



Mediations of Social Life in the 21st Century

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ACTION AND REACTION: RESPONSE TO BRADFORD

John Levi Martin

ABSTRACT

Purpose – This paper attempts to rebut criticisms of, and give further clarifications to, arguments about the nature of sociological explanation previously made by Martin (2011).

Design/methodology/approach – Here, arguments initially derived through historical reconstruction of theory are instead drawn out from our common stock of experiences. Aspects of the argument that were complex as initially presented are simplified here, and the maximum contrast between this approach and the more conventional is made.

Practical implications – The implications for practice are many; most important, the claim of Martin (2011) – rejected by Bradford (2013), as critiqued herein – to offer a coherent alternative to our current understanding of the task of explanation, if successfully demonstrated, suggests a reorientation of sociological research toward the production of intersubjectively valid cartographies and away from causal or pseudo-causal accounts.

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Findings – *Social theorists who are willing to seriously think about what lies in between our practice and knowledge as sociologists and as actors – to do the research.*

Originality/value – *The value of the paper, therefore, derives from its capacity to dispel common misunderstandings of Martin (2011), and to allow social researchers as well as social theorists, to make use of a coherent vocabulary for the development of social research, which otherwise would remain inaccessible to them.*

Keywords: Esthetics; social theory; explanation; field theory

INTRODUCTION

I am grateful for the close attention given to *The Explanation of Social Action* (henceforward, ESA) by John Hamilton Bradford in his 2013 critical review (*Current Perspectives in Social Theory*, 31, pp. 309–332). We all should be blessed with such a serious and close critique, and a careful critic is always to be prized above a sloppy supporter. Not surprisingly, I will argue against almost all of Bradford's conclusions. In most cases, he has fallen prey to a common misunderstanding that I set up; in some cases, it is a more particular misunderstanding; in some cases, he is making unfounded assumptions. In two cases he is right and I was wrong.

I refer to his main points as enumerated in his abstract in square brackets; I organize my response in a way that allows me to reproduce some of the key arguments of ESA as opposed to always following his order.

WHAT ARE WE EXPLAINING

What is ESA About?

First, Bradford (2013, p. 310) begins with a misinterpretation that others have shared, thinking that I attempt to answer the question of why people do what they do: that is, that the book *The Explanation of Social Action* is the explanation of social action. That isn't the way the title sounded in my head: I meant that the book was *about* the explanation of social action, not

that it *was* it. Thus, it isn't the answer to the question, it is hoping to be the answer to the question of how to answer the question, and the answer is, change the question. (To be sure, Bradford himself later [p. 313] says something just like this.)

I think perhaps because of this, the scope conditions of the book, which I thought were clear, were missed not only by Bradford but by others. Given that it is called *The Explanation of Social Action*, I had imagined that it would be clear that I was only talking about the explanation of social action. It is true that I think that most American sociologists underestimate the degree to which sociology *should* involve the explanation of action, but it isn't clear that it *all* should be an explanation of action, and it *is* clear that current sociology includes types of explanation that *aren't* explanations of action. Thus, I totally agree with Bradford [p. 318] that there are other things that we can explain, and thought that ESA had been clear on this, but if not, I concur. Thus contra his first point [pt. 1] I am not arguing that we *must* only focus on the explanation of social action; it is merely that this is my focus.

The central argument of ESA – and I will return to this below – starts from the fact that we have two registers, or sets of vocabulary, for trying to explain such action. One, “first person” accounts, come from the ways in which actors orient themselves to the world, and refer to things that they believe that they have experienced. The second, “third-person” accounts, explain some action by employing causal forces outside of the phenomenological experience of the actor in question. I argued, and I think quite well, that third-person accounts *for the explanation of action* are inherently problematic and paradoxical. That does not mean that such third-person terms might not well be used to explain something else, such as an *outcome*. However, given that in sociology, most outcomes “come about” via social action, it seems that a fuller, more scientifically valid, explanation will sooner or later involve us grappling with action, and here the third-person vocabulary fails us. Rather than accept first-person *accounts*, what we must do (or so I argue in ESA) is to compile experience to recreate the way in which actors respond to the environment, and I argue that we can here build on previous work by the Gestalt/field theorists and the pragmatists on this environmental relation. This is because (and I will return to this below), following the Gestalt theorists, I believe that one of the things that we *experience* (as opposed to *infer* or *reason*) is what we should do with things in the environment. In the language of ecological psychology, we directly perceive what things about us “afford” us as actors. But this all depends on us trying to explain social *action*.

Keeping this scope in mind suggests that some of Bradford's points, while accurate, are hard to construe as *critiques* of ESA. Bradford [p. 317] correctly points out that I have little to say about the unintended consequences of action. But (to use a familiar distinction and vocabulary, at some risk of misinterpretation) there isn't any reason to think that to understand the *causes* of rainfall, we must understand its *effects*, is there? I believe that the recognition of the importance of the unintended consequences of action, well developed in the Weber-Merton-Giddens tradition, has been one of the most important developments in sociological theory, and I have nothing to add here. I even think that it may well be that pursuing this rigorously supports a notion of autopoietic systems arising (as argued by Luhmann). Because of the feedback loops, such coagulated dynamics are of course *relevant* for the explanation of any particular set of actions ... but they don't change the issue of how we should analytically approach the situation that confronts the actor.

ESA, then, does not aim to give a general social theory but rather to determine what sort of minimal vocabulary for the explanation of social action is internally consistent and avoids assuming what is to be proven. This point about a minimal vocabulary turns out to be relevant to a number of other points made by Bradford, as well as by others.

Definitions and Action

Bradford [pt. 2] thinks it is a problem that I never define "action" or "social action;" many readers may agree with him that this is unacceptable in an identifiably "theoretical" work; I do not, and think this issue is an important one. Our sociological nominalism – coming from the assumption (critiqued in ESA) of a fundamentally disorganized world awaiting top-down imposition of mental categories – leads us to imagine that without definitions, we are lost. I do acknowledge that there are cases when definitions are required (especially when employing new terms), but not all cases are of this sort. In other cases, defining may give the appearance of rigor, but is actually only a form of distortion. Consider a zoologist studying mating behavior among some avian species. It might be that in the species in question, there is no confusion as to what mating behavior is – where it stops and starts. In such a case, it is a silly waste of words to define mating behavior as opposed to just getting down to studying it.

