THINKING THROUGH THEORY

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CHAPTER 6

"The Gentleman Is to Let the Lady Enter the Door First"

Or How the Environment Tells Us What to Do

MOTIVES AND TENDENCIES

Go Figure

When we're tired, or in a hurry, or focused on something else, almost all of us in sociology fall back on what I'll call "good old-fashioned action theory" (GOFAT). Almost no sociological theorist would defend it, and yet we use it almost all the time. We try to attach fancier or newer terms to this basic structure (and recognize the "practical" nature of this or that), but such qualifications don't seem to have gone very deep into our way of thinking about action. (Of course, there are some people who put forward very different ideas, but these haven't convinced the rest. I think that we *should* be able to convince them, and that's the goal of the current chapter.)

GOFAT links actions to motivations, first and foremost. These motivations, in turn, usually come from some sort of mixing process that, even if not a true calculus, can take inputs from "values" and from "interests," and use some form of rational or quasi-rational deliberation if necessary to make all this work together. The fact that people are being moved by the same values, or by systematically related interests, produces regularity in actions and allows us to explain them. In fact, for a long time, interests alone were understood as sufficient to guarantee the possibility of social science. One thing about rational interests, as Hirschmann (1977) said, is that unlike other passions, they are predictable. Although there are conflicts of interest between persons, we can at least learn to make a social order that contains them or works with them, as opposed to the conflict implying chaos.

However, this sort of order wasn't quite orderly enough for the sociologists, and so they emphasized the importance of *values*. Parsons (1968 [1949]) in particular argued that just having actions be motivated by interests would lead to a

war of all against all which, even if predictable, lacked the kind of order that springs to mind when we use the term "orderly." Nowadays, we compromise, admitting that *both* interests *and* values lead to action (Parsons joined these in a great-chain-of-normative-being). Actors think about things and decide to do one of a number of possibilities, taking into account the means and materials at their disposal, and the likely results, as well as all the moral prescriptions and proscriptions around. Further, we assume that an act is a naturally meaningful unit. People commence an act as a means to their end, and when they emerge out of it, with success or failure to their credit, the act is done.

This might seem so obvious that we forget other ways of beginning — other ways in which the orderliness of action can be approached. One of these is related to older ideas of action, prescriptive accounts turning on virtue, and this is to understand people as having "characters"— abiding profiles of tendencies that come from the inner nature of each one. And indeed, I will argue that "character," despite its antiquated ring, will turn out to be a nontrivial form of regularity for social action. Another possibility has often been considered only in terms of *weakness* of character, and this is the importance of "situational factors"— certain situations tend to call out certain types of actions from persons, no matter what their values or interests. Recognizing the importance of situational factors is often understood to imply positing the weakness of character, for those with strong characters are the same in all circumstances (think of Scott's men politely freezing to death in turn at the South Pole).

Now I think there are good *empirical* reasons to jettison GOFAT. That is, I think the evidence doesn't support it, and instead points in other directions. However, that isn't always enough to lead us to reject a theory, and in some cases, it *shouldn't*. This is especially true if we can't come up with any coherent alternative. We're always going to think in *some* way, and if we stick with a false theory, at least we'll know *which* way. But in this case, there *are* alternatives. Here, drawing on the results of the previous chapter, I want to propose that there is a coherent, largely situation-based (though character-sensitive), approach to explaining action.

What Are Motivations?

One of the greatest pieces in sociological theory was C. Wright Mills's (1940) article on motivations. Inspired by American pragmatism and its (I think still) revolutionary reappraisal of the nature of action, Mills turned his attention to motives. According to GOFAT, motivations precede actions: "First you gotta tell yourself you're gonna do it, and then — you gotta do it!" In Hobbes's (1909 [1651]) version, a stirring in the imagination rattles around in the nervous system, sending some sort of impulse—*"Now I want you to pick up that pipe"*—to the hand. The motive is the cognitive version of the act; in some ways, it is the ideal expression of the act itself. In the beginning were the words—*"Pick up the pipe"*—and then, lo, there was the action.[1]

Mills basically asked us to put our folk theories on hold, and be empiricists, at least for a while. Where do we actually *see* "motives" appear? Introspection actually has a tough time uncovering it. I just typed eight words and fifty characters in the previous sentence. Did I have a motive to type the "p" in *introspection*? Or was that not an act? Did I have a motive to type *introspection*? Was *that* an act? Or was the act the typing of the sentence *Introspection actually has a tough time uncovering it.*? But when I started, I didn't even know I would end with the word *it*. Was it writing this book that was the act that was preceded by the motivation? But I'd already formulated portions before I had the idea of writing a book, and the decision to put them together in a book came as I was impulsively lying to an editor, assuring him that I planned to write a book of essays on theory while working on a larger project, so that people would know I was still alive. "Sounds like a good idea," I thought to myself, as I heard the blatant fabrication.

This actually seems to be rather typical of actions. They usually don't have a "pre-" action stage whereby the motive *could* be formulated.[2] The means are often their own ends. And so on. So if we don't find a clear "motive to write this book" or "motive to go to college" or "motive to go shopping" when we seriously introspect recent concrete activities, where *do* we see motives? Mills's answer was that we see them *after* the event. Sometimes these come in the form of "accounting"— when we attempt to excuse ourselves if our actions are challenged. "Why didn't you water the lawn?" "Well, it looked like it might rain, so I thought I'd save the water and wait to see . . ." But even outside of the challenge-account couplet, motives emerge after the act, when the actor is involved in social

[1] Most neurologists would say that there's strong evidence against this, since instruments will pick up changes in the neural system that precede our conscious awareness of wanting to do an act. But it isn't quite a closed case, because it's so hard to compare the timings of different parts of the nervous system. As you can imagine, it might also take some time to go from wanting to do something to knowing you want to do something to remembering to press a button to actually have your finger press a button indicating "I want to make a movement." So I'm happy to consider this an open issue, and one that might go nowhere if it turns out we are attempting to split up things that are indivisible.

[2] Colin Campbell's (1996: 58f) response to this critique is to say that the critics are looking in the wrong places (such as, right before typing "it"), so of course they aren't finding the formulation of a coherent subjective motive. Rather, we should use the presence of such a motive to determine where an act begins and ends. So, for example, if today I go to the post office, as I currently intend, that would be a good example of an act, and it has a motivation that in fact existed in a stage of deliberation ("I should go to the post office to mail that letter.") before the act. This is entirely acceptable, but it means that, from the moment the alarm clock rang today to the time I fall asleep, this is likely to be the *only* "act" I have accomplished — as I didn't think like this before I made coffee, went to work, picked up reading where I had left off, and so on.

dialogue.[3] It turns out that often these professed motives aren't empirically very sound; even when they're trying to be honest, people seem to do better at explaining why they *would do* that action *now* than they are at recalling the precise constellation that existed *then*. But even if our offered motives are as accurate as accurate can be, they're still a selection, said Mills, of different *vocabularies* of motive. A person can "become" a doctor "because" she didn't want to be poor, because she wanted to help people, because she found math (her true love) too hard, and so on. In certain circumstances, Mills argued, one of these vocabularies may be favored over another, and for no (scientifically) good reason.

So I have said (correctly) that I didn't have "an" idea for this book before the writing. Even further, in some ways, I didn't write it so much as it used me to write itself. This section was added because it became obvious that it needed to be here, when I read over the third draft. If you had read it, you'd probably have added this section, too. Of course, maybe not — there are branches in the paths that lead through the space of all possible outcomes, which is why we can assume that the thousand monkeys don't all type the same thing. But the book as it was, my local environment, called for someone to add this in. I just happened to be here as the first responder. This idea — that action is really a dialectic, a continuous interaction with the world, yada yada — certainly isn't new (an elegant formulation tied to more specific claims should be forthcoming in a book by Ira Cohen). And that's why I hadn't put it in the earlier drafts — because everyone knows it already.

Or do they? We often assent to this vision of action, yet also claim that action can be explained as rule following, not understanding that this completely contradicts the dialectical understanding of action. As we'll see, this dialectical vision instead is compatible with a different understanding of the nature of social action, one more in tune with pragmatism. That is, most of the sociologists I talk to do acknowledge that GOFAT might exaggerate the degree to which motivations are always conscious. But they reject Mills's idea of considering motives as coming *after* action. "So just call the mental things that come before the action 'motivations,' even if they aren't well-formed, propositional beliefs." *Before*? This still assumes that action naturally breaks up into droplets, unit acts, each with its own subjective preface. If this isn't so — and I think every last bit of evidence tells us, and has told us, for 2000 years, that it isn't so — then this is to stick with a fundamentally misleading understanding of action, just to avoid rethinking our vocabulary. The chunking whereby one part of the stream of

[3] Some ethnomethodologists distinguish between *excuses* — when we lessen our degree of attributed control over some outcome, recognizing that it was a dispreferred outcome — and *justifications*, when we believe that we can demonstrate that our actions were defensible and thus do not need to lessen our agency (Scott and Lyman 1968).

behavior turns into "an" act only comes in the accounting after the fact. The "act" as such doesn't exist for it to have a motive. That doesn't mean there aren't cognitive components of action, but we can't rely on a minor modification of GOFAT to understand them.

Deliberation and Liberty

It is not that the pragmatist tradition, on which Mills drew, denied that people ever thought before acting. Rather (and here Dewey [e.g., 1930 (1922)] is our chief exemplar), their understanding was that action is an alternation between nonproblematic, habitual action, and problems. Problems are thrown up by the world. They call for readjustment, recalibration, and sometimes even deliberation.

Thus if we want, we can certainly find examples of deliberation before action through introspection. Indeed, many of us spend countless hours (often around 1 AM) deliberating over what to do. But when translating these data to our theoretical understanding, we can easily make two mistakes. The first is to hold on to this "deliberation-first" mode as the fundamental one for our theory of action, when it seems to be the exception. This is an easy mistake to make, for if we try to think about our action, by definition, we are reflecting. It's easy then to say that a key part of *anything* is the presence of this sort of reflective consciousness. But that's formally akin to concluding that the light is *always* on inside the refrigerator, because every time you open it to check, it's on![4]

The second mistake is that we easily confuse the fact that deliberation often arms us with justifications for our action, which is certainly true, with the thesis that deliberation is responsible for the choice — this is an empirical question, and the evidence is not great in its favor. Indeed, it may well be that deliberation is not a part of a process of *making* a decision but more what it feels like when one is *unable* to make a decision. At least, when I am awake at night, you could call it "deliberation" or "obsession" or "indecision" with equal justification. It doesn't have much to do with those (few) actions that I successfully implement.

