causally valid social science, empathetic explanations will always be limited relative to what we might hope and expect. Two reasons for this are that (a) contextual factors are often so overwhelming that it is not clear how to formulate predictions based on prior research (cf. the "frame problem" discussed by

Watts) and that (b) human beings are themselves lay theorists who consume social scientific theory and incorporate it in their action strategies, and, as they do, they render many social scientific predictions invalid. Watts sees such issues as problems with certain ways of doing social science. In fact, they are inherent features of the social world. The core challenges of causal explanation of social behavior lie in the nature of the subject matter, not in the method pursued. In our opinion, the solution does not lie in bypassing the critical work of trying to figure out "what the devil is going on," no matter how hard it is to answer that short, deceptively simple question.

Catherine J. Turco and Ezra W. Zuckerman
MIT

REFERENCES

Arthur, W. Brian. 1994. "Inductive Reasoning and Bounded Rationality." Papers and Proceedings of the Hundred and Sixth Annual Meeting of the American Economic Association. *American Economic Review* 84:406–11.

Bikhchandani, Sushil, David Hirshleifer, and Ivo Welch. 1992. "A Theory of Fads, Fashion, Custom, and Cultural Change as Informational Cascades." *Journal of Political Economy* 100 (4): 992–1026.

Bourdieu, Pierre. 2008. *Bachelor's Ball.* Chicago: University of Chicago Press.

Burawoy, Michael. 1991. *Ethnography Unbound: Power and Resistance in the Modern Metropolis*. Berkeley: University of California Press.

Burt, Ronald S. 1992. *Structural Holes: The Social Structure of Competition.* Cambridge, Mass.: Harvard University Press.

Carroll, Glenn R., and Michael T. Hannan. 1989. "Density Dependence in the Evolution of Populations of Newspaper Organizations." *American Sociological Review* 54:524–41.

Centola, Damon. 2010. "The Spread of Behavior in an Online Social Network Experiment." *Science* 329:1194–97.

———. 2013. "A Simple Model of Stability in Critical Mass Dynamics." *Journal of Statistical Physics* 151:238–53.

Centola, Damon, and Michael Macy. 2007. "Complex Contagions and the Weakness of Long Ties." *American Journal of Sociology* 113:702–34.

Geertz, Clifford. 1973. *The Interpretation of Cultures.* New York: Basic Books.

Gould, Roger V. 2002. "The Origins of Status Hierarchies: A Formal Theory and Empirical Test." *American Journal of Sociology* 107:1143–78.

Granovetter, Mark S. 1978. "Threshold Models of Collective Behavior." *American Journal of Sociology* 83 (6): 1420–43.

Hahl, Oliver, and Ezra W. Zuckerman 2014. "The Denigration of Heroes? How the Status Attainment Process Shapes Attributions of Considerateness and Authenticity." *American Journal of Sociology* 120:504–54.

Hempel, Carl G., and Paul Oppenheim. 1948. "Studies in the Logic of Explanation." *Philosophy of Science* 15 (2): 135.

Hedström, Peter. 2005. *Dissecting the Social: On the Principles of Analytical Sociology*. Cambridge: Cambridge University Press.

Jerolmack, Colin, and Shamus Khan. 2014. "Talk Is Cheap: Ethnography and the Attitudinal Fallacy." *Sociological Methods and Research* 43 (2): 178–209.

Katz, Jack. 1988. *Seductions of Crime.* New York: Basic Books.

