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## Chapter Five: Relativism in Philosophy of Science

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### I. Introduction

Relativism in current philosophy of science is driven by the twin problems of induction and underdetermination, which threaten the claims of science to have a rational structure.

Tensions are created by two separate motives: first, to picture contemporary science as a supremely rational enterprise, possibly our most successful epistemic activity; second, to “tell it like it is,” which can either serve or conflict with the first motive. Conflict here has real bite. Scientists feel threatened or angry in the face of claims that real practice is not rational; and those who propound views about rationally must be seriously disturbed if science does not meet their standards.

Relativisms cluster along two main axes: those that oppose scientific realisms and those that oppose claims that science is/can/should be value free. For each node on one of these axes, there are a variety of proposals on offer about what the disputed aspect of science is supposed to be relative to. The most common are concepts and methods, models and theories, aims and purposes, cultural and socioeconomic viewpoints, kinds of negotiation or critical discussion, historical location, and kinds of epistemic, moral, political, and aesthetic values. As usual, the debates occur at different levels: the ontological—there are no truths of the kind in dispute; the epistemological/methodological—there may be such truths but we do not have reasonable access to them; and the normative or descriptive—whether there are such truths and whether we are able to learn them. This is not what science ought to be doing, or not what science is, in fact, doing.

In all discussions of relativism, it is easy to slip between levels, especially in contemporary philosophy of science where issues are frequently framed with the term “accept”: “What are the standards that make a claim or practice acceptable in science?” This can mean, “What characterizes the kinds of claims we would like to admit in science,” or, “What are the standards we should use in admitting claims and practices,” or, “What are the standards we do use.”

The natural starting point for discussion might be the ontological level. In addition, there with truth, we can ask whether scientific truths are relative and if so, what are they relative to, and in what sense? But the truths that concern science are usually thought of as hidden—Nature’s secrets. That is why we need special experimental and mathematical techniques, the care and precision that characterize modern science, to unearth them. But can the carefully honed methods of science give sufficient warrant for accepting its results? This question provides the seedbed for relativism about science.

## II. Induction, overdetermination, and the ship of life

Hardly anyone disputes that science is and should be in the business of building outwards from claims and practices that are already accepted and that have some established reliability to new claims and practices that can be used reliably in new situations. So David Hume’s problem of induction is immediate. There are, Hume argued, no methods whose reliability can be defended in a non-circular way for going from what is known to what is unknown. The problem invites a general solution: Just suppose that Nature’s truths are accessible by this or that favored method of investigation. Hume and John Stuart Mill both discussed a solution of this form for the method of simple induction (i.e., infer that unobserved cases will be like observed ones): some kind of principle of the uniformity of nature. The solution suggests a fundamental relativism: Scientific practices and claims are acceptable relative to

the assumption that Nature is uniform in the appropriate way or, for more sophisticated methods, relative to the assumption that Nature does appropriately underwrite the method.

Presumably, though, Nature either is or is not uniform in the requisite manner, or does or does not have the appropriate structure to vindicate a given method for inferring the unknown from the known. So it seems the method is either reliable or not, *simpliciter*. No relativism here, certainly not at the ontological level. But principles like these, so the standard arguments go from Hume onwards, cannot have a non-circular basis. So our entitlement to count the method reliable is relative to an unsupported *assumption*, as in consequence is our entitlement to any claims we infer with the method.

Call this “epistemological” if you will, but there is far more at stake than whether our beliefs are mistaken. Consider Otto Neurath’s famous remark that in constructing knowledge claims, we are like sailors who must repair their ship at sea, never able to put in to dry dock to build from solid ground. Neurath worried not just about the quality of the planks available, but about the plan for how to use them. There are, he claimed, no properly rationally-grounded scientific methods to guide the new construction: Evidence neither confirms nor disconfirms hypotheses; at best it “shakes” them. For him, the decision to accept a new knowledge claim is always relative—relative to a “free” judgment, a judgment rising out of experience, thought, and inherited practices and beliefs, but not licensed by a properly grounded method, in the end a kind of “plumping.”<sup>1</sup>

Neurath’s is not a Kantian worry that we cannot ever grasp the “real” noumenal world but will always live in the world of experience. His worry is about how we can survive in the world of experience. The concern is not that we may fail to build a ship that matches the original plan of the master builder but that we may not be able to build a ship that is seaworthy. Neurath’s relativism threatens not just the truth of our new knowledge claims, but the effectiveness of the practices we base on them.

