

## REDESCRIPTION AND DESCRIPTIVISM IN THE SOCIAL SCIENCES

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**ABSTRACT:** In its quest to become more scientific, many have held that social science should more closely emulate the methodology of natural science. This has proven difficult and has led some to assert the impossibility of a science of human behavior. I maintain, however, that many critics of empirical social science have misunderstood the foundation for the success of the natural sciences, which is not that they have discovered the “true vocabulary of nature,” but—on the contrary—that they have realized the benefits of flexibility in “re-describing” familiar phenomena in alternative ways, in the pursuit of scientific explanations. In this paper I argue that this same path is open to the social sciences and that its pursuit would facilitate the prospects for the scientific study of human behavior.

*Key words:* re-description, descriptivism, social science, explanation, methodology

### Introduction

Ever since the Enlightenment, the ambition of social scientists has been to emulate the success of natural science. During the Scientific Revolution, the natural sciences emerged as empirical disciplines driven by the desire to conduct experiments, test hypotheses, and ruthlessly root out theories that did not fit with reality. The natural sciences, in short, became self-conscious about their methodology. This naturally led to the question of whether the social sciences might duplicate the success of the natural sciences by emulating their methodology. Does there exist a potentially unified method of inquiry across the natural and social sciences? Despite centuries of labor to achieve such unification, the results have been disappointing and are widely believed to have foundered with the eclipse of logical positivism.

Before we can legitimately assess the prospects for achieving a unified methodology across the natural and social sciences, it seems important first to examine the methodology of natural science. How does science work? How do scientists formulate new theories? How do they discover new laws and systematize them within general explanatory accounts? Such questions, of course, are part and parcel of the philosophy of science and have generated book-long answers to the question of what is distinctive about scientific inquiry.

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In this article I do not seek to defend a full-blown account of how science works, nor do I desire to dismantle any of those currently in existence. Yet I do hope to shed light on one important aspect of what is distinctive about science that may further illuminate the debate about unified method across the natural and social sciences, for I feel that many of the most popular accounts of scientific progress have overlooked (or at least underemphasized) what may be the most distinctive and valuable feature of scientific inquiry: its flexibility in describing familiar phenomena in novel ways, employing perhaps new theoretical terms that divide nature differently than we might in our common sense inquiry. In short, I claim that what is distinctive about science is that it allows us to understand nature only once it has been “redescribed” for us within the language of a scientific theory. What I then seek to show is that the prospects for a science of human behavior have been prematurely dismissed because of a stubborn and unfortunate misunderstanding of what is behind the success of the natural sciences.

### **Redescription**

Scientific progress is explanatory progress. Explanations, however, depend upon theories, and theories, in turn, depend upon a language of description. We cannot know the world except as it fits into the language that we have used to describe it. Thus, scientific phenomena are never explained “as such,” but only under some description, given by the language of a theory. Scientific theories, moreover, are more than just a reformulation of the correlation between the elements of those theories that precede it, expressed in a new way. Rather, a scientific theory offers us a new picture of reality that depends on its unique vocabulary not just to determine the appropriate correlations between some pre-set “kinds” of nature but to offer us a potentially new understanding of what nature is in the first place. When we describe reality we are not just passively recording the “joints” in nature; rather, we are deciding what is “real.” When we formulate a new theory we are in some sense creating a new world.

Scientific knowledge is based on our sensory experiences, yet this sensory input is ambiguous before we filter and categorize it. One world may offer us an infinite number of possible ways to describe it. Of course, there are genuine ontological connections within nature, yet ontology alone does not fully determine our scientific explanations, for the connections in nature need to be described and expressed in some theoretical vocabulary before they can be explained. Thus, while the ontology of nature *constrains* scientific inquiry, it cannot lead us to one unique and complete *explanation* of nature because there is no unique and complete *description* of nature to support it. The ontological connections of nature may be a necessary element for us to perceive nature as ordered, but order itself is only “as described.” This view of scientific explanation as illumination of the “order” of nature must necessarily depend upon our belief in the importance of descriptions. Scientific knowledge is more than just direct apprehension of the connections within reality, for empirical knowledge must always be filtered through a language of description.

