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SCIENTIFIC REALISM AND THE PESSIMISTIC META-MODUS TOLLENS

1. INTRODUCTION

Broadly speaking, the contemporary scientific realist is concerned to justify belief in what we might call *theoretical truth*, which includes truth based on ampliative inference and truth about unobservables.[1] Many, if not most, contemporary realists say scientific realism should be treated as 'an overarching scientific hypothesis' (Putnam 1978, p. 18). In its most basic form, the realist hypothesis states that theories enjoying general predictive success are true. This hypothesis becomes a hypothesis to be tested. To justify our belief in the realist hypothesis, realists commonly put forward an argument known as the 'no-miracles argument'. With respect to the basic hypothesis this argument can be stated as follows: it would be a miracle were our theories as successful as they are, were they not true; the only possible explanation for the general predictive success of our scientific theories is that they are true.[2]

In light of historical concerns, realists have modified the basic position, changing their focus from general predictive success to novel success, from truth to approximate truth, etc. In this paper, I will challenge a number of variations on this explanationist argument and the hypothesis it is meant to justify. I will clarify that the threatening historical argument is not a 'meta-induction' from which we infer the falsity of contemporary theories; rather, it is a valid deduction that bears on the justification of our beliefs. After articulating the implications this argument has for realism, I will show that this argument poses a serious threat even when success is understood as novel success. I will also challenge the claim that the approximate truth of a theory can explain its success and will offer an alternative non-realist explanation. My approach will be to work my way from the naive formulat-

S. Clarke and T.D. Lyons (eds.), Recent Themes in the Philosophy of Science, 63–90.
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-ion of realism toward more sophisticated versions in light of these non-realist objections.

2. LAUDAN'S CONFUTATION

With the basic realist position spelled out above, we can turn to an important historical argument against realism. Larry Laudan, in his 'Confutation of Convergent Realism', provides a frequently cited list of theories and existential postulates that were successful but which are, according to contemporary theories, false (1981, pp. 121-122):

- — the crystalline spheres of ancient and medieval astronomy;
- — the humoral theory of medicine;
- — the effluvial theory of static electricity;
- — 'catastrophist' geology, with its commitment to a universal (Noachian) deluge;
- — the phlogiston theory of chemistry;
- — the caloric theory of heat;
- — the vibratory theory of heat;
- — the vital force theories of physiology;
- — the electromagnetic aether;
- — the optical aether;
- — the theory of circular inertia;
- — theories of spontaneous generation.

Realists have been responding to Laudan's 'Confutation' for two decades now. One question that has become salient: what sort of inference is being drawn from this list? Many realists (I dare say nearly all) assert that the article containing this list provides an exemplary articulation of 'the pessimistic meta-induction'. In regard to Laudan's 'classic paper "A Confutation of Convergent Realism"', Jarret Leplin tells us Laudan 'argues *inductively* that our current picture is *unlikely*, in retrospect, to have achieved referential success or approximate truth from the vantage point of future science, and that realism, accordingly, has no application' (1997, p. 137) [my italics]. Herman De Regt interprets Laudan's argument in a similar way, 'if so many successful theories turn out to be ontologically false we may *induce* the conclusion that current successful theories will *turn out to be* ontologically false as well' (1994, p. 11) [my italics]. 'Laudan's "historical gambit"', says Stathis Psillos, '*concludes, inductively*, that *any* arbitrarily successful scientific theory Tn+1 is *likely to be false* (or at any rate, *more likely to be false* than true)' (1999, p. 105) [my italics]. Each of these philosophers is taking Laudan's Confutation paper as an articulation of the pessimistic *meta-induction* toward the conclusion that *our present theories are probably false.*

However, I wish to suggest that this very common reading constitutes a significant misinterpretation of Laudan's argument. First, and briefly, contra the quotes above, Laudan's list is not presented as evidence that our contemporary theories are probably false (or even that it is *unlikely* that we have reached the truth). He is concerned with the justification of our beliefs, not drawing conclusions regarding the truth value of our contemporary theories.[3] A second and more important point: against each of the quotes above, I submit that Laudan is not arguing inductively at all.[4] Before outlining the proper understanding of his argument, we can note that construing Laudan's argument as an induction gives the impression that his inference is insufficiently supported. In specific reference to Laudan's 'Confutation', Psillos writes, 'This kind of argument can be challenged by observing that the inductive basis is not big and representative enough to warrant the pessimistic conclusion' (1999, p. 105).[5] Additionally, we can note that the intuition that drives us to consider many inductions credible may well not ground an induction of this sort. While we may arguably be justified in assuming, for instance, that there is a natural uniformity in respect to some *physical* entities and their properties, such a uniformity need by no means carry over into the realm of scientific theories. We have been given no reason to think there would be a uniformity of overall falsity here.[6]

However, I contend that Laudan is making no induction and that such criticisms are therefore misdirected. His list of historical theories that were successful but false is presented (and ought to be presented) as phenomena which serve to falsify the epistemic realist's hypothesis (and to challenge the explanationist argument that grounds that hypothesis). Any argument of this form is quite properly distinguished from a deductively invalid induction. It concludes with no universal claim, nor even a prediction. No inference is drawn about the future, or even the present. The list serves instead as fuel for the second premise of a logically valid *modus tollens* argument.[7] Again, in its most basic form, the hypothesis the realist says we can be justified in believing states that theories enjoying general predictive success are true. The *modus tollens* argument against that hypothesis reads as follows:

Premise 1: If the realist hypothesis is correct (A), then each successful theory will be true (B)

Premise 2: We have a list of successful theories that are not true (Not B)

Conclusion: Therefore, the realist hypothesis is false (Not A)

And, given the argument, we are not justified in believing the hypothesis, etc. (Further specific implications will be spelled out below.)

Even viewing the list as occupying the second premise of a *modus tollens*, one may be tempted toward two related contentions against that premise. First, though the non-realist is questioning whether we can justifiably believe that our theories are true, the non-realist appears to be assuming the privilege of *knowing* that these past theories are false. But, given, say, the Duhem-Quine problem, what would license the non-realist's claim to have such knowledge? Second, to say that past theories are false, the non-realist has to assume present day theories are true; this is surely unacceptable given that the non-realist is ultimately trying to show we have no justification for such a belief.[8] However, in employing the second premise of *the modus tollens*, the non-realist is not *required* to believe any such claims. Since successful present science tells us that much of successful past science is false, successful present science and successful past science cannot both be true. Therefore, we have a subset of theories that are successful yet false.[9] The only claim being made, in the end, is that there are some successful yet false theories in the set considered, and acknowledging this necessitates no specification of where the falsity resides. For ease of discussion, in what follows, I will nonetheless refer to *past* theories as being 'false', as being 'counterinstances', etc. But the richer understanding of such phrasings should be kept in mind.

