

Beyond action

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Ethnography

2017, Vol. 18(1) 88–96

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DOI: 10.1177/1466138115592420

journals.sagepub.com/home/eth



Abstract

Isaac Reed, like the great early work of Talcott Parsons, is focused on social action and continental theorization and largely blind to the broader American interactionist tradition. Absent are Blumer, Dewey, Mead, James (though Peirce appears fleetingly), or the more phenomenological tradition of Schutz or Garfinkel. Absent too is the relational theorizing of gender and race scholars. Given all that is in Reed, it is absurd to suggest that there isn't enough. The point of this paper is different. It is simply that there are alternate important traditions that ethnographic work draws upon that might aid in pushing Reed's arguments in different directions. I hope to show how an interactionist perspective proves fruitful in moving us beyond some of the basic challenges of the classical tradition.

Keywords

social action, ethnography, interactionism, theory, interpretivist

One begins Isaac Reed's *Interpretation and Social Knowledge* expecting something quite new and radical. And by the end, what we get is something quite old-fashioned and classic. This is not a criticism; it is praise. But still it remains a curiosity. The opening is bold. 'Our understanding of social knowledge is due for a massive transformation' (p. 1). The end is certainly no whimper. Upon wading through Reed's difficult and erudite argument – effectively an impassioned case for interpretivism – we are told, 'Perhaps we now have the conceptual resources to make this project our calling' (p. 162).

Interpretivism is the position that man is meaning-making and meaning made. The implication of this position is that as researchers we must pay attention to the meanings that actors make through their activities, the ways in which meaning systems create the conditions of that making, and the fact that the social observer is embedded within and participating in the same processes he is observing. This last part has important implications for those of us who claim to be knowledge

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generators of any kind. With this basic description, the reader is no doubt justified in wondering, 'What, then, is old-fashioned?'

Simply this: Reed's argument is effectively a new solution to 'structure/agency' debates. The elements are renamed – structures are now 'mechanisms' and agency is 'motivation'. And the interpretivist solution rests on a something new: 'landscapes of meaning' (the culmination of Reed's argument is Figure 12, on page 160). But the element to be explained is social action. The unit of analysis (or for the most part in Reed's case, the unit of example) is the social actor.

It's not clear how an elegant re-working of structure/agency debates constitutes a massive transformation in our thinking. This is not to say that such a re-working is trivial, or that the reader will not glean deep and important insights from this ambitious and important work. But for the ethnographer, the basic observation that man is a meaning-making agent and this has consequences for one's practice is rather banal; indeed, it tends to be the starting point for most studies. Further, it has become standard practice among ethnographers to embed themselves within their texts, so that author and reader alike must take into account the ways in which their location influences their observations. In this sense, the value of Reed's work might be greatest for those areas of the discipline where such a position is viewed as problematic. The more important point for ethnographers relates to the empirical and methodological consequences to interpretivism that Reed elucidates. One such consequence is that of causality. These are interesting arguments. In this paper I will ignore them.

I will instead focus on a different area of this work to argue that Reed's classicism holds the argument back. By his classicism I refer here not to Aristotle, to whom Reed owes a deep debt for his arguments on causality. We need not go that far back. Reed seeks to return us to a simple definition of sociology that many of us learned in our introductory classes. Some of us might be able to recite it by heart. You should all recognize your Weber. 'Sociology... is a science which attempts the interpretive understanding of social action in order thereby to arrive at a causal explanation of its course and effects' (1964: 88). It's all there for us; we have the three core elements of Reed's argument: interpretive, social action, causal explanation. The point of *Interpretation*, it seems to me at least, is effectively that we've gone astray in how we've built upon Weber. Positivism and realism have proved poor guides. Instead, mix a little Dilthey and Saussure with our Weber, and we can be off and running again.

There is something Parsonian in this project. Again, that is not a criticism; it's a compliment. Where *The Structure of Social Action* sought a convergence thesis around the importance of action, *Interpretation and Social Knowledge* seeks a more pluralist path, but one that has at its core a Weberian attention to social action, and a suggestion that Parsons and others sent us down a blind alley when relying more on positivism than interpretivism (to say nothing of the realists!). *Interpretation* is a small book with big ambitions. One of those is to make the reader seriously reconsider our understanding of social theory. On this level, it succeeds marvelously.

