No Refuge for Realism: Selective Confirmation and the History of Science

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Realists have responded to challenges from the historical record of successful but ultimately rejected theories with what I call the *selective confirmation* strategy: arguing that only *idle* parts of past theories have been rejected, while truly success-generating features have been confirmed by further inquiry. I argue first, that this strategy is unconvincing without some *prospectively applicable* criterion of idleness for theoretical posits, and second, that existing efforts to provide one either convict all theoretical posits of idleness (Kitcher) or stand refuted by detailed consideration of the very examples (optical/electromagnetic ether, caloric fluid) to which they appeal (Psillos). I also argue that available avenues for improving on these proposals are unpromising.

1. Introduction. The explanationist or "miracle" defense of scientific realism stands challenged by a historical record of theories ultimately rejected despite apparently enjoying, at one time, just those same kinds of success that the realist finds indicative of truth in their current counterparts (Laudan [1981] 1996). One recent response to the challenge,

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pursued in most sophisticated detail by both Philip Kitcher and Stathis Psillos (see also Leplin 1997, 130, 146–151), stands out for its willingness to take the details of the historical record seriously. These thinkers argue that while there are indeed genuine examples of mature past theories that enjoyed impressive empirical successes, only some *parts* of those superseded theories were involved in the successes they enjoyed, and those parts have turned out to be confirmed by further inquiry: The successes of geological theories laboring under the constraint that lateral movements of the continents not be permitted, for example, came in areas in which the constraint was irrelevant (Kitcher 1993, 142).

Kitcher and Psillos each challenge Laudan's ([1981] 1996, 116) claim that realists must embrace confirmational holism in order to allow a theory's successes to confirm its deeply theoretical claims and posits along with its observational implications. Indeed, Kitcher argues that Hempel showed long ago why any successful account of confirmation will have to recognize "that confirmation does not accrue to irrelevant bits of doctrine that are not put to work delivering explanations or predictions" (1993, 142n). And both go on to use historical examples to try to show that the successes of past theories did not depend on the parts or presumptions of those theories that were subsequently abandoned.

But this defense of realism faces a crucial unrecognized problem: Of any past successful theory the realist asks, "What parts of it were true?" and "What parts were responsible for its success?", but *both* questions are answered by appeal to our own *present* theoretical beliefs about the world. That is, one and the same present theory is used both as the standard to which components of a past theory must correspond in order to be judged true and to decide which of that theory's features or components enabled it to be successful. With this strategy of analysis, an impressive retrospective convergence between judgments of the sources of a past theory's success and the things it "got right" about the world is virtually guaranteed: It is the very fact that some features of a past theory survive in our present account of nature that leads the realist *both* to regard them as true *and* to believe that they were the sources of the rejected theory's success or effectiveness. So the apparent convergence of truth and the sources of success in past theories is easily explained by the simple fact that both kinds of retrospective judgments about these matters have a common *source* in our present beliefs about nature.[1]

1. The same challenge applies to Richard Boyd's persistent defense of several distinctive realist theses (maturity of recent theories, increasing reliability of scientific methods, "retrospectively sustained mutual ratification" of theoretical developments) as perhaps he acknowledges in insisting that realism comes as a "package" that must be compared against competing empiricist or constructivist packages.

This virtual guarantee of substantial convergence between our *retrospective* judgments of the truth of parts of past theories and the sources of their success makes clear that any convincing defense of realism by selective confirmation must offer criteria that *could have been* and *can now be* applied *prospectively*—in advance of future developments—to identify the idle features or components of scientific theories.[2] But I will now argue that this is just what existing appeals to selective confirmation do not (and perhaps cannot) provide.