3. NAIVE REALISM AND THE IMPLICATIONS OF THE META-MODUS TOLLENS.

Let us determine the effect Laudan's argument has against the naive version of the realist's explanationist argument introduced above. Actually, because our present concern is with naive realism, we do not need to limit ourselves to Laudan's list. Any theories — for instance, the theories of Copernicus, Galileo, Kepler, Newton, Lavoisier, Darwin, Mendel, Wegener, Bohr, etc. — that were successful but are now taken as 'strictly speaking, false' are relevant candidates. That noted, looking at history, we see many such theories. The following, more specific, points make clear the extent to which naive realism is threatened by the list of counterinstances. (I will draw the same implications regarding the more sophisticated realisms discussed below. Doing so, I will direct the reader back to this section. I ask the reader, when returning here, to replace the naive versions of the realist hypothesis and explanationist argument with the hypothesis and argument at issue in the relevant section. The same implications hold).

Take first the explanationist argument the realist employs to justify belief in her hypothesis. For naive realism, the key premise states, 'It would be a miracle were a theory successful but not true'. Given that there have been successful theories that are false, we must either take this key premise of the realist's inference to be absurd and therefore unacceptable, or we must hold the success of these false theories to be miraculous. Since no-one in this debate wants to say the list of concern is a list of miracles, the key premise must be rejected. A similar but separable point: the list constitutes a list of historical theories whose success the realist cannot explain (of the implications I'm drawing, this is one that Laudan himself emphasises). How then can naive realism constitute a good or even adequate explanation of the success of scientific theories in general? Realists say non-realism is to be rejected for the very sin that realism itself is here committing, failing to explain success. Further, since we are rejecting miracles, and since realism cannot explain these successes, some explanation other than the truth of these theories is required to do so. And once that needed explanation is introduced (whatever it may be), the claim that truth is the only explanation for success is rendered futile. Moreover since that explanation would have to be acceptable for explaining the success of those theories on the list, it would also have to be acceptable for explaining the same sort of success of all other theories, including those of contemporary science. Thus it would have greater breadth than the realist's 'truth explanation'. Given that truth could at best only explain the success of some theories, and given that another explanation will be needed to explain the success of others, the truth of our theories would appear to be neither a good explanation nor the only explanation for the success of scientific theories in general. The argument by which the realist seeks to justify her hypothesis is rendered unacceptable.

Continuing, let us directly address the specific hypothesis the naive realist wishes to say we are justified in believing. For naive realism that hypothesis reads, 'successful theories are true'. Testing this correlation, we see first that, without begging the very question being addressed, i.e., without simply asserting the realist hypothesis to hold, we can establish not a single confirming instance of the hypothesis, not a single instance in which both success and theoretical truth obtain. Thus the hypothesis attains no solid inductive support whatsoever. Further, having now interpreted this historical argument as offering a set of counterinstances, in the proper spirit of Laudan's paper, we ask with Martin Carrier, 'What could count as empirical counter-evidence' for the realist hypothesis (1991, p. 25). And we answer: successful theories that are false. Given the high number of such theories, the hypothesis that successful theories are true has an exceptionally high number of falsifications. With the counterinstances in hand, we have a non-universal falsifying hypothesis that contradicts the realist hypothesis:

theories can be successful without being true. The high number of counter-instances provide significant empirical confirmation (or corroboration) to this hypothesis which directly falsifies the unconfirmed realist hypothesis. Testing the realist hypothesis, we discern not a single genuine confirmation and numerous falsifying instances. Thus, we cannot even say the realist hypothesis *probably* holds; even a mild Popperian *conjecture* that there is a positive correlation between truth and success would have to be rejected. (One is tempted to suggest that the realist hypothesis shares the evidential status of the hypothesis that all ravens are white.) We are prompted to ask ourselves whether any scientific hypothesis would be retained with such a weak track record. The answer appears to be, no. If we are to be sincere about testing our ideas against the world, then we need to take this evidence seriously: the evidence strongly suggests we must reject the realist's hypothesis as false. And, again, this is no induction: no universal generalization, nor even a prediction, has been made. As we address other realist formulations, we will be led to these same general conclusions against the realist inference and hypothesis. I will refer back to these conclusions collectively as *the implications of the meta-modus tollens*.

As I've implied, naive realism is a straw man. In response, the realist will here point out that we are not and ought not be *naive falsificationists*: we should be afforded the opportunity to modify our hypothesis. For example, our naive realist might deny that the tacit universality in the realist hypothesis is required. We can simply conjoin to our realist hypothesis the phrase, 'usually' or 'typically': successful theories are typically true.[10] A similar response would be to modify our hypothesis to say that a certain percentage of our successful theories are true.[11] However, these modifications do not get us very far — even putting their suspiciously *ad hoc* character aside. Without begging the question of realism, these modified hypotheses have no positive, confirming instances, thus no standard inductive evidence. Moreover, they fail to explain the counter-instances. Finally, and perhaps most importantly, these modifications clash with the very argument the realist is using to support them, the no-miracles argument. For that argument says it would be a miracle if a theory were successful but not true. The tacit supplementary claim is that we ought not accept miracles. Yet the phrase 'typically' and the specification that a certain percentage of successful theories are true entail the possibility of theories that are successful but are not true; thus these modifications leave us conceding to the existence of miracles in science. So even ignoring their *ad hoc* nature, the meta-modus tollens cannot be averted by weakening the naive realist hypothesis to say that successful theories are typically true (see Brown on this (1985)) or that a certain percentage of them are true.

4. NEW REALISM: NOVEL SUCCESS AND TRUTH

We have thus far been concerned with general predictive success. Primarily in response to Laudan's list,[12] a new version of the realist argument is appealed to by many, one which focuses not on mere *predictive* success but on the *novel* success of scientific theories (Musgrave (1985, 1988); Lipton (1993, 1994); Psillos (1999); and Sankey (2001)). Alan Musgrave writes, 'The fact to be explained [by realism] is the (novel) predictive success of science' (1988, p. 239). Novel success can be divided into two sorts: temporal novelty, in which the phenomena predicted are unknown to scientists at the time the prediction is derived; and use-novelty, a less demanding form, in which the phenomena may be known but were not used in formulating the theory. For our present purposes, then, a novel prediction should be understood as a prediction of phenomena that were not used in the formulation of the theory, and either sort noted will qualify. The following are examples of novel successes favoured by realists:

General Relativity

- light bends around massive objects (confirmed by the Eddington expedition)
- the gravitational redshift of spectral lines

Special Relativity

- time dilation (confirmed by jets carrying atomic clocks)

The appeal to novel rather than predictive success brings an element of wonder that ostensibly gives new force to the realist's no-miracles argument. It is extremely unlikely — in fact, says the realist, it would be a miracle — if a false theory would lead us to 'hitherto unknown' phenomena, or phenomena that were never *used* in the formulation of our theory. Novel success, it is held by the realist, has obvious epistemic significance. Further, redefining *success* in this way appears to hold the promise of eliminating many if not all of the theories in Laudan's list. Musgrave claims in response to Laudan's list that 'few, arguably none, of the theories cited had any *novel* predictive success' (1985, p. 211, footnote 10). If the theories on Laudan's list enjoyed *only* general predictive success, they do not qualify as genuinely successful, and the realist hypothesis is not falsified.