My argument in this short response to Reed is not that *Interpretation* is wrong. It's that like Parsons, Reed is so focused on social action, continental theorization, and classical texts that he is largely blind to the broader interactionist tradition here on our own shores. Absent are Blumer, Dewey, Mead, James (though Peirce appears fleetingly), or the more phenomenological tradition of Schutz or Garfinkel. Absent too is the relational theorizing of gender and race scholars. Given all that is *in* Reed, it is absurd to suggest that there isn't enough; the only argument a reasonable reader could possibly make upon reading *Interpretation* is that there is too much to get one's head around in this tightly argued book. So my point is different. It is simply that there are alternate important traditions that ethnographic work draws upon that might aid in pushing Reed's arguments in different directions. In order to develop this point, I draw upon my own ethnographic work, *Privilege*, to show how an interactionalist perspective proves fruitful in moving us beyond some of the basic challenges of the classical tradition.

How *Privilege* works

My work, *Privilege*, is a fairly straightforward, standard ethnography (Khan, 2011). I moved to a place for a year, interacted with and observed people, wrote notes about the experience, and then wrote these notes up into a book. The method might be better called a participant observation. The theoretical apparatus is similarly straightforward. I draw upon a variety of theoretical traditions – from Tocqueville to Bourdieu to Butler – to explain my observations.

The place I observed was St. Paul's School: one of the more elite boarding schools in the world. In 2013 the estimated cost of this high school was \$53,000 a year. From the 2004 to 2005 academic year, I worked as a faculty member and lived at the school (located in Concord, New Hampshire). My project was public knowledge. I was interested in getting a broad sense of the life of the place – namely how its 500 students, its faculty, and its staff experienced their lives there and were formed (or transformed) by their experiences at the institution. My status as an alumnus of St. Paul's School (the class of 1996) helped me gain access to the site and build rapport with people there.

My primary interest in St. Paul's – a place that vigorously embraces the importance of being an 'open' or representative institution – was making sense of how moral commitments to equality can be so tightly coupled with a context where inequality is increasing. From a research perspective, this meant asking students how they made sense of their lives, the world around them, and what their lives were like at a place like St. Paul's. And then it meant watching how they actually lived. For an analytic perspective, one of my primary interests was the juxtaposition of what people said about the world, and then how they interacted within it (Khan and Jerolmack, 2013). One might say that one of the great analytic advantages of ethnographic work is in showing the relationship between sentiments and acts (Deutscher et al., 1993; Jerolmack and Khan forthcoming). That is to say that their cognitive orientations to the world are not always very good predictors of

their actions within the world – this has important implications for Reed’s argument; I will return to this point below.

In terms of my own work what I found was that when talking with students, most constructed a narrative of having achieved through their own capacities and, importantly, through their hard work. Yet I contrasted this with observations I made in the field. Those observations revealed that most students did not work particularly hard. Beyond this, in social interactions they worked to discipline other students who went beyond working hard and actually manifested this behavior. In short, they marginalized the few students who manifested a strict work ethic (importantly, I found dimensions of difference across social categories like race, class, and gender; I will gloss over those here). The culminating argument of *Privilege* is that the reason for this discrepancy between students’ words and actions is that in a world marked by greater openness, students recognize the importance of rhetorically embracing meritocracy – even as they work in practice to protect the advantages they have most often inherited. The combination of rhetorically embracing openness while practicing protection is what I have termed ‘privilege’.

Theoretically, this work rests on fairly simple, or at least well-trodden, ground. It argues that one of the central aspects of learning privilege is embodied ease. To quote somewhat at length:

This elite ease is also an embodied interactional resource. In looking at seemingly mundane acts of everyday life – from eating meals to dancing and dating – we will see how privilege becomes inscribed upon the bodies of students and how students are able to display their privilege through their interactions. In being embodied, privilege is not seen as a product of differences in opportunities but instead as a skill, talent, capacity – ‘who you are.’ Students from St. Paul’s appear to naturally have what it takes to be successful. This helps hide durable inequality by naturalizing socially produced distinctions. (p. 16)

Such an argument is essentially an extension of Bourdieu’s idea of habitus. At its core is the idea of an interaction – the basic unit of analysis. An example can prove fruitful here. Imagine that two people can perform with the same ‘ease’ at a classical music concert. They know the same amount about the music about to be played. They are comfortable in their seats, dressed in business casual attire, and are able to have informed conversations about the evening. Now imagine that one is Asian and the other is black. What are the chances of both being able to express the same ease? Or, in Bourdieuan terms, to have their equivalent capital be recognized as having the same value? I argue in *Privilege* that the value of resources is interactionally determined. Capital is not like money in your wallet; \$20 is only worth \$20 if others are willing to accept your currency. And when it comes to cultural competencies, social ties, or symbolic invocations, we see many instances where similar amounts of social, cultural, and symbolic capital do not have equivalent values for differently situated actors. This is because such capital exists as an

exchange value, and others within interactions must recognize the value of one's capital; they are not always willing or able to do so. This argument points to the importance of situations and interactions.

