

1 *Seven myths of American sociology*

What is critical realism (CR) and who needs it?

Let us take the second question first. Sociology, particularly American sociology, needs CR. In the United Kingdom (UK) and elsewhere, it is already known. You might need CR as well. Are you unconvinced that a regression equation constitutes an explanation but do not quite know what does? Are you equally incredulous that ethnography and historical narrative do not explain? Maybe you hold the heretical view that not all reality is socially constructed but wonder how to formulate this counter case. Are you perhaps troubled by what various perspectives in sociology do with human personhood – decentering it, dissolving it into discourse, or otherwise deconstructing it? Perhaps you harbor doubts about the posture of value freedom that is supposed to characterize science. If these and other such disquieting thoughts about sociology fail to trouble you, carry on: You do not need CR. Otherwise, you do – at least a discussion about it.

This book is about CR and the contribution it can make to sociology. It is a book for those devoted to sociology who, nevertheless, are troubled by its current guiding assumptions. And sociology does have current guiding assumptions. All intellectual endeavors do. We may not notice them, but they are there. They exist at the level of presuppositions.

Sociologists are good at calling on others to recognize their presuppositions. Presuppositions are important, we tell them, because our presuppositions underlie and shape everything we do. Presuppositions determine what we think about our country and ourselves. They underlie and shape what we think is normal or deviant. Presuppositions shape how we think about criminality and poverty and religion.

Presuppositions are thus crucial to our current behavior and to the most radical changes we can make for the better – in ourselves, in our society, in the world. The word *radical* comes from the Latin, meaning root. So a radical shift or change is one that begins at the roots.

A radical change is thus deeper, more thoroughgoing than one that affects life only farther up the stem. And under the surface, the roots of our thinking are our presuppositions.

So we sociologists urge others to examine their presuppositions. We call this activity critical thinking, and when accrediting agencies come by to ask what it is we do and what value we offer society, it is the development of critical thinking we often tell them we provide. And in truth, we do get our students to think critically about their lives.

Critical thinking is a form of reflexivity, of thinking about ourselves. As sociologists, we assuredly do reflect on our collective lives more than most people. Reflection on our collective lives is our profession. What we do not do much of is critical reflection on our critical reflection. If that was one critical reflection too many, let me put it another way. For all our talk about critical thinking, we sociologists as a body do not tend to think very critically about our practice of sociology.

Admittedly, there is some, scattered, reflection on our discipline. There have been calls now for a “historical turn” to sociology, and historically framed pieces now show up in our top journals. Likewise, we now see isolated calls for newer thinking on causality or structure.¹ Such reflection, however, remains scattered and desultory.²

In the main, we rather repress critical reflection on sociology. It is a feature that distinguishes American sociology from, say, British. Think of the status that theory holds as a sub-discipline within American sociology. It is not one of the areas for which one sees many calls in the employment bulletin. The general assumption is that theory requires no specialized knowledge and that just about anyone can teach it.

That assumption, which itself is a disciplinary presupposition, follows from the function we expect theory to serve in the discipline.

¹ See, for example, Charles Tilly (2007) “Three Visions of History,” *History and Theory* 46: 299–307; David Diehl and Daniel McFarland (2010) “Toward a Historical Sociology of Historical Situations,” *American Journal of Sociology* 115 (6): 1713–1752; and Andrew Gelman (2011) “Causality and Statistical Learning,” *American Journal of Sociology* 117 (3): 955–966. Likewise, see the 1998 “Symposium on Historical Sociology and Rational Choice Theory” in *American Journal of Sociology* 104 (3).

² See George Steinmetz (2005) *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others (Politics, History, and Culture)* (Durham, NC: Duke University Press). Steinmetz makes the further point that at the top departments in the discipline, positivist assumptions go largely unquestioned.

We run our doctoral students through one or two courses in sociological theory to ensure they are grounded in the work of the three major founders of sociology – Karl Marx, Emile Durkheim, and Max Weber – and to ensure that they are sufficiently familiar with the different paradigms current in sociology that they will be able to pick the one in which they will be most comfortable working.

Quickly, though, students are urged to leave behind the big questions that divide the paradigms and settle on some concrete, empirical project within one. In other words, students are urged in the direction of what Thomas Kuhn called *normal science*.³ Normal science is science within a paradigm. Such science does not question the paradigm's basic premises – or if you will, its presuppositions. On the contrary, normal science is devoted to what Kuhn called *puzzle solving*. In normal science, the paradigmatic presuppositions are taken for granted and deployed to explain within their terms why something happens or is what it is. To the extent that the explanation is successful, then from the paradigm's perspective the puzzle has been solved. It is then onto the next puzzle.