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The central ideas behind this account of scientific explanation are the twin notions of “underdetermination” and “nominalism.”<sup>1</sup> These are the doctrines that counsel us to be skeptical of the realist notion of “natural kinds,” whereby we see scientific explanation as a process of theory building using the pre-set “brute” elements of nature. Instead, underdetermination holds that our theories are *underdetermined* by our experience; that the data of nature alone would fit infinitely many smooth curves that we might draw to describe and explain them. According to underdeterminism there are potentially an infinite number of possible scientific explanations for one and the same set of sensory experiences because there are potentially an infinite number of possible descriptions of any given correlation. *Nominalism* supports this view by telling us that we should be wary of believing that the names of things given by any particular theory pick out “real” divisions within nature. Instead we should see names as merely categorizing the phenomena of nature in an efficacious way. Does this mean that genuine explanation is impossible? Far from it. Rather, upon this view we may see explanatory progress within science as a process of “re-describing” nature in a new way, offering a novel explanation precisely because we are free to reconceptualize the appropriate categorizations within nature. Thus, scientific progress is progress in developing the theories and vocabularies that afford us new explanations of familiar phenomena, when the old explanations have failed to satisfy us. The backbone of progress in the natural sciences, I contend, is just this kind of progress in reconceptualizing familiar phenomena; scientific progress proceeds through “re-description.”

Perhaps the best way to get purchase on this idea is to consider those examples, drawn from natural science, that demonstrate the power of re-description. In the transition from theories about “phlogiston” to those concerning “oxygen,” one sees a clear role for re-description. Indeed, the history of natural science is littered with the remains of numerous other outdated descriptive terms and their correlative theories, where scientific progress was made only after giving up such notions as: “animal spirits,” “final causes,” “absolute rest,” “ether wind,” “caloric,” and “the four humors” in favor of more efficacious descriptive terms. Within the transition from alchemy to modern chemistry, in Mendeleev’s patient consideration of over sixty alternatives before settling on the currently accepted periodic table of the elements, through current efforts involving the taxonomic reclassification of several kinds of dinosaurs, we can see the importance of re-description to the progress of natural science.

### **Descriptivism**

One might now appropriately ask whether re-description may play a similar role in the social sciences. To the extent that social science has been mired in the same old explanations and theories of human behavior, can we make progress in

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<sup>1</sup> I have said more about the consequences of underdetermination in McIntyre (2003). For the locus classicus discussion of underdetermination and nominalism see Quine (1969) and Goodman and Quine (1947).

developing better ones through redescription? I believe that we can. But here we must consider an obstacle to the use of redescription that was also faced in the history of natural science: “descriptivism.”

“Descriptivism” is the idea that the subject matter of some area of inquiry is not the *phenomena* as such, but the *phenomena as captured by a particular descriptive vocabulary*.<sup>2</sup> This doctrine presents a real barrier to redescription and thus, I contend, to the progress of scientific explanation. Indeed, imagine the barriers that descriptivism would (and did) present to progress in the natural sciences. If one had insisted, in the face of growing anomalies, that “phlogiston” just captured a true “natural kind,” and that any reasonable explanation of combustion must be given using its theoretical vocabulary, one can imagine the poverty of the explanations that might have resulted. Indeed, in those instances in which scientists insisted that they *had* discovered the very “kinds” of nature (“phlogiston,” “ether,” “the four humors,” etc.) and felt that any genuine explanation must be given in these terms, sooner or later scientific progress slowed and ceased. This is because those scientists failed to remember that the job of science is to *explain the phenomena*, not necessarily to do so within the confines of some favored vocabulary.

In science the quest for explanation is primary. When faced with the dilemma of choosing between a descriptive vocabulary that seems sacred (but is not producing good explanations) and the challenge of producing a new categorization of nature that may yet reveal better explanations, science has relentlessly (although sometimes over great periods of time) chosen the latter course. This is because it has been recognized that, in the long run, it is nearly impossible to discern intuitively the best descriptions for nature. There is, moreover, no guarantee that the best descriptions will be salient to our common sense. Thus, the job of science is not merely to find correlations between a given set of referents; it is often to redefine the phenomena in such a way that we must come up with new referents altogether. When science has resisted this path it has eventually been mired down in unwieldy explanations. It is only through redescription that scientists have gained the flexibility to develop alternative theories—sometimes alien to common sense—that allow them to offer better explanations. And yet, isn’t this insistence on producing explanations only within a favored vocabulary exactly what seems to be taking place in contemporary social science?