The problem of “underdetermination” opens a similar road to relativism. If scientific hypotheses are to say anything new, there will always be a number of incompatible hypotheses compatible with all facts already accepted. So taught Pierre Duhem,<sup>2</sup> W. V. O. Quine,<sup>3</sup> and many others. What makes one of these hypotheses more acceptable than the others? Nelson Goodman’s “new riddle of induction,”<sup>4</sup> which has plagued science since its start, is a special case of underdetermination. What are the correct concepts to use in making inductive inferences? In studying falling bodies, Medieval physicists focused on the rate of change of velocity with *distance fallen* ( $dv/dx$ ). We make more reliable predictions from  $dv/dt$ . Or, in a Euclidean space, which is approximated by the space around us, where we have traditionally made our observations, bodies traverse Euclidean straight lines. Modern physics teaches that they will not do so in curved geometries. The appropriate concept for projecting from the observed to the unobserved is that of a “geodesic”—the shortest distance between two points in the geometry.

At the ontological level, again perhaps there is no relativism. There is only the question of whether a given hypothesis involving the concepts it uses is true or false, *simpliciter*. Matters are different at the epistemological level. There are two main kinds of “solutions” to the problem of underdetermination on offer, and both have been accused of falling into relativism.

First, employ some additional factual claims and use what Clark Glymour describes as “bootstrapping”: Given the “background” assumptions, the known facts logically imply the hypothesis.<sup>5</sup> This seems to be the method employed in Francis Everitt’s Gravity Probe experiment<sup>6</sup> to test the general theory of relativity. The theory implies that a gyroscope should be caused to precess from coupling with the space-time curvature. To design the gyroscopes and get them into space took twenty-five years. But the set-up was so carefully engineered with such voluminous knowledge of what else can cause precession that when the

Gravity Probe gyroscopes precessed, nothing but coupling with the space-time curvature could have been responsible.

One may then say that the coupling hypothesis is acceptable *relative to* these background assumptions, assumptions that use already a specific choice of concepts. This seems to amount to the dual claims that the space-time coupling is true if the background assumptions are true and that a belief in its truth is well grounded if the background assumptions are well grounded. Since these are factual claims, that may not seem a serious problem—probably they are indeed well grounded. It matters greatly that they should be well grounded for there are many examples where the same fact can count either as strong evidence for a hypothesis or strong evidence against it depending on what background assumptions are made.

But worries about relativism to these claims escalate if, as would be the case on standard reconstructions, these background assumptions must include general claims whose grounding seems problematic: Nothing causes a precession except a torque; every precession must have a cause; all sources of torque are controlled for in the experiment. Some of these—like “every precession has a cause”—are so fundamental and grand that they might get labeled as “metaphysical” to mark the impossibility of confirmation by standard empirical procedures (independent of worries about how well grounded these procedures are in themselves). This label is backed up by the suspicion that if we trace the cairn of support back stone by stone, we are likely to encounter assumptions clearly eschewed by even mild Positivists as metaphysical, like “Every event has a cause.” It is hard to be confident about the seaworthiness of our boat if its safety is relative to claims like this.

Consider as a second example the randomized controlled trial (RCT), widely regarded as the “gold standard” for testing causal claims in the human sciences, especially in medicine. Various proofs are available that in an ideal RCT, a higher probability of the effect in the

treatment group than in the control group deductively implies that the treatment causes the effect in the experimental population. Proofs, of course, require premises. One premise is that in the ideal RCT “confounding” factors are distributed in the same way in the treatment and control groups. (Real RCTs famously use random assignment to try to make this likely.)