[4] Archer (2003) has suggested structured differences in the type of reflection that people use, which gives us a much richer sense of the nature of internal dialogue, though the question of its proportionate weight in explaining action remains open. Indeed, it may well be that her empirical work is not immune to the refrigerator problem — by asking respondents about their reflection, and then categorizing them by their responses to *her*, Archer may not simply be assuming what some of us doubt, the centrality of deliberation, but indeed *creating* the particularities (one cannot help noticing that the category of "meta-reflexives" are PLUs — "people like us" — who, themselves academics, know how to express a thoughtful accounting that will push another academic's buttons). Thus, what appears to be an avenue to the innermost subjectivity of others may be a joint production of the interview situation.

Although deliberation, then, often does take place, it does not do so "before" the act has begun. Instead, any truly weighty decision is actually a conglomerate, most of which consists of nondecisions and nonactions: before you can "decide" to be a doctor, you need to have taken enough biology courses to go to medical school; if you took a lot of religion and history classes, you've already embarked on a path that makes the decision to forget about medical school a *fait accompli*; and so on. By the time of the "deliberation" and the discursive reasoning, almost all the "decision" has already taken place, because we have already shaped our environment so that we only need to look around to figure out where we have already guided ourselves, by means of previous actions that have foreclosed enough options that a calculus is not as silly as it would be for a more open-ended choice. If deliberation is prolonged, it may simply be that we have failed at a successful trajectory.

In sum, the fact that we often deliberate — run through different possibilities, attempting to weigh the positives and negatives — doesn't mean that we have found evidence for GOFAT. We only count it as evidence if we assume what is in question — that these sorts of psychic events are the causes of later action. And here is where orthological (and not substantive) considerations come in.

Tendentious Tendencies

What are motivations? Clifford Geertz (1973: 96) gives a classic formulation: "A motivation is a persisting tendency, a chronic inclination to perform certain sorts of acts and experience certain sorts of feeling in certain sorts of situations." That, at least, seems a pretty safe definition. But it can encourage some dangerous leaps of faith.

When one is trying to explain a phenomenon but doesn't quite understand it, the most appealing sort of explanation is often a pseudo-explanation, whereby we try to take the phenomenon we're trying to explain, and stuff it back into the object of explanation. We can then pull it out like a rabbit from a hat. Perhaps the most generic one is the idea of "tendency." We try to explain X's appearing some way or doing something by saying that it has a tendency to do so. You might think this is so obviously silly that no one with a brain would propose something this vacuous — at least, not since the early modern period. After all, thanks to Molière, we're all familiar with the foolish scholastic explanations that only restated what was to be explained — thus opium's effects are due to its "soporific tendencies."

Yet it turns out that it is often hard to avoid the tendency to invent tendencies — indeed, the realist theorist examined in Chapter 3, Roy Bhaskar (1975), deliberately tried to rescue the idea of tendency for scientific explanation.[5] I confess that I found myself relying on the idea of "structural tendencies" as I tried to formulate an understanding of how social structure emerges (Martin 2009: chapter 1). Once I realized that (in early drafts) I had actually inserted a tautology and dressed it up like an explanation, I was distraught, and it took quite some time before I was able to specify it nontautologically if vaguely as actual sentiments experienced by actors.

It turns out that it isn't quite the tautological nature of the idea of tendency that is the problem. That may bring the problem to our attention, but it is only a polar position of a continuum of problematic usages. In all cases on this continuum, there is something, some phenomenon X, that we are attempting to explain. So we take X and somehow push it, in some materialized form, into the entity responsible for the phenomenon. For example, the phlogiston theory of combustion was that there is something potentially fiery in objects (phlogiston), and objects burn because the phlogiston in them comes out and is actualized. These theorists were too hasty, and needed to remember that the only place they empirically saw evidence of fire was in the burning, not the pre-burning. It was overly convenient to assume it into the objects. And that's our theory of motivation. It's the action, stuffed into the head of the actor, idealized, but not fundamentally changed. It should raise a dozen danger flags.

Now consider another form of the tendency argument, one on which I've relied a great deal, namely, the idea of "quality." A quality, Peirce (1955 [1875–1910]: 85ff) said, is a *potential* for experience — that doesn't mean it isn't *real*, for it can be a real potential.[6] Thus we may say that the quality of redness is the potential of something to induce a qualia of "looking red" in a human observer who sees it under certain conditions. To say that its redness is *explained* by its quality of redness is completely circular and a waste of breath.

But that doesn't mean that the idea of quality is useless. Formulaically, we can say that if a quality is an attribute of some set of elements S (not necessarily a single element), say, $Q(S)$, that is defined by its propensity to produce some event on S, say, $E(S)$, that is a transformation in other attributes of S, we don't get any purchase by saying $Q(S) \rightarrow E(S)$. However, that doesn't mean that we don't get purchase if we say $Q(S) \rightarrow E(T)$, where $T \cap \sim S \neq \varnothing$ (that is, T includes something that S doesn't). Further, we may be able in many cases to say something about $Q(S)$ other than $\rightarrow E(S)$. It turns out (as we remember from Chapter 3) that we can say a lot of interesting and nontrivial things about the quality of redness in

[5] Bhaskar (1975: 175, 229–231) has a great deal to say about this issue, as he often does for things about which there isn't much to say.

[6] In general, there is a close formal relation between tendency and potential, though we seem to use them somewhat differently, and, though perhaps I can't defend it here, it seems less problematic to claim that a "potential" exists even when it isn't actualized than it is to claim that an entity has a tendency not being put into practice.

addition to its leading things to look red to us. It can signify infection, heat, receding, or what have you.[7]

Further, there is a way in which "quality" turns out to be a *relation* (e.g., a wine tastes fantastic to you and stanky to me), and we might argue that rather than some action being due to the quality of an object (I bought it because it was good wine) or of some characteristic of the actor (I am an oenophile), it is a function of their interaction, giving the two analytic independence from the act. We'll return to this interaction below.

But for now, our lesson is simply that we should always be suspicious when an explanation is of the tendentious form. In many cases, it is a pure tautology that makes us appear to be saying something when we are actually saying nothing at all. However, sometimes inserting a nearly tautological tendency is not at all problematic, if it allows us to link other theoretical elements.[8] Further, there are times when the tendency argument *isn't* tautological—it is wrong. This is when the tendency we insert is not of the contentless variety—something like "whatever-it-is-that-leads-things-like-this-to-do-things-like-that"—but rather, has a specific nature. In the case of motivation, our tendency is a homunculus version of the act, an iconic representation of what the motivation is supposed to cause. And that's our voo-doo doll theory of action.

So it isn't that there aren't things in the actors' heads, or bodies, or both, that explain the action. There certainly must be. But it's overly convenient to imagine that these are action-ideas, representations of the action, or anything like that. It's just like the early modernist scientists who scoffed at the scholastic explanation of opium. The true explanation, they were sure, was that the opium particles were round and smooth, and thus soothed the nerves as they rolled up and down them (Shapin 1996: 57). This isn't a tautology, but it's overly convenient, hoping that "soothingness" can be seen, as such, in the entity. Looking for "motives" for our action in our minds is like doing an fMRI (functional magnetic resonance imaging) on subjects as they talk and expecting to see the words they speak lighting up, spelled out in Times New Roman letters on their brain.

If this model of "motives" is as wrong as I think it is, this realization has some serious implications for one of the issues that is most associated with what are often understood as "cultural" emphases in sociology—namely, the idea that we

[7] Take the idea of "habitus," especially as used by Pierre Bourdieu. I'm sure that somewhere this has been criticized for being tautological. Indeed, I myself have argued that there is something wrong when we try to explain, say, someone's choice of an authoritarian political candidate by the actor's "authoritarianism." Isn't this an argument by recourse to habitus? It certainly is, but Bourdieu's wasn't. That is, Bourdieu didn't announce, I can explain everything anyone does—I'll just tell you what his or her habitus is. Rather, if habitus itself can be partially explained, then it is neither tautological nor irrelevant as part of an explanation as a whole.

[8] Note that I am not in any way saying that quality or habitus *is* tautological—both are subject to empirical exploration and have specifiable measurable aspects. However, pursuing their empirical and physically measurable aspects is unnecessary for the argument at this point.

follow rules (or norms, or schema, or whatever). We saw in Chapter 2 that it doesn't make much sense to say that culture constrains us—but does it make sense to say that rules do?

WHO FOLLOWS RULES AND WHEN?

Rules Are for Fools

Since so many of us don't like rules and feel ourselves beset and oppressed by them, it seems undeniable that they are there. Further, since many of us also suffer grievously, time and time again, for violating them, it seems that they matter. Finally, it also seems that these rules must be stored in our heads, and when we don't break them, it's because these rules, in our head, are guiding our actions. Left to your own devices, you are free to drive your automobile any way and anywhere you want. In circles in the middle of the park, up on the neighbor's porch, 100 miles per hour on the left side of the street. But if you drive 55 on the right side, it is because you know that this is what you are supposed to do.

This can't, and so shouldn't, be denied. Yet there seems to be something wrong in our current understanding of rules. For one small thing—probably, you *don't* drive 55, even where that's the law. You might still feel like you are following some sort of rule when you do what basically everyone else is doing. You believe, and you know it isn't so, that it's okay if everybody's doing it. You have a set of things rattling about in your mind—an understanding of the law, a sense of what is typical, and many other different justifications that you can use to explain to your child why you're breaking the law, and which you should probably keep to yourself if you're pulled over by a cop.

More generally, there are indeed patterns of action that we *call* rules, things like you're not allowed to initiate touch with strangers who are your status equals, or that you say "Good morning!" in a perky voice to co-workers but not to good friends, or that when someone asks you "Howzit goin'?" you say "Fine." And so, when we ask (if we do), "Why do people say 'Fine' when asked 'Howzit goin'?" we come up with the answer, "Because they are following the rule."