Is that not what scientists are supposed to do and is sociology not supposed to be a science? There are a number of presuppositions in these questions that themselves generally go unquestioned in sociology. Coming from those so insistent on critical thought, the twin “supposed” are particularly ironic. Who says that normal science is what scientists are supposed to do? Well, of course, Kuhn for one, but does sociology take its marching orders from Kuhn?

Interestingly, Kuhn himself did not say scientists necessarily should pursue normal science or pursue it exclusively. He said that in mature sciences, normal science is what scientists typically do. What did Kuhn consider to be a mature science? One where after considerable, pre-scientific debate, a single paradigm has won out over all others. That paradigm thus becomes the established way of approaching things in that science. The paradigm is the established way of approaching things because the scientists in that discipline have managed to reach consensus – at least for the time being – on fundamental reality in their field.

Kuhn's prime model of a mature science was physics and, as is well known, it was a social physics that was Auguste Comte's aspiration for

³ Thomas Kuhn (2012) *The Structure of Scientific Revolutions* (Chicago, IL: University of Chicago Press).

sociology. Technically, however, in Kuhnian terms, physics might not be so mature after all. Physics, it turns out, is not governed by a single overarching paradigm but by two: Einstein's theory of relativity and quantum mechanics. One governs the physics of the large and very fast and one governs the physics of the very small.

Each of these paradigms, physicists tell us, is very highly corroborated. Neither has failed any empirical test to which it has ever been submitted. The credentials of these paradigms are thus enviably strong. Certainly, we have nothing like them in sociology and, likely, never will.⁴

Still, physicists know there is something wrong with each of these two governing paradigms. How do they know? Because, for one thing, the two paradigms are conceptually incompatible with each other. Thus, as currently formulated, at least one must be wrong. Physicists strongly suspect the problem lies with both.

Now, if physicists were sociologists, they would receive something like the following counsel: We are scientists, not philosophers. Don't spend too much time worrying about conceptual issues, and, for the love of God, please don't distract yourselves with endless debate over them. Doing so holds us up from our real job. Which is? Normal science: processing data. Thus, just settle on one of these two paradigms and get onto productive work.

Is physics following such advice? Well, certainly, most physicists do go on with normal science within one of the two paradigms, and I am hardly disparaging normal science as an activity. Scientific paradigms avail nothing if they are not applied to concrete questions in the form of normal science.

But where paradigms are incompatible, some very big questions – in fact, the biggest questions – remain. And physicists do not ignore them. In fact, in physics the biggest questions, the theoretical questions, attract the biggest names: Stephen Hawking, Stephen Weinberg, Brian Greene, Paul Steinhardt, and so on. So-called *theories of everything* (TOES) are, among other things, ways of reconciling relativity and quantum mechanics.

⁴ Although I would argue that what philosophers of mind call "folk psychology" is every bit as predictive and successful. See Ian Ravenscroft (2010) "Folk Psychology as a Theory," *Stanford Encyclopedia of Philosophy Online*. <http://plato.stanford.edu/entries/folkpsych-theory/>.

Interestingly, at the edges of physics, where physicists debate matters like string theory or cosmic inflation or even multiple universes, theoretical development exceeds knock-down empirical evidence or even any current empirical evidence at all. At that point, physics finds itself in the position of sociology, where theory is largely underdetermined by data and where all physicists have is inference to the best explanation. Some physicists decry theorizing in this situation and call for a return to considering only those questions that data can answer.⁵ Still, the theorizing at the edges goes on, and it goes on a lot. It is where the big questions lie.

Let us now return to sociology. Our field is much farther than is physics from any consensus on paradigms. Whereas physics has only two governing paradigms, both very successful, we have many, none of which can be described as very successful. Some cannot even be described as good.

If big questions about paradigm choice remain in physics, many more remain in sociology. In fact, in contrast with physics, sociology cannot at all be described as a mature science. In Kuhnian terms, sociology is not even a science. It is instead what Kuhn called pre-science. The appropriate activity in pre-science, Kuhn suggested, was not normal science but continued work toward paradigmatic consensus.

American sociologists do not want to work toward paradigmatic consensus. That prospect sounds too much like endless debate. It sounds too much like philosophy. It does not sound like science. American sociologists want to do normal science.

So what do we do? We christen ourselves a *multiple paradigm science*.⁶ And voilà! With that designation, all our problems appear to disappear. We have no need to arrive at consensus, no need to address those big questions that divide us. We can each do normal science within our own chosen paradigm. All that is further necessary is to erect a mutual non-aggression pact. We will not attack anyone else's silly paradigmatic presuppositions if they will not attack ours.

⁵ John Horgan (1997) *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age* (New York: Broadway Books).

⁶ George Ritzer (1975) *Sociology: A Multiple Paradigm Science* (New York: Allyn & Bacon). Ritzer himself, it must be said, at least tried to make the case for meta-theorizing. That case was much less well received than his designation of the discipline.