Further premises are necessary to connect causality with probability. This can be done through the probabilistic theory of causality, which also undergirds the powerful Bayes nets methods for causal inference. On the probabilistic theory, an earlier event-type, *C*, causes a later, *E*, just in case *C* increases the probability of *E* once all “other” causes of *E* simultaneous with *C* are taken into account. The idea is that once other reasons for *E* to be probabilistically dependent on *C* are eliminated, the dependence must be due to *C*’s causing *E*. Clearly this presupposes that probabilistic dependencies must have causal explanations. This is again a grand claim. As before, as we look down through the cairn of support for claims about treatment efficacy we encounter assumptions near the base that have not been subjected to the kinds of careful empirical investigation we demand in science.

The second way of resolving underdetermination is by appeal to various “virtues” that acceptable claims or practices should have, or certain aims they should meet, where of course the virtues themselves may be “relative”: Different virtues matter for different occasions, different scientists, different sciences, different uses, and different agenda. Those who are keen to keep values and local agenda out of science, to “rescue” science from relativism, often pick a subset of these virtues as special for science, generally labeled “epistemic” virtues. These include, usually first and foremost, compatibility with already accepted fact. Further, these include simplicity, unifying power, precise novel prediction, fruitfulness, coherence with other accepted hypotheses and theories, practical usefulness, explanatory power, and survival of critical public scrutiny.<sup>7</sup> In addition, specific disciplines may add

subject-specific virtues. In physics for example, one may demand that theories be renormalizable or satisfy certain high-level symmetry principles.

### III. Where is the episteme in epistemic virtues and how much help are they anyway?

The label epistemic matters since it is often supposed that relativising scientific practices to epistemic virtues is not pernicious, not contrary to the spirit of science. Whereas relativising to other kinds of values is. But in what sense do any of these earn the label epistemic, which suggests that they conduce to knowledge or to truth? If the phenomena in a domain are complex or diverse, why should choosing the simplest claim or the one that unifies the most help in arriving at true claims? We may suppose that truth and our favorite epistemic virtues march hand-in-hand, but that looks to be one of those grand “metaphysical” assumptions not confirmed by the detailed scrutiny demanded of proper scientific claims.

Beyond this, there are other worries about how far epistemic virtues can fend off relativism. First, they are vague, so do not provide real grounds for choice. Attempts to make them more precise seem to require further metaphysical assumptions to back them up. Second, to the extent that they do have grip on a particular claim or practice, it is not guaranteed that they all point in the same direction. There seems no natural hierarchy or weighting scheme among them. Third, it seems that without what look to be very arbitrary definitions, they will not narrow the options to a single choice, so the problem of underdetermination remains.

Once the issue of epistemic virtues is raised, even the demand that acceptable claims be compatible with already-known facts comes into question. Imre Lakatos famously maintained that every theory is born, refuted, and stays that way all its life. We should not be aiming for true theories, he urged, but those that get a sufficient number of the known facts right and are progressive.<sup>8</sup> Others urge that theories be good at problem solving,<sup>9</sup> that they

produce correct predictions about targeted phenomena,<sup>10</sup> or that they be the ones best suited to serve democratically chosen aims.<sup>11</sup> So for many, the truth of a claim is not a trumping virtue. What is acceptable in science is always relative to a choice of goals, with truth just one among many we might embrace. This naturally escalates the problem one step. Are some goals legitimate in science and others not and what are the grounds for legitimacy?

Even if truth is taken to be the natural goal, the sole arbitrator of what is acceptable, it is otiose as a guideline for what we *should* accept, because we cannot directly tell what is true. That is why even staunch truth advocates engage in laying out other more readily ascertainable characteristics to aid in the quest for truth. This brings us back to the idea of epistemic virtues and their problems. Since it seems unlikely that the epistemic virtues, whatever their defense, can be honed fine enough to provide judgments about specific claims and practices, what fills in the gap? Standard answers include other values—political, moral, cultural, aesthetic; personal and group motives, fair and foul; local cultural and scientific practices; tacit knowledge; plumping; charisma; money; available technologies.