Of course, sometimes there *is* confusion – we aren't sure whether something is a case of mating behavior. In those cases, *defining* it is the worst

thing we can do. Because then we give the *appearance* of knowing what we are talking about, when in fact we don't. We may still need to begin by explaining precisely *what* we looked at – which of the large class of potential observables was the initial focus of attention – but in such a case, I insist (and I call the mighty Hegel and Marx to my side) the definition of our phenomenon must be the *conclusion* of our investigations (if they are successful), and not the preface.

Bradford also takes me to task for not defining “explanation;” again, this is a deliberate choice (defended in the book). I think we are better off starting out with “whatever most folks commonly understand by explanation,” seeing what this entails, and then trying to do it in a more scientific way. Otherwise, we can define – and, I believe, we have defined – “explanation” in such a way as to make even our more incredible and preposterous endeavors seem not only scientifically valid but also necessary. That is, we defend our (poor) choices of *criteria* for good as opposed to bad explanations by conveniently redefining explanation in an idiosyncratic way so that our choices seem unquestionable. In particular, we try to make a distinction between “description” and “explanation,” which turns out to lack the epistemic justification we believe it to have.

Description Versus Explanation

An answer to a “what” question, like “What is that man doing?” (Answer: he is sawing a log) is a *descriptive* act. An answer to a “why” question, like “Why is that man sawing?” (Answer: he wants to make a house) is an *explanatory* act. On this, we all agree. I firmly side with those who argue that this distinction is not an inherent one in the nature of the processes we study, but rather, indexes the particular combination of knowledge and ignorance that we have. Because sometimes we would find the question, “What is that man doing?” to provoke the answer “He is making a house.” This now *seems* descriptive as opposed to explanatory, yet the same answer before appeared as an explanation! How can this be? It is because we ask *Why* about the actions of others either when we do not see the purpose of the action or when we think that the actor can be taken to task for having acted thusly. There is nothing inherently wrong with this, though soon I will derive some problematic aspects, but once we recognize that this difference has to do with the state of our knowledge, it becomes implausible that we should urge all people to “explain” and not describe, as one person's explanation can be another's description.

When it comes to this distinction between description and explanation, Bradford seems not to accept my rejection of this distinction, yet it may be that there is simply a confusion of emphasis. If I understand him correctly, Bradford takes my argument that the difference between the two is not a scientific one but a social one as undermining my claim that this is a false dualism. He gives [p. 320] as an analogy – I think a good one – that “it would be equally absurd to argue that the difference between left and right constituted a ‘false dualism’ and, therefore, invalidated egocentric coordinates.” I see the takeaway from this excellent analogy quite differently, because I find myself in the position of one arguing with those who are claiming that the world is divided up into left and right, that this is not an egocentric division, and that we as scientists, can reach our goal by always “going right!” I say that this is a false dualism in that (1) what is on the right is not different from what is on the left; (2) what is right and what is left depends where you stand; (3) there are other, less egocentric, ways of orienting; (4) we’re not going to do much until we understand this. Same thing here: others are saying description and explanation are intrinsically different, and we need to explain, and not do mere description. I claim that this depends on where you are and what is of interest to you, there are better ways of orienting, and we’re not going to make any progress as a science until we get past this *false dualism*, and this requires resolving it into the *true* one – classes of social situations.

In sum, explanation – like its close kin, causation – turns out to be an anthropocentric concept. That’s as it should be. But pursuing this further requires, then, that we take into account the social relation of a researcher and researched, and not fetishize the explanation as if it were something independent of that relation. Of course, others have said this before; where ESA differs from most such arguments (but sides with the Gestalt, ecological, and field theoretic traditions) is to derive from this not an irreducible subjectivity of explanation, but rather the need for the production of inter-subjectively valid social cartographies – not piecemeal “explanations.”

What is Explanation?

It is because of our tendency to define “explanation” in an indefensible way, one that begs the question of what we should be doing, that I think we’re better off not defining explanation but rather starting with what we’ve learned about this distinction between description and explanation, namely, that explanation is something of interest to those with a certain kind of

ignorance. ESA then tries to sort out which sorts of these “somethings” are of greater or lesser scientific status ... even without defining explanation.

Bradford assumes this to be impossible. But there is no inherent difficulty to making a clear, and well-defined, partition of an unclearly defined class. Mental illness, for example, is a very murky class, and some are not sure if it really should be treated as a class at all. Yet that doesn't mean that we might not be able to divide mental illnesses up into two classes, those associated with lesions in the brain and those not so associated.

So too for the case at hand, where the class is “explanation,” ESA proposes (among other things) the partition “those that make reference to terms lacking first person correlates” as opposed to “those that do not,” and argues that the latter should be preferred to the former. In other words, I merely say that if you pick up different explanations of action, and read them to the actors and talk about them, in some cases, the actors will (whether they agree or disagree) understand each term and accept that it points to something existent, even if these elements are (to an analyst) somewhat abstract (“voters,” “Republicans,” “taxes,” “faith”). In other cases, unless cowed by authority, the actors will not accept that the terms have real referents (perhaps, “false consciousness,” “repressed fear,” “anomie”). (In still other cases, actors may accept or propose explanations that make reference to terms out of their phenomenal experience yet accepted by habit or tradition.) Thus, making a partition between the first two types of cases, the partition that is central for ESA, is not difficult, does not require defining the class of explanations, and does not imply that first-person *accounts* should be treated as veridical. It does, however, suggest that we should confront explanations employing third-person terms with a great deal of suspicion – the burden of proof is on those who would explain action with terms actors do not recognize. I do not believe that the weight of the evidence leads us to accept *any* such terms for explanations of action. But my arguments here did not convince Bradford, and indeed, seemed to him wildly implausible. I want to go on to reconsider these.

RELATIVISM AND AUTHORITARIANISM

An Implausible Thesis?

A major critique that Bradford makes is that I cast the history of sociology as one in which relativism leads to authoritarianism. Put this way, this does

sound contradictory at best, ludicrous at worst. My argument, however, was not about relativism in general, and certainly not ethical relativism; rather, it has to do with the specific theory of the arbitrariness of perception associated most notably with Mary Douglas. I am not saying that Freudianism “came from” relativism [p. 314], but rather that Freudianism was used to shore up a critique of a doctrine of the fundamentally arbitrary categories used to perceive the world, a doctrine that was often associated with relativism. But this doctrine, often proposed by those who were advocates of the reasonableness of the particular group they studied (and intended as an attempt to do justice to “their culture”), had destabilizing implications, by implying that people had no more authority to say what their own experiences were than did others. It may well be true that those we study do not deserve any special position of authority, but my argument is about this specific linkage, and not about relativism in general. In *ESA*, I tried to use an investigation of intellectual history to make this point. Let me re-derive the bare bones of this argument more abstractly, which I think may be more convincing.