Consider here the example of how traditional social science has used the terms “belief” and “desire.” Salient to our intuitive sense about what causes our actions, it is rare for social scientists to attempt to explain our behavior without using these terms. For many social scientists, human action is taken to be irreducibly intentional<sup>3</sup>—and what better way to capture our intentions than to talk

<sup>2</sup> For a fuller discussion of descriptivism see McIntyre (1996).

<sup>3</sup> Despite his commitment to physicalism, Donald Davidson believes that intentions are irreducible. This claim is also made by those who favor “interpretive” over scientific explanatory methodologies in the social sciences, such as Charles Taylor (1971), G. H. Von Wright (1971), and Peter Winch (1958). For further remarks on this trend see Staddon (2004).

about the beliefs and desires that motivate them? One recognizes that, despite the optimistic assumption of free will that motivates most social inquiry, there may in fact be outside causal factors—be they environmental or genetic—that shape our actions; yet, normally these are assessed by social scientists solely in terms of how they might effect our beliefs and desires!

The challenge to this model that has recently been put by evolutionary psychology—which seeks to explain human action outside the realm of human intentions—is a good example of the potential for redescription in the social sciences, for there are many actions that the belief–desire model would seem unable to account for that might be better explained in terms of our evolutionary inheritance. Yet, such proposals have been greeted with skepticism by many social scientists, while some others who have advocated such theories have argued that they are not really social scientific.<sup>4</sup> They have maintained, in other words, that true social science must be pursued in terms of the vocabulary of belief and desire: that the very subject matter at hand is human action as filtered through intentionality. One can imagine no clearer example of “descriptivism.”

How can it be that the subject of inquiry is the phenomena as such, yet we cannot explain the phenomena until they have been described? Indeed, doesn’t this admission undermine the earlier statement that the job of science is to explain the phenomena, and not necessarily to do so in terms of some favored vocabulary? Isn’t descriptivism just a prerequisite for explanation? No—for to say that we need to *describe* something in order to *explain* it is very different from saying that it can only be explained in terms of some *particular* vocabulary (i.e., there is a difference between saying that you need *some description or other* versus saying that you need some *particular* description). So the point of view that I have defended here is not descriptivist. While I endorse the idea that scientific explanation requires some description of the phenomena, I reject the idea that any particular vocabulary is necessary to do the job.

The potential confusion here is between (1) the idea that phenomena are only explicable under *some* description (which I think is true), and (2) the idea that the phenomena are exhausted by some *particular* description (which I think is false). What is the difference? It is that in the first instance, even while you recognize that you need a description, you also admit that any particular one could be false and may eventually need to be discarded in the name of scientific progress. In the second instance, you are wedding yourself to some particular vocabulary as

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<sup>4</sup> In his book *Sociobiology and the Preemption of Social Science*, Alex Rosenberg (1980) argues that an evolutionary science of human behavior preempts traditional “belief–desire” social science (though he hardly mourns the loss). In his own argument about “changing the subject matter” of social science (which I discuss in this article) Donald Davidson famously *does* mourn the loss, and maintains that the belief–desire model of social science should be preserved, though he holds that it will never be reducible to a true physicalist science of human action. Despite such differences about whether to preempt, eliminate, reduce, or otherwise tolerate traditional social science, such theories seem bound together by their commitment to the idea that the attempt to explain human action without reference to beliefs and desires is not genuine “social science.”

definitive or constitutive of the subject matter, which, if you are wrong, may eventually halt scientific progress. Unfortunately, social scientists have often confused these two issues (e.g., by insisting that human behavior is exhausted by the vocabulary of belief and desire as a condition of its explanation). This, of course, is just “descriptivism,” and we may now begin to see why the prospects for a science of human behavior have appeared to be so dim.

While natural science has moved far beyond the common sense vocabulary of our ancestors’ first inquiries into nature, in our social investigations we are still using a vocabulary that has largely been in place since the time of the ancient Greeks.<sup>5</sup> Is it any wonder that we have failed to produce a science of human action? The good news, however, is that such limitations on the prospects for a science of human behavior are largely artificial; they can be overcome—as they were in the natural sciences—through redescription.