At this level the realist is trying to justify our belief in the following hypothesis: theories enjoying novel predictive success are true. The realist justification for this claim is that the truth of the theory is the only explanation for its novel success; it would be a miracle were a scientific theory to enjoy novel success were it not true. In his 'Confutation' article, Laudan points out that realists 'say little about what … success amounts to'

(1981, p. 23). Presumably for this reason, Laudan was not concerned to present a list of novel successes. Testing the new realist package, then, calls for new data. We ask, in the history of science, are there novel successes from theories that have nonetheless turned out to be false? Attempting to begin the relevant list, I find there have been numerous examples such as the following:

Caloric Theory

- the rate of expansion is the same for all gases — confirmed by Dalton and Gay Lussac;[13]
- the speed of sound in air —predicted by Laplace;[14]
- the depression of freezing point by pressure — predicted by J. Thomson, using the Carnot theory, confirmed by W. Thomson;[15]
- steam engines with higher pressure have higher efficiency — predicted by Carnot;[16]
- the adiabatic law — derived by Poisson.[17]

Phlogiston Theory

- sulfuric acid and phlogiston (emitted from glowing coal) combine to create sulfur — predicted/confirmed by Stahl;[18]
- heating a calx with inflammable air (hydrogen) turns it into a metal — predicted/confirmed by Priestley.[19]

W.J.M. Rankine's 19th Century Vortex Theory

- the specific heat for saturated steam is negative (prior to Clausius's prediction of the same phenomenon);[20]
- the value of the specific heat of air under constant pressure (this conflicted with the data then available, yet was later confirmed);[21]
- the existence of the entropy function (predicted well before Clausius's formulation of the principle of entropy increase);[22]
- the cooling effect for carbon dioxide as later found in the Joule-Thomson expansion experiment.[23]

Newtonian Mechanics

- the existence of Neptune;
- the return of Halley's comet;[24]
- the oblate shape of the earth; [25]
- the non-Keplerian perturbations of planets;[26]
- the ability to use the moon in propelling a rocket back to earth — confirmed by NASA, Apollo 13.

Fermat's Principle of Least Time

- light slows in a dense medium — confirmed by Fizeau and Foucault.[27]

Fresnel's Wave Theory of Light and Theory of the Optical Ether

- the 'white spot': directing a point-beam of light at a small opaque disc will result in the appearance of a bright spot in the disc's shadow — predicted by Poisson, confirmed by Arago;[28]
- the 'black spot': directing point-beam of light at a circular hole in an opaque screen will result in the appearance of a black spot located behind the screen along the line of the beam — suggested by Poisson, derived and confirmed by Fresnel;[29]
- Numerous quantitative details regarding (previously known) straightedge diffraction cases, predicted and confirmed by Fresnel;[30]
- the existence of internal conical refraction;
- the existence of external conical refraction — both predicted by Hamilton, confirmed by Lloyd.[31]

Maxwell's Ether Theory

- the existence of radio waves — confirmed by Hertz.[32]

Dalton's Atomic Theory

- the varying weights of oxalate acid (and of carbonic acid, and of sulfuric acid) that react with a given weight of potash to make different compounds are in simple numerical relation — confirmed by Thomson and Wollaston; 33
- the same relation holds for the oxides of nitrogen and ethene and methane — confirmed by Dalton himself (all other post-1803 confirming instances of Dalton's Principle of Simplicity should be added here as well);[34]
- the individual constituents in a mixture of gases exert the same pressure as they would alone (Dalton's law of partial pressure);[35]
- the atomic compositions of carbon monoxide, carbon dioxide, nitrous oxide, nitric oxide, and nitrogen dioxide;[36]
- carbonic acid has a linear compound atom (ie., is a linear molecule) and sulfuric acid has a triangular compound atom (ie., is a triangular molecule).[37]

Kekulé's Theory of the Benzene Molecule

- the substitution of one hydrogen atom with five chorine atoms in Benzene results in one (rather than two) isomers — confirmed by Landenburg;[38]
- the number of isomers of the derivatives that come about by substituting one hydrogen, two hydrogens . . . six hydrogens — confirmed by Beilsten and Kurbatow;[39]
- benzene displays the properties of having three carbon-carbon double bonds — confirmed by J.W. Bruhl;[40]
- the ozonisation of oxylene (a benzene derivative) followed by hydrolysis yields glyoxal, methyglyoxal, and dimethyglyoxal — confirmed by Levine and Cole.[41]

Mendeleev's Periodic Law

- the existence of gallium — confirmed by de Boisbaudran;[42]
- the existence of germanium[43]— confirmed by Nilson;[44]
- the existence of scandium — confirmed by Winkler;[45]
- the existence of technetium — created by Perrier and Segre;[46]
- the existence of rhenium — confirmed by Noddack, Tacke, and Berg;[47]

- the existence of francium — confirmed by Perry;[48]
- the existence of plutonium — confirmed by Marie Curie;[49]
- beryllium has an atomic weight of 9 rather than 14;[50]
- uranium has an atomic weight of 240 rather than 120;[51]
- gold has a higher atomic weight than platinum, iridium, and osmium;
- platinum has a higher atomic weight than iridium and osmium;
- iridium has a higher atomic weight than osmium.[52]

Bohr's 1913 Theory of the Atom

- three series of the hydrogen line emission spectrum (in addition to the Balmer and Paschen series) — confirmed by Lyman, Brakett, Pfund.[53]

Dirac's Relativistic Wave Equation

- the existence of the positron — confirmed by Anderson and Blackett.[54]

The Original (pre-inflationary) Big Bang Theory

- the existence of the cosmic background radiation — predicted by Gamow, confirmed by Penzias and Wilson.[55]

It is important to emphasise that I've restricted my list to the most rigidly demanding sort of novel success, temporal novelty. We nonetheless have a list of novel predictive successes, all coming from theories that are false by present lights. (I will add to this list below.) According to contemporary science, heat is not a material fluid, and is not conserved; no substance is emitted from a flame; Newtonian mechanics holds neither at high speeds nor small scales; gravity is not a force; time is not absolute; mass is not independent of energy; there is no aetherial medium through which light as an elastic vibration travels; elemental properties are not a function of their atomic weights; the principle of least time has been replaced by the principle of least action; electrons do not have circular orbits, etc. Against the move to novelty, we have in hand a substantial new list of counterinstances. Adding perhaps the implication that novel success is not as special as the realist considers it, testing this form of realism, we are again led to the implications of the *meta-modus tollens* discussed in Section Three.