Now we have reached the point at which the question of how the sciences do settle on what to accept and how they should blend into each other. Contemporary history and sociology of science abound with studies of what was actually going on as particular scientific results and practices stabilized.<sup>12</sup> What was the material and cultural environment in which the stabilization occurred, who spoke to whom, what kinds of techniques/ideas/equipment were available, and so forth? These can describe well what did in fact resolve the underdetermination problem. But it seems to leave the scientific result relative to the historical accidents of what was happening where and how it developed.

For many this outcome is welcome. It paints science as a human activity, not a magic tunnel to the basic truths of the universe. For others, it is totally unacceptable. Science can not only help build a boat seaworthy enough to keep us afloat in life; it can also build the

right boat: There is something about scientific practice when done assiduously, honestly, and continually, that brings the accepted claims of science closer and closer to the truth. Their actual acceptance at any time may be relative to facts about their origins but if the history goes as it should, they will also approach the truth. They will be not only accepted but also *acceptable*, judged by the standard of truth, taken as the virtue that trumps all others.

Those who believe that science gets closer and closer to the truth face two main challenges. First, to explain this notion of “closer to,” especially across scientific revolutions. Consider: Aristotelian physics has a totally different conception of the universe from that of Newton, and Newtonian physics a totally different conception from the physics of Einstein. In what sense do the central theoretical claims get closer to the truth as we go from one to another? Second, what are the scientific methods that get to the truth and how can we defend that they do so? Attempts to answer these two questions are still the meat of philosophy-of-science debate, both writ large—across the sciences—and writ small—within small corners of specific disciplines.

#### IV. Are all scientific truths relative?

Induction and underdetermination plague our decisions about what to accept in science; the relativisms they give rise to are at the epistemological level. Is there place for relativism at the ontological level, especially with respect to the kinds of truths science investigates, assuming it is sometimes in the business of looking for truths?

Start with scientific truth in general. Two related lines of thought suggest a “yes” answer, lines of thought that can capture the imagination but are difficult to explicate. The first is that all truths of interest in science are relative to a perspective or point of view. All qualities in Nature are in some sense secondary qualities, like colors or tastes. Just as one might argue that what is true of the world for a bat is different from what is true for a human,

what is true from one scientific perspective is not true from another. The difficult task is to explain what a scientific perspective consists in. This, too, is one of the salient topics about which philosophers of science argue,<sup>13</sup> and to which feminist epistemology has made a special contribution.<sup>14</sup>

If all features of the world are like secondary qualities, truth will be relative to perspective. There are a number of similar-sounding but less radical accounts that are at base not relativist, often formulated in terms of “aspects.” A scientific theory/model/treatment can never give a *complete* image of the phenomena studied. We study only some aspects or other, and at some “level” of coarse graining. The truths that are acceptable in a particular study are *ipso facto* relative to the choice of aspect and level. This however is not a relativism at the ontological level so long as the aspects pick out features that are supposed really to be there. Incompleteness of description, even if inevitable, does not generate the serious ontological relativisms that perspectivism does.

The second line of thought leading to relativism about scientific truth in general is a bowdlerized Kantianism, that Nature is a kind of undifferentiated sea that demands a conceptual scheme before it gives rise to truths. Yet, it seems, not any conceptual scheme will do: some do—perhaps only one? Some do not allow for the emergence of claims and practices that pass scientific tests for acceptability. Consider Neurath. He would talk only about the world of our experiences. But that world seems to consist of “*Ballungen*”—loose clusters with vague edges (like a “*Ballungsgebiet*” or metropolitan area).<sup>15</sup> These can, however, for various purposes be represented by the kinds of precise concepts we use in science, though different ones for different purposes. Or Duhem. He can plausibly be read as claiming that Nature is qualitative; the quantities of science are just abstract symbols we use to represent it.<sup>16</sup>

