

McIntyre  
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BU Colloquium

## MCINTYRE ON MACINTYRE:

### A TRIBAL DISPUTE ON THE MESSINESS OF THE SOCIAL SCIENCES

On the first day of graduate school at the University of Michigan I went to the new student “mixer” and was approached by one of the faculty members. He said “Lee McIntyre, now there’s a good philosophical name. Are you by any chance related to Alasdair MacIntyre?” Unable to think of a clever reply (or a lie that I could easily get out of later) I just said “no,” at which point he said “oh, too bad,” turned around, and walked away.

Over the years I’ve come to anticipate a similarly puzzling—though somewhat more polite—reaction to the news that Alasdair MacIntyre and I are not related, in which the questioner seems to assume that because we share a surname, we must share similar philosophical views as well. Since we both work in the philosophy of social science—and Prof. MacIntyre’s views are so well known and mine are not—I’ve begun to prepare myself to discuss my own views in relation to his. At least it gets the conversation flowing.

Given all this, you can’t know how much satisfaction it gave me when Fred Tauber asked me to be a commentator on this paper for, in a way, I’ve been stalking Prof. MacIntyre’s work for something like twenty years now. And the topic that he has chosen for today is a delight: the messiness of the social sciences.

Over the years that I’ve worked in the philosophy of social science, I am often asked by social scientists (in a sometimes hostile tone) what philosophy could possibly tell them that they

didn't already know. For the social sciences *are* inherently messy and what makes them messy is the empirical details that the practitioners are experts on (and philosophers often are not). Add to this the fact that there is often a healthy resistance to the idea that some expert in the logic of science could come in, make a few prescriptions, and we would suddenly have laws, predictions, and better theories at the other end, and you sometimes get outright rebellion. And some of this is of course deserved.

Still, today I will maintain that philosophy can play an important role in the social sciences. More contentiously, I will also defend the idea that the model of explanation put forth in the natural sciences *does* provide a good prescription for how to deal with some of the messiness in the social sciences, even if I will also suggest that explanation in natural science is not quite what we have thought it to be.

Now in this crowd I don't have to tell you that the job of the philosopher who studies science is often a thankless one. As Sidney Morgenbesser so aptly put it: "you make a few distinctions. You clarify a few concepts. It's a living." But what *is* the proper role of philosophy in analyzing the work of the social sciences and how can we use our philosophical tools to better understand the ongoing attempt to make sense of human behavior? Here Prof. MacIntyre has put an excellent proposal on the table, which is that we try to be much less prescriptive—and certainly less rude in the unflattering comparisons that philosophers have sometimes drawn between the natural and social sciences—and instead get around to admitting that a lot of what the social sciences actually do in their day to day work is quite messy, and doesn't look much like what is going on in natural science, and—if we hope to really understand human behavior—it probably shouldn't.

And I love the way that he has framed the problem here, which is to suggest that social science is not the same as natural science because social science is inherently messy. But this naturally leads us to the question of *what* is so messy about the social sciences and *why* that must be so. (And, of course, the follow up question of whether we should try to do anything about it).

To the latter question, Prof. MacIntyre responds that no, we shouldn't try to do anything to "clean up" the social sciences, for an appreciation of the diversity and complexity of its explanatory tasks is essential when aiming at a complete understanding of human behavior. In service of this appreciation for the messiness of social science, Prof. MacIntyre has provided us with an excellent example, through Burns's work on the sociological explanation of adaptive organizations and the individuals who make them up. Through this example we really begin to understand that a full account of innovation must proceed along three different levels: historical, economic, and sociological, and that none of these is capable of filling in for any of the others. This anti-reductionist roadmap feeds precisely into what he means by "messiness."

Still, one is tempted to push here and question what sort of foundational ontology could be behind such messiness. At no point in Prof. MacIntyre's account does he say anything to suggest that what we are dealing with here are indeterminate phenomena. No suggestion is made, pace Popper, that human systems are "open" and therefore that they are messy because there is an uncaused element behind human affairs, perhaps springing from some sort of complication that arises as a result of free will or human subjectivity or consciousness. Rather, the messiness that Prof. MacIntyre describes seems to be much more like that proposed by F.A. Hayek, who famously claimed that social phenomena are too complex to be studied scientifically. And maybe that is enough. No ontological barrier is needed, perhaps, when the epistemological one is this

large.