5. SOPHISTICATED REALISM: APPROXIMATE TRUTH AND NOVEL SUCCESS

The natural response (and the response the reader has probably been anticipating) is to concede that our appeal to truth is too bold and to make an appeal to approximate truth. This sophisticated realist package now asserts the hypothesis that theories achieving novel success are at least approximately true. And the argument meant to justify our belief in that hypothesis now reads, *it would be a miracle were our theories to achieve novel success were they not at least approximately true*. Before concerning myself with the historical counterinstances against this version of the package, I will challenge the sophisticated realist position from another, though no less significant, direction.

On the Likelihood of Success given that a Theory is Approximately True

Let us clarify the realist's situation. The realist wants to say those theories in the list are approximately true. These are theories that have not only been replaced by later theories, they have also, by present lights, encountered empirical failure. The latter point imposes a requirement on our notion of approximate truth. It must be sufficiently nonrestrictive to allow that an approximately true theory can significantly fail empirically. The desire to accommodate the list therefore mandates that our notion of approximate truth be quite a *permissive* notion. However, our realist is pulled in another direction as well. She says that the predictive success of science can only be *explained* by the approximate truth of our theories. This of course requires that approximate truth can explain success in the first place. While realists will likely include a number of additional premises, they employ a Peircean (abductive) argument whose fundamental claims and structure are as follows:

> Premise 1: If a theory is approximately true, then it will (at least be likely to) enjoy novel success[56]
>
> Premise 2: We have had theories that have enjoyed novel success
>
> ---
>
> Conclusion: Those theories are approximately true

Given the centrality of the first premise, the realist's explanationist argument demands that our notion of approximate truth be quite *strict*. In short, realists have to make approximate truth permissive enough so that an approximately true theory can fail empirically; yet they must make it strict enough so that such a theory will be likely to be successful. That there is a tension between these requirements should be apparent and become more so.

Is premise 1 acceptable? It is based on the assumption that theories that approximate one another make the same (or approximately the same) empirical predictions. Laudan charges realists with failing to show that an approximately true theory will be successful (1981, pp. 29-32).[57] I wish to argue toward the stronger conclusion that the approximate truth of a theory *will not*, in itself, make success likely. First, a slight change in claims about unobservables can lead to dramatically different empirical predictions. Let us consider the most basic and non-contentious sort of approximate truth. Say an experimenter gets a value within one one-thousandth of the *actual* value. The realist (and many others) will surely want to say such a result would be approximately true. (In agreement with Brown (1985), I'd suggest it should be a condition of adequacy that a notion of approximate truth or verisimilitude include such scenarios.) Consider a set of theory complexes, C1-Cn that approximate one another in this fundamental way. C1 is the entire corpus of contemporary science. C2-Cn are identical to C1, except

- in C2, the charge of the electron is higher by one one-thousandth of its value in C1;
- in C3, the charge of the electron is higher by one one-billion-billionth of its value in C1;
- in C4, the charge of the electron is *lower* by one one-thousandth of its value in C1;
- in C5, the charge of the electron is lower by one one-billion-billionth of its value in C1;
- in C6, the charge of the *proton* is higher by one one-thousandth of its value in C1;
- in C7, the charge of the proton is higher by one one-billion-billionth of its value in C1;
- in C8, the charge of the proton is *lower* by one one-thousandth of its value in C1;
- in C9, the charge of the proton is lower by one one-billion-billionth of its value in C1;
- in Cn, . . .

We have a set of theory complexes all well within a range of being approximately the same. Indefinitely many additional complexes could be generated within the range of approximation delimited here. C1 enjoys an amazing and unprecedented degree of empirical success. However, despite the fact that each of the many alternatives C2-Cn approximates C1, each of these predicts that all matter would *repel* all matter. While C1 enjoys amazing empirical success, the approximating theories predict no universe at all. Therefore, *all theory complexes in our set of complexes approximating C1 completely fail empirically*.[58] I've used this example to illustrate that a slight change in claims about unobservables can — and in this case, does — lead to dramatically different empirical predictions.[59] Imagine for a moment that every statement in C1 is true. Despite the fact that C2-Cn would be approximating the truth (to an amazing degree!), no members of that class would enjoy any degree of empirical success, let alone that enjoyed by C1.

A second point toward the claim that the approximate truth of a theory will not, in itself, make success likely: the mere stipulation that a theory is approximately true makes no restriction on auxiliary hypotheses. If no auxiliaries connect the theory to the world, the theory need not enjoy empirical success. Assume for a moment that Leibniz's theory of monads is approximately true. In the context of contemporary science, this theory would not be conjoined to any auxiliaries that connect it to the empirical world. Thus, despite its approximate truth, it would not be successful. And even if the realist were to conjoin to her explanation the claim that the theory is connected to auxiliary statements, any auxiliaries whatsoever are allowed, from the irrelevant to the absurdly false. That given, an unlimited number of theory complexes can be generated, far more of which will fail than succeed. Thus, with no restriction on the nature of the auxiliaries, our approximately true theory is far more likely to fail empirically than to succeed. (This holds, as well, for the mere stipulation that our theory is true, but I will not pursue this issue here.)

The only hope the realist might have is to modify her position by adding the claim that the auxiliaries are, like the theory itself, approximately true. This restriction does not come without consequence. It makes the realist hypothesis bolder and more demanding. It thus decreases the potential approximate truth realism has for accommodating the list. (The tension I've noted above is at play.) Most importantly, this necessary restriction is not sufficient to solve the explanatory problem. Assume for illustrative purposes that we have a theory complex consisting of a true theory conjoined to a full set of true auxiliary claims. We then consider every possible modification of our theory, T1-Tn, within the range of being approximately true, while keeping the auxiliaries as they are. We've seen the results of the electron/proton example above. In the realist's favour, however, let us stipulate that, for some reason, in this case, modifying only our theory we are not led to such a dramatic situation. Nonetheless, a theory that is approximately true is false; false theories have an infinite number of false consequences; and, (to accommodate the list) our notion of approximate truth must be sufficiently broad that it allows for a great deal of empirical failure. Even though the resultant complexes contain a full set of true auxiliary statements, it is quite likely that a number of the complexes will encounter empirical failure.

Now take just one auxiliary constituent, A1. Consider the potentially numerous modifications of A1 within the range of being approximately true, A1a-A1n. We then consider every possible T + A1 conjunction that can be brought about from A1a-A1n and T1-Tn. The quantity of complexes generated would inflate by multitudes. Even holding all other auxiliaries true, we will likely have a great many complexes that fail. Take a second auxiliary, A2, and all modifications permitted within the range of maintaining its approximate truth, A2a-A2n. Adding each of these possibilities to the legion of T+A1 theory complexes, we have another expansive increase in the set of complexes. We imagine continuing this process, modifying each member of the full set of true auxiliary statements in our initial complex. Three factors will serve to proliferate the number of resultant complexes:

- the number of auxiliary statements included in the original complex;
- the number of ways each given statement can be modified so that it fits into our (necessarily permissive) notion of being approximately true;
- the number of possible conjunctions of all the statements that are available.