In order to do make such an argument compelling, *Privilege* is structured, again, in a rather conservative way. The basic plan of the book is to proceed by what might be called 'the logic of science' – and that logic is negation rather than confirmation (Stinchcombe, 1970: ch. 1). Each section of the text makes a claim. The subsequent section attempts to negate that claim, generating a new claim. In the second chapter, for example, I begin with the importance of every student finding a 'place' at the institution, and how such places allow for students to construct arguments about their own hard work and success and understand the world as a place where hard work pays off. The following section then focuses on the staff, a group whose hard work is recognized, but who are seen to never advance. They also are often ignored, largely 'placeless'. In the third chapter I argue for the importance of 'ease', but then show how certain students are less at ease (focusing on black students and, in the next chapter, girls), and then try to explain why. And so the book goes: claim, negation, claim, negation.

This structure or argumentation is fairly standard in ethnographies and has its roots in a kind of old-fashioned view of science where ideas progress through their attempted refutation rather than confirmation (breaching 'experiments' in ethnographic texts are an example of this basic impulse; one constructs negative cases of a phenomenon to see if its potential impact is as expected). To better evoke this structure, readers might consider a work they are likely to be more familiar with: say, Mitchell Duneier's *Sidewalk*. As Duneier develops his arguments about street vendors, he tests them in various contexts, attempting to negate potential observations. The most obvious might be comparing how his mostly black magazine sellers compare to the mostly white Christmas tree vendors who perform similar tasks in similar spaces.¹ In this sense, the epistemology behind the structure of most ethnographic arguments is one of the most basic (almost positivist) science.

The consequences for *Interpretation*

For the purposes of this special issue we were asked to use our own ethnographic practice to think through the implications of Reed's *Interpretation*. I do not believe my work is particularly unique, but instead broadly representative of ethnography in highlighting two lessons that might give us something to consider in relation to *Interpretation*.

1. The unit of analysis is typically the interaction and the situation, not the social actor. This points to a greater emphasis on relations rather than actions.
2. Cognitive orientations to the world are not always very good predictors of actions in the world, and are thereby best juxtaposed against and evaluated relative to observed action.

I will deal with each of these points in turn. The first is that the emphasis on social action almost always preserves the sanctity of the individual as the core unit of analysis. The impulse is to suggest that we must understand how individuals make meaning (and are made by it). But as ethnomethodologists (Garfinkel, 1967) are quick to note, people often don't 'know' how to act until they are in an interaction. Meaning happens within the context of other social actors, contingent upon the situation, which the actors themselves help define through collective negotiation.

To see what is at stake more clearly, let us turn momentarily to a classic argument of Emile Durkheim's. *The Elementary Forms of Religious Life* argues against religion as simply something actors believe – either consciously or unconsciously. Religion is also the practice of a community. In his now famous definition of religion, Durkheim (1995 [1915]) argues, 'A religion is a unified system of beliefs and practices relative to sacred things [...] which unite into one single moral community called a Church' (p. 44, emphasis added). Again, I hope to highlight a common shared understanding among sociologists, and after reading *Interpretation* one immediately recognizes that Reed knows his theory better than most (and indeed better than I do). He would thereby no doubt acknowledge that interaction is an important aspect of culture. But throughout most of the discussions in *Interpretation*, social action is operative and explanatory on the level of motivations and mechanisms in relation to individual actors. Yet the interactionist perspective forces us to focus more clearly on the collective act. As Durkheim (1974: 91) argues about collective action:

It is, in fact, at such moments of collective ferment that are born the great ideals upon which civilizations rest. These periods of creation or renewal occur when men for various reasons are led into a closer relationship with each other, when gatherings and assemblies are more frequent, relationships closer and the exchange of ideas more active.

Durkheim identifies the Reformation, French Revolution, Renaissance, and the 12th and 13th centuries that generated scholasticism as such periods of collective ferment; these resulted in cultural resources that changed the world. Meaning is not made, then, by individuals, nor is it contained within them. Instead it is constituted *between us*. And so to locate meaning within the motivations of actors is to enter into the kind of individualist attitudinal fallacy (Jerolmack and Khan, 2014) that Durkheim and other social scientists guide us away from. Social action becomes meaningful within interaction. If we are, then, to take such actions as a major basis of our theory, we must move away from motivations, and instead focus on how such dynamics are *enacted* with others under particular situational conditions. 'Mechanisms' and 'motivations' are insufficient for us to do so.

