# *The Relativity of Theory* by Moti Mizrahi: On the Necessity of History in Philosophy of Science

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Who is the best ever, Steffi Graf or Serena Williams? The 1995–96 Chicago Bulls or the 2015–16 Golden State Warriors? Wayne Gretzky or Bobby Orr? These are the sorts of questions that swirl in television studios, barrooms, and bleachers when fans and pundits debate the superlative accomplishments of their favorite sports. They rarely produce consensus. Commenters defend arguments for why team or individual A should be regarded as superior to team or individual B, and sometimes can reach agreement on such assessments, but debates about the GOAT (Greatest Of All Time) rage on precisely because consensus is elusive. Commentators often disagree about which statistics are most relevant, how to interpret them, or how to control for changes in games across eras. But they have nevertheless settled on pairwise comparisons as a mechanism for generating defensible assessments of *relative* sporting greatness.

So it is with scientific theories, according to Moti Mizrahi (2020). *The Relativity of Theory* contends that we will never be able to claim that our best scientific theories are superlatively true, or even approximately true, but that we can establish that some are *truer* than others, by holding them up side by side and comparing their empirical success. This is the essence of "relative realism," the position Mizrahi defends as an amicable middle ground in the realism/anti-realism debate.

Before developing his case, Mizrahi embarks on a survey of that debate, which is longstanding and marked by intransigence. The first five chapters introduce the realism/anti-realism issue and the stakes of the controversy over it, review the key realist and anti-realist positions, and outline the central arguments supporting each. The book's central conceit is to reconstruct those arguments in standard form, identifying problematic premises and suspect arguments in order to pinpoint the sticking points between realists and anti-realists.

This structure makes the book an excellent pedagogical tool. Mizrahi's reconstructions are clear and deliberate, and although readers are likely to find many opportunities to quibble, that can be construed as a feature, rather than a bug, of a book targeted at readers new to philosophy of science. As a lucid and accessible introduction to one of the key issues in philosophy of science, this book is the best since Larry Laudan's *Science and Relativism* (1990), which has now grown somewhat out of date.

I approach this book not as a philosopher, however, but as a historian of science. And from that perspective, a few of the aforementioned quibbles blossom into worries. Mizrahi takes a staunchly ahistorical approach to the realism/anti-realism debate, casting his lot with those who are skeptical that historical cases have much to offer philosophers, at least by way of evidence for their claims (e.g. Pitt 2001). Mizrahi's worry is that "scientific realists and antirealists often use historical evidence from the same historical record of science to support either scientific realism or antirealism, respectively" (129), as a consequence of which the historical record offers no basis for preferring one position over another. He positions relative realism as superior in part because his arguments for it proceed without asking for support from historical cases.

Let us consider this rhetorical strategy in light of a historical case. IBM's laboratories in Zurich hosted a landmark discovery in 1986, when K. Alex Muller and J. Georg Bednorz observed the phenomenon of superconductivity at unprecedentedly high temperatures, in a

class of ceramic materials known as cuprates (Muller and Bednorz 1987). Superconductivity—the phenomenon in which all electrical resistance within a material vanishes when the material dips below a critical transition temperature ($T_c$)—had been known since 1911, but only in metals, and only at temperatures within twenty or thirty degrees of absolute zero. The cuprates, with transition temperatures on the order of 30, 40, 50, or even 100 degrees Kelvin, tantalized experimental physicists and engineers with the promise of room-temperature superconductivity, and potentially transformative technological applications.

The phenomenon was no less seductive to theoretical physicists. High-$T_c$ superconductivity did not seem to obey the vaunted BCS (Bardeen-Cooper-Schrieffer) theory, which had successfully accounted for the behavior of previous superconducting materials. Theorists scrambled to account for this new class of superconductors. Competition was fierce among prestige-conscious physicists aware that accolades such as the Nobel Prize awaited the first to succeed. One theorist quipped that, like in the Cinderella fable, everyone lined up to see if they would prove a fit for the glass slipper (Zangwill 2021, ch. 14). But even today, that slipper still seeks a foot to fill it; competition remains fierce among multiple theories of high-$T_c$ superconductivity, despite the fact that theorists have at their disposal a rich and shared body of carefully collected empirical data about the cuprates and their behavior.

Now imagine that a theorist surveying this state of affairs and suggesting that, because the same data is used to support rival theories of high-$T_c$ superconductivity, the most appropriate way to craft a successful theory is to *disregard the data entirely!* This, needless to say, would be a curious proposition, and one unlikely to win much support from fellow physicists. But that, in essence, is what Mizrahi has asked philosophers to do vis-à-vis history.