But this type of messiness, no matter how bad it may be, is merely explanatory messiness. Epistemological messiness. The type of messiness that may result from an incomplete understanding of the causal forces at work. Think here of Alexander Pope's famous dictum that "disorder is order misunderstood." Here we encounter a familiar criticism of social explanation: that it is not essentially complex so much as it just cannot get its language right and so capture all of the causal forces at work in sociological phenomena in one neat conceptual package in the same way in sociology as we do in physics.

But if this is the sort of messiness that we are facing in social science, then the norms of scientific explanation seem well-equipped to help us to deal with it. First, we should recognize that some of this type of messiness is surely characteristic of some of the natural sciences like, for instance, meteorology or even—god forbid—particle physics, where we are never tempted to say that "messiness" prevents us from having a science.

Still, MacIntyre is surely right that we do not customarily pursue three different levels of description and explanation in physics or chemistry. In the normal course of things we are not really interested (at least as scientists) in the economic, historical or sociological aspects of physical or chemical phenomena. Yet, we do so seem to be interested in these questions in the social sciences. Is that what makes them messy? Even if this is an epistemological matter, it is a deep one.

Here we may begin to see the appeal of the principle of reduction. For if we could just find a clever way to eliminate all of these competing social scientific explanations and reduce them down to the essence of what is going on at the causal level, we would have our foundation

in scientific understanding. But I believe that this is a misunderstanding of what is required for science. Science does not require reduction. In the last twenty years of the philosophy of science, we have come to recognize that there can be many different levels of explanation in science and that sometimes the phenomena we are interested in just do not emerge at one level, so they must be studied at another.

A good example here, which illustrates not only the point against reduction but also about the messiness of some of the questions in *natural* science, can be drawn from the philosophy of chemistry. Despite its status as the default example of reduction in natural science, the reduction of chemistry to physics turns out to be no trivial matter. Take for instance the concept of smell. The smell of something has no correlate at the microphysical level. Even though it is true to say that phenomena at the secondary level of description are materially instantiated in physical (indeed subatomic) relationships at the primary level, there is nothing at that level of description that can recover for you the concept of what it means for a compound to have a particular smell. Some of the pathbreaking work in this area has been done not by a philosopher, but a chemist, Roald Hoffman, who has defended the idea that “smell” is an emergent property. There are numerous other examples: surface tension, liquidity, color, even molecular shape, all of which are chemical concepts that begin to lose their meaning when you try to reduce them to the physical level. Chemistry, as it turns out, is also sort of messy. Yet who would deny that it is a science?

It is attractive here to rethink what we mean by science and so allow for this kind of diversity of explanation, as long as one crucial parameter is met: in order to be scientific, an explanation must be focused on giving the tightest descriptive account of the phenomena that we

wish to have explained, without losing that phenomena in the attempt to capture it in language.

I maintain that this type of diversity of explanation is to be tolerated and even welcomed in social science, as it is in natural science, for this is to recognize a fundamental insight about science that Kuhn taught us. Language matters. Description matters. Truth may be grounded in the causal relationships of nature, but this is not where science starts. Science starts with a human being who has a question, and for this question to be formulated we need language. We need a description of the problem and the phenomena before we can do any science. And this is as true in natural science as it is in social science.

What science provides us, therefore, is a way to cut through the “messiness” of our competing, alternative accounts of description, to find the one that best fits the question that we have in mind, while still remaining true to the facts as we find them. If that question is causal, then I submit that the social sciences would do well to emulate the standards of natural science by trying to eliminate complexity wherever possible, not by pursuing reduction, but by recognizing that some of the messiness in science may be an artifact of our misdescriptions. We must be willing to redescribe the phenomena in social science, just as we do in natural science, in search of a causal pattern that will inform a scientific explanation. Redescription, not reduction, is the best way forward when dealing with a messy subject matter.