This does seem to adequately explain the cases of rule following, but it still isn't an acceptable answer if we're looking for a theory of action. We can indeed explain every instant in which we did not put our elbows on the table by appealing to our internalization and memory of "Don't put your elbows on the table," but if this is taken as support for the norms view, then it is only fair that every instant in which we *do* put our elbows on the table is taken as evidence *against* this theory. The problem is that we *do* put our elbows on the table and we *do* hit, and so on. To get a complete "explanation," we must allow one to bring in *other* norms and explain why *in this case* it was permissible to hit or put our elbows on

the table.[9] We find that rather than there being "some" norms, there are many, so many, in fact, that it seems that basically *any* action could be described as being in consonance with the larger set of norms. We realize, then, that these "norms" are "ethnomethods"—vocabularies that actors use to *justify* their actions, not *causes* of their actions.

Impressive empirical studies—most important, D. L. Wieder's (1974) work—suggest that, at least in everyday informal action, the rules are something that actors invoke *after* the action to explain, excuse, or condemn it. The "rule" exists only in interactions, when one person attempts to manipulate another's action (whether for good or bad reasons) or to account for his or her own. Thus, argued Wieder, rules aren't things that lie behind and *shape* action, they are things that actors *use* in action—with typical flexibility and perplexing illogicality. There may be a pattern, and the pattern may be theorized by actors, and the actors may formulate this as a rule, but we must not therefore assume that the rule causes the pattern.

The similarity to our previous finding—that the motives appear *post actum*, not *pre actum*—should be worrisome. We saw in Chapter 3 that we need to beware of any argument that sounds substantively impressive but works just as well no matter what we drop in. There our pattern was "X couldn't exist in a people without language," because we had inadvertently substituted '*X*'—that is, the linguistic ideas associated with the term '*X*'—for X itself. So, too, there is a danger in arguments of this form: "Y doesn't lie behind action, because *Y* exists only in social action." The reason is that if empirically we can see things only when they are "in social action," then this is always true and doesn't add much.

Or perhaps it does. There are times when a statement that is true for everything you substitute in is still nonvacuously true. A humorous example is, "You cannot read this sentence without thinking of an *X*." Substitute elephant, submarine, or violin, and it's actually true. And there are claims that we may not be able to dismiss even if they are false, which is frustrating, but they are neither empty nor tautological. Imagine I claimed that blood in your arteries was red, because it was oxygenated, and blood in your veins was blue because it wasn't. Let's also say that even the fair skinned among us get outside enough that we can't see right through pale skin (we are imagining, remember!). You ask where a big vein is, and I trace one on your arm. Emboldened by the quest for knowledge, you stick a pin in your vein and draw out red blood. "Your theory is wrong!" you triumphantly conclude. "But any blood we see has mixed with oxygen and is therefore red," I respond. "Hey, no fair. Your theory is then unfalsifiable! There is

[9] We could also appeal to our animalistic or satanic nature, but this has become unpopular in sociology, because admitting that we all might have enough devil in us to override society requires understanding humans as having a presocial "essence," which we've all agreed to ignore as a possibility. I actually think that's a wise choice, though maybe that's just one of the devil's tricks . . . .

no variation," you complain. I understand your frustration. Without going beyond pinpricks, we can't know the truth. The answer, though, isn't to reject my argument, it's to go beyond pinpricks. The limitation isn't in our thoughts but in our methods.

So, too, there may be something very important in the realization that all the things we're putting in people's heads are ideas that have actually come from their interactions with others. That doesn't mean that *nothing's* in their heads, but if there is something there, it isn't necessarily things like "rules." We must be cautious, but we don't need to automatically withdraw from exploring this idea simply because it might be—not *is*, but *might* be—one of these formulae that work for any X we put in.

Indeed, we shouldn't refuse to entertain that problematic thought experiment, our imaginary world of speechless human beings. These individuals interact but don't talk. In this imaginary world, would there be anything like a "motive" in the sense we understand it? Well, we know there wouldn't be one if we defined motive as the thing-that-is-raised-in-social-interaction. We need to first imagine what we might mean by "motive" in such a way that it isn't impossible in our speechless species by definition. But I actually have a hard time figuring out what sort of motive they might have. It seems that our very idea of motives, our belief that there are such things, may come from the social practice of answering questions and explaining things. We can imagine a motive being something like "drink to quench my thirst." But I find it hard not to conclude that in this speechless world, to the extent that we thought about why we did things, we would not be thinking about ourselves but about our *environments*. Our drinking of water would likely appeal to the qualities of water, not the thirst of the actor.[10] It is, just as Mead would have it, the switch to reflexivity in a certain type of interaction that puts a self in our thoughts, and a motivation is an account that makes use of this self.

Well, this exercise isn't determinative. First, as said above, our inability to imagine something is never a proof; further, even if it were true that speechless beings wouldn't have motives at all yet would still act, this doesn't mean that *we* act without these motives. Maybe speech changes us big time. But perhaps now we can start to imagine a kind of rule following that is dissociated from ex post facto challenge and defense. We can use this to approach the issue of who follows rules and when.

[10] Armchair theorizing often claims that at the root of any action must be a *lack* that motivates us. Phenomenologically, this is extremely implausible and represents an introjection of self-monitoring where there is no reason to expect it. The presence of thirst isn't necessarily experienced as an absence of drink, and water looks refreshing (because it is), often even when we didn't *know* we were thirsty until we saw the water saying "drink me." Sure, the starving person or the stranded crawler in the desert might indeed have a different relation of his inner sensation to his action—but using the stranded crawler as our default model for action might be like using the person blinded by sunlight for our default theory of vision.

Good Morning!

Now let's handle a simple imaginary problem: At 9:04 AM on Friday, Reginald enters his new workplace and says "Good morning!" in a perky voice to Archibald, Elizabeth, and Ronnie, who all work in the same office room. Why did he do this? Was it because of the rule, "You say 'Good morning!' in a perky voice to co-workers but not to good friends"?

Perhaps we can distinguish between:

1. A *pattern*: For example, on Monday, Tuesday, Wednesday, and Thursday, all four people in Reginald's office said "Good morning!" in a perky voice to whoever was already in the office when each arrived.
2. Reginald *seeing* this pattern: "You know, on Monday, Tuesday, Wednesday, and Thursday, everyone in the office said 'Good morning!' in a perky voice to whoever was already in the office when each arrived." I'm going to follow my previous (2011: 303) usage and call this his subjective understanding of (1) *patterning*, and use *pattern* for the underlying regularity.
3. Reginald *extrapolating* from this: "Here it seems that, in general, people say 'Good morning!' in a perky voice to whoever is already in the office."
4. The *descriptive rule*: "People [always] say 'Good morning!' in a perky voice to whoever is already in the office."
5. The *prescriptive rule*: "People should say 'Good morning!' in a perky voice to whoever is already in the office."

If Reginald held (5), this could be used to explain his action. Of course, there could be a somewhat different version that does not involve (5). Instead, there are two other planks:

6. Those who violate descriptive rules are conspicuous.
7. I do not want to be conspicuous.

It seems to me that the "rules" account is happy with either the (1–5; prescriptive) or (1–4, 6–7; descriptive) versions of these. Although in a philosophical sense they are worlds apart (one normative, the other instrumental), both are indeed saying that the rule guides action. But what if we come up with a modified form of (6), call it (6′), that goes as follows: "Those who violate patterns are conspicuous," and propose an account that accepts 1–3, 6′ and 7, but not 4 or 5? This seems to be just as satisfactory. Yet it lacks the idea of the rule (though it does assume that the focal actor will only avoid violating *patternings*, because he

won't know about *all* the possible patterns). Of course, it might be that this is splitting hairs—there is no real difference between (2) and (3) and (4). And yet, it seems that there is. There must be thousands and thousands of empirical behavioral regularities ("patterns") in Reginald's office. Not all are recognized as such (and therefore become "patternings"). And so they certainly aren't all rules, which means that breaking such patterns might not be noticed at all, and certainly wouldn't make one conspicuous. For example, if one is left-handed and picks up one's coffee cup with one's left hand, it might not be noticed, and if it was, it might be immediately forgotten.

Some of these patterns can be important for understanding social action. For instance, it appears that in large unstructured spaces with many people going in opposite directions (e.g., the lobby of a train station in rush hour), people dramatically increase their average speed of travel by tending to walk behind someone going in the same direction (Couzin and Krause 2003: 19). They may know they do this, and they certainly wouldn't deny it were it brought to their attention, but that doesn't mean that they do it because they hold the norm. They might actually be swept into the slipstream of other people simply by following a heuristic such as "try to go forward and keep a 2.5′ radius of space around you." Yet they don't have it in the form of a rule that could itself explain their action as due to rule following. And that's because someone who *doesn't* follow isn't perceived as violating the rule, he's just perceived as standing in your way.

Still, so far I've really only demonstrated that patterns (2) aren't the same as descriptive rules (4). But are "patternings" (3) really distinguishable from rules? Can we identify patternings that aren't rules? Or can we figure out what makes a patterning a rule? To do this, let's consider cases in which four things hold: first, an actor *A* can reach generalizations of form (3) above; second, *A* believes that such generalization is generally available—that is, every other *B* in the situation should have a similar generalization. It seems that this second criterion is necessary for a patterning to be a descriptive rule: if we didn't think that others were aware of the patterning, we wouldn't say they were following a rule. In any case, the third thing we'll assume is that there is some *B* who violates the patterning; and the fourth, that this inconveniences *A*. In such a case, *B* may well be perceived as violating *something*, and this may need to be ascribed to *B*'s lack of *knowledge* of the patterning, to *B*'s lack of *ability* to execute the patterning, or to *B*'s lack of *goodwill*. Especially when we go with the last of these, we are likely to see the patterning as more rule-like.

What we are finding—and this is by no means a new argument, it was taught to me by my advisor, Ann Swidler, and I'm stealing her ideas right and left here—is that it isn't the regularity itself that leads to (6), it's that some sorts of regularities have *meaning*. Not saying "Good morning!" is different from how you pick up your cup. Why? Because it is one of the things that can be scrutinized by

those who are trying to draw out information on others, with or without their knowledge. Not following a general pattern when it inconveniences *me* means that you are not respecting me, which means that you are a jerk, and a great way to explain your jerkiness is to appeal to the widely understood rule. That is, following the results of the last chapter, something moral emerges as we see the expectations associated with the patterning as legitimate expectations to hold.[11]

On Valentine's Day—which is a pretty stupid holiday honoring a religious figure that even most Catholics don't give a hoot about—we give presents to our true love. Maybe most people do this. Maybe it's a rule. But that's not why we do it, or not exactly. And certainly it's not because we want to pay tribute to Saint Valentine (since no one is even sure who he was). Forgetting Valentine's Day can be a big deal because it's the sort of thing someone who didn't care about his or her true love might do. That's why we all give presents. And it's because "we" all do it, that it has meaning that *you* didn't (and no, it's *not* okay!). If we all didn't, your noncompliance would carry no information.