All possible modifications of the true statements in the initial complex and all possible conjunctions of these statements render together an innumerable mass of complexes whose statements are all approximately true. And the greater the number of possible complexes available, the greater the quantity of empirical predictions made by the set of all complexes. Among the set of complexes, there will be indefinitely many that will be empirically *unsuccessful*. Most importantly, failure is not restrictive; the ways in which, and degrees to which, a complex can fail to be empirically successful are not limited. By contrast, success is restrictive: the ways in which, and degrees to which, a complex can be successful are very restricted. Put another way, with a finite number of *types* of claims about observables that have to be sufficiently correct in order to render empirical success, the likelihood of getting those sufficiently right is far lower than the likelihood of getting any other possible results for observables. These considerations strongly suggest that the number of approximately true complexes that will be successful will be lower than the number of approximately true yet unsuccessful complexes. In short, despite the fact that all the statements in our set of complexes are approximately true, empirical failure will be more likely than success. I should point out that, our above considerations pertain to the capacity for an approximately true theory to bring about *general predictive* success. *Novel* success — at least insofar as it is sought in conjunction with general success — constitutes an even greater demand. It is even *less* likely to be brought about by the approximate truth of the theory.

The crucial explanatory premise of realism states that, if a theory is approximately true, then it will (be likely to) enjoy novel success. This premise appears to be false, rendering dubious the claim that the approximate truth of a theory can explain its success. It looks as though the sophisticated realist argument is unable to get off the ground.[60]

Approximate Truth, Reference, and the Meta-Modus Tollens

Let us consider whether sophisticated realism is also subject to the meta-*modus tollens* and its implications. We must ask whether my list above includes theories that were successful but not approximately true. On what grounds might we say that a theory is not approximately true? Laudan draws on the intuition that in order for a statement to be approximately true it must genuinely refer to things in the world. 'If there were no entities similar to atoms, no atomic theory could be approximately true; if there were no subatomic particles, no quantum theory of chemistry could be approximately true' (1981, p. 33). If reference is required for approximate truth, theories that were successful but do not refer cannot be approximately true. Thus theories that have achieved novel success and do not refer (by present lights) will constitute counterinstances to sophisticated realism.

While I think there are a number of reasons for accepting Laudan's claim that the terms of a statement must refer for that statement to be approximately true, I'd suggest the following is crucial. I've just argued that the approximate truth of a theory does not imply or even make it likely that our theory will be successful. This is so even when our notion of approximate truth is one that requires reference. But, were our notion of approximate truth to be applicable to statements that contain altogether nonreferring, *or even partially referring*, terms, it will be even less likely that an approximately true theory will be successful. Eliminating reference from approximate truth threatens to render approximate truth completely non-explanatory. In fact, even appealing to a significantly permissive, charitable notion of reference, e.g., a causal theory of reference, leads to precisely this same problem. Given the list of counterinstances, the realist may be tempted to invoke a notion of approximate truth that allows permissive/charitable reference or no reference at all; however, such a notion of approximate truth will not be compatible with the realist's goal of *explaining* success. For the realist of concern, approximate truth requires reference.

We can now address our list of novel successes as a threat to the sophisticated realist's hypothesis. Construing approximate truth in such a way that we can attribute it to theories containing such terms as 'caloric', 'phlogiston', 'ether', 'absolute space', 'atomic vortices', etc. is not an option for the realist of concern. So the theories including such terms constitute genuine counterinstances even when we make the move to approximate truth. (And since reference is necessary but not sufficient for approximate truth, those theories that can still be construed as referring — in the non-permissive sense required — may still also fail to be approximately true.) Though our list of counterinstances may be decreased with the move to approximate truth, it remains significant. And we are led to the implications of the *meta-modus tollens* discussed in Section Three.

6. AN ALTERNATIVE EXPLANATION FOR THE SUCCESS OF SCIENTIFIC THEORIES?

Let us step back a bit. Our concern here has been with the argument that realism, in some form or another, is the *only* explanation for success. We've now seen that, unless each counterinstance we've considered thus far is inexplicable, another explanation must be available for that theory's success. Here's a contender:

> MS: The mechanisms postulated by the theory would, if actual, bring about all relevant phenomena observed, and some yet to be observed, at time *t*; and these phenomena are brought about by actual mechanisms in the world.

This is my modification of a non-realist contender suggested by Arthur Fine, but which goes back at least as far as Vaihinger. Fine's version — the world is *as if* the theory were true — is deemed *strong surrealism* by Leplin (1987; 1997, p. 26). I call the above version *modest surrealism* (MS). Notice that, provided the phenomena observed are sufficiently wide-ranging, a theory with the property of MS will achieve both general and novel predictive success. The claim that a theory has the property of MS can therefore explain both sorts of success. Moreover, MS expresses a relationship between (a) unobservables in the world and (b) the entities posited by the theory. It also makes causal claims about (a) and (b). Thus, MS is far from being a mere reiteration of the explanandum. On the negative side, MS is predictively vague in that it entails no specification of when the success of the theory will break down. However, the same holds for the claim that our theory is approximately true. Finally, and quite crucially, because MS is not falsified by any of the counterinstances to realism, MS appears to explain the success of every scientific theory we've discussed, while realism gives no explanation at all.

The realist will quickly suggest that, despite the fact that MS goes deeper than reiterating the explanandum of success, we are still left wondering why a theory is modestly surreal. The non-realist replies by pointing out that MS describes something about the unobservable structures of the world and something about the postulates regarding unobservables in the theory: MS specifies that the former does, and that the latter would, bring about the specified phenomena, P. The non-realist will ask, why need we say anything more? The realist will respond, we want to know why *both* have this property. She will then suggest that she has the answer: the theory is approximately true.

In reply, the non-realist will simply point to our considerations above: crucially, we cannot invoke approximate truth to explain why any counterinstances — successful theories that cannot be approximately true — have the property of MS. Second, the approximate truth of T does not even appear to make empirical *success* at *t* likely. And since P goes beyond the phenomena observed at *t*, the approximate truth of T does not make it likely that P will occur. Thus, as with success, the approximate truth of the theory cannot explain MS. On these grounds, the non-realist will contend that if modest surrealism must or even can be explained, it cannot be by appeal to the approximate truth of our theory.

7. DEPLOYMENT REALISM

Given the serious problem with the invocation of approximate truth, the realist may wish to concede that the listed theories are neither fully true, nor approximately true, but suggest that *those constituents of the theories that contributed to success* are true. In other words, the false parts were not responsible for the novel success of the theories. The claim that ether exists, for instance, is not obviously deployed in the derivation of Fresnel's white spot (see Kitcher (1988, pp. 144-149)). A deployment realist hypothesis can be expressed as, 'those constituents that were *used* in the derivation of novel predictions are true'. And the argument meant to justify that hypothesis: it would be a miracle were those constituents not true. In contrast with the approximate truth of the theory, the truth of the deployed constituents *would* make both novel success and MS likely. Thus, this form of deployment realism appears to be a genuine contender for explaining these properties.