Kleinman, Sherryl. 1996. *Opposing Ambitions: Gender and Identity in an Alternative Organization.* Chicago: University of Chicago Press.
Kollock, Peter. 1994. "The Emergence of Exchange Structures: An Experimental Study of Uncertainty, Commitment, and Trust." *American Journal of Sociology* 100:313–45.
Kuwabara, Ko. 2011. "Cohesion, Cooperation, and the Value of Doing Things Together: How Economic Exchange Creates Relational Bonds." *American Sociological Review* 76:560–80.
Lieberson, Stanley. 2000. *A Matter of Taste: How Names, Fashions and Culture Change*. New Haven, Conn.: Yale University Press.
Lieberson, Stanley, and Freda B. Lynn. 2003. "Popularity as a Taste: An Application to the Naming Process." *Onoma* 38:235–76.
Obukhova, Elena, Ezra W. Zuckerman, and Jiayin Zhang. 2014. "When Politics Froze Fashion: The Effect of the Cultural Revolution on Naming in Beijing." *American Journal of Sociology* 120:555–83.
Platow, Michael J., Margaret Foddy, Toshio Yamagishi, Ji Lim, and Aurore Chow. 2012. "Two Experimental Tests of Trust in In-group Strangers: The Moderating Role of Common Knowledge of Group Membership." *European Journal of Social Psychology* 42:30–35.
Rabbie, Jacob M., Jan C. Schot, and Lieuwe Visser. 1989. "Social Identity Theory: A Conceptual and Empirical Critique from the Perspective of the Behavioural Interaction Model." *European Journal of Social Psychology* 19:171–202.
Reagans, Ray E., and Ezra W. Zuckerman. 2008. "Why Knowledge Does Not Equal Power: The Network Redundancy Tradeoff." *Industrial and Corporate Change* 17: 903–44.
———. 2009. "The Short Cut to Inefficiency? Implications of the Small but Stratified World." Manuscript. MIT Sloan School of Management.
Ridgeway, Cecilia L., and Shelley J. Correll. 2006. "Consensus and the Creation of Status Beliefs." *Social Forces* 85:431–53.
Salganik, Matthew J., Peter Sheridan Dodds, and Duncan J. Watts. 2006. "Experimental Study of Inequality and Unpredictability in an Artificial Cultural Market." *Science* 311:854–56.
Salganik, Matthew J., and Duncan J. Watts. 2008. "Leading the Herd Astray: An Experimental Study of Self-fulfilling Prophecies in an Artificial Cultural Market." *Social Psychology Quarterly* 71:338–55.
Schelling, Thomas. C. 1960. *The Strategy of Conflict.* Cambridge, Mass.: Harvard University Press.
———. 1978. *Micromotives and Macrobehavior.* New York: Norton.
Shapira, Harel. 2013. *Waiting for José: The Minutemen's Pursuit of America.* Princeton, N.J.: Princeton University Press.
Simpson, Brent. 2006. "Social Identity and Cooperation in Social Dilemmas." *Rationality and Society* 18:443–70.
Swidler, Ann. 2001. *Talk of Love: How Culture Matters*. Chicago: University of Chicago Press.
Tajfel, Henri. 1978. *Differentiation between Social Groups: Studies in the Social Psychology of Intergroup Relations*. London: Academic Press.
Turco, Catherine. 2012. "Difficult Decoupling: Employee Resistance in the Commercialization of Personal Settings." *American Journal of Sociology* 118 (2): 380–419.
Walder, Andrew, G. 2009. *The Fractured Rebellion: The Beijing Red Guard Movement.* Cambridge, Mass: Harvard University Press.
Watts, Duncan J. 2014. "Common Sense and Sociological Explanations." *American Journal of Sociology* 120:313–51.
Watts, Duncan J., and S. H. Strogatz. 1998. "Collective Dynamics of 'Small-World' Networks." *Nature* 393 (6684): 440–42.

Weber, Max. 1968. *Economy and Society: An Outline of Interpretive Sociology*. New York: Bedminster Press.
Willer, Robb, Ko Kuwabara, and Michael W. Macy. 2009. "The False Enforcement of Unpopular Norms." *American Journal of Sociology* 115:451–90.
Yamagishi, Toshio, and Toko Kiyonari. 2000. "The Group as the Container of Generalized Reciprocity." *Social Psychology Quarterly* 63:116–32.
Yamagishi, Toshio, and Nobuhiro Mifune. 2008. "Does Shared Group Membership Promote Altruism? Fear, Greed, and Reputation." *Rationality and Society* 20:5–30.
Zhang Juanjuan. 2010. "The Sound of Silence: Observational Learning in the U.S. Kidney Market." *Management Science* 29:315–35.
Zuckerman, Ezra W. 2012. "Construction, Concentration, and (Dis)Continuities in Social Valuations." *Annual Review of Sociology* 38:223–45.
———. 2016. "Optimal Distinctiveness Revisited: An Integrative Framework for Understanding the Balance between Differentiation and Conformity in Individual and Organizational Identities." Chap. 10 in *The Oxford Handbook on Organizational Identity*, edited by Michael G. Pratt, Majken Schultz, Blake E. Ashforth, and Davide Ravasi. New York: Oxford University Press.