Of course, there have been other critics of the belief–desire model in the explanation of human behavior who, even while they have not overtly embraced redescription, have not been at all reluctant to offer alternative explanatory models. One forceful challenge, made by Paul and Patricia Churchland (and also by Stephen Stich), lumps all such intentionalist vocabulary under the dismissive term “folk psychology” and attempts to show that in order for psychology to make progress it must jettison such talk in favor of a more scientific/empirical approach, such as neuroscience.<sup>6</sup>

While the model that I am defending in this paper may be thought to be generally sympathetic to this approach—and it does share some key similarities—it also has important differences. While my model agrees with theirs in recognizing the linguistic and explanatory limitations of intentionalist vocabulary, unlike them I do not seek to eliminate it altogether in favor of some replacement theory. This is not because I do not think that we can do any better; it is rather for two reasons. First, I think it is an empirical question what descriptive model should replace another. This should be decided based on science and not a philosopher’s speculations about who will win the empirical “horse race” in the proper model for mind and behavior. Second, a very real disadvantage of this approach is that it may end up committing the very sin that descriptivism has warned us against (i.e., being wedded to some particular vocabulary to the exclusion of other alternatives).

Whereas the Churchlands’ position is avowedly “eliminativist,” I seek to defend a true “pluralism” of explanatory stances in the social sciences, not seeking to prejudge what outcome the scientific study of human behavior might produce. For me, the appeal of redescription is that it is *flexible*, that it allows us to move from one vocabulary to another when one is not working. I fear that if we start to place bets, or to simply choose the best currently available theory, we may end up replacing one descriptivist theory with another!

<sup>5</sup> This point has been made by many psychologists, particularly behaviorists, as well.

<sup>6</sup> See Churchland (1979, 1981, 1998). Patricia Churchland’s *Neurophilosophy: Toward a Unified Science of the Mind–Brain* (1986) offers neuroscience as a replacement theory for philosophical speculations about belief and desire. Stephen Stich’s argument can be found in his book *From Folk Psychology to Cognitive Science: The Case Against Belief* (1983).

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Furthermore, it seems a central weakness of the Churchlands' model that it does not draw—as I hope to—an obvious parallel between the methodological situation that we are now in in the social sciences, and that which we were once in in the natural sciences. To me, the interesting thing about the debate isn't that the vocabulary of belief and desire needs to be replaced (or by what) but that the obsessive commitment to one particular vocabulary in social explanation is an almost identical rerun of what historically almost did in the natural sciences. That is, the most important part of the methodological debate is not which theory ends up being better (for, as an empirical matter, this could end up being revised over time); rather, it is that the social sciences have now gotten themselves into almost exactly the same descriptivist mess that we were in in the natural sciences 400 years ago.

What I am offering as a prescription for the social sciences, therefore, is not commitment to some ready-made replacement theory or vocabulary that has better empirical prospects than the vocabulary of belief and desire, but rather a guiding principle in scientific reasoning, which I think has served us well in the natural sciences. The idea is to recognize the power of redescription, to be willing to give up a favored descriptive categorization when it is not working, and to embrace the flexible and explanatory advantages of seeking other approaches.

Yet, we must now contend with a formidable objection to the use of redescription in the social sciences that is given throughout the literature, most prominently by Donald Davidson, who claims that to redescribe human behavior is to *change the subject matter* of social science. In his essay "Psychology as Philosophy" Davidson claims that in the social sciences we face a barrier that "has no counterpart in physics." Namely, he tells us that in the social sciences we are constrained by the fact that we must describe human behavior using "a system of concepts in part determined by the structure of beliefs and desires of the agent himself" (Davidson, 1982, p. 230). In short, Davidson tells us that in the social sciences we are constrained by the vocabulary of "beliefs" and "desires." But this is just descriptivism. Indeed, Davidson's claim seems to be predicated on the assumption that the subject matter of social science is *not* merely human behavior, but human behavior as fixed by a certain vocabulary that we have found salient in thinking about our own action. Yet why should scientific explanation be so constrained? Surely there is no guarantee that any explanatory account of our behavior must be given in terms of a vocabulary that is acceptable to our common sense. Worse, there may in fact exist better explanations, but Davidson's program would not allow us to find them.