In response, one must grant that Fresnel's ether theory is a case in which certain false theoretical claims were not actually employed. However, it is not obvious that many other theories can be broken up in this way. Often the parts of a theory hang together very tightly. I find it highly doubtful that *no* false constituents played a role in the derivation of the predictions on the list.[61] For instance, the numerous novel predictions Mendeleev made are direct consequences of assembling his periodic table. Since that table is a correlation of chemical properties given their atomic weights, it appears undeniable that Mendeleev's periodic law — the claim that atomic *weights* determine chemical properties — played significantly in the construction of that table. To say that absolute acceleration was not used in derivations from Newtonian theory seems implausible. In fact, given the apparent need for absolute acceleration, it is not even clear that Euclidian geometry, absolute space, and absolute time were not involved. Regarding the prediction of the positron, Stephen Brush writes,

> the existence of an anti-particle [ie., the positron] followed directly from [Dirac's relativistic wave] equation, provided one accepted his interpretation of 'holes' in a sea of negative-energy states as particles with the opposite charge and positive energy. But Dirac's theory was replaced by quantum electrodynamics . . . (Brush 1995, p. 139)

Given Brush's account of the history of Benzene theories (1999a, 1999b), one would be hard pressed to deny that Kekulé's theory, now seen as false in light of molecular orbital theory, was employed in the the predictions noted above. Martin Carrier shows that the claim that phlogiston is the principle of heat and the claim that 'sulfuric acid was in fact dephlogisticated sulfur' were directly involved in Stahl's prediction that the synthesis of phlogiston and sulfuric acid would result in sulfur (1993, p. 402). The postulate that charcoal is '[h]igh in phlogiston' and that inflammable air is pure phlogiston, were used in deriving Priestley's prediction that inflammable air would, like charcoal, turn calx into metal. (1991, p. 30.) Following the reasoning of Laplace and Haüy, Carrier also shows that the following constituents, which are now seen as false, were used in predicting that 'the rate of expansion is the same for all gases' (1991, p. 31).

- heat is a weightless fluid called caloric
- the greater the amount of caloric in a body, the greater is its temperature
- gases have a high degree of caloric
- caloric, being a material itself, is composed of particles
- caloric particles have repulsive properties which, when added to a substance, separate the particles of that substance
- the elasticity of gases is caused by this repulsive property of caloric heat particles
- the elasticity of all gases is due to the repulsive properties of a single substance, caloric

Psillos acknowledges the following regarding Poisson's prediction of the adiabatic law: 'To be sure, Poisson did *rest* his derivation' of the law of adiabatic change on the state function hypothesis (1999, p. 120), [my italics] which is the 'fundamental hypothesis of the mature caloric theory' (1999, pp. 120-121). However, the state function hypothesis requires that heat is always conserved, and, by present lights, heat is not always conserved.[62]

Keith Hutchison has given a detailed history of W. J. M. Rankine's (often overlooked) mid-nineteenth century vortex theory, detailing its exceptional success.[63] This theory made at least four temporally novel predictions plus half a dozen or so use-novel predictions.[64] The beauty of Rankine's theory, in regard to deployment realism, is that Rankine himself sought diligently to achieve the same results while eliminating as much of his vortex theory as possible. Despite his efforts, however, he had to make explicit appeal to the vortex hypothesis. As Hutchison puts it, 'Rankine's "phenomenological" theory . . . remained logically dependent on the vortex theory' (1981a, p. 13). And Rankine's theory is dramatically false by present lights. This theory appears to provide exceptional counterevidence to the realist's suggestion that the false parts of theories were not used in the derivations of novel predictions.

Dalton derived his law of partial pressure from an early version of his (false) atomic theory in 1801. More specifically, as John Hudson points out, he 'deduced' it from 'the idea that each particle could only repel others of its own kind, and that dissimilar particles exerted no forces on each other' (1992, p. 80). Dalton's law of multiple proportions was also, as James Partington puts it, 'a consequence of his Atomic Theory' (1957, p. 159). That law states

> when two elements combine to form more than one compound, the weights of one element which unite with identical weights of the other are in simple multiple proportions. (Partington 1957, p. 159.)

H.M. Leicester notes that this law 'followed so essentially from Dalton's theory that he did not even express it as a distinct principle.' (1965, p. 155). Among those specific claims that were crucially employed in the derivation of the law of multiple proportions, and thereby more specific novel successes, is Dalton's principle of simplicity:

> where two elements A and B form only one compound, its compound atom contains one atom of A and one of B. If a second compound exists, its atoms will contain two of A and one of B, and a third will be composed of one of A and two of B, etc.
> (Hudson 1992, p. 81)

This principle, however, is false by present lights.[65] Yet it was — at times by way of its deployment toward the law of multiple proportions — genuinely employed in numerous confirmed temporally novel predictions (see the list in Section Four). Going beyond our list of novel successes, it should be recognised that thousands of previously unknown chemical compounds have been predicted by way of chemical principles such as Proust's law of definite proportions and Dalton's law of multiple proportions. Yet, as Maureen Christie points out (in a different context), these principles cannot be regarded as strictly speaking true. Each is 'exceptioned' and/or approximate, at best (1994, pp. 613-619).

I must also reemphasize that the list above includes only examples of *temporally* novel success. Yet the realist (in following Zahar's discussion from a separate debate) commonly appeals to use-novelty as well. When a theory complex makes a prediction after a new hypothesis is added, and was not *seen* to make that prediction before, it is clear that the new hypothesis is genuinely employed in predicting the phenomena. Noting this, we see that accommodating hypotheses with little external support can very well be and often have been use-novel. Within the context of phlogiston theory, the claim that phlogiston has negative weight was introduced to explain why calxes are heavier than their metals. Once formulated, however, that hypothesis could also be deployed in the use-novel prediction that fire behaves differently than other substances: because fire has negative weight, it moves away from massive bodies rather than toward them. In order to reconcile the discrepancy between the behavior of Mercury and Newtonian mechanics, it was proposed that a collection of invisible planetoids, small planets, serve to offset Mercury's perihelion. However, that postulate also predicted, in the use-novel sense, future instances of the glow in the sky observed post-sunset on the western horizon and pre-sunrise on the eastern horizon, known as 'the zodiacal light'. Astronomer Simon Newcomb wrote: 'if the group exists the members must be so small as to be [individually] entirely invisible. But in this case they must be so numerous that they should be visible [collectively] as a diffused illumination on the sky after sunset. Such an illumination is shown by the zodiacal light'.[66]

Now in contrast with most discussions about use-novelty, our concern is truth and justified belief, rather than rational theory choice. That given, coupled with the fact that use-novelty entails no temporal restrictions, ancient theories that make use-novel predictions of modern phenomena would have to qualify as well.[67] Even setting aside such anachronistic cases, since use-novelty is very common in science, we can likely find many more historical examples of false constituents being deployed toward use-novel predictions. For instance, Newtonian mechanics predicted the behaviour of the tides, the behaviour of unseen stellar objects, the precession of the equinoxes; Bohr's theory of the atom predicted the Balmer series of the hydrogen spectrum, etc. Rankine's vortex theory achieved a significant amount of *use*-novel success as well. I submit that, upon a close search, the list of counterinstances to deployment realism would be very substantial.