None of us need reflect on our presuppositions. Doing so would not be normal science. Doing so would not be science.

If we were social constructionists studying sociology as a tribe, we would know what to do. We would notice how the tribe constructs itself as a science by barring certain kinds of questions and by *othering* certain outsiders like psychologists and philosophers.⁷ But then social constructionists rarely turn social constructionism on themselves. Doing so would raise all kinds of embarrassing questions: Are social constructionists' constructions just social constructions? If so, what epistemic weight do sociological constructions bear that warrants anyone's heeding them? Generally, social constructionists, too, do not want to consider such philosophical questions but rather pursue their own brand of normal science within their own paradigm.⁸

I began this book with two questions: What is CR and who needs it? From there, I went on to speak of sociology's presuppositions and of its reluctance to confront them. My point was that sociology does have underlying presuppositions that shape what we do.

Basically, the sum total of our presuppositions about science constitute a philosophy of science. And CR is a philosophy of science. That is what CR is. As such, CR is not a theory that directly explains anything, but a metatheory that establishes the boundaries between good and bad theorizing. It does so by advancing basic ontological and epistemological assumptions.

⁷ Philosophers generally repay the compliment. Although the philosophy of science is a very important area of disciplinary philosophy, philosophy of social science is not. When disciplinary philosophers think of science, they think characteristically of physics, particularly in the philosophy of mind, which charmingly refers to all else, including sociology, as "special sciences," meaning fated ultimately for reduction to physics. For a critique of this orientation in the philosophy of mind, see Steven Horst (1996) *Symbols, Computation, and Intentionality: A Critique of the Computational Theory of Mind* (Berkeley, CA: University of California Press). For a notable exception in the philosophy of science, see Daniel Little's (2012) *Varieties of Social Explanation*, Amazon Digital Services. It is a book with which I often disagree but which shares with the present volume a similar (although less confrontational) sensibility and coverage of topics.

⁸ The most reflective practitioners of the approach recognize the problem. See the debate on this point that pitted Steve Woolgar against Harry Collins and Steven Yearley in Andrew Pickering (ed.) (1992) *Science as Practice and Culture* (Chicago, IL: University of Chicago Press).

Do we need a philosophy of science or metatheory? Well, yes. The fact is you already have one. The question is whether you have the right one.

In Chapter 7, we will talk more about philosophies of science, finally putting together everything we discuss. There, we will exhaustively compare and contrast CR with other philosophical presuppositions on offer:

- positivism;
- postmodernism, poststructuralism, and discourse theory;
- social constructionism;
- analytical sociology;
- pragmatism;
- Marxism/Frankfurt School;
- symbolic interactionism and Verstehen sociology;
- actor–network theory (ANT);
- practice theory;
- relational sociology; and
- Bourdieusian thought.

We will end up with a chart, marking all the different positions of each perspective on a range of important questions. Here, let it suffice for me to say a few things. First, why do I say you already have a philosophy of science? Well, look at what I said a philosophy of science is: a body of presuppositions about science. Certainly, if you are practicing science, your activity rests on some presuppositions about what you are doing. Ipso facto, you are operating with a philosophy of science.

Second, as indicated above, prevailing in sociology is not one, but multiple different philosophies of science. In fact, as suggested, many of the different sociological paradigms listed come with their own philosophy of science. These philosophical or metatheoretical differences are what make inter-paradigm dialogue and adjudication so difficult. Our disputes concern not only the data but which data, what to do with them, and what to make of them. Many of our sociological paradigms differ in all these regards.

Historically and still today, the philosophy of science dominant in sociology has been positivism. Many sociologists today do not consider themselves positivists because they do not believe in running statistical regressions mindlessly, but no positivist ever really believed

in doing that. The truth is that positivism is the philosophy of science behind empiricism, and most sociologists today remain empiricists in deep ways they do not even realize. Positivism is a philosophy of science for those who do not want to think about philosophy, and not wanting to think about philosophy is a salient characteristic of empiricism.⁹

As the term positivism or positive philosophy was coined by our own Auguste Comte, this philosophy of science aligns closely with sociology's founding myth. The founding myth is Comte's *Law of Three Stages*.

The *Law of three Stages* is a putative law about history, according to which human collective consciousness advanced in three stages. The first stage was religious consciousness, in which human beings derived their truths, their sense of reality from religion and religious authority. Stage two was the stage of speculative philosophy. It was a stage in which religion and other verities were challenged on philosophical grounds. As critique was prominent in this stage, there was a certain negative aspect to collective thought. The French *philosophes*, for example, were always coming down on this or that human practice or belief system.¹⁰

The modern period began, according to Comte, with a new, more positive way of thinking. That way was science. Whereas in the second stage of human consciousness, people thought through matters conceptually, in this new third stage, empirical inquiry becomes paramount. The search now was for the actual laws through which all of reality operates, and that search had to be empirical, that is, based on observation.

