"Methodological Realism and Modal Resourcefulness: Out of the Web and into the Mine"[1]

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Abstract

Psillos (1999, 2011), Kitcher (1993), and Leplin (1997) have defended convergent scientific realism against the pessimistic meta-induction by arguing for the divide et impera (DEI) strategy. I argue that DEI faces a problem more serious than the pessimistic meta-induction: the problem of *accretion*. When empirically successful theories and principles are combined, they may be inconsistent with each other, or the combination may be an empirical failure. The shift from classical mechanics to the new quantum theory does not reflect the discarding of "idle wheels." Instead, scientists had to contend with new principles that made classical calculations difficult or impossible (the Maxwell-Boltzmann equipartition theorem), and new results (the anomalous Zeeman effect) that were inconsistent with classical theorems (the Larmor theorem), and that suggested a new way of conceiving of atomic dynamics. In this shift, reference to atoms and to electrons was preserved, but the underlying causal explanations and descriptions of atoms and electrons changed. I propose that the emphasis on accurate description of causal agents as a virtue of background theory be replaced with Ruetsche's (2011) advocacy of pragmatic, modal resourcefulness.

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1. Introduction

Arguments about scientific realism have centered on the dialogue between Putnam's no miracles argument and Laudan's confutation of convergent realism. I investigate a distinct, but related tack we might take in analyzing arguments about convergent realism based on the history of science. An often cited virtue of scientific theories is their ability to produce causal explanations. Stathis Psillos (1999) links explanation to entity realism, arguing that scientific methodology allows for associating stable causal descriptions with terms for scientific entities and processes. Psillos and Jarrett Leplin argue that the methodology of science requires that scientists be committed to the theory-independent reality of those entities and processes. In particular, Psillos (2011, p. 309 and *passim*) argues that realism grounds the coherence of our picture of the microscopic and the macroscopic.

The confutation of convergent realism has supported anti-realist arguments that entities, processes, and relations to the reality of which past scientists were committed no longer feature in empirically successful theories. I cite another feature of the history of science, possibly more damaging to realism linked to scientific explanation: the problem of accretion. Two or more empirically successful theories or principles, when combined, may fail to be explanatory or empirically successful, even in contexts in which one of the original theories or principles was successful.

Ruetsche (2011) has argued that a most empirically successful theory, quantum field theory combined with quantum statistical mechanics ("QM_∞"), has and can have no single, unifying physical interpretation. She argues, in consequence, for a theoretical virtue I will call "modal resourcefulness": that a theory be able to function as a guide in varying modal contexts, without requiring a unifying physical interpretation of the theory as a depiction of reality. I argue that there is a tradeoff, in theory building, between the modal resourcefulness of a theory, and the specificity of the causal explanations and causal descriptions associated with it. I conclude that emphasizing the modal resourcefulness of a theory solves the accretion problem: inconsistent principles or explanations may be associated legitimately with a theory if they are considered to be valid in distinct modal contexts. Requiring a unifying realist interpretation and a single set of associated causal descriptions impairs the modal resourcefulness of a theory. There are reasons to temper methodological realism in favor of modal resourcefulness in theory-building.

2. Empirical success and the realist debate

Convergent realists connect arguments for realism to the empirical success of science. Abductive arguments include Putnam's general "no miracles" argument, but also more local arguments that take the form of inference to the best explanation, for instance. If these arguments succeed, they constitute a "scientific" argument for realism: if we read the history of science as a body of scientific evidence, we can conclude on the basis of this evidence that there is justification for realism as a philosophical position (Putnam 1975, 73).

Putnam argues that if the statements of science did not refer to objects, or were not statements about the world with "objective content", the continued success of science would be a miracle (Putnam 1975, 73). Following the empiricists, we could take the statements of science to have only observables as their referents, and remain agnostic about whether these observables are linked to or grounded by real objects or relations. An empiricist might assert that scientists construct definite descriptions of scientific objects and phenomena by making substantive reference to conventions or implicit definitions, using a priori laws and axioms (see Putnam 1976, 182-3). If a theory of the objects thus defined were to succeed, then definite descriptions, parts of which are constructed analytically and a priori without reference to objects of interest with which we are in causal contact, would happen to dovetail with scientific laws so that the theories can generate novel, well-confirmed empirical predictions. Putnam argues that such an account of the history of science makes the success of science seem miraculous.

The "pessimistic meta-induction" associated with Laudan (1981) is a "confutation" of convergent realism. For Laudan, convergent realists claim that "scientific theories […] are typically approximately true"; "the observational and theoretical terms within the theories of a mature science genuinely refer"; and that "successive theories in any mature science will be such that they 'preserve' the theoretical relations and the apparent referents of earlier theories" (Laudan 1981, 20-1). These claims entail two "elaborate abductive arguments", one of which is:

1. If the earlier theories in a 'mature' science are approximately true and if the central terms of those theories genuinely refer, then later more successful theories in the same science will preserve the earlier theories as limiting cases;
2. Scientists seek to preserve earlier theories as limiting cases and generally succeed.
3. (Probably) Earlier theories in a 'mature' science are approximately true and genuinely referential (Laudan 1981, 21-2).

Laudan observes that the realist takes a risk in linking the success of science to realist claims. If "mature," empirically successful theories of the past, which made new predictions and produced detailed explanations, can be shown to contain non-referring terms, and thus to make false claims, the realist argument will fail. This is the pessimistic meta-induction (PMI). Laudan points out that many of the terms of past scientific theories no longer are taken to refer: "ether," "caloric," and "phlogiston" are cited often in this context. Scientific propositions that contain these terms now are considered to be false: "The ether is a carrier of transverse waves", "Caloric flows from hotter bodies to colder bodies". But Fresnel's ether theory, especially, gave detailed explanations and made well-confirmed novel predictions.