The Problem of Instability

First, let me give the problem of instability in its most delicious form. Freud and Jung are off to travel together. Jung speaks enthusiastically about some prehistoric remains found in Bremen. Freud, understanding that Jung sees himself as possibly Freud’s student and successor, interprets this as Jung’s Oedipal wish to have Freud (the “prehistoric remains”) disposed of. Freud faints dead away. Jung interprets this as Freud’s own inverted Oedipal obsession (Gay, 1988: 208f). In other words, each thinks that the other is crazy, and each explains *the other’s* theory as part of his aggressive craziness. If we provisionally assume either is right, then the other is completely wrong and has no place from which to make any legitimate critique. But either one seems as good a place to start as the other.

This, in a nutshell, is the problem of instability – we have a doctrine that can produce completely opposite statements of truth given the same empirical inputs; it is only stabilized when there is some *external* anchor of social authority to determine who is the crazy (or stupid, or ignorant) one.

Now one could argue that it’s always this way. You could go to the optometrist and read the chart, “E, F, P, T, O, Z, L, F, . . .” “That’s wrong,” the eye doctor says, “that last letter is supposed to be a P. You need

glasses.” Well, maybe *she* needs the glasses. Sure, maybe. But it makes a difference if you and the doctor can both go, together, closer to the eye chart and come to an agreement. “You were right, doc, it *is* a P.” We don’t always do this, but we know we can, and we probably would if the glasses we got made our head hurt, and we noticed the doctor bumping into things.

Now the pragmatist understanding of science and truth, which is associated with Peirce, James, and Dewey, basically held that what we mean by “truth” is what we’d eventually agree on, if we all did in fact walk up to the eye chart and take a good look at it. If you don’t accept that definition, and in fact you find yourself in a room with three eye doctors who are having a fierce disagreement as to whether after the P it goes “E, D” or “E, O,” you might wonder whether you should really believe them when they all turn on you and insist *you’re* wrong, at any rate, about the F. My argument is that this is the position we’re in, and though it may sound funny, it does come, though indirectly, from the type of relativism that sociology has incorporated into its fundamental world view.

The Relation of Relativism to Decision

Relativism by itself can’t be bad, because it’s *symmetric*, in that it refuses to give any one view a head start in the race for truth. That sounds to me like the right place to start a science. The problem comes because we haven’t just started with relativism, we’ve started with two specific planks about the relation of cognition to action.

The first (which we’ll denote as *PA*) might be thought of in terms of what Bourdieu (1990) has called the “scholastic fallacy,” namely, the academic’s tendency to give a primacy to theoretical accounts. But it’s even more fundamental than this (and Bourdieu himself understood this quite well): it is that we tend to model actors’ subjectivities in terms of propositional beliefs: things like “those elitist liberals are taxing hard working Americans to support the lazy poor.” We retroject, as Mills (1940) said, actors’ accounts into fundamental cognitive elements that we then assume to be core structural elements of mind.

We’ll put on hold the precise nature of such verbal (whether spoken or silent) productions for a bit, but we can note that such an emphasis doesn’t necessarily sit well with what seems to be the widespread agreement among social theorists that we should give a primacy to the practical, that is, we understand actors as actors first, thinkers second.

The second plank (which we'll denote as *PB*) is that our cognizer/actor takes in a set of *particular* sense impressions that can only be integrated and identified as an instance of some species due to our presence of a cultural template. With admirable symmetry, this notion of actors' cognition parallels our epistemology — our theory of our own knowledge production — namely, the theory of concept formation we're taught in school. This is a voluntarist form of neo-Kantianism: each of us must form "concepts" on the basis of our "theory" so as to be able to best reach our own, possibly idiosyncratic, analytic goals. If I happen to be interested in the relation between "global migration" and "crimes against persons," I establish these two boxes, define them clearly, and put in them whatever particularities satisfy my definitions.

As a result, there isn't any natural basis on which the superiority of one template over another can be demonstrated. Who should be included in the species "elitist liberals?" They aren't a natural kind; rather, this is category constructed by *some* actors and is different from those of other actors, especially the accused elitist liberals themselves. But who's to say they're wrong? We're relativists, after all?

The Intolerance of Tolerance

Yes, but only up to a point. I think sometimes the attempt to catch relativism as being inherently contradictory ("so if relativism is relative then you refute yourself!") has been considered far more important and intelligent a statement than it is. However, it is also the case that we do need to do something, and say something, which means, sooner or later, someone will predicate *something*. And as Latour (2004) in particular has argued, we generally *have* to make some statements of the starkest and most unquestionable form if we are to carry out the sort of destructive critique that was integral to this relativist approach.

Now I simply don't think that relativism is inherently flawed in this way; here I am only talking about that specific form that is about the arbitrariness of general terms (*PB*) when joined to a theory of action that emphasizes propositional cognition (*PA*). My argument — the one that sounded so ludicrous when recounted in compressed form by Bradford — is simply that in order to be able to say *something*, given these presuppositions, social scientists needed to assume a position of experiential authority that they had no right claiming. And here I don't mean no *political* right, I mean no *epistemic* right. A sociologist might say something like this: "Conservatives

say ‘those elitist liberals are taxing hard working Americans to support the lazy poor,’ but this is not accurate. Rather, they construct their ideas of ‘elitist liberals,’ ‘hard working Americans,’ and ‘lazy poor’ on the basis of an uncritical integration of low quality information and pernicious chatter, and use this to support their own political project. So *really*, the conservatives are attempting to repress the stirrings of an alliance between the downtrodden and exploited (on the one hand) and certain progressive factors of the educated elite (on the other) so as to preserve their own dominance.”