This particular source of relativism has a venerable tradition in the social sciences. Max Weber, for instance, argued that the study of society could not be a science because the concepts with which we should be concerned in that study do not name precise quantities figuring in regular laws.<sup>17</sup> Supposing—as is not uncommon—that these precise quantities are what really exist in Nature, truths in social science are in trouble at the ontological level. If they are true at all, it must be relative to something. Often the underlying idea appears to be that they are relative to some way or another that to which we human beings are responsive. We group the “true” features of Nature together to assign concepts where no proper referent exists in Nature. Here values and purposes are likely to play a significant role, and plumping of the Neurath style may be inevitably involved if there are no strict rules in the offing for how the assignment is done.

Consider for instance attempts to operationalize the important social concept of poverty. Should poverty be taken as an absolute concept or as a relative measure, and if relative, relative to what? One standard approach defines that one is poor in a targeted society if one has less than two thirds the median income there. But is it individuals or households that are judged to be poor? Surely children in rich households are not poor. Within households, do we demand that a family of seven, including elderly grandparents and small children, have seven times two-thirds the median income? The first thing to notice is that there seems no natural or “scientific” way to settle these questions. Second, the ways we settle them will give rise to very different poverty figures and count different individuals as poor—thus advantaging some individuals and disadvantaging others in the face of attempts to help poor persons.<sup>18</sup>

Perhaps this does not seem so surprising with respect to poverty. Consider then *price*, which figures prominently in economic theory. To calculate real price we need an index of inflation. How shall we calculate that? Many goods have increased in quality so that owning

them now provides more utility than before. But many people cannot afford the new higher quality goods at all; or the goods are available at reasonable cost in suburban outlets, which require cars for access. Veterans' benefits are usually pegged to inflation indices. But the recent suggestions of the American Boskin commission on how to treat quality improvement in calculating inflation does not take account of the fact that veterans who need the benefits cannot afford cars.<sup>19</sup> So, is there a proper quantity, *price*, that figures in true economic relations, Or is every version of this concept relative, either consciously constructed relative to purpose or willy-nilly serving one moral/political aim rather than another with no ontological or scientific justification for so doing?

#### V. Scientific realisms and anti-realisms

It may seem natural to some to take practically anything that science talks about as a good candidate for existing in reality.<sup>20</sup> But much of philosophy of science has been in the business of stripping reality of one thing or another, leaving only some favored set of things as really there. The basic divide is between those who take the science—or favored bits of the science—as right and the rest as, at best, relative, and at worse, chimerical or nonsensical; and those who do the reverse.

Wilfrid Sellars distinguished between the scientific and the manifest image.<sup>21</sup> The manifest image involves the concepts that we have been in the habit of responding to the world with, refined but not discarded since the dawn of humanity. The scientific image involves the concepts that science uses to describe the world. These, so say scientific “realists,” describe what is really there (at least when science is done at its best). The others involve true descriptions only when they can be reduced to those of science (and in general it should not be supposed that this is possible). The concepts of so-called folk psychology are a special target nowadays, with the demand for the reduction of features of the mind to those of

the brain a special case. If the terms of folk psychology do not reduce to those of a proper science, then if they are truths at all, they must be relative. We might suppose them to be relative to perspective or experience, but for a staunch reductionist it is hard to see what there can be for them to be relative to.

Similar considerations apply to various other reductionisms, like that of chemistry or biology to physics, or of group characteristics to those of individuals or of the macro to the micro. Failing strict reduction, for those who think that only one kind or level of phenomena is basic, the other kinds or levels, it seems, can only be true relatively. The task is to explain what these truths can be relative to, and how. The alternative looks to be that we talk nonsense when we use these non-reducible concepts, which is itself not very palatable given how reasonable our discussions often seem.