Methodological scientific realism is one form of what often is called "robust" scientific realism. Robust scientific realism is defined here as:

1. Committed to the theory-independent existence of the scientific entities to which scientific terms refer.
2. Committed to the truth, or at least the "truthlikeness" in Psillos's terms, of scientific claims.
3. Committed to the claim that what is preserved through scientific theory change is not limited to mathematics or structure.

Leplin and Psillos recognize the challenge implicit in the above discussion: robust scientific realists should give a substantive account of how the progress of science tracks truth, an account not limited to the preservation of reference or the continued validity of laws. Leplin and Psillos argue for a methodological realism that derives its force from analyzing the methods behind the empirical success of science. Leplin (1986) argues that realist assumptions underlie the "rational reconstructibility" even of quantum mechanics (p. 37). For Leplin, scientists' success must be attributed in part to methodological realist commitments. He attributes to scientists a

> methodological prescription to seek a unifying theory where our current theoretical account of a domain of phenomena is so far too eclectic to be believed. Methodological realism appeals to truth as a goal. If empirical adequacy alone is sought, what motivates attempts at unification? (p. 40).

Instrumentalists or empiricists may respond that unification need not be valued for realist reasons, but for reasons of simplicity, for instance. Leplin responds that the empirical success of science can be explained as a product of rational behavior only by attributing realist commitments to scientists (Leplin 1986, 43).

However, science could be reconstructed entirely as a rational practice, and the theories arrived at by that practice could be empirically successful and false. Scientists can aim at successful, true theories, follow a rigorous methodology in pursuing them, and fail. In response, Leplin gives at a methodological prescription with two suggestive features: first, that commitment even to entities in which one does not believe fully serves as a heuristic guide for probing the world experimentally (pp. 46-9), and second,

> at the level of methodology, if not epistemology, a distinction must be drawn among theoretical entities between those treated realistically and those proposed as mere possibilities to be exploited for their heuristic and explanatory potential (p. 49).

The second move is a form of the divide et impera strategy, formulated by Psillos, Kitcher, and Leplin in the context of the Putnam-Laudan debate. The divide et impera (DEI) move attempts to separate constituents of scientific theories from one another in characterizing empirical success:

> Philip Kitcher and Stathis Psillos contend that we can justifiably believe those, and only those, constituents that are deserving of credit for the significant successes of the theory. Kitcher says, we must "distinguish between those parts of theory that are genuinely used in the success and those that are idle wheels" (p. 143, footnote 22). […] And if, say, the proposition "the ether exists" (Kitcher 1993) was not "deployed in" – that is, if it were not among the constituents responsible for – successful predictions, it is not deserving of credit for those predictions. And despite its falsity, it would not stand as a counterexample (or an apparent "miracle"). The historical argument against realism is thought to be deflected (Lyons 2006, 538).

Psillos (1999) goes further, to argue for the claim that certain hypotheses of a theory (H'), when conjoined with existing theoretical commitments (H), are not only *deployed in*, but also *essential to* the derivation of certain results of a scientific theory, formulated in propositions (P), derived with the help of auxiliaries (A). As Lyons summarizes the position,

> For H to be essential
> 1. It must be the case that H + H' + A leads to P.
> 2. It must not be the case that H' + A, alone, leads to P.
> 3. It must not be the case that any alternative, H*, is available
>   a. that is consistent with H' + A and
>   b. that when conjoined to H' + A leads to P and
>   c. that is non ad hoc (which for Psillos means, among other things, that it does not use the data predicted by P (Psillos 1999, 106), that [it] is potentially explanatory, etc.) (Lyons 2006, 539).

This picture of DEI treats science as a working mechanism that can be analyzed into the parts that are "deployed" or "implicated" in scientific results, and the parts that are idle wheels.

Psillos (1999, chapter 12) links DEI to the theory of reference. Psillos distinguishes a theory's preservation of (bare) descriptions or of (bare) reference from its preservation of *causal descriptions* linked to explanation. Kripke (1980) proposes "determining the reference of a name by description, and not by ostension":

> Neptune was hypothesized as the planet which caused such and such discrepancies in the orbits of certain other planets. If Le Verrier indeed gave the name "Neptune" to the planet before it was ever seen, then he fixed the reference of "Neptune" by means of the description just mentioned. At that time he was unable to see the planet even through a telescope. At this stage, an a priori material equivalence held between the statements "Neptune exists" and "some one planet perturbing the orbit of such and such other planets exists in such and such a position" (p. 79n).

Kripke makes this remark in the context of distinguishing apriority from necessity. But the focus here is Kripke's remark that "Neptune was introduced as a name rigidly designating a certain planet" (p. 79n), and that rigid designation takes place via a description.

Psillos argues that Le Verrier's naming of Neptune in this context is not a bare description or conventional baptism. It is an implicit appeal to inference to the best explanation, and it includes an implicit causal description:[2]

(1) *Inference to the best explanation*. Le Verrier's characterization of his causal relationship to Neptune depends on Laplacean mechanics and Newtonian gravitation, applied to the available data. Le Verrier hypothesized that the best available explanation for the perturbation was that a planet caused it.

(2) *Causal description*. The implicit causal description of that "planet" is "the massive object capable of causing the observed orbital perturbations according to Laplacean mechanics and Newtonian gravitation."