What’s wrong with this statement? It might well be true. But we can’t avoid noticing the parallel to the subjects’ *own* way of thinking; thus we can imagine a different expert who might argue, “Liberal social scientists say, ‘Conservatives say “those elitist liberals ...,” ... so as to preserve their own dominance,’ but this is not accurate. These sociologists do so to preserve their own dominance.” And so on. This formal parallel is not in itself problematic, so long as there is some empirical way of deciding who is right. But, since each side is using concepts that each is free (or so we say) to construct in his or her own way, for his or her own “analytic purposes,” there is no reason to think that there *can* be such an empirical adjudication. That is, it isn’t simply that our two social scientists disagree. It is how they understand the project of marshaling evidence. Our first social scientist could – and, as I discuss in [Martin \(2001\)](#), actual social scientists did – support this argument through concept formation and data gathering. One could argue that there is a personality trait, say “right wing authoritarianism” (RWA), that leads people to react punitively to the weak based on their own neurotic fear of weakness. Our scientist could construct measures of this based on his theory, and find that conservatives were high in this trait. He could then find that conservatives were higher in RWA, and that this explained their unwillingness to support programs for the poor.

But our second social scientist could support *his* argument with a different set of concepts. “There is no such thing as RWA,” our scientist might argue. “The reason liberal social scientists believe that there is, is that they tend to be high on a different, and a real, trait, ‘left wing wimpiness’ (LWW), which leads elites to fearfully identify with any critic of elites based on their own neurotic guilt.” He could construct measures of this based on his theory, and if he could somehow get social scientists to fill out *his* surveys, he could find that indeed, his liberal social scientist subjects were high on this trait, and this explained *their* action.

You might respond, but this is obviously *bad* social science: our first social scientist measuring the existence of RWA on the basis of responses

to questions like “do you disapprove of nude beaches” and our second measuring the existence of LWW on the basis of responses to questions like “do you find reggae music interesting.” But my point is that what each is doing – proposing (potentially idiosyncratic) concepts, using his own theory to derive testable implications, using these to construct measures and then seeing whether the resulting data are compatible with his theory – is just what we say that social scientists *should* do. In essence, we take a flawed theory of science, one that assumes the incommensurability of different theories, and use it as our guidebook.

It is for this reason that I made the claim – and I stick by it – that our particular form of relativism, namely, the belief in the voluntarist construction of concepts and the arbitrary nature of species in social perception, coupled with our focus on propositional knowledge, undermines our attempt to find a rigorous social science. It is not objectionable in itself simply because it delegitimizes the perspectives of actors – you can’t make an omelet without breaking a few eggs. It’s that there after all that, we don’t even have an omelet.

Now if there were no other way to start, we’d just conclude that there can’t be a social science, which would save us all a lot of trouble. But it seems to me that this conclusion isn’t warranted. The problem is that we’ve made some assumptions that we shouldn’t have, and if we rip them up, we find a stable footing. Just as there was something that our optometrists could “go up to” to determine who was correct, so there is something that we – sociologists and actors – can go up to.

The Option for the First Personal

I noted that where I believe that Bradford misinterpreted the argument of ESA, he was not alone. The most common problem that I set myself up for was the belief that I was trying to rehabilitate first-person *accounts* – answers to the question, “Why did you do this?” (Bradford [p. 310] begins by arguing that I attempted to do exactly this.) It is indeed the case that I argued that *third-person* accounts to the question “Why did *A* do this?” are problematic, and to do so, I negatively compared them to first-person accounts. Even though I did make various asides that I did not mean for us to accept first-person accounts, because I pushed this off until later, a number of readers misunderstood me as *defending* these first-person accounts. Bradford does in fact recognize and discuss my attempt to distinguish between such accounts (talk *about* experiences), which I believe to be

problematic as data, and the underlying experience itself, and that it is the retrieval of the latter that I think must be our goal (though it is invariably accessed indirectly). But still, Bradford [p. 317] thinks that the fact that first-person accounts may be implausible counts against my claims.

In ESA, I do indeed criticize mainstream sociology for, in effect, first asking actors “Why did you do y ?” and then dismissing the answers. But not because I think that we should *accept* the answers! Rather, we have asked a question that is (1) rude and (2) of dubious scientific value. The rudeness might seem scientifically irrelevant, but I believe that it is not; by first provoking a response worthy of dismissal, and then dismissing it, we convince ourselves that lay reports are worthy of dismissal. As I will go on to argue, rather than being dismissed, these need to be compiled and organized by the analyst.

A reasonable critique, however, is that very few of us actually do this: those who ask actors “why” tend to accept the answers, and those who reject actors’ explanations do not even bother asking “why.” Now to the extent that there are “why” questions that do not involve explaining action, I completely accept (and accepted in ESA) that such cases are outside of the scope of the argument. For example, we might ask why the rate of savings in the US population dropped in the early 20th century, and conclude that the correct explanation is simply a compositional one, having to do with the decreased number of agriculturalists and the rise of an urban white collar population. No one’s behavior had changed, just the number of each type in the population, and so we are not trying to explain the action (we treat this as nonproblematic). That’s a successful explanation that doesn’t require fishing for first-person experience.

Yet more often than we would think, when we ask a “why” question, we are implicitly asking a question that calls forth a first-person account, and indeed, is only stable thusly. For example, if we ask “what caused people in place p at time t to rebel” and we understand this in first-person terms, it would seem that we should be interested in what they have to say about it. I think we would be *wrong* to do that. Because one of the robust findings in social psychology is that when we ask people “why” they did something that they have already accomplished, we tend to get them to generate an “account,” which usually explains why what they did was a *right* or *reasonable* thing for someone (especially the *questioner*) to have done. When speaking to a religious person, we might emphasize religious values; when speaking to a secular person, secular values. All these things may be true “in a way,” but trying to sort out “an” answer from such accounts is a fruitless task.

Even when respondents are doing their best to help us, they tend to be much better at explaining why they would undertake such an action *now* than they are at reproducing the constellation of factors that confronted them *then*. So it makes a great deal of sense to reject these accounts as an answer to our question.

CAUSAL EXPLANATIONS AND INFERENCES

Third-Person Causality

But should we therefore accept a third-person account? And what sort of an account would this be? In sociology, we take for granted that this would be a “causal” one, and our interpretation of this causality tends to revolve around *necessitation* and *counterfactual dependence* (though generally watered down to some “probabilistic” version). For example, had people *A* not experienced relative deprivation (*x*), they would not have rebelled (*y*). Further, to the extent that our ideas about causality make sense, we are required to imagine that our cause *x* could be randomly allocated to persons. This means two things. First, because counterfactualism introduces imaginary worlds, there are an infinite number of causal explanations that we treat as equally valid, even though many are silly. (For example, had people *A* been wearing mind control caps [*z*], they would not have rebelled.) Second, people cannot cause their own actions, though they can cause *others'* actions. That's because any time we are doing something *ourselves*, we have a form of selectivity that wrecks our understanding of causality. Note the importance of ESA's focus on action – it is here (and not everywhere) that we are on strong grounds for rejecting a third-person causal explanation.