This final stand in the series of retreats I've traced is, in a very significant way, a bolder position than the others: there are far more potential falsifiers for this thesis than for the more broad-based theory realisms. Many constituents will be involved in the theories listed above, and each false *constituent* that is employed in a confirmed novel prediction constitutes a counterinstance to deployment realism. Thus many counterinstances can come from a single prediction. For example, it looks as though we have at least seven false constituents employed in Haüy and Laplace's reasoning toward the prediction that 'the rate of expansion is the same for all gases' (Carrier 1991, p. 31). Further, the deployment realist's hypothesis pertains to auxiliary assumptions no less than broad ranging theoretical claims. It seems altogether untenable to claim that every constituent, invoked explicitly or implicitly, that was deployed in the predictions we've discussed is true. Moreover, a given false constituent stands as a separate counterinstance *each time* it is employed in the derivation of such a prediction. Deployment realism has the potential to fare far worse than most versions of realism given the historical argument. Consider Dalton's principle of simplicity. Or better yet, Mendeleev's periodic law: this law was genuinely employed in, at least, those dozen predictions listed in Section Four — and that number is setting aside his prediction of individual *properties* of each of the predicted elements (see my footnote on Mendeleev in Section Four). Thus, that law would stand as a counterinstance at least a dozen times, once for each of the predictions drawn from it. Consider as well the use of the laws of definite and multiple proportions in the predictions of thousands of chemical compounds. My bet is that on close examination, the falsifying instances for this final stand of theory realism will be far more numerous than they are for the more naive realisms considered above. Though further in depth analysis is needed, suffice it to say that each of the claims above appears to be genuinely employed in novel predictions, yet each is strictly speaking, if not dramatically, false by present lights. We are again led to the implications of the *meta-modus tollens* in Section Three.

Regarding the ability to explain modest surrealism (MS), deployment realism affords no explanation for novel successes from false constituents. Nor, then, can it explain why the respective broader theories have the property of being modestly surreal. Though MS may have less *depth* than the realist desires, it has an incomparable degree of *explanatory breadth*: MS explains the success of both the general predictive success and the novel success of, not only every theory mentioned in this paper, but all that we've neglected, including the corpus of today's science. In this sense, it is a far superior explanation for the success of scientific theories than any version of realism we've here considered.[68]

8. CONCLUSION

The following number among our conclusions. The threatening historical argument is neither an induction nor an inference to the falsity of present theories, and numerous successful novel predictions have come from false theories. It looks as though the approximate truth of T does not make T likely to succeed. And a nonreferential notion of approximate truth is in conflict with the explanationist argument for realism. We've seen further that, given the counterinstances at each level, there are a number of significant implications of the meta-*modus tollens*. False theories are rendered miracles; thus the first premise of the realist's inference is unacceptable. And, despite the realist's own insistence on the need for explanation, the much-advertised explanatory ability of realism is rendered dubious given its inability to explain these instances. The realist hypotheses — the asserted correlations between success and truth, approximate truth, etc. — have no confirming instances without presupposing realism. And each realist hypothesis is falsified many times in the history of science. Finally, we've seen a non-realist competitor that, in great contrast to realism, explains the success of all successful scientific theories. Though I consider myself to be a scientific realist of sorts, a theory-based scientific realism invoking (approximate) truth has yet to make its case.

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NOTES

[1]In this paper, I am restricting my discussion to truth-based theory realism and am setting aside the entity realisms of Ellis (1979) (1990), Hacking (1983) Cartwright (1983) and Clarke (2001).

[2]Since the attribution of miracles is seen to be equivalent to giving no explanation at all, we can read this argument as claiming that the theory's truth is the *only possible* explanation for predictive success.

[3]While knowing that our theories are likely to be false would bear on whether or not we are *justified in believing* that they are true, this is not Laudan's approach.

[4]Genuine versions of the pessimistic meta-induction can be found in Rescher (1987, p. 5) and Putnam (1984, p. 147). In fact, Laudan himself did state a version of the pessimistic induction four years earlier (1977, p. 126). Nonetheless, I submit that Laudan is making no pessimistic meta-induction in the 'Confutation' article.

[5]While Psillos refers to Laudan's list as an inductive inference, he also puts an appropriate spin on it (1999, p. 99, p. 102). However, he is inconsistent in this, to the detriment of his own defence against Laudan. His wavering makes salient the fact that these two arguments are often conflated.

[6]I've recently read a preprint by Peter Lipton in which he presents a careful analysis that details the precise factors that render this induction weak (2000).

[7]While, on occasion, Laudan's argument appears to be *implicitly treated* as I've construed it, in order to appreciate the full implications of the argument (to be spelled out below), I consider it important that the proper formulation be made explicit.

[8]Jarret Leplin makes this point against Laudan (again, misconstruing Laudan's argument as a pessimistic induction) (1997, pp. 141-142). See also Devitt (forthcoming, p. 19, footnote, 30), who appears to share Leplin's concern.

[9]Laudan does speak of the theories on his list as being 'evidently false' (1981, p. 35) and false 'so far as we can judge' (1981, p. 33). In order to keep the argument clean, he should instead only say they are false 'according to present science', 'by contemporary lights', etc.

[10]Putnam (1984, pp. 143-144) makes this move, albeit, in his more sophisticated realism.

[11]In the same breath, realists may introduce the claim that it is possible to justifiably believe something that we later conclude to be false. This, however, would serve as no response to the meta-*modus tollens*. The counterinstances challenge the justification for believing the realist hypothesis *in the first place*, irrespective of whether a particular theory is later concluded to be false.

[12]Novel success is also appealed to in response to van Fraassen's Darwinian explanation of the success of science. See Worrall (1984), Brown, (1985), and Musgrave (1985, 1988).

[13]Carrier (1991, pp. 30-31)

[14]Hacking (1983, p. 69)

[15]Hutchison (1973)

[16]Knight (1989, p. 120); Hutchison (1973)

[17]Psillos (1999, p.120). This is an appropriate place to acknowledge, along with Whewell, Duhem, Psillos, *et al*, the great significance of the derivation of novel *theoretical generalisations* as well as *singular empirical* predictions. In fact, I think few would disagree that, so long as such predicted theoretical generalisations are retained in a later theoretical scaffolding, they are even *more* valuable than singular empirical predictions. For from the predicted theoretical generalisations, one can derive further individual empirical predictions. And, so long as the empirical successes of those generalisations don't rest on assumptions that contradict the original theory, these empirical predictions count no less as successes for the original theory.