Now here's the thing: if we all *really* gave chocolates on Valentine's Day, it wouldn't be a rule. Why? Because there would never be occasion to say "You (jerk) *broke the rule. You* forgot *Valentine's Day.*" In my elementary school, there were rules against giving wedgies, cheating, and leaving the campus to get candy, because we—some of us, sometimes—gave wedgies, cheated, and left the campus. There were no rules against lighting children on fire, eating glass, or teaching the class yourself, because no one ever did this.

Sometimes it turns out to be true—that things don't exist unless they're talked about. It's not always true, and in the 1970s some sociology seemed very silly because folks liked the shock value they got by implying that if you didn't mention it, a Sherman tank would bounce right off of you. Power doesn't come from talking. You can call a policeman "my little pony" and not "officer" but that doesn't mean he won't sock you with a billy club. But rules, at least in the way in which we use the idea in sociology, might well only appear *as rules* from the talking about them.

Talking 'bout Rules

This doesn't mean that everything we might want to call a rule has this communicative nature as its core. So thinking back to elementary school, I remember

[11] I recognize that one could also argue that there is something pre-existingly moral about the nature of the individual, and it is this that leads to the belief in the legitimacy of expectations here. But this doesn't seem to get at what is distinctive about these situations, at least among people and places where we don't treat people as behaving immorally for accidental wrongs made in ignorance (though they still might be held responsible).

observing and breaking some rules. Some fit the semiotic version: I didn't play with the girls, because that would have semiotic import, so long as other boys weren't playing with the girls; or I didn't pledge allegiance *because* it was a rule, and so disobeying could be meaningful. But other observances and nonobservances lacked this meaning-focus: I didn't tattle, because I didn't want to be punched (pure self-interest); or I didn't copy my neighbor's paper even if it would have been in my self-interest to do so. (Though, interestingly, note that I can't say "I wouldn't have cheated, even if it weren't a rule not to," because if there isn't a rule against it, it's not cheating!)

And yet, there is a way in which to the extent that the *rule-ness* of the rules acted in a distinctive manner for my action, the semiotic component comes to the fore. That is, there were lots of things I didn't do because I didn't want to be punched (not that I wasn't, anyway). Whether they were rules or not didn't really enter my subjectivity. In fact, it might be that (as Hobbes would have said) there is really only one rule of the playground: do not do to another that which is likely to lead you to end up getting punched. Although you could *describe* the action using the idea of rules, there's no reason to think that the subjectivity of the actors was oriented to these rules. The regularity generally comes from somewhere else. Rules appear as rules when there is contestation over them, and when it can be meaningful (or can be made meaningful) to be seen as in or out of compliance with them.

If so, then we can't stick with an explanation of the regularity of action that turns on our walking around with catalogues of rules and following them. What else do we have?

I CAN ONLY OPEN THE DOOR

A Field-Theoretic Account

In other works (especially Martin 2011), I have argued that the importance of field theory is that it provides us a more plausible account of how persons act in regular ways; more plausible, that is, than GOFAT and its kin. I personally think that the field-theoretic account does not fall afoul of the strictures laid out in the preceding chapters, yet I acknowledge that it has been substantively weak. Here I want to see if—in part, drawing on the results of the last chapter—we can push forward an attempt to offer a serious alternative to the norms-driven account.

The running example here, for reasons I will explicate below, will be the norm "The Gentleman Is to Let the Lady Enter the Door First." According to GOFAT, when I am a Gentleman and in the environment is a door and a Lady, I recall the norm, find that it fits the condition ("when a Gentleman and a Lady come to a door . . ."), understand the conclusion (". . . the Gentleman should let the Lady enter the door first."), and hence I let the Lady enter the door first.

In contrast, I have claimed that actors are not guided by such norms and that they do not even carry them around in their heads (though I will clarify this misleading claim below). There have been two justifications for this claim. First, there is the argument of implausibility: it seems hard to imagine that we really carry around norms for every form of regularity in social action. The second justification was parsimony: if we do not need to have an internal rule for all forms of regularity, and we therefore have some other way of producing regularity from the environment, it may be more parsimonious, or at least more intellectually pleasing, if we imagine that all forms of regularity arise thusly—they do not correspond to general norms but rather to regularities in the environment that we experience (here see especially Reed 1996).

These are not actually very strong justifications, especially given what should be an apparent weakness in the critique of GOFAT, namely, that on the one hand, we *do* have verbal formulae for many norms (e.g., don't put your elbows on the table, don't hit, and so on), and on the other hand, it is quite obscure what it means to claim (as I did) that the environment contains the information on how we are to act. It is therefore reasonable for explanations to revert to the norm-driven version until stronger justifications are provided for the field-theoretic approach.

I will try here to provide such stronger justifications—and thereby demonstrate that the principles introduced by Bourdieu (1984 [1979]) in *Distinction*, often taken to be a good if limited account for "culture" (whatever that is) but obviously inadequate as a general theory of human action, are in fact of the broadest relevance. But first, I must turn to what I think is a possible misstatement that those of us who put forward an ecological approach to action in place of a normative one may be prone to make. We often make it seem as if it were impossible that the rules could be "in the heads" of the actors because there isn't sufficient room. But this can't be so. If people can use the rules to account for the action *after the fact*, presumably the rules were there *before* the fact as well. So why could they not inspire the action, as well as account for it?

One can't deny that we can memorize many verbal formulae and later recite them. That doesn't mean that they guide our action.[12] Think of some comic-relief character from a novel (probably an old-timer) who makes self-righteous I-told-you-so comments in the form of proverbs but will alternate between opposing

[12] We saw something like this with the example of baboon baby handling in Chapter 5. Strum's argument was that the mother (*A*) who wants to prevent the other female (*B*) from handling her infant turns her back to *B*. *B*, seeing a back, and being a bit agitated, grooms *A*'s back. This relaxes *A*. Seeing *A* relaxed, *B* now successfully grabs the baby. I doubt that baboons have propositional norms, but a naïve student might propose that the baboons have internalized the rule "you must first groom before you may handle." This might seem to explain the behavior but be quite incorrect; the chances are good that in a different environment (one in which, say, turning around was not a practical strategy of the mother), this "norm" would not be followed.

pairs such as "look before you leap" and "he who hesitates is lost," depending on the situation. Just as this character is unable to know which maxim should guide action in the current situation *before* the fact, so, too, we with our norms. And that is because it is not the "rule" in the abstract that would be costly to hold in our heads, but the detailed information on the *applicability* of the rule.

To return, then: the immediate problem with the rules approach is that we often violate the rules, and although a rules-theorist can always explain why in this case but not that we observed the rule, one feels less like an impressed observer of scientific progress and more like a teacher having to listen to one excuse after another from a student. We don't have a science, we have special pleading. It isn't that these claims are wrong, it's that because they are mobilized selectively to account for discrepancies, we are pretty sure that nearly anything can be explained—not in the good way, but because there's an instability or unidentifiability in our system. We have enough rules, meta-rules, counter-rules, precedents, and so on, that in any particular situation, we could account for an action or its opposite with equal confidence.

More important, thinking about the situations in which we *do* put our elbows on the table and so on may help us understand in what way the environment tells us what to do. We do not put our elbows on a *fancy* table—but a fancy table is not merely recognized as such by its physical construction and what sits on it, but by who sits around it and how they act at the table. Drawing from the principles reached in the previous chapter, we can accept that the way in which others influence our actions comes not (or not *always*) through a direct transmission of subjectivity (or one mediated by symbolic communication), nor by modeling others (I watch how they act to figure out how *I* should act)—though these *do* occur—but rather, by changing the perceived qualities of the environment.

Let's now consider the action for which the norm "The Gentleman Is to Let the Lady Enter the Door First" may be expected to be relevant. I find this a good example for the following reasons.[13] First, it is one that not only can be and has been expressed in propositional form, but it is one that many readers may well have been explicitly taught at some time. Second, it is one with differential implications: unlike "thou shalt not kill," the action imperative of the "same" norm is different depending on whether we are a Gentleman, a Lady, or neither. Third, it is one whose moral status has been contested and we can imagine subpopulations among which the norm has different meanings.

[13] Schutz and Luckmann (1973: 176) also considered a very similar norm; I find their treatment unsatisfactory, though only because of their starting point.

Ladies and Gentlemen . . .

Let's first begin in a simple world in which everyone is either a Gentleman or a Lady, and everyone accepts the validity of this norm. What does it mean to tell someone, "The Gentleman Is to Let the Lady Enter the Door First"? Most obviously, it is not that "The Gentleman Is to Let the Lady Enter the Door First" in any literal sense. For example, a Gentleman and a Lady walking into the large garage-door style opening that constitutes the door of a Costco warehouse would not slow down to make sure that the Lady preceded him across the threshold. That would be strange and rude. On the other hand, should a Gentleman and a Lady be walking down a corridor that begins to narrow to a width of 36 inches, though there is no door, the Gentleman may slow down so that the Lady goes first.

There are two likely ways of saving the norm account. The first is the many-norms response that I raised above and found wanting (that is, we say that in the Costco warehouse a *different* norm was governing action). The second is to say that the literally expressed norm is not the *actual* norm. This *actual* norm, say, pertains to deference, or to symbolic protection. But because "deference" is a complex issue and hard to explain in simple rule form, children are taught a number of less ambiguous norms that lead the socialized child to re-create the underlying principle of deference and be able to make practical adaptations when it comes to particular circumstances. Thus the norm in question is really a subnorm of the supernorm "The Gentleman must show deference to Ladies."