Now Bradford rejects my arguments about causality, and indeed, one of his arguments seems to be that my own notion is defined in terms of counterfactuals [pt. 5]. Contrary to the implication, no counterfactualist would assimilate the approach I review sympathetically, which identifies candidates for causality on the basis of their deviation of actuality from what members of community treat as normal, to current counterfactualism, which turns on a general issue of our capacity to construct imaginary worlds. Bradford's critique is at its weakest here and it sheds little light on the most important issues; so I will pass over this somewhat quickly, but I do want to respond to two parts of his review here.

On Necessity

Bradford [p. 324; p. 311n18] has a few minor glitches in his discussion of causality; we would say that x is necessary for y when there is no y without x , which means that x 's absence implies y 's absence ($\sim x \rightarrow \sim y$), which we also say as “only if x , then y .” Counterfactual dependence is understood to mean a relation of necessity, because the question of whether, in this factual world, where x and y both existed, x actually caused y , is answered by asking whether ($\sim x \rightarrow \sim y$). Sufficiency is when ($x \rightarrow y$), which we say as “if x , then y .” “If and only if x , then y ” means both of these ($x \rightarrow y$; $\sim x \rightarrow \sim y$). Some of the things Bradford brings out here are interesting and important (such as a point he attributes to Alan Garfinkel, one earlier made by Davidson (1967: 698), regarding the fragility of events – something I discussed, but in abbreviated manner). Further, his overall takeaway is correct – that I criticize unreasonable practices, not thinking in terms of causality per se, nor in terms of counterfactualism. However, Bradford [pt. 6] says “no one endorses these ideas anyway.”

Were that true, I would be right, but silly. However, not only do people endorse them – that is, that causality *is* an inherent and mind-independent part of the world, that it is equivalent to simple counterfactualism, and that there is no explanation that does not turn on causality – but many believe them to be so obviously true that anything that violates these assumptions must be banished from sociology. Courses are taught, articles are rejected, and books criticized, on the basis of these ideas which Bradford, I think rightly, calls unreasonable. It is true that some of the most important theorists of counterfactual statistics, such as Morgan and Winship (2007: 4f), emphasize the *difference* between the counterfactual approach to causality in philosophy (on the one hand) and their use of a framework for alternative treatment values (on the other). However, statisticians are able to begin by setting up conditions for the rigorous deduction of their techniques that, as I point out in ESA, are rarely satisfied by the consumers of their products, and the issue for us is how practicing sociologists think.

Since Lieberman's (1985) wonderful work on making sociology more rigorous, there has been a widespread conception that scientific analysis in sociology is causal, that causality implies simple counterfactualism, and that this implies an experimental or quasi-experimental research design. An explanation of action that turns on some factor x is pushed to conceptualize x as a cause; once this is done, the explanation is pushed further and further toward an experimental set up involving the allocation of x . If we

fail, in that we cannot push our explanation very far in this direction, it is criticized as inconclusive and we hang our head in shame; if we succeed, we find, to our puzzlement, that all of the action, the phenomenon in which we were interested, has disappeared.

This does not mean that *all* employments of causality are flawed, nor even that we are *unable* to use the concept of causality in a helpful way, but it *does* mean that, so long as we hold to conventional understandings of what causality is and how we use it in our work, we have no way of defending the good and rejecting the bad. Indeed, when it comes to the explanation of action, the more seriously we take these definitions, as opposed to using rough-and-ready common sense understandings of causality, the more confused we are going to become. Like Bradford, I side with MacIver (1964 [1942]) (in his wonderful – though flawed – treatment of social causality), at least to the extent that causality, as used in sociology, is about differences: it is a mental operation, not an aspect of the things.

And, as I have said, this type of mental operation becomes cumbersome and contradictory when applied to action. But we have also seen a problem with the first-person accounts. What do we do? ESA's answer is that rather than look for retrospective accounts from actors, we want to get a sense of their prospective orientations and their present experience. Even though we generally cannot quite get their actual present experience, we can come a lot closer than we do. Unlike *accounts*, such experience does not need to be subjected to the purifying flame of destructive critique before it is of use to us as social scientists.

FROM CAUSATION TO EXPLANATION

The Veridical Nature of Experience

ESA's emphasis on treating experience uncritically appeared quite batty to Bradford. A way of clarifying this might be to use the distinction between *sensation* (which in the 18th century, philosophers argued we could never be "wrong" about) and *perception* (which is the identification of an object that is the cause of the sensation, and thus involves judgment – and which therefore can be mistaken). Bradford – who thinks I have wasted people's time talking about judgment in close detail – doesn't see any distinction between perception and sensation [p. 316], thereby illustrating the problem at hand. When he thinks that I am stuck on a dilemma about "how to

square the validity of first-person experiences with the apparent fact that people are wrong about why they do what they do" [p. 323], he is finding himself in the same position as one who would argue that a philosopher (like Reid, 1969 [1785]) who maintained that *sensation* was always veridical ("you really do have a cold sensation on your fingertips ...") was inconsistent to deny the perception ("... but you are wrong to think that this sock is wet"). Following Dewey, I make a similar opposition, between experience (on the one hand), and propositions *about* experience (on the other).

It is of the utmost importance to be clear here as to the nature of this distinction between experience and accounts (propositions about experience), and what ESA claims as to the status of each. The reason it is crucial for ESA that experience be veridical is that, following the Gestalt school, I argue that we perceive what we can and should do with objects (their "affordances"), and this is where sociology must start (though not end) in our explanation of action. If actors' experiences are misleading and illusory, this class of explanation is obviously invalid. But this does not require that actors be able to explicate the nature of their experience, store it away accurately, and/or regurgitate it upon request. In fact, the direct nature of our perception of affordances means that in many cases, it is *because* our experience is veridical (potato chips really *are* delicious) that we are unable to give a good account of our actions (for our hand may be reaching for the chips and bringing them to our mouth without us really thinking it through).

The difficulty in adopting this framework for social science is that we have to accept that there is *disagreement* about what things "afford" – that some will see the Democratic party as sickly and pathetic, while others see it as courageous and patriotic. Much of ESA tries to work through how we grasp these sorts of non-random differences in veridical experience, but fortunately, this was not the focus of Bradford's critique, as a defense would be quite lengthy. Here, we need to simply emphasize the separation between the nature of experience and accounts, and that ESA requires the first (but not the second) of these to have a form of ecological validity.