The fundamental problem with Davidson's argument is that he fails to recognize that the subject matter of social science is most appropriately conceived of as *human behavior*, not that behavior as captured by the vocabulary of belief and desire. True, we do need to describe our behavior before we can explain it, but why wed ourselves *a priori* to some narrow vocabulary, no matter how salient, for describing human action? Of course, perhaps Davidson is right that if we confine ourselves to this narrow conception of human action we will not be able to have a science of it—but such a claim is not surprising or even very interesting. If we had

stuck with the descriptive terms of alchemy, would it be surprising if we had never invented modern chemistry? Why should we buy into such a narrow definition of the possibilities for explaining human behavior? Perhaps Davidson's conclusion is right if we allow him to define social science as the study of "belief" and "desire," but why should we allow this? At what cost do we allow descriptivism to limit the horizons of the social sciences, especially in light of the amazing success of the natural sciences when it has rejected this doctrine?

It seems uncontroversial to conclude that Davidson's argument does not actually delimit the prospects of producing a science of human behavior, but only whether it is possible to have a science of human behavior *given* some particular vocabulary. When Davidson claims to have refuted the possibility for a science of human action, we must understand his argument for what it is. Yes, he may insist that the vocabulary he favors is crucial to the proper understanding of human action, but if one allows the legitimacy of a potentially wider interest in explaining human behavior it becomes clear that we should not pay too much attention to arguments against the prospects for a science of human action that are given by descriptivists. It is important to remember that the limitations that descriptivists are prepared to put on scientific inquiry are only limitations from a particular point of view, and that the *very same limitations* that descriptivism now puts on social investigation were once used against the natural sciences as well. The real possibilities for a science of human behavior have not yet been plumbed by most contemporary social sciences.

### An Example

One might here complain that if my goal in this essay is to criticize current social scientific research I should be prepared to offer something in its place. What sort of alternative vocabulary do I have in mind for the investigation of human behavior? Can I give an example of a successful redescription? But this is unfair. One cannot insist that in order to evaluate an argument in favor of the redescription of human action I must first anticipate the future progress of social science. Indeed, this would be impossible *a priori*. The redescription of human action awaits further empirical work that may go forward only when one is prepared to go beyond the artificial barriers of descriptivism. What I am arguing for here is not the replacement of our current vocabulary in the social sciences with some favored alternative, but rather a shift in attitude that allows us to be more flexible in developing alternative vocabularies in the first place.

Nonetheless, with these qualifications in mind, by considering a hypothetical example at this point I hope at least to be able to indicate the *direction* that social science may take in employing redescription and show the advantages that it may offer over more traditional modes of social explanation.

Consider a fairly typical piece of human behavior: someone sitting in a movie theater watching the previews of coming attractions while waiting for the feature to begin. Suppose, however, that along with the movie previews the nefarious theater owner has inserted a "subliminal cut" that says "BUY POPCORN." Such

advertising, presented at speeds just below the threshold of human perception, can be surprisingly effective in manipulating human behavior, and for this reason was banned from theaters in the early 1960s.<sup>7</sup> In this example our moviegoer, along with a number of others in the audience, is seized by an overpowering desire to go to the snack counter, which is suddenly mobbed. Can this behavior be explained? It depends on our theoretical model.

If we choose the “intentionalist” model we might have a difficult time explaining this behavior. In concert with this model we might try to account for the behavior of the theater patrons solely in light of their beliefs and desires, their values and intentions. In order to discover these we would probably need to interview them to determine whether they actually had seen the hidden message, what their normal propensity is for buying popcorn when they go to the theater, and so forth. In conducting such interviews we would probably find that the movie patrons were well equipped to talk about their beliefs and desires, and that they could provide no shortage of reasons for their behavior.<sup>8</sup> They might say things like “I just wanted some popcorn, and I suddenly realized that there was still time before the movie started” or “I remembered that I had forgotten to buy popcorn on my way in, and that I had better hurry because the movie would soon be starting.” What probably would become clear from such interviews, however, is that the subjects had *no knowledge of the subliminal cut*, and that even if they were made aware of its presence, they would be prone to discount the effect that it might have had over their own behavior.<sup>9</sup> Thus it seems clear that if one is trying to explain the behavior only in terms of the intentions of the subjects—knowing that such a sudden simultaneous desire for popcorn is unusual in theater crowds where there are no subliminal cuts—one might have a very hard time explaining the behavior. The subjects, after all, have no cognitive access to the critical influential factor that manipulated their desire for the popcorn. If we nonetheless insist that since their

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<sup>7</sup> Although the original evidence for subliminal perception was based on a hoax—perpetrated by a failed advertising executive who was seeking to save his business—recent psychological experiments conducted in laboratory settings have confirmed the effect that subliminal presentations of word primes can have on human behavior. Significantly, these effects are realized only in cases in which subjects are already primed for the suggested goal (e.g., they are already hungry or thirsty), which does much to discount the fear of mass manipulation that originally led to the ban. But, also significantly, suggestible subjects *have* been shown to be more susceptible to subliminal influences than supraliminal ones, so the potential for unconscious manipulation is genuine. Thus, despite the odd history of the phenomenon of subliminal persuasion, its effect on human behavior is real and therefore must be explicable by social scientists (Dijksterhuis et. al., 2004; Epley et. al., 1999; Strahan et. al., 2002).