Oppositely, there are those who are suspicious about the scientific world picture. Perhaps only claims about what is observable are true *simpliciter*, or are acceptable without relativization. Often the motivation is some version of the problem of induction: It is hard to see what the license can be for moving beyond what we can be sure about from our own observation.

Others take issue with specific kinds of scientific claims, or—more usually—admit only certain kinds. Causal talk has been one main target. Rudolf Carnap urged that many things that are expressed in the material mode—that is, many claims that to surface appearances are about the world—are more correctly construed in the formal mode, as about our representations.<sup>22</sup> So to claim that  $x$  causes  $y$  can mean roughly that  $y$  is deducible from  $x$  within our best theories. Causation thus becomes relative to theory, or as others might have it, to our choice of model.<sup>23</sup> Others take causation to be relative to what we can manipulate<sup>24</sup> or to a range of situations across which the regular association between the so-called cause and

so-called effect remains stable.<sup>25</sup> Others even more radically maintain that what we single out as causes is fixed by prejudice or political agenda.

Laws are another favorite target. I argue that we have insufficient empirical warrant for most of our abstract high-level laws, like the fundamental laws of physics.<sup>26</sup> Our warrant is far better for long, complicated, detailed claims about very specific systems in very specific circumstances. The more general our law claims, the more they try to encompass, the shakier they become. What is acceptable at the abstract level then becomes relative to a choice among a variety of desiderata, including the so-called epistemic virtues. This does not necessarily make the acceptability of either theoretical entities or theoretical features of the world (for example, the quantum state of a SQUID) relative.

Others despair of high-level laws because they have been replaced repeatedly as we have reconceptualized the world. But this need not lead to complete relativism about them. Rather, many look for something about the laws that is retained across conceptualizations. Currently one favorite is the structure or relations represented in laws, which may be true independent of conceptualization or acceptable across reconceptualizations even if the contents, the very features that figure in the structure, are not.<sup>27</sup>

These are but examples. Realisms versus anti-realisms are hot topics nowadays. Philosophers and scientists alike declare themselves realists or anti-realists about broad scientific categories such as entities,<sup>28</sup> laws,<sup>29</sup> mechanisms,<sup>30</sup> causes,<sup>31</sup> fields,<sup>32</sup> mathematical structures,<sup>33</sup> groups and institutions,<sup>34</sup> or minds.<sup>35</sup> They debate equally about very subject-specific topics such as the acceptability of concepts of group selection,<sup>36</sup> strings,<sup>37</sup> utility,<sup>38</sup> or the natural level of unemployment.<sup>39</sup> In each case, relativism lurks as a solution to account for why talk of the flawed category might nevertheless be acceptable.

An alternative is to call into question the very idea of scientific acceptance. What is it and where is it? Science, I would argue, is just a conglomerate of people, practices,

techniques, materials, technologies, books, journal articles, lab notebooks, lectures, professional meetings, funding bodies, conversations, and the like. Is a particular claim acceptable? Or accepted? That depends on the context and the purpose. Are we trying to solve a specific scientific problem ourselves? Then much of what matters are the claims and techniques we are master of. Or, are we recommending where a student should go to graduate school—to work with a group that supposes this rather than that about strings or about selection or about how to model the economy? This decision depends a lot on where funding is, what the students' capabilities are, and what are their aspirations and work habits. If we are sitting on a funding council, our considerations about what to “accept” will be different, and different yet again if we are dispensing funds earmarked for “blue skies” research. Shall we use a particular assumption in building our bridge, or rocket or auction? Costs and benefits, safety and responsibility loom large here.<sup>40</sup> In each case, the acceptability of the scientific claim or practice is relative, but relative in a myriad of different ways in a myriad of different contexts.

Many will find this extreme relativism unpalatable. They long for an encyclopedia in which is written all and only what science accepts or should accept, at least for the nonce, or even, ideally, just what is true, or significant and true. But who will do the peer review on this encyclopedia, and how?<sup>41</sup>

#### Notes

<sup>1</sup> See N. Cartwright, J. Cat, and T. Ubel, *Otto Neurath: Philosophy Between Science and Politics* (Cambridge: Cambridge University Press 1996).