Psillos's strategy picks up on the link between causal descriptions, attributions of responsibility, and inference to the best explanation:[3]

> If we take the line that reference is fixed by means of detailed descriptions associated with a theoretical term, then, typically, it will turn out to be the case that no entity posited by a newer theory will satisfy them. […] Not only, however, were the advocates of both the new theory and the old one dealing with the same phenomena, they were trying to identify the causes behind these phenomena. To that end, they posited certain causal agents to which they attributed several properties which were taken to bring about these agents' effects. […] The claim here is not merely that some subsequent posit has taken the place of the entity posited by the older theory as the putative cause of a set of phenomena. What is important is that the subsequent posit is invested with some of the

[2] Psillos's causal descriptivism owes a debt to David Lewis, and seminal work on causal descriptivism about theoretical terms is found in Enç 1986, Kroon 1987 and 1989, and Nola 1980. I am grateful to an anonymous referee for suggesting consultation of these works.

[3] Chapter 4 of Psillos (1999) contains further discussion of the relevance of IBE for scientific realism.

> *attributes* ascribed to the abandoned putative entity, attributes by virtue of which the abandoned entity was thought to produce its effects (p. 294).

For Psillos, what warrants realist commitment in this context is the causal description of Neptune: "the massive object capable of causing the observed orbital perturbations according to Laplacean mechanics and Newtonian gravitation." As Psillos puts it,

> The reference-fixing mechanism should have the following form:
>
> R(x) = x causes phenomena Φ *and* D(x)
>
> Term t refers to x if and only if R(x).[4]

Reference is not fixed, as it is for the causal theory of reference, merely by the subject's standing in an appropriate causal relation to x. "Reference is fixed by means of descriptions of the causal role attributed to the putative referent," and "The descriptive component D(x) is required because the referent (the cause of Φ) should be attributed some properties – those that capture its causal role – if cognitive (as opposed to merely causal) access to it is to be had" (*ibid*.).

Psillos's causal descriptivism can be formulated as follows.[5]

> For $\{A_i\}$ to be a set of necessary attributes that warrant belief in the reality of an entity x with those attributes,
>
> 1. [IBE] $\{A_i\}$, along with a set of relevant contextual facts, must identify x as causally sufficient for the observed phenomena Φ.
> 2. [IBE] $\{A_i\}$ must describe x as the best explanation for the observed phenomena Φ; that is, no available alternative object and causal attribute set, x^* and $\{A_i\}^*$, should be a better explanation of the observed phenomena Φ.
> 3. [DEI] If one of the A_i, say A_1, should turn out to be unneccessary to (1) and (2) above, then only the set $\{A_i-A_1\}$ warrants belief.
> 4. [DEI] If only the set $\{A_i-A_1\}$ warrants belief, then x should be described as having only the attributes $\{A_i-A_1\}$.

The move to block the PMI is straightforward. Step 3 is used to shave off unnecessary, false, or non-referring posits within a causal explanation. To warrant realist commitment, the existence of the ether need not be necessary to deriving results in Fresnel's theory. Instead, the causal attributes of Fresnel's ether must be essential to a sufficient causal explanation of the relevant phenomena. The causally relevant attributes of the ether include its capacity to allow for the propagation of transverse waves, and its behavior as a dynamic system governed by the

[4] Psillos (2012), 222.
[5] This formulation is based on Psillos's work, but is not to be found there.

Lagrangian. Commitment to whatever entity has these causal attributes is kept distinct from commitment to the specific entity, "the ether", hypothesized to be the carrier of these attributes.

A number of critical responses to DEI have been put forward (Chang 2003, Carrier 2004, Chakravarrty 2007, Lyons 2006, Saatsi and Vickers 2011, Cordero 2011). Many of the current objections to Psillos's, Leplin's, and Kitcher's DEI move focus on their claim that realist commitment is limited to working posits, not idle wheels, and that discarded entities and relations of past theories (phlogiston, caloric) were idle wheels. But, the objections go, there is historical evidence that scientists treated these entities as working posits. Scientists appear to have been committed to the actual existence or to the causal-explanatory significance of elements of theories that DEI identifies as idle wheels.

Psillos and Leplin have a response to these objections. It is possible for scientists to propose idealized or even fictional or counterfactual models, and to be committed to them as instruments for inquiry. In the initial stage of inquiry, scientists can propose any models or hypotheses they choose to, for heuristic purposes. But after sufficient investigation has taken place, once scientists have built an explanatory theory, the criteria for arguing that a theory justifies ontological commitment to an entity become stricter. Psillos defends three criteria:

1. The entity must be indispensable to a justified causal-nomological explanation. There must be a well-founded argument that the entity is responsible for a stable effect.
2. The entity, under a certain causal description, must be the best explanation for a well-confirmed phenomenon. This description must provide the right kind of link between the properties of the entity and the causal properties appealed to in the explanation.
3. The entity's causal properties as given in the description in (2) must be coherent with an overall "causal-nomological framework" of scientific explanation (Psillos 2011).

The initial "anything goes" phase of theory construction thus gives way to the mature phase, in which causal explanation requires ontological commitment. On Psillos's account, we should appeal to "the realist framework" to obtain "coherence" in our picture of the microscopic and macroscopic worlds, for instance (Psillos 2011, 309). But there is reason to doubt this, from the development of the theory that should bring strongest support: the theory of atoms.

3. The problem of accretion: atoms and electrons in classical and quantum theories

On a methodological realist's reading of the history, if we are in the right kind of causal contact with atoms and electrons, then

(1) The existence of atoms and electrons is the *best explanation* for the observed effects (Putnam 1975, Psillos 1999); and

(2) Reference to atoms and electrons is *indispensable to* our causal-nomological picture of the world (Psillos 2011, 309; see discussion just above).