Further, I think that Bradford makes a serious error (flagged by the use of "of course," which can indicate an indefensible assumption) in saying that "people's experiences are, of course, not independent of their statements about experience." Of course they aren't independent (they can't be if the statement is *about* the experience), but the question is whether someone's experience x is shaped by what she will *later* say about it. It is hardly so obvious to me that this is the case that I would employ an "of course" here. If the argument is that our experience of some x_2 is not independent

of our statements about an earlier x_1 , that is fine, but completely in harmony with what (following Dewey) I said about the “funded” nature of experience (that it folds in our state of being, a point also made by Heidegger and Merleau-Ponty). If the argument is that x is shaped by our thoughts about what we would/could/should say about it, that is certainly an empirical possibility (pointed to by Mills, 1940), but it isn’t obviously *always* the case.

Bradford is, however, still right about a key part – I (like Dewey) struggle to explain how the *experience* has veridicality without the *statement* partaking of this veridicality, just as Reid struggled to say how a sensation was real and a perception not, given that a sensation is often, immediately, the sensation *of* something. I think I am on stronger grounds in that in this case, the talk-about-experience is temporally disconnected from the experience itself. Still, I confess that this problem (how to understand where the “error” creeps in in social perception) is not completely solved in ESA, but as far as I am aware, no one else has solved it either. Our normal way of making the partition is (as I argued in Chapters 4 and 5) completely untenable, as it relies on a demonstrably false theory of cognition in general and of vision in particular. Perhaps this will turn out to be a problem that theorists cannot solve on their own.

First Persons and Scientists

The only place where I think Bradford makes a serious misstep, and I confess it seems to be a side thought, is his attempt to argue that by saying that we shouldn’t allow social scientists to invoke third-person entities in their explanation, I am contradicting my own advice, because such third-person explanations are first-person to the social scientists, and their action *is* explaining action. This strikes me as sophistry – the production of a statement that does not survive serious reflection (that is, do you *really* experience third-person causality in social data? I spend a lot of time analyzing data, and I never have). However, this gets to a real point: if social scientists were *not* to slap a third-person account on their own *experiences*, these would indeed provide valuable data. I think the most important example here is Christie’s (1993, p. 88) admission that when he was applying sociometric tests to subjects to determine if they were “authoritarian” – an attribution that most of these subjects would not only reject as applying to them, but dispute as a having an actual referent as intended – he could *tell* who would score high from his initial interactions with them. Those he

disliked would turn out to be authoritarian. This *dislike* was a first-person experience, and has fundamental validity. His *account* of this dislike could, with suitable cleaning, compilation, and organization with other such accounts, be useful data. The abstraction placed on the person (*A* is an authoritarian) actually *removes* the fundamentally relational account, and takes what is in fact a joint production, and misconstrues it as an abstract individual quality, one requiring the utmost theoretical nicety to uncover.

This confusion between social scientists' *own* experience and their own *accounts* of this experience gets to a more fundamental one that I think we see in Bradford's argument [pt. 7] that my attempt to separate *experience* from *reports about experience* is inherently useless, because sociological findings are invariably reported in propositional form. Although I will argue against Bradford's conclusion here, I should emphasize that he is thinking in precisely the right direction – taking for granted that if ESA (or any work of theoretical methodology) is to be acceptable, it must have that minimal reflexivity of being able to exist in the same world as other explanations without requiring some sort of total war. My argument was not that propositions about experience could *never* be accurate, but that if we are looking to stabilize our analyses by building in an epistemic “option for the actor” – that is, an “innocent-until-proven-guilty” that means that not all ways of *seeing* things (note, not *talking about* them) are equal – we cannot do this by giving actors' *accounts* this credibility, but rather, their *experiences*. This – compiled, organized, and aggregated experience – is thing “we go up to” to see if we are correct.

At the same time, I have emphasized that our social scientific work must not only be understood as a refined and improved form of that social competence that actors have, but must be restricted to the employment of terms that have phenomenological validity to those whose actions we explain. It might indeed seem that this implies that social scientists are simply some actors among others, no different at all.

And yet I do not think that this is so. A true social science – one that involved the systematic compilation of experiences into an organized terrain in which both actors and social objects can be positioned – readies us (and any actors who would like to use our products) to begin crawling toward defensible propositions, and mutual critique can turn such a crawl into something more like a walk. Again, I would throw my lot in here with Latour – it is quite true that the “universal” is a local configuration, and the view-from-above is not the same as a view-from-nowhere. But the locality of the universal, the fact that it requires abstraction of some sorts and the loss of many specific characteristics of experience, in no way counts

against its objectivity. Thus, when we make an analysis, a social cartography, and then make statements based on it, we are not making statements about our *own* first-person experience, but about the principles of organization of others' experience. There is simply no reason to think that the problems that come with actors' accounts arise here, for the social situation and associated exigencies are completely different. That does not mean there are *no* problems with these situational demands, but they are different and must be analyzed in terms of their own specificity.

Finally, I think that Bradford – again, like others – assumes that because I object to many sorts of experiments that mete out treatments to actors so as to cumulate evidence for a third-person explanation of action, I must reject all such experimental studies [pt. 8]. I certainly do not – Gestalt psychology was an experimental science, and the evidence that lies at the heart of my understanding of the nature of cognitive processing all comes from experiments. In fact, the greatest works showing the problems with experiments (namely, the ways in which experimenters were somehow unconsciously guiding their research subjects to the “right” results) were all experimental (and done, it should be noted, by descendants of the Gestalt tradition).

There is nothing wrong with third-person knowledge, even of human beings. And there is nothing wrong with experiments. The question is whether we can found a science of human action on the principles that (1) said action should be explained in causal terms; (2) our understanding of causality should be that underlying the classic treatment/control experiment. None of the successes of the experimental method have suggested that this is the case and indeed, when we put them together, they strongly suggest that it is not.

From Experience to Fields

The argument of ESA that we have reproduced so far is that we need to take as our data the experiences of actors, because their experiences are of objects that “tell us what to do with them,” and hence explaining the nature of the *objects* (and their relation to the actors) is equivalent to explaining the *action*; ESA thus argues that the most promising direction for this task, termed therein a “social aesthetics,” will be a field theory.