<sup>2</sup> Pierre Duhem, *The Aim and Structure of Physical Theory* (Princeton: Princeton University Press, 1906/1982).

<sup>3</sup> W. V. O. Quine, *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press, 1953).

<sup>4</sup> Nelson Goodman, *Fact, Fiction, and Forecast* (Cambridge, Mass.: Harvard University Press, 1986).

<sup>5</sup> Glymour talks about the facts implying an *instance* of the hypothesis on which an induction is then performed. But many scientific methods can be reconstructed in the stronger way I describe.

<sup>6</sup> C. Will, "Relativity at the Centenary," *Physics World* (January 2005): 27–32; J. D. Fairbank, B. S. Deaver, Jr., C. W. F. Everitt, and P. F. Michelson, *Near Zero: New Frontiers of Physics* (New York: W. H. Freeman, 1988).

<sup>7</sup> Cf. Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970); H. Longino, *Science as Knowledge* (Princeton: Princeton University Press, 1990); E. McMullin, *The Social Dimensions of Scientific Knowledge* (Notre Dame, Ind.: University of Notre Dame Press, 1992).

<sup>8</sup> I. Lakatos, *The Methodology of Scientific Research Programmes* (Cambridge: Cambridge University Press, 1978).

<sup>9</sup> L. Laudan, "A Problem Solving Approach to Scientific Progress," in *Scientific Revolution*, ed. I. Hacking (Oxford: Oxford University Press, 1981).

<sup>10</sup> M. Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953) and other instrumentalists.

<sup>11</sup> P. Kitcher, *Science, Truth, and Democracy* (New York: Oxford University Press, 2003).

<sup>12</sup> Cf. C. Smith and N. M. Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989); H. Chang, *Inventing Temperature: Measurement and Scientific Progress* (New York: Oxford University Press, 2008); H. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Chicago: University of Chicago Press, 1985); H. Collins and T. Pinch, *The Golem: What Everyone Should Know About Science* (Cambridge: Cambridge University Press, 1993); P. Galison,

*How Experiments End* (Chicago: Chicago University Press, 1987); S. Shapin and D. Schafer, *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life* (Princeton, N.J.: Princeton University Press, 1985).

<sup>13</sup> R. N. Giere, *Scientific Perspectivism* (Chicago: University of Chicago Press, 2006).

<sup>14</sup> Cf. S. Harding, *The Science Question in Feminism* (Ithaca, N.Y.: Cornell University Press, 1986); *Feminism and Methodology: Social Science Issues* (Bloomington: Indiana University Press, 1987); Noretta Pinnick, Cassandra L. Koertge, and Robert F. Almeder, *Scrutinizing Feminist Epistemology: An Examination of Gender in Science* (New Brunswick, N.J.: Rutgers University Press, 2003).

<sup>15</sup> See Cartwright, Cat, and Uebel, *Otto Neurath*.

<sup>16</sup> Duhem, *The Aim and Structure of Physical Theory*.

<sup>17</sup> Max Weber, "Objectivity," in *The Methodology of Social Sciences*, eds. and trans. E. A. Shils and H. A. Finch (Glenco, Ill.: Free Press, 1949).

<sup>18</sup> See A. B. Atkinson, *Poverty in Europe* (Oxford: Blackwell, 1998).

<sup>19</sup> See J. Reiss, *Error in Economics* (Oxford: Routledge, 2007).

<sup>20</sup> Cf. A. Fine, *The Shaky Game* (Chicago: University of Chicago Press, 1986).

<sup>21</sup> W. Sellars, *Science, Perception, and Reality* (London: Routledge and Kegan Paul, 1963).

<sup>22</sup> R. Carnap, *Philosophy and Logical Syntax* (London: Kegan Paul, 1935).