(1) and (2) support the claim that atoms and electrons are responsible for the relevant effects. One indication that we are in the right kind of causal contact with atoms and electrons is that we are able to give the laws and principles governing their behavior and their interaction. This is necessary to showing that their existence and action is the best explanation for the observed effects. Moreover, a causal-nomological framework that supports, not only our assertion that we are in causal contact with atoms and electrons, but also our assertion that they are indispensable to the best explanation for observed phenomena given the laws in force, must survive theory change. As Leplin writes, "Where past theories have met the standards imposed for warranting theoretical belief, their eventual failure is not a total failure; those of their theoretical mechanisms implicated in achieving that warrant are recoverable from current theory" (1997, 145).

But what if one of the triggers of the development of a new theory is a new law or principle that makes the derivation of certain causal explanations using reference to the entities in question indeterminate or impossible? Boltzmann and Maxwell developed a classical program in statistical mechanics, including the theory of heat and, especially in Boltzmann's case, the theory of gases. Both used the equipartition theorem, according to which "at equilibrium, the total kinetic energy of a system is equally distributed among all its degrees of freedom" (Barberousse 2012, 277). At the macroscale, this theorem was empirically successful. Not only that, it was treated as a "remarkable theoretical result" that spanned multiple theoretical and empirical contexts (Brush, Garber, and Everitt 1986, xxi). As Staley (2008) points out, "the equipartition theorem… was *made* classical, a process that simultaneously extended the conceptual grasp of that word [classical] to cover far broader theoretical expanses than mechanics alone" (p. 376). The theorem contributed to the empirical success of the classical theory, and even to the extending of the domain to which the classical theory applied.

Nonetheless, the equipartition theorem always was controversial (Kuhn 1978, 150-1 and *passim*). A striking fact about its history is that its increasing empirical confirmation at the macroscale was accompanied by two compelling problems in explanation at the microscale: the problem of specific heats, and black-body radiation and the ultraviolet catastrophe. The specific heat of a substance is the quantity of heat required to raise the temperature of one unit of mass of the substance by one degree of temperature. The classical explanation of specific heats, using the equipartition theorem, was unsuccessful, and a solution to the problem had to wait for the quantum theory.

> Without any detailed knowledge about the internal structure of atoms and molecules, nineteenth-century physicists… had to content themselves with conjectures deriving from analogies with observable cases... One of the consequences of these macroscopic analogies was that an important theorem of statistical mechanics, the so-called "equipartition theorem,"… necessarily had wrong consequences whenever polyatomic gases were at work, because in that case, the number of degrees of freedom of their molecules was erroneously determined. It is only when quantum theory was adopted that the famous "specific heats problem" was solved, because the quantization of the authorized levels of the atoms' energies allowed [scientists] to understand why the equipartition theorem failed in classical statistical mechanics.[6]

> When combined with one of Clausius's formulas, [the equipartition theorem] gave a rigorous expression for the ratio γ of the specific heats of a gas… that was in sharp disagreement with experiment. This result… was to become the subject of interminable debate over the nineteenth century, *the difficulties only increasing as proofs of the equipartition theorem were widened and deepened by Boltzmann, Maxwell, and their followers*. Equipartition became one of the grand mysteries of classical physics and was not to be resolved until the emergence of quantum mechanics.[7]

Ass the equipartition theorem proved more and more empirically successful at the macroscale, the problem of specific heats became more and more intractable. It was only resolved with the development of the quantum theory. The equipartition theorem was empirically successful. Classical mechanics was empirically successful. Employed together to explain specific heats, the two produced "mysteries," failures of prediction that were not resolved until the emergence of the quantum theory.[8]

The classical analysis of black-body radiation raised a related, deep problem. Salmon (1998) puts it picturesquely:

> According to the theories of electromagnetic radiation available at the end of the nineteenth century, a light beam entering a dark box with a small hole will produce inside the box an infinite amount of radiant energy in the ultraviolet region of the spectrum, thus giving rise to a holocaust more terrible than the worst nuclear bomb. This consequence was later aptly called the "ultraviolet catastrophe" (p. 32).

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[6] Barberousse (2012), 277. The problem of specific heats is discussed by Gibbs (1960/1902), 162-86, and by Kelvin (1904/1884), 494-504 and 526-7.

[7] Brush, Garber, and Everitt 1986, xxi, emphasis added.

[8] Renn (2000), Uffink (2006, especially §2), and Darrigol and Renn (2014) emphasize the importance of Boltzmann's and Maxwell's equipartition theorem in the development of the quantum theory, especially in the work of Einstein.

It is in part the combination of classical statistical mechanics, as a theory of radiation, with the equipartition theorem that gives rise to the “catastrophe,” which is really a failure of calculation. The classical theory, when combined with the equipartition theorem, breaks down in the case of black-body radiation.

Planck, especially, continued to pursue classical approaches to the theory of radiation. But, as Büttner, Renn, and Schemmel (2003) point out, it was precisely the attempt to pursue a classical approach that led to crisis for the old quantum theory.[9] Generally, the “old” quantum theory was a hybrid between classical and quantum theories, in which Planck, Lorentz, Landé, and others tried to use techniques continuous with the classical theory to solve novel problems for atomic theory, including the failure of Larmor’s theorem (Massimi 2005, 38ff.) and the anomalous Zeeman effect (Massimi 2005, 47).[10]

The normal Zeeman effect is the “magnetic separation of spectral lines” (Kragh 2012, 156). Lorentz quickly explained the normal Zeeman effect in classical terms, using “the Larmor precession of negatively charged electrons in magnetic fields” (Hentschel 1998, 190). Larmor’s theorem is that “for every motion [of electrons] without the magnetic field there is a corresponding motion in the field, which is the original motion plus a uniform rotation”.[11] Electrons were an indispensable element of Larmor’s theorem. But,

> according to the classical theory, all systems of electrons would precess with the *same* angular velocity. (This is *not* true in quantum mechanics.) This result is related to a theorem in classical mechanics [Larmor’s theorem].[12]

The anomalous Zeeman effect “was first reported by the Irish physicist Thomas Preston, who in 1897 noticed that the sodium doublet lines D_1 and D_2 were split into a quadruplet and a sextuplet, respectively” (Kragh 2012, 156). The anomalous Zeeman effect was “anomalous” for the “old” atomic core model within quantum theory. The atomic core model was Heisenberg’s attempt to reconcile quantum theory with the classical atomic and electron theory, and to make causal explanations possible that were continuous with the classical framework.