2. Hardworking Posits. Kitcher invites us, for example, to distinguish "working posits (the putative referents of terms that occur in problem-solving schemata) from presuppositional posits (those entities that apparently have to exist if the instances of the schemata are to be true)," and he goes on to suggest that "[t]he moral of Laudan's story is not that theoretical positing in general is untrustworthy, but that presuppositional posits are suspect" (1993, 149). Of course, it seems easy enough to reconstruct the predictive and explanatory schemata of past theories so as to avoid appealing to entities abandoned by contemporary theories and then declare them "merely presuppositional," but Kitcher elaborates how such a merely presuppositional or idle status is to be established with a specific example, arguing that the mechanical ether in which nineteenth-century wave theorists held light waves and/or other electromagnetic phenomena to be propagated is "a prime example of a presuppositional posit" (1993, 149) because the wave theory's successes did not require (and therefore should not have been taken to confirm) its theoretical postulation of any such mechanical medium:

> Modern treatments do not make any reference to an all-pervading ether in which light waves are propagated. But, for Fresnel and many of those who followed him, the existence of such an ether was a presupposition of the successful schemata for treating interference, diffraction, and polarization, apparently forced upon wave theorists by their belief that any wave propagation requires a medium in which the wave propagates. All the successes of the schema can be preserved, even if the belief and the presupposition that it brings in its train are abandoned. That, to a first approximation, is what happened in the subsequent history of wave optics. (1993, 145)

2. John Worrall's Structural Realism suffers from a related problem, in that those "structural" elements of theories that Worrall claims are typically preserved in their historical successors seem *identifiable* as distinctively structural only in retrospect.

On this ground he replies to Laudan's invocation of James Clerk Maxwell's contention that "the aether was better confirmed than any other theoretical entity in natural philosophy":

> Although we can understand this claim, based as it was on the multiplicity of phenomena to which schemata appealing to wave propagation had been successfully applied, Maxwell was wrong. The entire confirmation of the existence of the ether rested on a series of paths, each sharing a common link. The success of the optical and electromagnetic schemata, employing the mathematical account of wave propagation begun by Fresnel and extended by his successors (including, of course, Maxwell), gave scientists good reason for believing that electromagnetic waves were propagated according to Maxwell's equations. From that conclusion they could derive the existence of the ether—but only by supposing in every case that wave propagation requires a medium. Thus the confirmation of the existence of the ether was no better than the evidence for that supposition. (1993, 149)

Thus, the ether was supposedly idle or merely presuppositional simply because there was an alternative account on which the empirical predictions and explanatory schemata of the wave theory survived elimination of this presupposition: to wit, that light and/or electromagnetic disturbances are transmitted *like* mechanical waves (and according to Maxwell's equations), but (somehow) without any medium of transmission at all. But *this* criterion of eliminability or idleness applies perfectly straightforwardly to virtually all those posits of contemporary theories that realists hope to defend as *genuinely* confirmed by their successes, including Kitcher's own paradigmatic examples of "working" posits: the electromagnetic field, genes, atoms, and molecules. We can just as easily suppose, for example, that characteristics are passed from parent to offspring in the patterns suggested by contemporary genetics, but (somehow) without the existence of genes themselves. The existence of genes *seems* to be confirmed by the role they play in innumerable explanations of the inheritance and production of the characteristics of organisms, but only by supposing in each case that the inheritance and production of phenotypic traits requires that there be some material causal basis for those characteristics that is transmitted between parents and offspring. Thus, if the existence of such a spare alternative as "transmitted like waves, but without any medium of transmission" suffices to render the ether "idle" or "presuppositional" (and therefore unconfirmed by the successes of the wave theory), then virtually *any* theoretical posit must be so regarded.

Indeed, this problem is even deeper than it first appears, for Maxwell himself *explicitly considered* whether the wave theory's successes could survive excision of the ether and *rejected the very intelligibility* of wave transmission without any mechanical medium. He concludes *A Treatise on Electricity and Magnetism* with the following remark:

> [H]ow are we to conceive this energy as existing in a point of space, coinciding neither with the one particle nor with the other? *In fact, whenever energy is transmitted from one body to another in time, there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other,* for energy, as Torricelli remarked, "is a quintessence of so subtle a nature that it cannot be contained in any vessel except the inmost substance of material things." Hence all these theories lead to the conception of a medium in which propagation takes place. . . . ([1873] 1955, 493; my emphasis)

Thus Maxwell regarded the *very idea* of wave transmission without any mechanical medium (or, "energy existing in a point of space") as incoherent. But if the alternative that eschews appeal to a given theoretical posit and thereby undermines its confirmation by the successes of the theory that posits it need not even be *intelligible* to the scientists who hold that theory, then the realist loses any hope of insulating any theoretical posits of current theories from idleness or merely presuppositional status.