<sup>8</sup> cf. Steven Pinker’s discussion of the phenomenon of “confabulation” in his book *The Blank Slate* (2002), wherein brain-damaged patients, well aware of their defect, nevertheless make up stories to justify their actions.

<sup>9</sup> Although this is a theoretical example, the situation that I have described here is fairly typical of those social psychology experiments in which subjects demonstrate a profound lack of awareness of the actual causal forces that influence their behavior and instead falsely attribute their actions to salient noncausal factors, cf. Nisbett & Wilson (1977).

behavior was intentional it should be explained within the vocabulary of belief and desire, it is obvious that we will miss the important causal role of the subliminal cut and we will not gain a complete understanding of the action. While we may still be able to piece together a simple explanation based on the verbal reports of the subjects concerning their beliefs and desires, it is clear that such an account would be moving only at the periphery of the actual causal factors that we know to be at work and that should inform any legitimate explanation of their behavior.

Through this example I believe it is clear that there is a potential role for redescription in the social sciences, for once we are free to redescribe accounts like “the subject had a desire for popcorn and believed that it was available in the lobby” with “the subject was exposed to a subliminal cut that stimulated the impulse to buy popcorn” we are in a much better position to explain the behavior. Indeed, given the intentionalist model it is not even clear *why* the subjects would have a desire for popcorn in the first place. Worse, the reported beliefs and desires of the subject would be at least irrelevant, and at worst misleading, concerning the actual causes that led to the behavior. Forced to explain such actions relative to the very limited level of description at which we were concerned only with beliefs and desires, we would have no good way to account for why the subjects bought the popcorn, which is the very point of our explanation. Moreover, given the limitations imposed by our level of description, we would be in no position to compare how this situation was different from those others in which the subject bought no popcorn *because there were no subliminal cuts*. However, once we have redescribed the action and put the beliefs and desires within their causal context, it *would be* clear why the movie patrons had a desire to buy popcorn. Indeed, once we had done this we might even be in a position to predict their behavior in advance in similar situations, knowing of the causal power of subliminal cuts to stimulate such behavior. Thus, while it is true that the movie patrons’ behavior *was* intentional, a complete explanation would demand that we identify the *cause* of those intentions that led to the behavior in question. By itself, the intentionalist vocabulary would fall short.

Thus, it seems clear that redescription may play an important role in the explanation of human action. Once we have broken free of the descriptive limits of the “intentionalist” model, we may come to realize that the reported beliefs and desires of many subjects may be irrelevant to the actual explanation of their action. For what *causes* our intentions? And *why* do we have the beliefs and the desires that we do? These are issues that a subject’s verbal report normally can scarcely illuminate, but they are crucial to the explanation of human action. Thus, even if we agree that a good deal of human behavior *is* intentional, and even that the subject matter of social science is at least partially concerned with a subject’s self-understandings, we would be wise not to accept the reported beliefs and desires of the subject at face value. Instead, it seems clear that if we really want to understand human behavior we must be free to redescribe both the behavior and its causal antecedents in nonintentional terms. The intentionalist perspective provides an important dimension to our understanding of human action, but it is not the whole

story, even in social science. In social science, as in natural science, our explanations may be liberated by the use of redescription.

### Conclusion

What I hope to have shown in this essay is that there is potential for progress in the scientific study of human behavior through emulating the method of “redescription” that is used in the natural sciences. If we eschew descriptivism and embrace redescription, who can foresee the potential successes that may await us? Once we understand the crucial role that redescription plays in affording scientific progress, the prospects for unified method across the natural and social sciences may be brighter than many have thought. Indeed, to recognize what the history of natural science may teach us about the challenges and prospects that now face a science of human behavior is what the focus on comparative methodology is all about.

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