[9] They write, “the crucial point of the early quantum revolution was indeed not a non-classical interpretation of Planck’s law. On the contrary, it was only the integration of the law into classical physics and the revelation of contradictions generated by this integration that altered its meaning and established its revolutionary character” (Büttner, Renn, and Schemmel (2003), 45). Planck’s law describes black-body radiation at thermal equilibrium.

[10] It is beyond the scope of this paper to give an exhaustive account of what makes a theory “classical” or “quantum”, and so I follow the usage of these terms in the sources cited.

[11] Feynman (2010), §34-7.

[12] Feynman (2010), §34-7.

In deriving an explanation for the anomalous Zeeman effect, Landé realized that his model including Larmor's theorem had the consequence that "**R** [the core angular momentum vector] precessed twice as fast… as **K** [the electron angular momentum vector] did" (Massimi 2005, 51). There were only two ways to explain this anomaly within the old quantum theory using the atomic core model:

> either to modify Larmor's theorem, or to postulate a further rotation of the core… Both alternatives were wrong and a conclusive understanding of this anomaly came only with Pauli's rejection of the atomic core model and the later introduction of the electron spin (Massimi 2005, 51).

The old quantum theory, continuous with the classical framework, was discarded by most (not all) quantum theorists, and new explanations were formulated.[13] As Feynman (2010) explains,

> The electron… has a spin rotation about its own axis (something like the earth rotating on its axis), and as a result of that spin it has both an angular momentum and a magnetic moment. But for reasons that are purely quantum-mechanical – *there is no classical explanation* – the ratio of **μ** [the magnetic moment] to **J** [the angular momentum] for the electron spin is twice as large as it is for orbital motion of the spinning electron… In any atom there are, generally speaking, several electrons and some combination of spin and orbit rotations which builds up a total angular momentum and a total magnetic moment. Although *there is no classical reason why it should be so*, it is always true in quantum mechanics that (for an isolated atom) the direction of the magnetic moment is exactly opposite to the direction of the angular momentum (§34-2, emphasis added).

There are explanations of atomic structure and dynamics available to the quantum theory that cannot be reconstructed within the classical theory. Feynman points out that the move to the new quantum theory even changes the theory's way of picturing forces and angular momentum:

> Now we would like to discuss the idea of angular momentum in quantum mechanics – or rather, the characteristics of what, in quantum mechanics, is called angular momentum. You see, when you go to new kinds of laws, you can't just assume that each word is going to mean exactly the same thing. You may think… "Oh, I know what angular momentum is. It's that thing that is changed by a torque." But what's a torque? In quantum mechanics we have to have new definitions of old quantities. It would, therefore, be legally best to call it by some other name such as "quantangular momentum"… because it is the angular momentum as defined in quantum mechanics. But if we can find a quantity in quantum mechanics which is identical to our old idea of angular momentum when the system becomes large enough, there is no use in inventing an extra word. We might as well just call it angular momentum (Feynman (2010), §34-9).

[13] I am describing the move to a novel, empirically successful theory, the new quantum theory. I do not wish to overstate the reasons for the move, nor the extent to which the old quantum theory was discarded – Dirac and others continued to try to find ways to incorporate classical explanations into quantum electrodynamics much later. This does not invalidate my point, which is that the new theory used distinct explanations that are empirically successful but that paint a non-classical picture.

In the development of quantum theory, reference to atoms and electrons was conserved, but the framework that supported the statement that these entities were indispensable to our best causal explanations of the phenomena was rebuilt from the ground up. The angular momentum of electrons, the properties ascribed to them, their dynamics, and the causal description of their behavior all were rebuilt in the new theory.

The intention to refer to atoms and electrons is conserved, and reference to atoms and electrons is also conserved, in the move from classical to quantum theory, so it may seem to be a good case for robust realists. But the methodological realist account also should explain *why* reference is conserved in this case. On Psillos's theory, for instance, reference is conserved through causal descriptions and through realist commitment to a causal-nomological framework for explanation. The innovative analysis of the ether in Psillos (1999) rests partly on the claim that "the term 'luminiferous ether' may be seen as referring to the electromagnetic field" (p. 286). The term "ether" referred to a system with certain causal properties, and with a certain kinematic and dynamic description. In particular, "the core causal description associated with the term 'electromagnetic field' takes up the core causal description associated with the term 'ether'" (*ibid.*). But ether scientists were not referring to the luminiferous ether, because the causal description they constructed really was appropriate to the field, and so they really were referring to the field all along.