Can we improve on Kitcher's account? Perhaps the ether is merely presuppositional because the theory also ascribes no *direct* causal role to it in the production of optical and electromagnetic phenomena. Although natural, this proposal seems to run afoul of any number of discarded theoretical posits that *were* ascribed direct causal roles in the production of phenomena by the successful explanatory and predictive schemata of their respective theories: Alongside such familiar examples as phlogiston and caloric, we might consider the vital forces of late-eighteenth- to mid-nineteenth-century physiology and embryology.

Such theorists as Reil, Blumenbach, Kielmeyer, and von Baer argued that the directive or teleological character of organic causal processes required the postulation of special formative or developmental forces *(Lebenskraft, Bildungstrieb, Gestaltungskraft)* unlike those operative in the inorganic realm. Like ether theorists, defenders of vital forces insisted explicitly on the *necessity* of postulating them, as the only way to account for the many organic phenomena in which an end product or goal, or a functional relationship not yet realized, seemed to direct specific causal pathways of interaction and development. While they insisted that such forces must be an ordinary part of the material world (rejecting the vitalism of Stahl and Wolff, which attributed the organization of organic entities to their direction by an immaterial soul), they held equally forcefully that neither vital forces themselves nor the organic processes they directed were reducible to or explicable by ordinary inorganic chemical and electromagnetic forces, affinities, or interactions, notwithstanding, they argued, Wöhler's successful inorganic synthesis of urea (see Lenoir 1981). Reil and Blumenbach defended the postulation of such forces by explicit analogy to Newtonian gravitation: That inert matter exerts and experiences an attractive force did not itself admit of further mechanical explanation, but the existence and properties of this force, like those of the vital forces responsible for organic phenomena, are nonetheless known through its effects. And von Baer went so far as to provide what he regarded as compelling empirical evidence for the physical location of such a force at the center of the mammalian ovum.

Such forces were ascribed a direct causal role in phenomena ranging from the production of functionally uniform organisms from widely varying states of early individual embryos to the production of related basic physiological and anatomical body plans in various animals. Perhaps most impressively, postulating a small number of specific vital forces directing and constraining the pathways of mechanical causal growth and development in individual organisms and passing through generations in the germinal materials, whose operation was systematically modified in interaction with local environmental or geological conditions according to general laws, offered a convincing causal explanation of the remarkable correspondence between animals' basic anatomical/physiological plans, their taxonomic groupings, the patterns of diversity found in the paleontological record, and the various stages undergone in their embryological development in both normal and teratological (monstrous) cases. Thus, causal role considerations will not help Kitcher, for it seems that successful schemata of past theories *have* routinely ascribed direct causal roles to subsequently abandoned theoretical entities.

Of course, none of this controverts Kitcher's Hempelian suggestion that any successful account of confirmation will have to refuse confirmation to unconnected parts of a successful theory that are not "put to work" in delivering its predictions and explanations. *What our historical cases suggest, however, is that the rejected posits of past theories, like ether and vital forces (and caloric fluid, see below), were simply not any less intimately involved in the predictive and explanatory accomplishments of those theories than genes, atoms, molecules, and the electromagnetic field are in our own. Thus, we have no reason to think that whatever solution we ultimately embrace to the quite general problem of avoiding excessive holism in our account of confirmation would prevent confirmation from accruing to the discarded theoretical posits of actual past scientific theories.*

3. Trusting Scientists. Stathis Psillos's intriguing alternative approach promises to finesse these persistent problems by evading the need for any *explicit* criterion of selective confirmation at all. He argues that working scientists *themselves* routinely judge different parts, features, or aspects of extant theories to be differentially confirmed by the empirical evidence, and that the historical record shows these judgments to be generally reliable. If so, we could have in the past and can now safely rely on *scientists' own* judgments in identifying the selectively confirmed, trustworthy aspects of existing theories, *however* they manage to reach such judgments. Sadly, Psillos's defense of this creative strategy itself turns out to depend upon a highly selective reading of the historical record.