In the next section I'll try to clarify my use of this terminology, but first, a brief clarification on whether I understand this project as requiring the abandonment of the project of inference and current statistics. I certainly

don't. I suspect that my critique of our normal way of doing statistics as *explanation* misled Bradford into thinking, first, that I opposed inferential statistics, and second, that I wanted us to only do case studies and aggregate them. What I would say is that, in *actuality*, we *always* only do case studies and aggregate them, and so we *misinterpret our own actions* when we read a study with an *N* of 3,000 as if it were about a single abstract person (a point I previously made in Martin, 2000), introjecting *inter-individual comparisons* as if they were *intra-individual dynamics*. Just as my critique of simple counterfactualism led some to mistakenly imagine that I was opposed to the counterfactual approach to causality as method, so this seems to imply that I am against inferential statistics, when my argument is that we should not be trying to infer to the *average man* of a "representative population," but to the *structured landscape of the distribution of the social*, and for this reason, I am extremely enthusiastic about the extension of new techniques (including "General Linear Reality" types such as hierarchical linear models and other mixed models) that make it easier for us to include heterogeneity of different types, and make the *right* inferences.

Thus Bradford is absolutely right that I think that we need to compile and organize our data. That's what we do anyway. I think we should use field theory to do this in a way that produces descriptions that are intersubjectively valid and relatively theory-free (in the sense that they can be used to answer *many* different questions, in the way that a good map can be used to go many different directions, even though maps are made for specific things). Here I want to respond to Bradford's critique of my use of field theory and again clarify.

FIELD THEORY AND EXPERIENCE

Field Theory and SAF's

Bradford repeatedly [pp. 312, 319, 327] cites me as speaking of "action fields," a term that I don't think I have ever used, though it is used by my friend, teacher, and colleague Neil Fligstein. Bradford says that what I am talking about are not in fact such action fields, but, rather, fields of experience [p. 327], and seems to think that this is a critique of ESA [pt. 4]. But this is indeed exactly what I, like the Gestalt psychologists, *want* to talk about. This is a conception of "field" that was developed by the Gestalt psychologists in the mid-20th century, and which influenced

Bourdieu via Merleau-Ponty (here one may see [Martin and Gregg, 2014](#)). I cannot overstate my conviction that this approach to social explanation was the most promising direction that we had, that its abandonment was a great setback for the social sciences, and that one of the many praiseworthy features of Bourdieu's work was his rehabilitation of this perspective from independent starting points.

Bradford reduces my claim, acceptably for this level of specification, to "Experiences drive actions" and "fields induce experiences." And it is also correct that, as Bradford [p. 328] says, the converse of these is true ... but this is not quite as relevant as he makes it seem. That is, experiences (via actions) do change fields, but it is that the *aggregated* actions of *many* participants change fields, and *fields* induce the experience of *single* actors. Thus, the argument of ESA is exactly what Bradford thinks should follow logically from its premises; whether it is correct or not is a different matter. The chain "fields induce experiences which drive actions" is derivable from two planks: First (*QA*) that, as the Gestalt theorists, like the pragmatists, emphasized, the objects in our field of experience have *qualities* that call out for us to do things with them; second (*QB*) that in social action, the objects we confront include not only simple material ones but the virtual social objects that are constellations or complexes of relations.

Thus Bradford's main critique seems to be that simply reproducing a field of experience, should it be possible, is not in itself sufficient to explain action. This is technically incorrect, because the basis of the Gestalt approach is that to do one is to do the other (*QA*). I will briefly reproduce this logic, but later return to Bradford's critique and link it to a stronger one he made.

Quality

I was quite sorry to find Bradford [p. 330n12] noting as if it were obvious that my use of the term "quality" is "out of place in the context of discussing human sensory perception," for this means that I failed at what was perhaps my chief object, namely, to convince sociologists that our theory of action was wrong because our model of cognition was wrong, and that our model of cognition was wrong because *we believe that quality is an arcane and unscientific expression* and that we can discuss human sensory perception *without* discussing quality. In fact, this is the only critique of

Bradford's that mystifies me. Does he not himself perceive qualities in his environment?

Such a rejection of the idea of qualities would account for Bradford's conviction that the Gestalt approach does not explain action. Let me briefly sketch ESA's arguments here. First, what do we mean by a "quality" in an object? We mean that it has a potential to induce a certain form of response in something else, and in this case, we mean a certain form of experience in a human. But the key thing about human perception, argued the Gestalt theorists and ecological psychologists, is that one of the types of qualities possessed by objects is that they "tell us what to do with them." A cup with a handle actually tells the human how to pick it up, in that when we see the cup, we know how to wield it without any further thought. A French fry indicates that it would very much be a good thing to eat, and if we try to ignore it, it will remind us. Its capacity to remind us is its deliciousness, and thus if one eats a French fry, and one is asked why, one need do nothing more than point to the deliciousness of the French fry. In this simplified rendition, *to explain the quality is to explain the action*.

Of course, this is simplified, but we always start with simplifications. Bradford makes a serious critique of this simplification, and I will return to this below – and agree with him. The problem, however, isn't that of simplification. But before we get to that issue, we need to understand why this perspective – which I believe to be empirically defensible as a first approximation, while our other simplifications are not – is also theoretically progressive, in that it solves problems that other theoretical approaches cannot.

Judgment

In particular, the focus on quality, and on what we should understand as qualitative predication (our capacity to attach a quality as predicate to a subject), is, or so I believe, central for understanding the relation of action to subjectivity. You will recall that I began by saying that I believed one problem with our current approach came from our assumption (*PA*) that the cognitive components of action should basically be seen as propositional beliefs, things like "2+2 is 4" or "those elitist liberals are taxing hard working Americans to support the lazy poor." I have, or so I do believe, demonstrated that this leads to an instability in our science that *can* be resolved if we give an epistemic priority to that which is experienced (and not to the accounts of actors). But that would be irrelevant at best,

sophistical at worst, if I were to try to smuggle in the propositions to the world, and say that certain actors, say, “experience” that “those elitist liberals are taxing hard working Americans to support the lazy poor.”

But the considerations sketched in ESA do not imply such smuggling, nor even allow for it. Statements like “those elitist liberals are taxing hard working Americans to support the lazy poor” are *explanations* (folk theories, we might say) that are given by persons to justify their actions and make sense of their experience. What they perceive in the environment is not such (synthetic) propositions, but objects with qualities. Although in *some* cases, these can certainly be unpacked into (analytic) propositions (thus seeing a red fire truck can turn into “the fire truck is red”), we do *not* perceive the propositions, nor the form of predication, but rather a unity, and some propositions almost certainly *cannot* be perceived. Thus, an actor may indeed perceive “elitist liberals,” “hard working Americans,” and “lazy poor,” but will need to theorize the conjunction.