The classical theory *is* taken to refer to the entities: to atoms and to electrons. But the "core causal description" of atoms and of electrons changes from the classical to the quantum theory. The classical theory involves commitment to the existence of atoms and molecules under a specific classical causal description, as the best explanation for heat and for temperature. The old quantum theory, which appealed to the core model of the atom, was an attempt to reconcile the quantum with the classical picture. But the old quantum theory was not as empirically successful as the new one. Once they had discarded the old theory and the core model, quantum theorists solved a number of problems, including deriving the spectrum of the hydrogen atom and the accounting for the anomalous Zeeman effect, that had not been solved within the old, hybrid quantum-classical framework.[14] But the causal-nomothetic explanatory framework

[14] The core model could be revived at some point, if a scientist wished to. But it can't be employed as it was by Landé and others, in a way that explains the Zeeman effect, with the same background assumptions they used.

changes from the classical to the new quantum theory, and the causal descriptions of atoms and electrons change with it.

The problem for realism posed by the pessimistic meta-induction may be less than the problem for realism posed by *accretion*. As theories develop, the same term or relation can be given empirically successful but internally inconsistent causal descriptions. Moreover, two empirically successful causal explanations or principles, when combined, can produce an empirical failure.

For instance, the Maxwell-Boltzmann equipartition principle, when used in the framework of the classical theory, resulted in unintelligible explanations in certain contexts. The hybrid theory provided a picture of the phenomena that was not just fuzzy at the edges, it was no longer possible to draw in a determinate way, because certain calculations went off to infinity. The propositions about black body radiation derived from the equipartition theorem in the classical context *cannot be approximately true*, because they are not intelligible, and so it makes no sense to say that they are true or false. The theory no longer works in that context.

But the hybrid theory combined the classical theory, which was empirically successful and made successful causal predictions about atoms and electrons, with the equipartition theorem, which became increasingly better confirmed over time and in distinct experimental domains. If the best explanation for the success of causal explanations is that the statements of the background theory are true, or at least correct, then why should combining an empirically successful theory with an empirically successful principle result in failure?

The history of the shift from classical to the new quantum theory is not quite consistent with the methodological realist story. On that story, quantum physicists would have retained a set of underlying classical basic laws, dynamic systems, and causal explanations that remained valid, but then would have had to discard the "idle wheels" of their theory, the ontological speculations that got in the way of the underlying truth or rightness captured by the empirically successful classical theory. But that is not what happened. Instead, the underlying picture of atomic and electron dynamics changed radically. Quantum physicists (1) gave up the classical model of the internal dynamics of the atom and replaced it with others, such as the Bohr model and the Heisenberg atomic core model, and (2) fundamentally changed a subset of causal explanations, descriptions of dynamical systems, and causal descriptions of atoms and electrons employed in the theory.

If a methodological realist account as described is to work, realist commitment to the extra-theoretical existence and causal efficacy of entities responsible for observed effects, under a specific causal description, must underwrite the persistence of those entities through theory change. There are two elements that must persist through theory change, then:

> 1. The causal descriptions of certain entities, including their role in the overall causal-nomothetic theoretical framework, since justification for the causal description is derived from the postulated entities' indispensability to causal explanations, and
>
> 2. The set of causal explanations involving those entities.

Neither of these persist through the move from classical to quantum theories of atoms and electrons.

My argument is not the well-known argument that quantum theory requires instrumentalism. My argument is that, even if we interpret the quantum theory realistically, there does not appear to be a sound argument for convergent methodological realism on the basis of the claim that a stable causal description of atoms and electrons underlies or grounds the transition from the classical to the quantum theory. The problem for this theory in the case of the classical-quantum shift is twofold:

1. No coherent configuration incorporating both the laws of interaction and the causal descriptions of atoms and electrons can be recovered, retrospectively, from the combination of the classical and the (new) quantum theory – in fact, revising these laws and descriptions was a central reason for the shift from the old to the new quantum theory.
2. No accurate history of the change from old to new quantum theory would describe the attempt to preserve the classical causal description of atoms and of electrons as the ground for the new quantum theory. Instead, the opposite is true: the shift from old to new quantum theory is motivated by the need to discard key elements of the classical explanatory framework, and of the classical picture of atoms and electrons, to solve new problems.

Methodological realism must derive its force at least partly from being an accurate description, not only of scientific practice, but of intentional scientific practice. According to methodological realism, the success of science requires that scientists be committed to realism. But the intention to preserve realist commitment to the existence of extra-theoretical entities captured by causal descriptions is not a motivation of, nor is it a basis for, the move from the classical to the new quantum theory.

4. Out of the web and into the mine

The argument from the empirical success of science to convergent realism relies on the claim that novel prediction requires "traction". The idea is that the best explanation for the fact that a theory allows scientists to make novel predictions and to formulate causal explanations is that the claims of the theory have "latched on," in Worrall's terms, to an underlying feature of reality. Psillos puts it this way:

> Suppose that a background theory *T* asserts that method *M* is reliable for the generation of effect *X* in virtue of the fact that *M* employs causal processes $C_1, \ldots, C_n$, which, according to *T*, bring about *X*. Suppose, also, that we follow *T* and other established auxiliary theories to shield the experimental set-up from factors which, if present, would interfere with some or all of the causal processes $C_1, \ldots, C_n$, thereby preventing the occurrence of effect *X*. Suppose, finally, that one follows *M* and *X* obtains. What else can better explain the fact that the expected (or predicted) effect *X* was brought about than that the theory *T* – which asserted the causal connections between $C_1, \ldots, C_n$ and *X* – was right, or nearly right? (Psillos 1999, 79).

Psillos points out two other requirements for such an abductive inference: *T* should be the single best explanation, and should be a sufficient explanation in its own right. In particular, the claims of theory *T* about entities should be restricted to those claims that employ a causal description according to which an entity of a certain kind is responsible for and even indispensable to a given causal process.

