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History and the Contemporary Scientific Realism Debate

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The scientific realism debate began to take shape in the 1970s, and, with the publication of two key early '80s texts challenging realism, Bas van Fraassen's *The Scientific Image* (1980) and Larry Laudan's "A Confutation of Convergent Realism" (1981), the framework for that debate was in place. It has since been a defining debate of philosophy of science. As originally conceived, the scientific realism debate is one characterized by dichotomous opposition: "realists" think that many/most of our current best scientific theories reveal the truth about reality, including *unobservable* reality (at least to a good approximation); and they tend to justify this view by the "no miracles argument," or by an inference to the best explanation, from the success of scientific theories. Further, many realists claim that scientific realism is an empirically testable position. "Antirealists," by contrast, think that such a view is lacking in epistemic care. In addition to discussions of the underdetermination of theories by data and, less commonly, competing explanations for success, many antirealists—in the spirit of Thomas Kuhn, Mary Hesse, and Larry Laudan—caution that the history of science teaches us that empirically successful theories, even the very best scientific theories, of one age often do not stand up to the test of time.

The debate has come a long way since the 1970s and the solidification of its framework in the '80s. The noted dichotomy of "realism"/"antirealism" is no longer a given, and increasingly "middle ground" positions have been explored. Case studies of relevant episodes in the history of science show us the specific ways in which a realist view may be tempered, but without necessarily collapsing into a full-blown antirealist view. A central part of this is that self-proclaimed "realists," as well as "antirealists" and "instrumentalists," are exploring historical cases in order to learn

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the relevant lessons concerning precisely what has, and what hasn't, been retained across theory change. In recent decades one of the most important developments has been the "*divide et impera* move," introduced by Philip Kitcher (1993) and Stathis Psillos (1999)—possibly inspired by Worrall (1989)—and increasingly embraced by numerous other realists. According to this "selective" strategy, any realist inclinations are directed toward only those theoretical elements that are really doing the inferential work to generate the relevant successes (where those successes are typically explanations and predictions of phenomena). Such a move is well motivated in light of the realist call to *explain* specific empirical successes: those theoretical constituents that play no role, for instance, in the reasoning that led to the successes, likewise play no role in explaining those successes. Beyond that, however, and crucially, the *divide et impera* move allows for what appears to be a testable realist position consistent with quite dramatic theory-change: the *working parts* of a rejected theory may be somehow retained within a successor theory, even if the two theories differ very significantly in a great many respects. Even the working parts may be retained in a new (possibly approximating) form, such that the retention is not obvious upon an initial look at a theoretical system but instead takes considerable work to identify. This brings us to a new realist position, consistent with the thought that many of our current best theories may one day be replaced. Realism and antirealism are no longer quite so far apart, and this is progress.

A major stimulus for, and result of, this progress has been a better understanding of the history of science. Each new case study brings something new to the debate, new lessons concerning the ways in which false theoretical ideas have sometimes led to success, or the ways in which old theoretical ideas "live on," in a different form, in a successor theory. At one time the debate focused almost exclusively on three famous historical cases: the caloric theory of heat, the phlogiston theory of combustion, and the aether theory of light. An abundance of literature was generated, and with good reason: these are extremely rich historical cases, and there is no simple story to tell of the successes these theories enjoyed, and the reasons they managed to be successful despite being very significantly misguided (in light of current theory). But the history of science is a big place, and it was never plausible that all the important lessons for the debate could be drawn from just three cases. This is especially obvious when one factors in a "particularist" turn in philosophy of science where focus is directed toward particular theoretical systems. These days, many philosophers are reluctant to embrace grand generalizations about "science" once sought by philosophers, taking those generalizations to be a dream too far. Science works in many different ways, in different fields and in different contexts. It follows that the realism debate ought to be informed by a rich diversity of historical cases.

It is with this in mind that the present volume is put forward. In recent years a flood of new case studies has entered the debate, and is just now being worked through. At the same time it is recognized that there are still many more cases out there, waiting to be analyzed in a particular way sensitive to the concerns of the realism debate. The present volume advertises this fact, introducing as it does several new cases as bearing on the debate, or taking forward the discussion of historical cases that have only very recently been introduced. At the same time, the debate is hardly static, and as philosophical positions shift this affects the very *kind* of historical cases that are likely to be relevant. Thus the work of introducing and analyzing historical cases must proceed hand in hand with philosophical analysis of the different positions and arguments in play. It is with this in mind that we divide the present volume into two parts, covering "Historical Cases for the Debate" and "Contemporary Scientific Realism," each comprising of seven chapters.