We have, of course, already seen enough historical evidence to be suspicious of Psillos's claim: After all, it was nineteenth-century physiologists and embryologists *themselves* who argued that vital forces were absolutely required by the distinctively teleological or directive character of organic causal processes, and it was nineteenth-century physicists *themselves* who judged the ether as well-confirmed by the successes of the wave theory as it is possible for theoretical entities to be. For Maxwell (a leading scientific intellect and arguably the foremost expert at the time) to have misjudged (by realist lights) the confirmational status of the ether in so spectacular a fashion casts considerable doubt on the idea that scientists' own judgments of selective confirmation have been historically reliable.

Psillos does not address nineteenth-century physiology and embryology, but his discussion of the ether quite rightly notes the agnosticism of many ether theorists regarding the exact constitution of the ether as well as caution and even outright skepticism regarding the details of such particular mechanical models of the ether as Green's elastic solid, McCullagh's rotational ether, and Stokes's elastic jelly. But this proves too little for the realist's needs, for this skepticism regarding particular mechanical models of the ether was more than matched by the same theorists' confidence in light of the available evidence that there must be *some mechanical medium or other* in which light waves were propagated: Green's claim, for instance, that we are "perfectly ignorant of the mode of action of the elements of the luminous ether on each other" (Psillos 1999, 132) expresses cautious agnosticism regarding the existing models of the ether's mechanical interaction, but *not* regarding the existence of the ether itself. Similarly, Psillos points out the difference in Maxwell's attitude towards his accounts of the dynamics of electromagnetic propagation in the ether and of the constitution of the ether itself (1999, 138), but that is not enough: Although Maxwell was (rightly) not at all confident that he had successfully identified any correct account of the mechanical medium in which electromagnetic waves were propagated, we have seen that he was nonetheless as confident as he could be that the evidence supported (indeed *necessitated*) the existence of *some such mechanical medium or other*. Thus, Psillos's claim that history has vindicated the selective confirmational judgments of ether theorists can only be defended by conveniently ignoring many such judgments made by the very scientists to whose more cautious and skeptical attitudes he appeals.

The same sort of selective attention is at work in the one other case Psillos considers in detail: the material fluid or caloric theory of heat. In this connection he effectively documents Joseph Black's scrupulous refusal to regard his experiments as conclusively establishing a material fluid theory of heat against the competing "dynamic" account of heat as motion. What Psillos misses, however, is that this restraint simply reflects Black's unusually strict but characteristically eighteenth-century Scottish commitment to Newtonian inductivism, that is, an official hostility towards *all* theories and theorizing *in general,* famously expressed in his warning to his student and editor John Robison to "[reject] even without examination, every hypothetical explanation, as a mere waste of time and ingenuity" (Black 1803, vii). Black ultimately judged Cleghorn's material fluid account of heat "the most probable of any that I know," but immediately reasserted his officially antitheoretical stance, reminding us that "it is, however, altogether a supposition" (1803, 33) and insisting later that all such hypothetical suppositions were best avoided "as taking up time which may be better employed in learning more of the general laws of chemical operations" (1803, 192–193). Even more troubling for Psillos's thesis is the confidence with which Black *rejected* the dynamical theory in light of the evidence: He argued that his own discoveries of latent heat could not possibly be squared with the dynamical account (McKie and Heathcote [1935] 1975, 28), while insisting that he could not conceive of an "internal tremor" of the particles of substances "that has any tendency to explain, even the more simple effects of heat" and that the dynamic theory's supporters "have been contented with very slight resemblances indeed, between those most simple effects of heat, and the legitimate consequences of a tremulous motion" (Black 1803, 31). Psillos does not, then, discover any *special* or *selective* reticence on Black's part to endorse the material fluid theory of heat in light of the evidence, and insofar as Black made any judgments of selective confirmation at all, they are ones that realists must regard as profoundly mistaken: that the material fluid account was the most probable theory of heat and that his own discoveries definitively refuted the competing dynamical view.