As argued above, the Maxwell-Boltzmann equipartition theorem is a counterexample to this reasoning.[15] Many of the causal explanations of the classical theory were empirically successful. Many empirical predictions and causal explanations using the equipartition theorem are empirically successful. But the combination of the classical theory and equipartition yields nonsense, in the case of black-body radiation. The best explanation for the empirical success of the two theories cannot be that both are correct. Why would the combination of two correct theories result in nonsense?

The no miracles argument depends on the empirical success of science. Causal explanations are one element of empirical success. How, then, do we solve the accretion problem? Ruetsche (forthcoming) points out that physical explanations may not be derived

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[15] Kuhn often cites another counterexample. In one of the first theories of the Leyden jar, the shape of the jar, the fact that it was made of glass, and the relation between the fluid and the jar's shape were all causally efficacious. Background theory *T* expressed all these claims. According to the theory, method *M*, building a Leyden jar, was reliable for generating effect *X*, that is, the function of the jar as a capacitor. On this old theory, one could follow *M* and *X* would obtain – those who endorsed the early theory could construct a Leyden jar, and it would function as a capacitor. But none of the causal claims they endorsed were right, or even nearly right.

directly from the abstract theory itself. Rather, in most cases, a theory must be given some physical interpretation. Moreover, "a successful scientific theory T underdetermines its own interpretation, and does so in such a way that different, and often incompatible, interpretations account for different elements of T's total success" (§3.3). The explanation cited by Psillos involves the background theory *T*, method *M*, and causal processes $C_{1,}$ .... $,C_n$. It also involves using "*T* and other established auxiliary theories to shield the experimental set-up from factors which, if present, would interfere with some or all of the causal processes $C_{1,}$ .... $,C_n$, thereby preventing the occurrence of effect *X*". But an experimental set-up involves material factors that must be connected to the statements of the theory – an experiment involves an implicit physical interpretation of the theory.

A robust realist account might have it that the possible physical interpretations of a theory are constrained by the causal descriptions of the agents involved, and by the laws and structure specified by the background theory. But Ruetsche (2011) argues that the interpretation of quantum theories appears to be irreducibly complex. *Interpreting Quantum Theories* focuses on the "interpretation of quantum field theory (QFT) and the thermodynamic limit of quantum statistical mechanics (QST)", collectively, "QM_∞" (p. 2). The overall argument is that there is "no single interpretation" of this combined theory that is adequate to all the circumstances for which physically inequivalent Hilbert space representations arise (p. 15). In consequence, either we must revise our notion of physical possibility, or we should revise the "standard", "pristine" semantic account, according to which theories give a picture of possible worlds that is invariant when applied to distinct material and factual circumstances. Ruetsche chooses the latter option, and proposes a way of interpreting quantum theories according to which the notion of physical possibility given a background theory "fractures" when applied to distinct "extranomic" material circumstances (p. 4).

Ruetsche's analysis is aimed principally at the general goal of the no miracles argument, to provide abductive confirmation of a general theoretical framework that is believed to be true. In the local case, Ruetsche appeals to a pragmatic modal notion of physical possibility:

> When physical possibility is pragmatized, there's a single way the world is, but (as gleaned by physics) there isn't a single set of ways it might be. Instead, there are many sets of ways it might be. Taking these sets seriously is part and parcel of commitment to a physical theory: to call something an "electron" is to take on commitments regarding how it would behave in a variety of circumstances, and to offer explanations, evaluations, and further theory constructions in ways constrained by those commitments. But we take on

> different, internally cohesive collections of such commitments in different circumstances. Provided we can keep the circumstances calling for commitments straight, this doesn't make us incoherent. It makes us resourceful (Ruetsche 2011, 353-4).

Ruetsche argues for a novel theoretical virtue, given this: good theories "foster manifold spaces of physical possibility". A good theory need not be a sharp camera that gives a univocal, determinate picture of a single way the world might be. Rather, a good theory can be a modal kaleidoscope that depicts manifold ways the world might be, and that guides scientific investigation in many or all of them.

One interpretation of the standard semantic account has had it that scientific theories are webs or fabrics of interwoven statements, which depict reality by specifying states of affairs that obtain if the statements are true. This view was expressed in a classic form by Wittgenstein, and was developed further in the work of Quine and others in the philosophy of science. The web of belief is supposed to impinge on reality at the edges, and its form is intended to be stimulated and corrected by observational data.

Exploratory science is more like a mine than it is like a web. That is, explanations constructed in particular material contexts, using specific physical interpretations of a theory, are mined until they no longer pan out, but mining may go on beyond the point at which a unifying, coherent web of physical interpretation can be woven. Moreover, if Ruetsche is correct, grand, overarching theories like QM_{∞} may have an infinite number of possible physical interpretations. Even our interpretations of terms of physical theories may vary, as "angular momentum", "atom", and "electron" do when moving from classical to new quantum theories. In the quotation above, Ruetsche emphasizes the conventional element of what we "call" an electron, for instance. But it is true that, once we have decided to call something an electron, and have chosen a specific description or physical interpretation, those choices constrain the inferential power of a given background theory of the electron.

That choice is like deciding to mine a particular seam. The metal we are after is the entity of interest; the reinforcing beams and structure are the laws assumed to be in place. If we consider empirical investigation to be a mining operation, not the weaving of a fabric, then it is no surprise – no miracle – when we are able to learn from the success or failure of a particular investigation, under a particular interpretation. For instance, if we find no gold in a particular place, we will close the shaft, even if it is still structurally sound: the laws may apply, but there is no more of what we're seeking to be found there. But if the shaft collapses, we know that the laws do not hold – this is what happened when scientists tried to reinforce a shaft with the Boltzmann equilibrium principle combined with the classical theory. It wasn't that the combination didn't reveal new effects. It wasn't possible for new effects to be revealed, because the entire shaft collapsed.