Many of the new historical cases are first and foremost challenges to the realist position, in that they tell scientific stories that are apparently in tension with even contemporary, nuanced realist claims. The volume kicks off in Chapter 2 with just such a case, courtesy of Dana Tulodziecki. For Tulodziecki, the miasma theory of disease delivered very significant explanatory and predictive successes, while being radically false by present lights. Further, even the parts of the theory doing the work to bring about the successes were not at all retained in any successor theory. Thus, Tulodziecki contends, the realist must accept that in this case false theoretical ideas were instrumental in delivering successful explanations and predictions of phenomena.

This theme continues in Chapter 3, with Jonathon Hricko's study of the discovery of boron. This time the theory in question is Lavoisier's oxygen theory of acidity. Hricko argues that the theory is not even approximately true, and yet nevertheless enjoyed novel predictive success of the kind that has the power to persuade. Just as Tulodziecki, Hricko argues that the realist's *divide et impera* strategy can't help—the constituents of the theory doing the work to generate the prediction cannot be interpreted as approximately true, by present lights.

We meet with a different story in Chapter 4, however. Keith Hutchison provides a new case from the history of thermodynamics, concerning the successful prediction that pressure lowers the freezing point of water. Hutchison argues that although, at first, it seems that false theoretical ideas were instrumental in bringing about a novel predictive success, there is no "miracle" here. It is argued that the older Carnot theory and the newer Clausius theory are related in intricate ways, such that in some respects they differ greatly, while in "certain restricted situations" their differences are largely insignificant. Thus the realist may find this case useful in her bid to show how careful we need to be when we draw antirealist morals from the fact that a significantly false theory enjoyed novel predictive success.

Chapter 5 takes a slightly different approach, moving away from the narrow case study. Stathis Psillos surveys a broad sweep of history ranging from

Descartes through Newton and Einstein. We have here a "double case study," considering the Descartes–Newton relationship and the Newton–Einstein relationship. Psillos argues against those who see here examples of dramatic theory-change, instead favoring a limited retentionism consistent with a modest realist position. Crucially, Psillos argues, there are significant differences between the Descartes–Newton relationship and the Newton–Einstein relationship, in line with the restricted and contextual retention of theory predicted by a nuanced, and epistemically modest, contemporary realist position.

Chapter 6—courtesy of Eric Scerri—introduces another new historical case, that once again challenges the realist. This time the theory is John Nicholson's atomic theory of the early 20th century, which, Scerri argues, "was spectacularly successful in accommodating as well as predicting some spectral lines in the solar corona and in the nebula in Orion's Belt." The theory, however, is very significantly false; as Scerri puts it, "almost everything that Nicholson proposed was overturned." Hence, this case is another useful lesson in the fact that quite radically false scientific theories *can* achieve novel predictive success, and any contemporary realist position needs to be sensitive to that.

Chapter 7 turns to theories of molecular structure at the turn of the 20th century. Amanda Nichols and Myron Penner show how the "old" Blomstrand-Jørgensen chain theory was able to correctly predict the number of ions that will be dissociated when a molecule undergoes a precipitation reaction. While *prima facie* a challenge to scientific realism, it is argued that this is a case where the *divide et impera* strategy succeeds: the success-generating parts of the older theory are retained within the successor, Werner's coordination theory.

The final contribution to the "History" part of the volume—Chapter 8—concerns molecular spectroscopy, focusing on developments in scientific "knowledge" and understanding throughout the 20th century and right up to the present day. Teru Miyake and George E. Smith take a different approach from the kind of historical case study most commonly found in the realism debate. Siding with van Fraassen on the view that Perrin's determination of Avogadro's number, so commonly emphasized by realists, does not prove fruitful for realism, and focusing on diatomic molecules, they emphasize instead the extraordinary amount of evidence that has accumulated after Perrin and over the past ninety years for various theoretical claims concerning such molecules. Taking van Fraassen's constructive empiricism as a foil, they indicate that, in this case, so-called realists and antirealists may really differ very little when it comes to this area of "scientific knowledge."

The second part of the volume turns to more general issues and philosophical questions concerning the contemporary scientific realist positions. This part kicks off in Chapter 9 with Mario Alai's analysis of the *divide et impera* realist strategy: he argues that certain historical cases no longer constitute counterexamples when hypotheses are *essential* in novel predictions. He proposes refined criteria of essentiality, suggesting that while it may be impossible to identify exactly which components are essential in *current* theories, recognizing in hindsight those which were *not* essential in past theories is enough to make the case for deployment realism.