In the spirit of Mizrahi's own exposition, let us consider how the argument above works. This is an argument by analogy; my aim is to convince you that the two cases are similar enough that if you find the neglect of physical data problematic in the case of high-$T_c$ superconductivity, then you should also find the neglect of historical data problematic in the case of philosophical discussions of realism. At minimum, the analogy should establish that the mere fact that data can be used to support multiple positions is insufficient grounds for dismissing that data as impertinent to the soundness of those positions. In standard form, the argument might look something like this:

(P1) In physics, theoretical success requires accounting for known, relevant empirical evidence.
(P2) High-$T_c$ superconductivity is a physical problem.

Therefore,

(C1) A successful theory of high-$T_c$ superconductivity should account for known, relevant empirical evidence.

(P3) Philosophical theorizing about realism and anti-realism is relevantly similar to physical theorizing about high-$T_c$ superconductivity.
(P4) Historical data constitutes an empirical evidence base for philosophy of science.

Therefore,

(C2) Philosophical stances in the realism/anti-realism issue should account for known, relevant historical evidence.

I take the first part of the argument to be uncontroversial. Accommodating existing data is widely recognized as a potent, if not the most potent desideratum for scientific theory selection (Brush 2015), and high-$T_c$ superconductivity is straightforwardly part of physics. C1 is therefore unlikely to excite much opposition.

The premises that establish the argument by analogy, however, require more careful defense. Consider P3: the argument depends on the cases being sufficiently similar, and in the right ways. But philosophy is not a natural science, and so we might reasonably expect to find a different relationship between theory and evidence in philosophy than we do in condensed matter physics. Nevertheless, the cases have plenty in common. Both involve theories attempting to account for a recognized phenomenon—the behavior of high-$T_c$ superconductors in one case and the success of scientific inquiry in the other. Most importantly for the purposes at hand, both are situations where the same data can be used in service of competing theories, and so the superconductivity case provides a counterexample against the notion that data that can be used to support multiple theoretical positions should be disregarded, as Mizrahi suggests. To justify dismissing history on these grounds, Mizrahi would have to establish the much stronger claim that we should *never* expect that historical data will be able to motivate a preference between competing philosophical views.

That brings us to P4. Whether or not one accepts this premise depends on how one conceives of the objective of philosophy of science. In what measure is its goal to develop prescriptive accounts for wringing reliable knowledge from the world, and in what measure must it account for the actual success of scientific practice? Some will contend that philosophy of science is a strictly normative enterprise—if historical scientists failed to act in accordance with the best philosophical principles, so much the worse for them. On this view, history can be illustrative, but never evidential. But many, if not most philosophers of science would sincerely hope that their accounts contribute to understanding the ostentatious success of the scientific enterprise over the past few centuries. To accept that as a goal is to accept P4, and Mizrahi's embrace of the realist's "narrative of triumph"—the notion that success of science is indicative of the increasing truth-likeness of its theories—indicates that accounting for the past success of science holds an important place among his goals.

This is not to suggest that manipulating historical evidence is easy. As Mizrahi rightly points out, philosophers all too often cherry-pick convenient historical cases to lend support for their positions without considering the broader class of cases to which they belong, or countenancing possible counterexamples. But the bad behavior of a few cherry-pickers should not lead us to dismiss historical evidence *tout court*. As I have argued elsewhere (Bolinska and Martin 2020), recognizing bias is quite often the first step toward instituting measures to address it. For philosophers, that means recognizing that not all historical cases are created equal. Like scientists, who must be critical of the conditions in which data were collected (much of the early data on high-$T_c$ superconductors, for instance, was taken from poor cuprate samples), and evaluate that data's pertinence to their theoretical problems, philosophers should select their cases in light of positive arguments for virtues such as salience, representativeness, and generality (see Bolinska and Martin forthcoming). And it bears emphasizing that historical evidence does not come in the form of discrete data that provide direct support for a theory. But nor is that how data typically work in science. Rather, data place constraints on what counts as a reasonable theory. Neglecting historical evidence therefore risks ignoring important constraints that might guide fruitful philosophizing.

How does the neglect of history bear on Mizrahi's positive program? I admit to some reservations about relative realism as a middle-ground position. Many of the arguments Mizrahi gives for it could be straightforwardly recast as arguments for instrumentalism by replacing

"truth" with "empirical adequacy" in his premises. It would be a tricky business to make the case for preferring truth over empirical adequacy in those premises without resorting to inference to the best explanation—a form of reasoning Mizrahi seeks to avoid—and, indeed, he appears to salvage a commitment to realism only by adopting an attenuated notion of truth that many anti-realists would accept, but that most realists would reject. These, though, are philosophical worries that I lack the space here to treat in full. In closing, I note instead the evident *possibilities* careful engagement with historical evidence present. Mizrahi's position raises promising historical questions, the investigation of which can be expected to yield results of philosophical interest. What actually happens when scientists compare theories vis-à-vis their empirical and predictive success? On what grounds do they decide that one is superior to another? How do they negotiate situations in which different theories have different comparative success in different domains? Answers to questions such as these will make or break relative realism as an account of successful science.

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