If we maintain the web of belief view, along with a general structural semantic view, then the choice to abandon a particular shaft or to build a new one seems ad hoc. Why should we choose to pull back from one particular interpretation or ontological commitment, or to build a model to test only one particular configuration of the laws and their physical interpretation? After all, what we are after is a comprehensive, maximally coherent web. But if we are mining and not weaving, then such choices are rational, though the rationality may be pragmatic. If we are mining a particular shaft and it is not panning out, then it's reasonable either to abandon that seam or to start digging elsewhere. When Pauli saw that the atomic core model was not going to pan out, he decided to pull back and to dig elsewhere.

The best mines are those that allow us to draw inferences about the possible construction of other mines. If we have constructed a mine using laws, and are digging for facts about atoms in that mine, then the fact that we can gather information about atoms using that structure has two consequences. One is that the theory being used is inferentially stable in the particular physical interpretation being used: that is, that the general theory can be given that particular physical interpretation and the inferences licensed by the principles of the theory will go through.

That inferential power may depend on the context. For instance, Landé adopted Heisenberg's atomic core model initially. When Landé tried to derive an explanation of the anomalous Zeeman effect using the atomic core model plus Larmor precession, he failed. But Lorentz was able to derive an explanation of the normal Zeeman effect using the extant electron theory. Classical electron theory has not been discarded, like the ether: it is still taught to students, with the caveat that classical explanations are not valid at the microscale. We can still mine using the classical theory, but not in some seams.

One virtue of the "new" quantum theory is that, not only does it solve novel problems, but it also expands our notion of the intelligible modal properties of atoms and electrons and of their behavior. This, in turn, sheds light on the powers and limits of the theory as an engine for inferences in specific material circumstances. Such illumination requires a clear picture of what counts as an atom or as an electron in any one context. And, in variable contexts, it requires that the theory be patched, if necessary, to retain an intelligible, coherent description of atoms and of electrons. The coherence of various theoretical descriptions of the same entity is not required in scientific practice. Scientists do employ inconsistent models and explanations of the same entity in distinct contexts. But, if an intelligible, consistent picture of an entity is not retained or extended coherently when contexts vary, the extension of explanations involving an entity to novel modal contexts may or may not contribute to our understanding of that entity or of the theory's modal properties. A second virtue of a theory associated with Ruetsche's modal resourcefulness, then, is semantic intelligibility, whether in a particular physical context or across contexts.

The virtues of inferential stability and of semantic intelligibility can be put to work as part of the goal of illuminating a theory's modal depiction of physical possibility, especially with respect to particular entities. There can be two distinct explanations of the same entity in distinct contexts, but even on Ruetsche's pragmatic account of modal resourcefulness, it will be most illuminating if scientists can show how the employment of explanations of the behavior of that entity across contexts reveals facts about the background theory's explanatory and inferential force. In the context of Ruetsche's novel theoretical virtue, we want to know how theories function as guides for inference and for investigation in variable physical contexts. Learning about this function requires stable, local information about how our picture of a given entity can be coherent, and whether a given coherent, stable description of an entity does or does not ground inferences about its behavior in distinct contexts. Without some local notion of semantic intelligibility, we can draw no inferences about a theory's broader inferential and heuristic power from its local empirical success.[16]

Choosing the virtue of modal resourcefulness solves the accretion problem. The accretion of principles that are empirically successful independently but not together, or of incoherent descriptions of entities, does not pose a problem for the mining enterprise. A theory should guide empirical investigation in multiple modal contexts. It should "foster manifold spaces of physical possibility" and make it easy for us to demonstrate how investigation across distinct contexts can license further investigation and inference. If the move from the classical to the quantum theory results in combinations of principles and theories that don't work together even if they may work

[16] The virtue of semantic intelligibility is intended to capture one aspect of Psillos's and of Leplin's view, that the success of science requires belief in entities.

separately (such as the classical framework plus the equipartition theorem), it is a virtue of the quantum theory that it can explain how, and why, the classical theory can be used in some contexts but not in others.

Theory building is often a tradeoff between rival virtues: simplicity can contend against explanatory depth, for instance. In this case, one of Ruetsche's preferred theoretical virtues, that a theory can act as a heuristic guide in distinct modal contexts (mines multiple ways the world might be), contends with one of Psillos's, that a theory give a targeted description of the causal agents and relations involved (depicts the way the world is). The more we describe a particular agent and its properties in a given material context and under a particular interpretation, the more our explanations begin to depend on specific features of that context and that interpretation.

According to the realist position, the goal of exploratory science ought to be to become acquainted with, and to describe, the causal properties of extra-theoretical, mind-independent entities. But the more scientists aim to do so, the more they hinge descriptions and explanations on particular cases and contexts – on specific physical interpretations. This makes the problem of assessing the relationship between particular evidence gathered under a specific physical interpretation (in a specific experimental context, for instance) and the overarching theory more pressing.

A scientist might construct a theory in an attempt to capture static causal descriptions of theoretical entities, and thus downplay the modal resourcefulness of the theory: its ability to guide research in distinct modal contexts, that is, in situations where the causal agents or material conditions are not as described or encountered previously. She might do so because she thinks the coherence of our picture of the macro- and microworlds depends on the realist depiction of theoretical entities: on depicting the world as it is. But Ruetsche's sharp analysis of QM_∞, and the history above, point in another direction. The coherence of our "depiction" is pragmatic: how well does a theory guide investigation in distinct contexts, given different ways the world might be? How good a map is it to the mining territory? In the tradeoff between modal resourcefulness and targeted description, modal resourcefulness should win. Philosophers should stop weaving and start mining.

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