Black's general hostility towards theories was certainly not shared by Lavoisier, who advocated instead a "systematic" approach to chemistry and argued against simply piling up empirical facts (see Guerlac 1976, 219); accordingly, the most impressive piece of textual evidence Psillos cites in support of his historical thesis is Lavoisier and Laplace's explicit insistence in opening their famous *Mémoire sur la Chaleur* that the experiments therein are consistent with either a material fluid or a dynamical theory of heat and that they will not decide between the two. Upon reflection, however, this evidence carries less weight than it seems to, for the point of this *Mémoire* is simply to present Lavoisier and Laplace's new ice calorimeter and its techniques for the *measurement* of heat and the specific heats of particular substances: "We have already said, and we cannot stress this fact too much, that it is less the result of our experiments than the method we have used that we offer to scientists" (Lavoisier and Laplace [1783] 1982, 32). Since these new calorimetric methods really were compatible with both the material and dynamical theories of heat, it is unsurprising that Lavoisier and Laplace address their new techniques to the widest possible audience of their interested contemporaries. Furthermore, the explanations of specific phenomena offered later in the joint *Mémoire* itself are indeed committed to the view that heat is a material substance (Morris 1972, 30–31), notably in their explanatory appeals to the chemical *combination* of caloric with other substances (e.g., in phase transitions).

Far more important than Lavoisier's reasons for wanting to present himself cautiously in (at least the first half of) the *Mémoire sur la Chaleur,* however, are his repeated endorsements of, explanatory appeals to, and confident judgments of confirmation for the material fluid theory itself. Throughout his many other works, Lavoisier defended a material fluid theory of heat in which the "matter of fire" could exist in either free or combined forms and whose central contention was that the state (solid, liquid, aeriform fluid) and changes of state of any matter depended upon its *chemical combination* with this "matter of fire," an account of the matter that cannot be straightforwardly transformed into dynamical terms. As early as his 1778 paper on the formation of elastic fluids Lavoisier claimed not only that the opinion that there is such a material fluid was "that of the great majority of ancient physicists" and that he could therefore "dispense with reporting the facts upon which it is founded," but also that "the collection of memoirs . . . that I have to offer will serve to prove it," in part by showing "that it agrees everywhere with the phenomena, that it everywhere explains everything that happens in physical and chemical experiments" (Lavoisier 1965a, 2:212, my translation). Lavoisier appealed to this account alone throughout his works to explain a wide range of phenomena, including latent heat, the specific heats of substances, evaporative cooling, the elasticity of gases, and (along with his "oxygen principle") the phenomena of combustion and calcination. Indeed, the material fluid theory of heat plays a critical role in his case against phlogiston and for the new chemistry: Lavoisier's discussion of an alternative to the phlogiston theory in the famous antiphlogistic memoir of 1785 consists almost entirely of presenting his material fluid theory of heat and showing how it can explain a wide range of thermal phenomena (including the relative quantities of heat released by various combustions and calcinations, the heat produced by mixing water and concentrated vitriolic acid, and the heat lost when salt is dissolved in water) and can make general predictions of whether heat will be gained or lost in a given reaction (see Morris 1972, 17f). Perhaps most important of all, the material theory of heat provided Lavoisier with a viable alternative explanation of the release of fire during combustion, the primary explanatory accomplishment of the phlogiston theory (see Guerlac 1976, 202–203; Morris 1972, 37–38).

The memoir on phlogiston does mark Lavoisier's increasing concern to integrate his earlier, exclusively chemical, discussion of thermal phenomena (in terms of free and chemically combined caloric fluid) with a physical account of these phenomena in terms of interparticulate forces and the availability of the spaces between physical particles to receive caloric fluid. But this shift does not strengthen Psillos's case, as Lavoisier's increasingly physical presentations of the account are still forcefully committed to the material fluid view:

> It is clear *a priori* and *independent of all hypotheses* that the greater the distance between the molecules of bodies, the greater their capacity to receive the matter of heat, and consequently their specific heat will be greater. (Lavoisier 1965a, 2:644 as translated in Donovan 1993, 172; last emphasis mine)

Most important of all, neither Lavoisier's endorsement of the account nor his judgment that it is well-confirmed by the existing evidence is at all reserved or qualified in the way Psillos suggests. Remarking on the expansion and contraction of bodies, Lavoisier says:

> One can hardly think about these phenomena without admitting the existence of a special fluid [whose accumulation causes heat and whose absence causes cold]. It is no doubt this fluid which gets between the particles of bodies, separates them, and occupies the spaces between them. Like a great many physicists I call this fluid, whatever it is, *the igneous fluid, the matter of heat and fire.* (Lavoisier 1965a, 2:640–641 as translated in Donovan 1993, 171; original emphasis; translation modified)