But this new version of the norm-theory produces its own difficulties. If the Lady clearly indicates that she is planning to hold the door for the Gentleman, the Gentleman may indeed go through and account for this as a successful application of the "deference" supernorm, and therefore explain the violation of the norm "The Gentleman Is to Let the Lady Enter the Door First." The vague nature of the supernorm means that it is possible to explain why an interaction that seems successful was in fact an application of a norm, and an interaction that seems unsuccessful was a violation. The appeal to vague supernorms is thus not very different from the solution invoking many particular norms. There can always be a post hoc explanation of any action. In other words, such supernorms don't actually save the norm-driven account at all because they don't provide the rules that could be used to guide action. Rather than being something that explains behavior, *this* now becomes what we need to explain: What counts as deference, to whom, in what situations? Once again, we are forced to accept the ethnomethodologists' point that such accounts are not causes of action, but are used by actors to justify or criticize actions.

Consider the following scenario: a Gentleman and a Lady in a hall come to a normal size exit door with a large metal bar that must be pushed to open the door; after the door, the hall continues (see Figure 6.1). There may be a number of norms that could be invoked in the quiet of retrospective debriefing: "The Gentleman Is to Let the Lady Enter the Door First," "The Gentleman Is to Do Strenuous Tasks for the Lady and Prevent Her from Dirtying Her Hands," "A Gentleman Should Not Turn His Back to a Lady," and "The Gentleman Should Not Force the Lady to Pass Closely to the Front of His Body." The problem is that if he does not open the door for the Lady, she will have to press the metal bar hard — perhaps it will require un-Ladylike exertion to open. But if he goes first (violating *one* rule) he has another problem. Yes, he has spared her the un-Ladylike exertion, but once he has opened and walked through the door, what does he do? It will only open 90°, meaning that he can neither get it completely out of her way nor stand behind it. If he holds it for her, he forces her to walk dangerously near his front, but turning his back on her while holding the door is also a problem. Finally, opening it and allowing it to slam closed toward the Lady might also be extremely rude.

Such practical issues sometimes do in fact arise, and sometimes lead to explicit and agonized deliberation of the form that I have outlined here. But the surprising thing is that similar situations are navigated all the time without such thought; they do not stick out, and no repair or explanation is necessary, and so no "norm" is invoked. The very smoothness of the action can make it hard to analyze; making things more complex can facilitate an understanding of the processes involved in regular (nonproblematic) action.

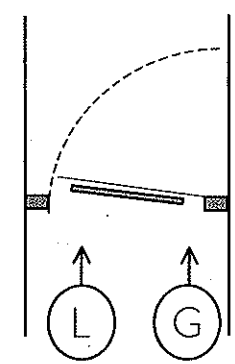

FIGURE 6.1: What to do?

Let's now introduce further heterogeneity in our population: there are two types of Ladies (or beings that a Gentleman is likely to believe to be a Lady — he may be wrong), one type possesses what we shall call "old-fashioned femininity" (OFF) and the other does not (~OFF). In other words, we introduce two types of "character." (Here one may see Gerth and Mills [1954: 13] for a discussion of the character of the OFF lady.) OFF Ladies agree as to the validity of the norm (which does not mean that this necessarily guides their behavior, it is just what they use to interpret behavior), whereas ~OFF Ladies do not, and in fact believe that a Gentleman should *not* spare Ladies from exertion nor engage in symbolic protection. Indeed, they believe that such acts communicate a *lack* of deference. Our one type of Gentleman, however, cannot tell the difference between OFF and ~OFF Ladies. What a potential recipe for disaster! There is clearly an insoluble dilemma: any action may not only fail to reach the normative goal, but also may send the wrong message and insult the Lady.

With that sort of mock objectivism so well fitted to mask anticipated *schadenfreude*, we situate ourselves near the door, ready to enjoy (I mean, observe) the pragmatic failures that are sure to result. Yet pair after pair of Gentleman–Lady duos proceed through without seeming to even notice the door. What has happened?

The situation as I described it above was reproduced in Figure 6.1. The door with the bar is ahead of the Lady (L) and the Gentleman (G). The arrows indicate the direction and speed that the Gentleman and the Lady are traveling. Now consider the sketches in Figures 6.2 and 6.3, respectively. Two things are different: the position of the Lady is altered somewhat, and we have added the "envelope" of personal space that surrounds her. Let's also postulate that all our subjects have a relatively clear agreement as to how much space a Lady must be afforded before one has changed the situation in some way that requires justification. Further, they have a phenomenological *sense* of this, as opposed to a *theory* of it. Finally, personal space toward the front increases with our velocity, and we notice that in Figure 6.2, the Lady is a tiny bit faster than the Gentleman, and her personal envelope is a bit larger to the front than it is in Figure 6.3, where she has slowed down a bit. We can see, merely for reasons of fluid dynamics, that the Gentleman in Figure 6.2 seems very unlikely to be squeezed to the front position, while the one in Figure 6.3 is very likely to open the door.

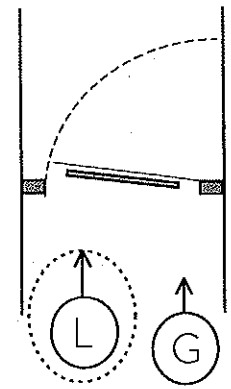

FIGURE 6.2: An ~OFF lady

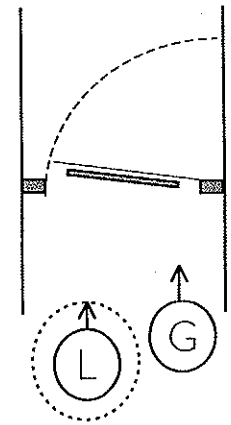

FIGURE 6.3: An OFF lady

The norm-driven response might be that the man has taken his lead from the woman, and that the woman has decided whether to treat the norm "The Gentleman Is to Let the Lady Enter the Door First" as a valid one for her or to respect it in the breach. And of course, there will never be a situation that cannot, with suitable creativity, be described ex post facto as in conformity with norms, since there are so very many of them. The question for us now is whether the field account has a different and a plausible explanation. It does.

Affordances and Action

As we saw in the previous chapter, objects have affordances for us, affordances that are available on inspection; in Gibsonian terms, these are qualities of the objects that are available in the ambient optic array (that is, it's there in all that light bouncing around that you are free to direct your eyes to). Further, these qualities that the objects perceivably have tell us what to do about them. Field theories, as I have emphasized, note that these qualities, though retaining intersubjective validity, vary in predictable ways for different persons. The force field of a heavy iron bar is different for an ~OFF Lady than for an OFF Lady — the first it perhaps attracts, the second it repels (see Figures 6.4 and 6.5, respectively). It is this that explains the different positions of the Lady in Figure 6.2 and in Figure 6.3.

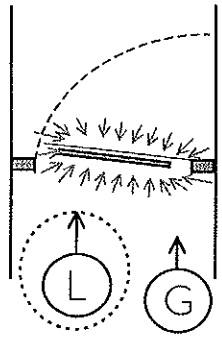

FIGURE 6.4: An ~OFF attracted

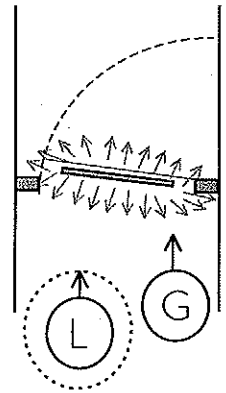

FIGURE 6.5: An OFF repelled

It is important not to impatiently dismiss what may seem to be an over-elaborated and over-schematized adumbration of a very uninteresting and obvious set of actions. "Of course," we might snap, "the OFF Lady doesn't want to touch the door, or wants to communicate that she doesn't want to, and so by slowing down, she signals to the Gentleman to go first." But that was never in doubt. The question was *how* she did this, and I am claiming that she does this *not* by invoking any of the norms but by responding to the qualities of physical objects in her physical environment. Even though we explain her response by the qualities, and the qualities are all qualities *of* physical objects, she responds differently than does an ~OFF because the qualities of the situation are conditional on character. Since the qualities of objects tell us about our potentials for doing things with them, their attributes such as "coarseness" or "heaviness" do not have merely to do with them, but with our own constitutions.[14] The same pole that seems to say "throw me" to one person, announces just as definitely "I am too big for you to wield" to another.[15]

Thus we explain action by the qualities of the objects in the environment; as these qualities are reciprocal to the actions of particular types of actors, they produce a regular, structured, but also differentiated world. This does not mean, however, that there are never times when things do not run smoothly when there

[14] And that's because it's about what *we* are going to do with the objects; people judge objects to be "heavier" when their distribution of mass is such that they are difficult to wield (Turvey et al. 1999).

[15] I used to be incredulous that Kurt Lewin (1951) thought his formula $B = f(P,E)$ — that behavior is a function of the personality *and* the environment — was even worth writing down, let alone that scores of his students saw it as a profound statement. Now I get it. It's the core of field theory — about the interaction of valenced objects in the environment and sensitized (hysteresized) actors.

are dilemmas that arise that are consciously grappled with.[16] For example, if our OFF Lady speeds up as she goes through the door, the Gentleman may not hold it, but fling it open, as her force field pushes him on; but if she is on the more sensitive side, and our Gentleman large, she may treat him as having a very large envelope, and thus slow down or remain slow, leading him to slow down, which will mean he is still in the door, possibly — given his large envelope — forcing her too close to the wall.

In such cases of failure, reasoning from norms may well be all that our hapless actors can rely on (and recall that *knowing* the norms doesn't automatically help our actor know how to *apply* them, because of the overlapping relevance of different norms with contradictory action implications). But this does not mean that these processes are those that will explain the nonproblematic situations. If Dewey was correct that we are jolted out of habit into intelligence (or what contemporary psychologists might speak of as a change from basal parallel assimilative processes to prefrontal serial reasoning processes), then simple reflection is not enough to determine which processes are used in nonproblematic circumstances. It is, as said above, akin to opening the door to see whether the refrigerator light is on, even when the door is closed. To reflect on action is to turn on that part of the mind that reflects, and we know from much neurological research (Gazzaniga 1970; Wegner 2002) that this portion boastfully if sincerely claims control of things it has not really done.[17]

Further, what the field-theoretical approach can explain is not just how there is regularity, but how we perceive our actions as regular and norm-conforming despite the variety of situations in which we find ourselves. Different halls, different arrangements of doors, different types of door pulls and handles, different placements of them call for improvisations that are cognitively demanding for a norm follower but *may* be traversed easily by attending to what the environment affords. It is only when there is turbulence — no easy way of traversing

[16] Further, although it goes beyond the purely theoretical goals of the current work, contemporary neurology does seem to support the idea that objects directly tell us what to do with them, and when we do not actually obey, this is due to a secondary suppressive reaction that can, among healthy individuals, be inhibited under load, and can be completely missing in persons with certain forms of neurological damage (e.g., they will reach out and pick up anything that goes by [McBride et al., forthcoming]; Lhermitte [1986] calls this "Environmental Dependency Syndrome"). Although this will be a tough sell, I think our best model of the actor is Reed's (1996) of roughly hierarchically organized coupled oscillators with higher orders suppressing lower ones, and an extremely perceptive but not omniscient consciousness that selects aspects of the states of these, should they become available, for narrative purposes.