It was not just the laws governing nature that were to be empirically uncovered. For Comte in particular, it was also the laws of society and human behavior that the third stage of human thought was after. Hence, Comte's vision of sociology as social physics. Just as physicists were to find the laws of nature, sociologists were to find the laws of society.

⁹ According to Steinmetz, *The Politics of Method*, at the top departments many positivist sociologists have actually adopted different ways of disguising their own positivism from themselves.

¹⁰ See Irving Zeitlin (2009) *Ideology and the Development of Sociological Theory* (New York: Prentice Hall).

What was the point of all this law-finding? The answer comes in Comte's famous quip: Explanation in order to predict; prediction in order to control. In other words, empirical work (i.e., observation) was necessary to find the laws operating in a domain, say society. Laws, in turn, were necessary for explanation (a premise that CR in particular will strongly contest). And finally, the point of explanation was the ability to predict and control.

Prediction and control were in turn important because the ultimate point was to fix society for the better. Hence the positive element in positivism, the element of scientific optimism. Sociology was to be in the service of social progress, and the inevitability of progress was part and parcel of sociology's founding myth.

I use the word myth here in the way it is often used by religion scholars – not to designate a belief that is necessarily mistaken but a belief filled with larger significance for some group, a belief that tells the group members who they are, where they came from, and where they are going. The postmodernists – when they were still around – referred to these founding beliefs or myths as *meta-narratives*.¹¹

A group's meta-narrative may well be historically false. I don't think, for example, that Rome really was founded by a pair of twins raised by wolves. At the same time, a group's founding myth or meta-narrative need not be false. Whether or not myths are false needs to be examined case by case. For all the criticism associated with Comte's sentiments, especially the connotations that prediction and control carry of failed social engineering, I do think there is something right about it. We do want to understand how the social world works and in part we want to do so in order to create a better world. Why else are we in this field? For all the criticism lodged against it, I also think something remains of the idea of human progress.

So I am not totally dismissive at least of the sentiment behind the positivist philosophy of science. It should be acknowledged, furthermore, that positivism has never been owned solely by sociology. On the contrary, Comte and Durkheim after him were part of a wider intellectual wave. For a long time – up, basically, through Kuhn in the late 1960s, positivism was the single, paradigmatic way of

¹¹ Jean-François Lyotard (1984) *The Postmodern Condition: A Report on Knowledge* (Minneapolis, MN: University of Minnesota Press). Now, everyone claims never to have been a postmodernist, just a poststructuralist. The postmodernist phase must have been a collective dream.

understanding science philosophically, so that anyone doing science in any field understood what he or she was doing in positivist terms. Sociology in fact is now much less positivist than economics and psychology, which even more stubbornly refuse to look at their founding presuppositions.¹²

There is also more to positivism than I have so far identified and developed. And, here, I do want to switch to a more colloquial understanding of the word myth, that is, myth as a false idea. After Kuhn and the post-positivist thought that ensued in the philosophy of science, we now know – to the extent that we know anything – that many of the tenets of positivism are wrong. Empirically and conceptually, they are untenable.

The problem is that various of those false positivist tenets live on comfortably in sociology as myths – strongly felt but demonstrably wrong ideas. What is worse, we will see, is that the positivist account of science is accepted even by many who oppose a sociology built on positivist principles. What positivism's sociological opponents often end up opposing is thus not just a faulty account of what it means for sociology to be scientific, but rather all scientific aspirations for sociology. In the process, the anti-positivists – the humanists, the post-Wittgensteinians, the postmodernists, and social constructionists – all end up endorsing counter-myths, strongly felt but equally wrong ideas. The problem is that with an empiricist-inspired, disciplinary repression of philosophy, we do not thoroughly think through these matters. Instead, we go with our instincts. We pick a paradigm in which we feel comfortable and prepare to conduct normal science for the rest of our lives.

The big questions remain unaddressed. In the process, the worst thing is, as Christian Smith points out, that sociology comes out with images of ourselves that we know cannot be true, images that we ourselves hardly carry with us into ordinary life, images of ourselves as structurally or culturally determined automata or as deconstructed congeries of subject positions. Like the economists with their fabled *homo economicus*, we are often content with the pretend people our theories offer instead of the real persons that as non-professionals we know we are.¹³

¹² See Steinmetz, *The Politics of Method*.

¹³ Christian Smith (2011) *What Is a Person? Rethinking Humanity, Social Life, and the Moral Good from the Ground Up* (Chicago, IL: University of Chicago Press).