<sup>23</sup> Cf. P. Suppes, "A Probabilistic Theory of Causality," *Acta Philosophica Fennica* 24 (1970); J. Heckman, "Econometrics, Counterfactuals, and Causal Models," Keynote Address, International Statistical Institute, Seoul, Korea, 2001.

<sup>24</sup> Cf. P. Menzies and H. Price, "Causation as a Secondary Quality," *British Journal for the Philosophy of Science* 44 (1993): 187–203.

<sup>25</sup> Cf. J. Woodward, *Making Things Happen* (Oxford: Oxford University Press, 2004).

<sup>26</sup> N. Cartwright, *How the Laws of Physics Lie* (Cambridge, Cambridge University Press, 1983).

<sup>27</sup> Cf J. Worrall, “Structural Realism: the Best of Both Worlds,” in *The Philosophy of Science*, ed. D. Papineau (Oxford: Oxford University Press, 1996); James Ladyman, “Science, Metaphysics and Structural Realism,” *Philosophica* 67 (2002): 57–76.

<sup>28</sup> I. Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983).

<sup>29</sup> D. M. Armstrong, *What Is a Law of Nature?* (Oxford: Blackwell, 1983); M. Tooley, *Causation* (Oxford: Clarendon, 1987); F. Dretske, “Laws of Nature,” *Philosophy of Science* 44 (1977): 248–268.

<sup>30</sup> C. F. Craver and W. Bechtel, “Mechanism,” in *Philosophy of Science: An Encyclopedia*, eds. S. Sarkar and J. Pfeifer (New York: Routledge, 2006); P. Machamer, L. Darden, and C. Carver, “Thinking about Mechanisms,” *Philosophy of Science* 67 (2000): 1–25.

<sup>31</sup> For antirealism see Wolfgang Spohn, “Bayesian Nets Are All There Is to Causal Dependence,” in *Stochastic Causality*, eds. Maria Carla Galavotti, Patrick Suppes, and Domenico Costantini (Stanford: CSLI, 2001); for realism, N. Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon, 1989).

<sup>32</sup> M. Hesse, *Forces and Fields* (Edinburgh: Thomas Nelson, 1961).

<sup>33</sup> S. Shapiro, *Thinking about Mathematics* (New York: Oxford University Press, 2000); M. D. Resnick, *Mathematics as a Science of Patterns* (New York: Oxford University Press, 2000).

<sup>34</sup> M. Gilbert, *On Social Facts* (Princeton: Princeton University Press, 1989); P. Sheehy, *The Reality of Social Groups* (Hampshire, UK: Ashgate, 2006); R. Grafstein, *Institutional Realism: Social and Political Constraints on Rational Actors* (New Haven, Conn.: Yale University Press, 1992).

<sup>35</sup> Cf. S. Hampshire, *Spinoza and Spinozism* (Oxford: Clarendon: 2005); P. M. Churchland, *Matter and Consciousness* (Boston: MIT Press, 1988).

<sup>36</sup> E. Sober, *The Nature of Selection* (Chicago: University of Chicago Press, 1993); S. Okasha, *Evolution and Levels of Selection* (New York: Oxford University Press, 2006).

<sup>37</sup> L. Smolin, *The Trouble with Physics* (New York: Mariner, 2006).

<sup>38</sup> A. Sen, “Rational Fools: A Critique of the Behavioral Foundations of Economic Theory,” *Philosophy and Public Affairs* 6, no. 4 (1977): 317–344.

<sup>39</sup> J. Reiss, “Natural Economic Quantities and Their Measurement,” *Journal of Economic Methodology* 8, no. 2 (2001): 287–311.

<sup>40</sup> Cf. Douglas, H., “Rejecting the Ideal of Value Free Science,” in *Value-Free Science: Ideal or Illusion?* eds. H. Kincaid, J. Dupre, and A. Wylie (New York: Oxford University Press, 2007).

<sup>41</sup> Thanks to Fernando Morett for his help. Also note: references herein are samples to help the reader get started, not meant to be comprehensive.