[17] It might seem that reflection could *never* produce or confirm the account given here. But this is not so. A good first step, one I presume Dewey used in his initial researches, is simply to have one's conscious mind first remember, then keep track of, the sorts of situations when it is present, or when it "comes to life."

lines of force — that we become aware of the "problem" that seemed to call for deduction.[18]

To sum up, it seems that the field approach's claim that what we are to do is stored not in mental norms but in the environment, is a plausible and consistent account. It *does* have a partiality; it is good at explaining how the environment tells us what to do, and not so good at telling us when (as discussed in note 16) we decline to obey. One of the puzzles is how to differentiate those aspects of character that *facilitate* our response to the environment (generally understood as arising from repeated experience *with* the environment — that is, habit) from those that *dampen* our responsiveness ("character" as understood in the old sense). *Logically*, we introduce all sorts of problems if we allow for an explanatory factor that is "resistance-of-the-agent-to-doing-what-my-theory-suggests." *Psychologically*, however, there is no doubt that such resistance to environmental affordances is a measurable factor that exists as both "state" (it changes for us as individuals over time) and "trait" (it differentiates one individual from another). *Sociologically*, we had better *know* something about such character before we shoot off our mouths about it, and it seems like the field approach is in as good a position as anything else to get us started here.

The reader may likely object that this example is trivial, and indeed it does seem trivial, but it is not clear why it seems so, and whether its seeming triviality is at all relevant for analytic purposes. First, we must acknowledge that the example does involve normative considerations, and these are considerations that have been taken quite seriously in the past century. Does the presumed triviality arise because we are speaking about actions that are merely persons moving their bodies through space? Perhaps. But how easy do we think it will be to claim that there are actions that are *not* about organisms moving their bodies in physical space? (I'll give this critique a stronger form below.)

The best objection would be that this account is not actually more parsimonious than that of the norm-driven account. Recall our finding that for GOFAT to account for actual behavior, we needed to bring into play more and more

[18] For a nice example, the Booth Graduate School of Business at the University of Chicago put in large double-glass doors that have hidden pivots, and steel rods as handles. On the *inside*, the handle is (as is conventional) horizontal, but on the *outside*, the handle runs vertically down the entire jamb side of the door. Since the horizontal handle is visible through the transparent door, the vertical handle is often mistaken as the pivot, and so many people first try to open the doors by pushing on the side opposite the vertical bar, even though this is the fixed point. A Gentleman who positions himself here to open a door for an OFF Lady finds himself in a difficult position. She will be near the door, and he must reach across her body, effectively blocking her path and perhaps violating her envelope, in order to get a portion of the handle with which he can exert sufficient torque to open the door. But if the Lady does not react to this as a violation of her envelope, the whole interaction will be nonproblematic. I suspect that the behavior at this door is more predictive of career success than anything that happens once they are inside.

norms other than "The Gentleman Is to Let the Lady Enter the Door First," some having to do with deference, some with exertion, and so on. But the field approach also begins to bring in more things, not the simple spatial constriction of the doorway, but the envelope of each person, the affordance of the door, and, were we to pursue other concrete cases, other qualities of the environment (perhaps placement of fire extinguishers on walls, whether the door leads to stairs and, if so, whether they go up or down, the color of the door, and so on).

It seems to me that the difference is that the things appealed to in the field account, unlike those appealed to by the norm account, are *already there*. Further, the properties of the things are there and the relation of these properties to action imperatives is usually clear. For example, let's say that an ~OFF Lady and a Gentleman approach a door in the same size hall, and yet, unlike the scenario graphed in Figure 6.4, in this case, the Gentleman holds the door for the Lady. If an analyst accounts for the discrepancy by pointing to the presence of a fire extinguisher that projected 10 inches from the wall, 3 feet from the door (see Figure 6.6), the explanation does not bring into the situation something that was not already obviously there. That cannot be said for something like the norm "the Gentleman should symbolically protect the Lady." There is no independent evidence that this norm *was* there before the analyst (whether or not this person is the same as the actor) has brought it into existence.

The claim that physical features of our spatial environment contain the information on how we are to act, including what it means to do the right thing,

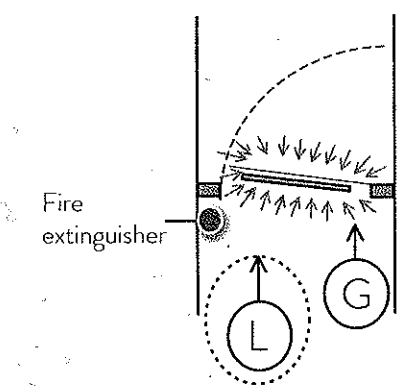

FIGURE 6.6: A new factor

might seem a stretch — but that is only because our conventional sociological theory has allowed us to smuggle in a form of scholastic idealism that none of us would defend. Bear in mind that you will have a great deal of hard work to do to establish that there is anything at all other than objects in space (including grimacing and talking objects like ourselves, and artifacts that we have made and put in places to shape our behavior). The problem with the field-theoretic account, then, is not that we think it is implausible. In a way, we all necessarily accept it. The question is whether it is *tractable*, and it seems to me quite likely that it is for a wide class of interesting problems (though see Latour et al. 2012: 7 for a serious critique). Movement of bodies in space is one, and a fine place to start, finally, trying to produce a serious science of social action.

Extensions

I noted above that this example might seem to be of a particular nature because it involves people moving their bodies in physical space. I gave a cute response to that, pointing out that unless you are a spiritualist, you accept that *all* social action is bodies moving in space. Still, one could respond that although all action may involve bodies moving in space, often the *meaning* of the action isn't as clearly tied to where the bodies are, but rather to what they accomplish. It's one thing to say that the environment holds information about what we are to do when it comes to navigating the environment in direct ways, but what about when we see our action in terms of meaningful accomplishments, and not position? Maybe then we'd find that, in fact, everything *is* about the observance or nonobservance of social rules?

I think we find that the principles uncovered here remain important. In many cases, the environment tells us what to do, but we have to remember that we (that is, we humans) construct the environment for that very reason. That might seem to make things more complex, but it allows us to simplify our theory of action for the same reason that it simplifies tasks for the actor. I don't know which fork you use for what, and I can't tell a salad fork from a dessert fork, but I do know that one is supposed to start with the implements farthest from the plate and work inward. The environment is set up so that I can follow the arbitrary norms without actually knowing them. And the person who sets the table need not know about the use–norm either, actually.

One may object that this example is just as trivial as the other. A true understanding of behavior needs to deal not simply with etiquette, or not with it at all, but with the more fundamental things like morality — not foofie forks but things like murder.

But as we recall from Chapter 5, we generally don't murder. And most of these "more serious" moral rules turn out to be (as Durkheim emphasized) about

what *not* to do. Of course, the environment doesn't have information on all the things we *don't* do. All that stuff is imaginary. If you *do* kill someone, however, it might well be that you follow some rules, and then the theoretical claims above should be relevant. But *not* doing things isn't automatically following rules — every day, I don't kill people, but I think it would be silly to say that I am following the don't-kill-people rule, because I don't *want* to kill any people. (Then again, I haven't read the reviews of this book yet. Just kidding reviewers! No worries!)

Put in other words, if morality is "negatively privative" in that goodness is the absence of evil, that would fit with what we've seen here regarding rules. The fact that conduct is moral doesn't mean morality is constraining conduct, and the fact that we act within rules doesn't mean we're following rules. Rather, when people believe — or can make a credible show to others that they believe — that another has acted in a way in which she or he can be taken to account, rules may well be invoked. The application of these cognitive constructs for meaningful disruptions of patterns doesn't need to explain the pattern following itself (see especially Vollmer 2013). That, rather than being privatively defined, is positively defined — what we *do* do — and it seems parsimonious, plausible, and consistent with other findings that this is largely guided by the physical aspects of our environment and what it affords.

CONCLUSION

The good-old-fashioned-action-theory feels right to most of us. But that doesn't mean it's correct. Its two main planks — the importance of conscious or paraconscious motivation before action, and the importance of rules in leading to the orderliness of sociological action — both have serious difficulties. Many of these difficulties are empirical, which is outside of the scope of the current work. But some of the support for GOFAT seems to come from making the sorts of errors that orthological investigations can uncover — in particular, the idea that rules constrain or guide action.

All this is old, indeed, very old, hat. But despite such critiques, as a discipline, we haven't moved very far from GOFAT, and not only because GOFAT seems intuitively correct. We also haven't had a compelling alternative spelled out for sociology. Although the basis of a plausible theory was offered by John Dewey, and later by Pierre Bourdieu, both were marginalized not only because they were vulnerable to a crushing critique ("You are saying we act *only* on the basis of habit?"), but because they had difficulty with the combination of, on the one hand, the culturally constituted and individually variable nature of the character of the actor, and on the other, the physical fundament for the qualities of

the objects that called out for action. They had difficulty, but they got it right. However, it was easy for readers to lose the thread of the argument, and think that things weren't solved that were solved. All theories have problems; good ones solve their problems. It seems to me that the approach discussed here comes from a tradition that is solving its problems. That's a good place to work from.

CONCLUSION

ON LUCK

I like watching automobile racing, including Indy formula racing, although I know that most people in my world don't share this appreciation. They think it is very boring to watch the nearly identical cars go around and around and around. But I find something aesthetically pleasing about watching a first-class machine at work. I have a somewhat similar feeling about most of what we consider sociological theory. Even if theorists are just going around and around in circles, as I think they do and must, we often see first-class minds in operation. Circles may not, as the scholastics thought, represent perfection, but you could do worse.