To make this point clearer we might first recognize that expressed ideologies are poor predictors of social action (Deutscher et al., 1993; Pager and Quillian, 2005; Jerolmack and Khan, 2014). This is a lesson well known to the ethnographer, who

juxtaposes subjects' expressed accounts with their situated activities. But for Reed it poses a problem: if expressed motivations are not particularly motivating of social action, what are they good for? The reply, it seems, can be gleaned from Reed's discussion of the Skocpol/Sewell debate around the role of ideology within revolutions. The way through this historical discussion is in locating the ideological not within the minds of individual actors, but instead, within the more Durkheimian sense of the collective – in my language, the interaction within the situation. This means thinking of social action slightly differently. We do not have structural mechanisms and agentic motivations. Instead, we have a more relational interplay. I feel it bears repeating here. This argument is not to suggest that Reed is wrong in any significant way. Only that his Weberian emphasis builds a kind of individualism within social action that creates tensions, particularly if *motivation* is to be a core element of one's theoretical framework. Meaning-making happens not through individual actors enacting their motivations within structural conditions, but instead through collective (interactional) coordination between actors.

This position that I am advocating can be leveraged to support Reed's interpretivist view rather than critique it. But here is a subtle but important difference: the focus on the motivational/agentic obscures that processes are collectively negotiated within interactions between units rather than constituted within them (motivations) or imposed upon them (mechanisms).

This point might be repeated and made clearer in the relation to the kind of standard ethnographic practice I've outlined in my own work. What ethnography often emphasizes is the importance of activity over speech (in this sense it is very different to interview research and much closer to experimental work). Or better, ethnography uses as its analytic leverage the distinctions between speech and action. To be clear, speech can be a form of activity. This is not the kind of speech I am thinking of. Instead, drawing on work I have done with Colin Jerolmack, I hope to point here to a distinction between actors' descriptions of their motivations or justifications of their activities and actual observations of what they really did. Part of the reason actors are bad at explaining or anticipating their activities is because such activities are often done in relationship to others in situations which they do not fully control.

The location of meaning, then, from an ethnographic point of view, is not in the motivations of actors, but in the situations of actors and the relations between that actor and other actors. Such attention to place and situation moves our theoretical landscape from Europe to America – forcing us to be more attentive both to the Chicago School and ecology, but also to more pragmatist theorizing and phenomenological and interactionalist social theory. The obvious challenge to such a view is that it is highly situationalist, presupposes the importance of direct observation of activity, and is relatively silent on how structure and history might be brought to bear within social exploration and explanation. This is to say, I have proposed a model that best fits the work ethnographers do, and thereby makes it difficult to imagine the conditions under which other social science work might be valuable.

It should not be terribly surprising that the ethnographer proposes a model of sociology where the ethnographer is king. This may well be good for me, but it could be bad for sociology. Reed's more catholic account has greater room for others. My immediate defense might be that I have done as I was told: responded as an ethnographer. But a greater defense might suggest something more. And that is that if we are interested in how man is meaning-making and meaning made, we might do well to remember that meaning is a social rather than an individual act. And so as we construct theoretical models to make sense of meaning, we might take as the unit of our analysis not the individual actor, but instead the social act.

Here, then, I return to Parsons. For the great contribution of *Structure* is not in its canonical ambitions. Instead, it is in using the analytic tool of the 'unit act' to challenge voluntaristic models as a satisfactory model of explanation for action. Individual motivations as explanatory of action are a type of such voluntarism – it is our motivations within us that drive us to perform in the world in particular ways. Structures may, like a switchman, guide toward a particular path. But motivations are our engine along such routes. Yet if we understand meaning as situation and interactional, as I tend to, then it is not motivation that matters. Instead, it's the rich context of relations – the between-ness, the Durkheimian collective act, the interaction – that helps constitute meaning. Perhaps we might not just mix a little Dilthey and Saussure with our Weber, but also add a pinch of Schutz, Durkheim, Mead, and Blumer, and we can be off and running again.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Note

1. You can also relate this to the point about saying/doing. Liebow, for example, structures his book around claims by those he observes and then works to negate those claims with observations about their lives/interactions.

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