Nor is Lavoisier's enthusiasm for the material theory—unlike his calculated expression of agnosticism in the *Mémoire sur la Chaleur* ([1783] 1982)—confined to just a single work or a single moment in his career: both the *Traité de Chimie* (1965b, 17–19) and the *Mémoires de Chimie* (1972, 1:3–5) offer accounts of the confirmational significance of expansion and contraction for the material fluid account that are virtually identical to that given in the antiphlogistic memoir of 1785. The *Traité,* for example, claims that

> [i]t is difficult to conceive of these phenomena without admitting that they are the result of a real, material substance, of a very subtle fluid,

> that insinuates itself throughout the molecules of all bodies and pushes them apart. . . . This substance, whatever it is, is the cause of heat, or in other words, the sensation that we call heat is the effect of the accumulation of this substance. (1965b, 19; my translation)

As Psillos notes, the *Traité* does contain Lavoisier's one other explicit note of caution recognizing the hypothetical status of the material fluid: Strictly speaking, Lavoisier says, it is sufficient "that it is considered as the repulsive cause, whatever that may be, which separates the particles of matter from each other" (Psillos 1999, 119). Once again, however, Lavoisier's later explanations of specific phenomena in the same work "take for granted the existence of heat matter as a fact of nature not requiring justification" (Morris 1972, 31). More importantly, this recognition of the hypothetical status of the igneous fluid did not *generally* lead Lavoisier to qualify his endorsement of the material fluid theory of heat or his confidence in its confirmation by the existing evidence: Morris suggests that "Lavoisier never faltered in his belief that the cause of sensible heat is a material substance" (1972, 30), and that his "true feelings regarding this 'singular hypothesis'" (1972, 31) are those expressed in his posthumously published final collection of essays, the *Mémoires de Chimie*. The discussion in that work is the most damaging of all for Psillos, as Lavoisier directly confronts the merely hypothetical status of the "igneous fluid" and responds with a forceful and explicit defense of its conclusive confirmation by the existing evidence:

> [O]nce I have shown, in the collection of memoirs I am publishing, that it is everywhere in accordance with the phenomena, that everywhere it explains in a simple and natural manner the result of experiments, this hypothesis will cease to be one, and one will be able to regard it as a truth. . . . (*Mémoires de Chimie* 1972, 1:2; my translation)

The strength of Lavoisier's confidence in the confirmation of the material theory of heat by the available evidence is all the more remarkable in view of the caution and diffidence with which he is famous for having typically presented claims that he regarded as theoretical or hypothetical (see Donovan 1993, 168). Thus, I do not see how this textual evidence can be reconciled with Psillos's claim that "scientists of this period were not committed to the truth of the hypothesis that the cause of heat was a material substance" (1999, 119). And once again, it seems that Psillos's case for the reliability (by current lights) of scientists' own judgments of selective confirmation must itself rely on an extremely selective reading of the historical record that ignores or dismisses most of those very judgments.

Finally, as these historical cases illustrate, Psillos's account also seems to require an implausible *homogeneity* in scientists' own judgments of selective confirmation: The assurance that we can trust scientists' own judgments won't help us decide what to believe in the routine case of *disagreement* among contemporary scientists concerning which parts of theories are well-confirmed by the evidence. And if Psillos means to claim only that we can rely on scientists' own judgments of selective confirmation when they are in fact univocal, then the realism he defends would be slender indeed, even if his claim of reliability for scientists' own judgments of selective confirmation were not historically implausible.

4. Conclusion. Of course Psillos might suggest that judgments of selective confirmation are complex and difficult and that Maxwell, Lavoisier, and others have gotten them wrong, but this escape concedes the pointlessness of the detour through the historical record of (unreliable, as it turns out) judgments of selective confirmation by scientists themselves, and thus inherits again the very problem Kitcher could not solve and Psillos sought to finesse—providing an explicit, historically reliable criterion for identifying the parts of theories selectively confirmed by their successes that *prospectively* distinguishes the rejected posits of past theories from those of the present theories she hopes to defend. I conclude that the historical record includes enough salient cases of ultimately abandoned crucial posits from intuitively mature and genuinely successful scientific theories to sustain a simple but compelling challenge to scientific realism.

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