My colleague Andrew Abbott once said that the history of theoretical innovation in sociology seemed, if not such a circle, then a wander over the surface of a globe, in which progress was just returning to a place where no one had been in a very long time. This is in some sense undeniable and might be taken as cause for despair or for cynicism, but now I think it shouldn't be. The circles we go in are where we want to be. They arise from the combination of our preexisting conceptual momentum and the pull of our dedication to understanding the world (call it the real world, if you want). That's what we're circling *around*. In this book, I've emphasized "thinking straight." But in a warped space, that doesn't rule out curves! If we were going in a straight line, we'd be shooting out into the space of silliness.

Pursuing the metaphor, we might say that the quality of our thoughts here is inverse to the radius of our orbit. There can be a better or a worse, even if it's hard for us to compare the better-worser-ness of very different theories. (Think in terms of polar coordinates — different theories may have very different angles, but that doesn't mean we can't compare their radii. Sometimes we need to change our angle to allow us to get around a blockage that prevents us from changing our radius.) It might be that we can never get all the way there, and certainly, as our radius decreases, it seems we might speed up to a dizzying extent. So far, it does

seem that no one's been able to get closer than a certain point without a crash landing into mysticism.[1]

So perhaps there's a chance of theoretical progress. Even if not, it does seem that in social science, we're often going to need to say things that require a certain amount of conceptual scaffolding. That is, we'll need some theory, even if our theory can't get any better. As a result, there isn't anything inherently wrong with system building, even castle-in-the-sky-building. Consider this the construction of the scaffold in a zero-gravity environment — it makes things easier, but might lead us to want to check our resulting structure very carefully before using it to bear weight. Still, in most cases, sociological theory is, as we might say, "mostly harmless." Even when it is very wrong, nothing terrible happens, because usually no one bothers to consult it before acting, including practicing social scientists. Even those who think they are strongly attached to one theoretical tradition or another, and that their work is focused on a contribution to this tradition, tend to write books whose first chapter you could pull out, switch it with the first chapter in another book at random, and probably no one would notice. I am actually inclined to think that this is a good thing, although for a sociological theorist it's professionally somewhat discouraging.

But sometimes, the way we think about things (and we must think about them in *some* way) makes it hard for us to understand what is going on about us. In such cases, system building doesn't help, and in fact makes things worse. We can distract ourselves from our real problems by repeating to ourselves our theories — that is, our mishmash of unfounded substantive assertions and our favorite words — as if this little prayer could keep scientific failure at bay. To pursue the structural analogy, we make it harder for the fundamental problem to be identified, as we nail up all sorts of temporary supports and wrap everything with duct tape. For such situations, orthological researches (as we might laughingly call them) are potentially important. Unfortunately, our biggest problem is usually simply coming to recognize that we are in a situation calling for such orthological investigation. Most of us seem to feel that our own ideas are just "obviously true, you idiot," or "already said by someone very famous, fool" or "the kind of thing only a bad person would deny, creep."

Often the only thing that can trigger our search for a serious consideration of our own formal theoretical structure is a kind of vigilance that is the last thing our discipline encourages, and this is a vigilance based on a suspicion of things that look too good. Most of us now understand that if a Nigerian prince needs *your* help to smuggle $12 million out of Russia and is willing to split it with you,

[1] The problem with this version of the metaphor is that, really, I think our progress is best understood as an *away from* error, and not a progress toward truth, which would imply that the quality of our ideas is proportional (and not inversely proportional) to the radius. But combining the two metaphors becomes ungainly.

this is too good be true. But we don't look a gift horse in the mouth in our own work, neither methodologically nor theoretically. Regarding the former, if we do what seems like a very conservative longitudinal analysis and yet we get three asterisks by our favorite coefficients, we go right to the press. We don't think, "Hmmm, have I rested too much on a linearity constraint for a temporal change, such that I'm reparameterizing time as if it were a causal effect?" He who hesitates is lost. Or so we think (which is why we're *all* lost — as Thurber said, "He who hesitates is sometimes saved.").

Similarly, we don't think, "Huh! My theory explains everything I was interested in, in just the way I thought it would, using my favorite terms, *and* it shows that I'm morally superior to most other social scientists! Too good to be true." But it is. Only one person was that lucky, and he's buried in the Highgate Cemetery — and even he was often a pain in the ass to his friends.

So, strangely enough, when things are good is precisely when we should be worried, and when we should see whether we are making an argument that sounds important, but isn't what it appears to be. In such cases, we may have slipped in the sorts of problems that are outlined here.

Such orthological considerations are the first word, not the last. We should all become so good at the rules of straight thinking that we know when and how to break them. Because, for example, at a certain point, the law of self-identity breaks down. Things *aren't* themselves. An acorn isn't an acorn, it is an oak tree, . . . and it isn't. There are mysteries pertaining to duration, to the limited transitivity of identity, and even (but don't tell your professors you know this!) problems of reification. But that's not the place to start. We start as simply as we can, and try to walk as straight as we can. Don't worry — the crazy landscape we walk on will introduce plenty of twists and turns. As we go along, there are a few principles that can be used to guide us in the right direction, think-wise. I close by providing what I believe to be some of the robust rules for sociology, mostly drawing on work by generations of others, and emphasizing their relevance for theory construction.

BONUS: THE NEWEST RULES OF SOCIOLOGY

The idea of the basic rules of sociology was started by Vernon K. Dibble, whose first rule, justly lost to history, was "the sociologist knows." His second rule is now known as the first rule of sociology, and the one I give as second is also from the Columbia school at around this time.

1. Some Do, Some Don't

This is so deeply important that every sociologist should have it tattooed on her or his forehead. We generalize, and that is fine. We talk about averages, and that is fine. But in part because of our disciplinary vision of social life as the average plus "error," we tend to be seriously hampered in thinking about variation. Admitting it often seems like confessing ignorance. Hence, when we are hostile to others, and they use a comparison, we may play a card that seems a version of this rule, misunderstood as some sort of triumphant negation: "You compared German X to French X — thereby assuming homogenization within the groups." Really? Since when does a comparison of means imply absence of variance? Unless you think Germany and France are random draws from the same bag of marbles, there's nothing wrong with a comparison.

Isn't it silly that we, who have a science *based* on the assumption that we should study groups, aggregates, societies, or whatever, because these things are fundamental, and that individuals have no epistemic priority, and so on and so forth, turn around and slash at each other for pooling individuals? I think this is because we really haven't come to grips with the truth that even when our comparisons are okay, and our normal methods are doing just fine, some do and some don't. It isn't bad sociology, it's not error of *any* type — it's life.

And the real problem with our ignoring this principle doesn't arise in comparisons like this illustration, it happens when we *aren't* comparing — when we are giving our "story" as people increasingly say these days, that is, our interpretation and conclusions. Then there isn't any comparison at all to alert us to the problem — there's just a fantasy of an abstract "what people do." At the time of writing this, I actually just finished an unintentionally humorous piece in a top journal on racial prejudice in hiring decisions that concluded "employers discriminate against . . ." and so on. (The evidence indicated that *some* employers discriminated, . . .) My point isn't that saying that employers discriminate is morally just as wicked as saying that nonwhite employees steal. It's that where it really counts — in our conclusions, what we take away — we are often unable to bear in mind not only that some do and some don't, but that this can matter.[2]

And most important, if you find yourself making a theory that starts out "people do X . . . ," watch out. Something's probably wrong. Even if you are sure that a society where people don't do X couldn't last five minutes, or that our ancestors *must* have been selected for X, it's just not that way. We've seen the errors

[2] Because this is outside the theoretical issues in this book, I won't go on about it, but the obsession with the "average treatment effect" in causal and policy studies is just as bad. The average is just a meaningless compositional artifact. Any serious investigation should be focusing on causal heterogeneity, and we now finally have statistical tools for this.

people made in animal studies by assuming homogeneity, and it's not going to get better with people.

2. It's Different in the South

This was understood as the third rule in 1960s Columbia, and it is indeed a good one, at least for any country that has a South (and that's lots of 'em). That's the cool thing about having a rotating sphere to live on: south versus north is different from east versus west. Going farther west can change things, but it doesn't have to. This seemingly wholly substantive lesson speaks to a more general point for sociological theory that we only admit when we can make it sound fancy-schmancy ("I draw attention to the emplacedness of human action . . .") but ignore otherwise. It isn't just that where we are matters, it matters in a material way. Because early sociologies (or the sorts of things that would turn into sociology) often were (by today's view) embarrassingly direct, blunt, and simplistic in how they linked culture to climate, it's been considered insane to say that the physical qualities of where people are matter in any way you could figure out. That's overly pessimistic. There are plausible things to say, and they matter. You can't go to the library where there isn't one. Rain and cold keep people inside. If the only road where you live goes east, you go east to get stuff. It matters where the people are and the stuff is. And as we saw in Chapter 5, culture is about how we transform the environment that shapes our action. And this gets to a third principle.

3. Composition Is Nine-Tenths of the Law

We often read about how America, say, is changing, again, as if there were a single person (or maybe two, Mr. and Mrs. America) who once did things one way, but now do them differently. But America — and not just America — is a big bunch of different types of people, and the relative proportion of these people has been changing. Guess what? Sociology actually works. We can find out a lot about social action, and even our usual suspects for explanations—the "master statuses" like age, sex, race, occupation, education, and income—do a lot here.

Most of what we think of as cultural change is really change in the composition of populations. Industrial revolution means fewer farmers. Deindustrialization means fewer union members. Aging of the population means (you guessed it) more old folks. Before we theorize about *aggregate* changes, we need to think through what would be a null model of no change in the *conditional* action of persons (conditional on the sorts of people they are).

Sociological theorists do need to be comfortable working with abstractions. But too often they seem to be comfortable *only* with abstractions — they run from any encounter of the concrete with terror. But theorizing about an abstrac-

tion like "the changing American character" isn't sociological theory. It isn't social theory. It isn't anything but the roar of the wind.

4. Ask a Sociologist a Question, You'll Get a Sociological Answer

This is true even if there is no reason to think a priori that the question should have one. Not all things that seem to be questions have answers, not all questions that could in principle be answered can be answered in practice, and not all of those questions that can be answered have sociological answers. For example, if you are wondering what caused ethnic cleansing in the 1990s, if you ask a sociologist you will probably get an answer that involves sociological phenomena. It is quite possible that the answer to this question is a political one that political scientists will get to but not sociologists, and so on. If you're a sociologist, you probably have an occupational blindness here. If you assume that the answer is always under your realm of professional competence, you're more likely to need orthological fixes in the future.