Indeed, perhaps the clearest proof that this is the correct prescription for the social sciences is that this path is already being pursued—with some success—in current social science. Take, for example, some recent work in behavioral economics done by Sheena Iyengar on the subject of economic “choice.”

The topic of “choice” turns out to be a complex matter. Our personal history, perceptions,

societal norms, cultural biases, economic class, personal preferences, etc., all play a part in determining whether we will choose any given consumer good, political party, profession, etc. When we choose one thing over another, there are always going to be competing explanations that can be offered at different levels of description. Did I choose to become a philosopher because, as a boy, I loved to think about the concept of infinity? Because my parents never went to college and so revered education? Because a teacher encouraged me? Because I didn't want to go to law school? Maybe those are all correct answers.

But there must be a simpler way to proceed. In her work, Prof. Iyengar has suggested that the best way to understand choice is to see if you can predict the outcome. You use Occam's Razor and that old positivist principle which says that if you can predict an outcome, then you don't need all of the other explanations. As Carl Hempel suggested long ago, once you can predict the behavior, you've got the explanation.

The experimental work on this topic was done with jam. Yes, you heard me right—jam!

Iyengar and her colleague Mark Lepper set up tasting booths in a grocery store and asked people to pick which jam was their favorite. As I recall, everyone who tasted the jam got a free coupon, which allowed the researchers to track their later purchasing behavior at checkout. In one display there were twenty-four choices of jam. In another, there were only six. They alternated the displays by time of day and instituted all sorts of other good experimental controls to make sure that they were following proper scientific protocol. And what they found was nothing short of amazing. Although the more extensive array of jams attracted more initial interest from the shoppers, their subsequent purchasing behavior was radically diminished when they were faced with more choices. In each display about an equal number of people actually

tasted the same number of jams. But when the researchers later measured the results they found that those shoppers who had visited the booth with twenty-four choices used their coupons only 3% of the time, whereas those who visited the booth with six jams used their coupons 30% of the time. That is a huge difference!

In their analysis of these results, Iyengar and Lepper speculated that the complexity of the first task might have overwhelmed the shoppers. They couldn't be sure that they had chosen the best jam, so they chose not to buy any at all. But in the second booth they could handle the choice. As it turned out, more choices were not necessarily better. Parallel work has now been done on the topic of choosing between options within 401(k) plans and the result has held up. For every ten options by which a choice matrix increases, the participation rate drops by about 2 to 3%. Apparently, people want fewer choices!

Now this is not perhaps what any of the people involved would say, but does that matter? Maybe this explanation is opposed to the sort of self-understanding that people have of their own actions—and may even run contrary to their sense of self—but there you have it. Their preferences are revealed through their behavior, not by what they say about what they want. And the beautiful thing about Iyengar's work is that this is still intentional behavior. And it is still causal. But it does not require us to embark on any sort of "messy" explanations whereby we have to take into account the historical, sociological, or even economic factors that people might take to influence their decisions. The psychological ones will do just fine. Whether they know it or not, people want fewer choices. This is the power of experimental social science. It can offer us perhaps surprising insights into the causal forces that shape our actions, in sometimes astonishingly simple form.

Perhaps such explanations are not as pleasing to us as the rich ones that we are used to from the more traditional ways of doing social science. But that is the price to be paid when we embark on a scientific understanding of familiar phenomena and must leave our “folk” explanations behind. Many levels of description and explanation will be lost because they are not cutting to the deepest causal level. Indeed, as we note from the history of science, this is not unfamiliar. We faced the same phenomenon when chemistry eclipsed alchemy, when physics gave up the concepts of “ether wind” and the “epicycle.”

We must sometimes redescribe the world that we thought we inhabited, in order fully to understand it. This need not (always) be reductive, but it can still be scientific. Scientific explanation encourages us to simplify. To get our explanations down to the most fundamental causal level. I submit that this is the best way to deal with a subject matter that is messy. We must still use a perhaps imperfect language. Our theories and concepts will often have to be revised. Sometimes our explanations will turn out to be downright wrong. But this is what good science—even social science—is all about.