Now ESA does not claim that the actors perceive physically (say) one elitist liberal after another, and compound the set after this series of experiences. Rather, what our actor perceives is a *social object*, a bundle of relations. I believe that this is (again) true as stated, that is, what such complexes are is nothing other than the relations that persons establish, following the logic laid out by Marx (e.g., 1906 [1867]) in his work on the fetishism of commodities, and explored again by Simmel (1978 [1907]) in his *Philosophy of Money*. The chief critique that I personally would make of ESA is that this statement is, at best, obscure. I do not know how we do this. But I am pretty sure that we do, because we are able to produce inter-subjectively valid knowledge of the properties of complexes like political parties without having *any* direct experience of the individuals as individuals who make up this set.

Obscurity is indeed troubling, but it is not fatal. I am not sure yet if anyone knows how our visual system fills in texture in areas of the visual field that are not really being seen with the degree of resolution necessary for the determination of said texture. Yet it occurs whether we completely understand it or not. One of the central arguments of ESA is that *when* we recognize that actors are capable of this sort of judgment, we have a more defensible conception of how what is in their heads relates to their actions. Further, we then find that our own, scientific, understanding makes use of actors’ capacities, but compiles and systematizes many judgments, as opposed to negating them.

Now Bradford, I think, takes my arguments about judgment in a way I did not intend. I do indeed emphasize that our conventional way of thinking

about generalization, one which only allows for *subsumptive* judgment (when we put a particularity in a more general *class* by attending to only a few of its attributes), is in a weak position for understanding social action. Such action, I argued (and here I appeal to the entire history of Western thought from Aristotle onwards) necessarily also involves *reflective* judgment, our capacity to attach a universal predicate to a particularity *without* such subsumption (e.g., “the rose is beautiful,” or “Dukakis is a pansy”).¹ I am not saying that we should *not* formalize (as Bradford [p. 322] takes it), but that (1) when actors *cannot* formalize, they may still have veridical cognitive components to action; (2) we should be studying these and indeed measuring these; and (3) we are fortunate that we can expect that these are the subjective correlates of the affordances of social objects.

So far, I have recapitulated what I believe to be the strengths of ESA’s analysis: by starting from the veridical capacity of actors to intuit the inter-subjectively valid (though differentially distributed) qualities of social objects, they “know what to do with them,” and action can unproblematically be derived as an interaction between objects and actors placed in the same overall configuration, or field. However, there is a serious weakness in this analysis, one which Bradford uncovered, and to which I now turn attention.

CHARACTER

Bradford [p. 326] charges me with “overemphasizing” the external causes of action [pt. 3]. I have long dismissed such sorts of critiques of theories for two very good reasons. The first is that there is no “metric” for the “degree of emphasis” in theorizing, so this seems to come down to personal predilection. The second is that it is better to have a clear and consistent theory that has a limited range of applicability than one that seems to produce reasonable statements for everything, but only because of adhocery and other forms of fakery.

However, I have come to believe that Bradford [p. 327] is fundamentally correct here, and I indicate the way that I think we need to approach this in the forthcoming (2014) *Thinking Through Theory*, though it is not fully worked out there (I am still not sure *how* to work it out). But first let me re-state the criticism in a way that I think is more powerful than the issue of “degree of emphasis.” The field theoretic account says not merely that action is a function of the situation and the habitus of the actor, but, more

particularly, that these actions are in response to the ways in which objects call out for us to do things with them. These affordances are differential to the actor: someone who has never eaten a paint chip may think one affords eating; someone who has never used a cherry pitter may not understand what it can do for us; someone who grew up on a farm in Iowa may have a different feeling about the Democratic party from someone who grew up in Milwaukee.

The field account can, *technically*, handle differences between people by simply allowing for differences in habitus. But there is good a priori reason to think that there is one type of difference that is different, and this would be a general capacity to withstand the beckoning of objects. I first started thinking about this seriously when reading work on neurological impairments that led certain people to be *over*-responsive to the qualities of objects: they would grasp anything that hovered in front of them. *They were the perfect subjects for the Gestalt field theory* and they were impaired. Most of us have a capacity to *suppress* such a reaction. If that is the case, then there may be (and I think there is good reason to believe there is) individual variation in degree of susceptibility to the demands of the environment.

In the old days, this was called “character,” and I now think that any theory of action that relies on field and habitus also needs something vaguely like “character” in order to be stable. There is one way of thinking of habitus such that it *encompasses* character, but I worry that simply trying to erase this distinction would produce a pathological theory in which both predictive success and failure are treated with equal satisfaction (habitus becomes the sort of thing which explains why habitus doesn’t behave as anticipated). This may turn out to be a big deal, or it may not. Of course, I am hoping it does not – that the outlines of the field approach remain stable when we allow for suppression of response – but I think the only way to go forwards is, once again, to triangulate sociological evidence, psychological evidence, phenomenological evidence, and the clearest thinking we can muster.

WHO IS ESA FOR?

The saddest part in reading this review, however, was that Bradford took for granted that those who would read ESA were not those whom I was really addressing – those engaged in research. In addition to my error

regarding the role of character, I erred in thinking that a book about the fundamental conceptual problems in the practice of sociology, if written forcefully and clearly, would reach practicing mainstream sociologists. Perhaps, this can be done, but it does not seem that I have succeeded in doing so. It would be nice if someone else could succeed here, but there may be another way.

When I was in graduate school, and the anti-theory movement was building momentum (I discuss this in [2014]), we were told that theory was too important to be left to theorists, and that all researchers were theorists. That might well be, but I haven't been consistently impressed with the results. So perhaps it's time for the theorists – those who identify themselves as such and are willing to seriously think about what lies in between our practice and our knowledge as sociologists, and for this reason, those who must seriously think about what lies in between our practice and our subjectivity as *actors* – to do the research.

NOTE

1. Bradford correctly [p. 322] notes that in reflective judgment we do not necessarily pay attention to every particularity (thus when saying a rose is beautiful I do not necessarily pay attention to its weight); I think a fair way of putting it would be that our attribution is, first, an attribution of a quality possessed *by* a whole and second, does not reach universality by *suppressing* particularities. This issue of the difference between subsumptive and non-subsumptive judgment, however, is well understood and need delay us no longer.

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