In Chapter 10, Kyle Stanford makes the case that so-called realists and instrumentalists are engaged in a project together. For Stanford, these traditionally diametrically opposed protagonists are now working together to "actively seek to identify, evaluate, and refine candidate indicators of epistemic security for our scientific beliefs." As philosophy of science, and the realism debate in particular, becomes increasingly "local," Stanford also argues for "epistemic guidance intermediate in generality" between the sweeping generalizations of 1970s and '80s realism, and a radical "particularism" where any realist claim should always be specific to one particular theory, or theoretical claim.

Chapter 11 turns to the relationship between the realist's *divide et impera* strategy and the structural realist position. James Ladyman argues that structural realism is not a form of selective realism (or at least doesn't have to be). For Ladyman, structural realism represents a departure from standard scientific realism, not a modification of it. He also argues that scientific realists face ontological questions (not only epistemic ones), and he defends a "real patterns" approach to what he calls the "scale relativity of ontology." This allows for equally "realist" claims to be made at the level of fundamental physics and at the macroscopic level.

In Chapter 12, Jennifer Jhun explores the realism debate in a different territory. In particular, she considers the possibility of taking a structural realist attitude toward macroeconomic theory. Taking the consumption function as a case study, she argues that a better take on macroeconomic theory involves a compromise between structural realism and instrumentalism. For Jhun, when it comes to economics (at least), "theories are instruments used to find out the truth."

In Chapter 13, Ludwig Fahrbach considers a prominent antirealist argument against the claim that realism can be defended against the pessimistic meta-induction (PMI) by invoking the exponential growth of scientific evidence. The antirealist response to this defense depends on the claim that realists could have said the same thing in the past. He introduces this antirealist response as the "PMMI," the pessimistic meta-meta-induction. Fahrbach's challenge focuses on a particular weak spot common to both the traditional PMI and the PMMI. Thus realists unimpressed by the traditional PMI will not be moved by the new PMMI.

Chapter 14 turns to another important aspect of the modern realism debate: the use of "radically false" theoretical assumptions such as infinite limits in many contemporary, highly successful theories. Patricia Palacios and Giovanni Valente note how "infinite idealizations" misrepresent the target system, and sometimes it appears that the introduction of such blatant falsity is necessary to achieve empirical success. Focusing on various examples in physics, such as classical and quantum phase transitions as well as thermodynamically reversible processes, Palacios and Valente propose a realist response to such cases.

Last but not least, in Chapter 15 Anjan Chakravartty tackles the standard model of particle physics, and in particular what the realist might say about representations of fundamental particles. Introducing the realist "tightrope," Chakravartty discusses the trade-off between committing to too little, and committing to too much. But in the end, he argues, this is a tightrope that is not too thin to walk.

These articles have been carefully collected for this volume over many years, and in particular during the Lyons/Vickers 2014–18 Arts and Humanities Research Council (AHRC) project "Contemporary Scientific Realism and the Challenge from the History of Science." The project enjoyed seven major events over its lifetime, out of which the fourteen substantive chapters of this volume ultimately grew. The seven events were:

(i) "The History of Chemistry and Scientific Realism," a two-day workshop held at Indiana University–Purdue University Indianapolis, United States, December 6–7, 2014.
(ii) "The History of Thermodynamics and Scientific Realism," a one-day workshop held at Durham University, UK, on May 12, 2015.
(iii) "Testing Philosophical Theories against the History of Science," a one-day workshop held at the Oulu Centre for Theoretical and Philosophical Studies of History, Oulu University, Finland, on September 21, 2015.
(iv) "Quo Vadis Selective Scientific Realism?"—a symposium at the biennial conference of the European Philosophy of Science Association, Düsseldorf, Germany, September 23, 2015.
(v) "Contemporary Scientific Realism and the Challenge from the History of Science," a three-day conference held at Indiana University–Purdue University, United States, February 19–21, 2016.
(vi) "Quo Vadis Selective Scientific Realism?"—a three-day conference held at Durham University, UK, August 5–7, 2017.
(vii) "The Structure of Scientific Revolutions," a two-day workshop held at Durham University, UK, October 30–31, 2017.

The editors of this volume owe a great debt to the participants of all of these events, with special thanks in particular to those who walked this path with us a little further to produce the fourteen excellent chapters here presented. We are also grateful to the unsung heroes, the many anonymous reviewers, who not only helped us to select just which among the numerous papers submitted would

be included in the volume but also provided thorough feedback to the authors, helping to make each of the chapters that did make the cut even stronger. The volume has been a labor of love; we hope that comes across to the reader.

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