5. Everybody Always Thinks Something

Sociologists are good at understanding that just because scientists say something, doesn't mean it's true. "We now know . . ." should set your b.s. detector on full power, especially if someone is trying to sell you (or your insurance plan) something. The fact that there is uniform agreement *right now* shouldn't sway you. There was a day when everyone knew that bloodletting was pretty much all you could do for a case of smallpox. And so it goes.

And yet, sociologists allow the fact of universal agreement (that is, among everyone we know, *now*) to dismiss sociological *theories*. Something that is no longer believed to be a good idea "by everybody" is assumed to be validly ignorable. But if indeed pretty much anything in sociology has been thought up at least *once*, everything's already been dismissed, as will whatever we all think now. That can't be a good way of proceeding—to dismiss something because we all know that it is worthy of being dismissed. And related to this is the one principle perhaps most important for theorizing, explained next.

6. The New Law of Large Numbers

You have heard it said that the law of large numbers pertains to issues of inference under random sampling. All that is well and good, but there is a different law of large numbers, which, if understood, would completely reorient our current theoretical practice. It is simply this: the world is a big place. Many things

come in "lots of 'em." For such things, chances are that whatever *kind* you think of, you can find one, if you look.

That means to have a general theory and to *illustrate* it with a case is a waste of everyone's time. You wouldn't propose that "in general," people are named "John" because you can find one, two, three, or however many of them, would you? Yet people make all sorts of claims based on the one thing that they have found—often because they were looking for just something like this.

Take for granted that you can find one of anything: unions that run bottom up, corporations that produce religious conversion, four-leaf clovers, even mermaids. Unless there was a strong theory that denies that such a thing should *ever* be found, it's just not important. Don't cross the street to look at them, let alone write a dissertation on them.[3]

7. Think about How You Know What You Think You Know

Every now and then, there are reasonable questions raised about the level of political debate in the United States. We wonder why, given that there are serious problems our country might want to deal with, we spend so much time yelling about whether, say, some senator likes to watch the giraffes in the zoo humping each other. It's easy to say that Americans are prurient, or idiots, or whatever, but more sociological analyses will remind us that we can only argue about what we know about. If the newspapers print scandal stories, that's what we'll know about. If they print negative stories—and, as Schudson (2011) shows, they disproportionately do—we'll tend to think that most politicians are creeps and liars. But we can adjust our ideas if we take into account the *processes* that bring this news to our attention.

So, too, when it comes to theorizing, a lot of the story is over before you even start thinking, because it all depends on what you have in your head to think with. You often need to take a step back and try to see if you can figure out what got into your head, and what didn't, and where it comes from. You can't do much of this, but you can do some, and most of us don't do any at all. Absolutely, we think "from a place," but the key thing here usually isn't about who we are ("I'm a guy") or what we value ("I hate fascism")—and those things are easy to take into account. More difficult is to understand how certain ideas and information reach us; even when we are active, going out and about to gather perceptions, we don't always understand how the paths we take affect what ends up in our heads. We have to—as well as we can—get a sense of the stuff that *didn't* reach us. We even need to go out of our way to get some of it.

[3] That goes for mermaids; remember, they're *really* bad luck.

8. *You Can Be Too Smart*

All men are mortal. Socrates is a man. Therefore, Socrates is mortal. QED. The logic is perfect. Of course, really, we're not 100.000 percent sure that all men are mortal. Perhaps someone born among us right now will never die. And who knows — maybe Socrates is a very unusual type of squirrel, or of angel, or he is an alien. Still, if we are even just 99 percent sure of the two first statements, we can still conclude that Socrates is a mortal with 98 percent (= 99% × 99%) certainty.

But suppose that instead of a classic syllogism, we had sixty-eight different links in a chain of reasoning. Now, even with 99 percent certainty in each link, our conclusion is basically a toss-up. What's the point? That you can be too smart.

This actually was a refrain my grandfather said, or that I thought he said, but with his thick Russian-Yiddish accent, I often had a hard time understanding him. My father thought it was "you can't be too smart," but as he pointed out, the two things really mean the same thing: there is a zone in which increasing smartness can make things worse, not better.

Why? To answer, let me give you a sad, bitterly funny scene that has unfolded time and time again in different ways. I'll give the classic form I grew up with. In a room, an ideologue who has no real idea what he is talking about is mouthing slogans. Someone else who knows much more and, for whatever reason, has a significantly greater share of smarts than the ideologue, is nodding appreciatively, and allowing himself to be corrected. Our anti-hero is quite willing to discard things he has learned, things that are perhaps true or reasonable or both, to gain the approval of the narrow-minded, perhaps insane, ideologue. What's up with this?

Our anti-hero was too smart for his own good. He could reproduce a long chain of reasoning, each link with some plausibility, that connected his fundamental goal such as the liberation of all humanity on one end, to his kissing butt to some numbskull on the other. Unfortunately, this is a very general phenomenon. Many con men will look for those who are "smart" in the sense of having a verbal and complex intelligence, and avoid those who just seem to have common sense. Because the "smart" person can turn into a co-conspirator, he'll be better than any con man at coming up with plausible explanations for why the deal is suddenly changing, and is better at reducing dissonance than someone who may not be able to talk uninterrupted for an hour but knows when he smells a rat.

Okay, so what's the point for sociological theory? It's a vital one — being too smart is working through a chain of reasoning that, if you properly took into account the uncertainty of each link, you'd realize was bringing you to a conclusion that seems air-tight, but is better seen as improbable. We need to understand the difference between *chains* and *ropes* of reasoning. Chains are "serial," one after the other. A chain is only as strong as its weakest link. Ropes are

"parallel," in that each thread is side by side with the others. If one breaks, the whole doesn't. In one kind of logic, adding more things makes your argument stronger, but in another, it actually makes it weaker. We need to know when we are piling up ideas in parallel, as opposed to in serial arrangement. The funny thing is that, as I think Thomas Reid (1969 [1785]: 730) suggested, it's common-sense stuff, the sort of thing you don't need a prodigious memory to comprehend, that tends to work in parallel. It's the complex chains that only a smart person can keep in mind that are serial. If there's a ratio in which you divide smarts by common sense, you want to keep that low. Many of us theorists don't have a lot of common sense. If we did, we would keep our reasoning in plausible bounds, where we might have a chance of sticking with things that we can understand.

9. *Everything Is Somewhere*

And this gets to one of the running themes of this work: we need to be attentive to where things are. We found a number of times that initially very impressive claims about the way things *really* happen, the sorts of things we're likely to march around trumpeting when we are in a very scientific mood, things like "causality" and "facts," turned out to be far more intrinsically *mental* than they first appeared. That is, they weren't in the things *outside* our minds, they were *in* our minds. There's nothing intrinsically wrong with using your mind — it's pretty hard to be smart otherwise — but it is important to know what we *should* be committed to as sitting happily in the world *outside* our minds (the transphenomenal), and what we should accept as a human way-of-working.

This is where the principle of everything-is-somewhere comes in handy. A zebra is not a giraffe, and no zebras ride the Staten Island Ferry. Now a zebra, say, Zelda the zebra, can be somewhere outside of your mind, and it is fine to say that the zebra, outside of your mind, isn't a giraffe (even outside your mind, that is, she isn't a giraffe — it isn't your mind that turns Zelda into a zebra). But the not-giraffeness of Zelda, if you want to treat it as an element, has to be somewhere. Where could it be? It *can't* be in the trans-phenomenal world, which only includes things that *are*, so chances are, it's in your mind. Where is the Staten Island Ferry? Probably somewhere in the Upper New York Harbor between Staten Island and lower Manhattan. Where is the absence of zebras on the ferry? It isn't there. It's only in your mind, and in mine.

Why should one be so punctilious about this matter? Because pathological theorizing always involves claiming that absences are in the trans-phenomenal world. We've seen in Chapter 2 how such claims throw our attempt to think through causality into a mess, but it gets worse. What would you think are the likely signs of being obsessed with, say, cheese? You might talk about cheese a lot

eat it a lot, or just put up pictures of it. But if absences can be real, you might propose that someone who is truly obsessed with cheese might *never* mention it—and refuse to eat it. Of course, this sounds reasonable, in a way, because it *could* be true. But there isn't any good reason to think it *is* true, or *always* true. And (again), it gets worse. If an absence of cheese-talk is a real thing in the world (cheese-avoidance) that (somewhat paradoxically) indicates a cheese-obsession, guess what else is real? The absence of cheese-avoidance, the double-whammy technique of the *truly* cheese-obsessed. Sometimes the truly cheese-obsessed will mention cheese, thereby *negating* their cheese-avoidance, which they employ to *negate* their cheese-obsession.

Do you think this is very silly? If so, don't tell your psychoanalyst, because this was exactly how Sigmund Freud was able to go from his botched experiences with a few patients that he mis-treated (and mistreated) to an elaborate set of doctrines that still compel the unreasonable adherence of many today. (And if you think I'm exaggerating, see his analysis of Dora, which precisely argued that the disorderly absence of an orderly absence indicated the truth of his own twisted projections.)

But it's not just that. Keep going. Why don't the working classes accept whatever version of dumbed-down Marxism you are trying to peddle to them? Well, again, make the *absence* of their doing what *you* think they should a real thing in the world, and start populating the real world with *equally* imaginary entities to explain this. We saw in Chapter 2 that this actually starts to feel *more* successful and *more* scientific than when we don't include all the imaginary thingamabobbies. But when you take absences out of mind, where they exist, and put them in the world, you open a hatch through which all sorts of your own demons will flood in, and contaminate your trans-phenomenal with the phenomenal to the point that there's no longer any reason to do science. You might as well just write fiction and call it nonfiction.

FROM HERE ON

These are the best of times and the worst of times. Methodologically, our field has grown far more sophisticated, and we have even learned some core principles that allow us to productively understand the processes whereby we produce data as social situations of specific kinds that have their own dynamics. That's pretty advanced for a social science. But we're not always doing a very good job with the tools we have, in part because they're difficult to use properly, but also because they need to be guided by a theoretical vision that has certain architectonic properties. Otherwise, we find things, but actually do not know what we find. This book, I hope, helps us produce such theoretical structures.

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