[18]Carrier (1993, p. 402)

[19]Carrier (1991, p. 30)

[20]Hutchison (1981a, p. 9)

[21]Hutchison (1981a, p. 9)

[22]Hutchison (1981a, p. 8). Rankine named this state-function 'the thermodynamic function' in 1854. But it was already present in his papers read on February 2, 1850. As Hutchison explains, 'All the involved calculation had been done by 1850, though Rankine still lacked a motive to single out the entropy function for specific attention' (p. 8). This was 15 years before Clausius's claim that entropy increases.

[23]Hutchison (1981a, p. 9). See also Hutchison (1981b, p. 104)

[24]Mason (1962, pp. 289-290)

[25]Mason (1962, pp. 292-293)

[26]Worrall (1989a, p. 142)

[27]Motz and Weaver (1989, p. 104, p. 123)

[28]Worrall (1989a, 1989b, 1994)

[29]Worrall (1989b, p. 145)

[30]Worrall (1989b, p. 152). See also the comments of Arago and Poinsot quoted in Worrall (1989b, p. 143)

[31]Worrall (1994, p. 335)

[32]Mason (1962, pp. 485-6)

[33]I've blended at least four confirmed predictions together here. Thomson found specifically, that acid salt contains exactly half the oxalate of normal salt. He found the same result when the base being held constant is strontium (Hudson, 1992, p. 84); (Leicester, 1965, p. 156); (Partington, 1957, p. 159). Partington states that Thomson varied the base rather than the

acid, but this is not important here. Wollaston added a third compound to Thomson's results, and found the three to have a 1:2:4 ratio. This third salt being potassium tetroxaliate (Hudson, 1992, p. 84); (Partington, 1957, p. 159). Just as with the oxalates, Wollaston found a 1:2 ratio between the quantities of the carbonic acid in the different carbonates. Likewise, the acid in the two sulfates was again in a 1:2 ratio (Partington 1957, p. 159).

[34]These confirmed predictoins came after his theory was put forward in 1803 (see Partington 1957, p. 158, p. 172); (Hudson 1992, p. 82).

[35]He arrived at this law from an early version of his atomic theory in 1801. He 'deduced' it from 'the idea that each particle could only repel others of its own kind, and that dissimilar particles exerted no forces on each other' (Hudson 1992, p. 80).

[36]Regarding the last four see Hudson (1992, p. 82).

[37]Hudson (1992, p.83)

[38]Specifically, in 1874, Landenburg showed there is no second isomer in pentachlorobenzene (Brush, 1999a, p. 25).

[39]Brush (1999a, pp. 27-28); (1999b, p.264)

[40]Brush (1999a, p. 32)

[41]Brush (1999a, pp. 58-61); (1999b, pp. 265-6, p. 290)

[42]This was confirmed in 1874 (see Leicester, 1965, p. 95). In fact, when Gallium was first discovered, its specific gravity was found to be a bit lower than Mendeleev had predicted. Mendeleev asked de Boisbaudran to recheck the value, and Mendeleev's value was confirmed (see Marks 1983, p. 313).

[43]This shorthand of merely *naming* these elements constitutes a great injustice to Mendeleev's actual predictions. In predicting germanium (which he called eka-silicon), for instance, what Mendeleev did was predict the existence of an element with the following properties: an atomic weight of 72, confirmed at 72.32; a specific gravity of 5.5, confirmed at 5.47; an atomic volume 13, confirmed at 13.22; a valence of 4, confirmed at 4; a specific heat of 0.073, confirmed at 0.076; a specific gravity of dioxide of 4.7, confirmed at 4.703, a molecular volume of dioxide of 22, confirmed at 22.16, plus many other such properties. (See Tables 1 and 2 in Leicester 1965, pp. 195-196). Mendeleev made amazingly detailed predictions for each of the first three elements listed here.

[44]Confirmed in 1879 (Leicester 1965, p. 195).

[45]Confirmed in 1885 (Leicester 1965, p. 196).

[46]That Mendeleev predicted these last four was pointed out to me by John and Maureen Christie. In the literature on Mendeleev, these are less commonly discussed than the first three. To see that he predicted these one can compare Mendeleev's table with the contemporary table. See, for instance, tables in Hudson (1992 p. 133, p.137). On Perrier and Segre's confirmation, see Isaacs (1991, p. 682).

[47]Confirmed in 1925 (Greenwood and Earnshaw 1986, p. 1211).

[48]Confirmed in 1939 (Isaacs 1991, p. 274).

[49]Confirmed in 1898 (Isaacs 1991, p. 544).

[50]Scerri (2000, p. 59)

[51]Scerri (2000, p. 59, footnote 27)

[52]Regarding the preceding five predictions (collapsed to three): Mendeleev's table reorders these elements, correctly contradicting the data then available; his corrections were later

confirmed. (See Hudson and compare the tables on 128 (data available) with 129 (Mendeleev's table)).

[53]Lakatos (1970, p. 147)

[54]Mason (1962, p. 558, p. 560)

[55]Guth (1997, Ch. 4)

[56]Peirce's construal of abductive reasoning requires that an explanation (A) makes a surprising fact (B) 'a matter of course' (1958, p. 189). I'd suggest that premise 1 is a liberal interpretation of this claim.

[57]Realists tend to neglect this issue. For instance, while Stathis Psillos attempts to elucidate just what verisimilitude is (Chapter 11, 1999), at no point there does he argue that such a property will even render empirical success likely.

[58]Notice that in pointing out that this consequence would follow from C2-Cn, no assumptions are being made about the world. It is known analytically given the corpus of science: we need only look at what the theories say to see that they make this prediction. (Of course, pointing out that such a prediction would fail empirically rests on the basic empirical claim that there is a universe.) The consequence of changing the charge of the electron, etc., is discussed in, for instance, Greenstein (1988, pp. 61-65).

[59]It should be clear that the result is not symptomatic of the fact that I've used the corpus of science. We can delimit our theory surrounding these changes in any number of ways and achieve the same result. Pre-reflectively, one might be concerned that my example fails for the following reason: the corpus of science simply prohibits the possibility that any such slight change in these charges can occur, thus, the alternatives cannot really be approximately true. (The claim internal to C1 that generates this concern would be the claim that a slight modification in charges causes matter to repel.) However, we cannot look to a given theory itself in order to determine what would make that theory approximately true. For the purposes of realism, our notion of approximate truth must, of course, be defined independently of any particular theory.

[60]If the realist argument does not involve Peirce's premise, it is most pressing that realists clarify just what their replacement premise is and then show that that premise holds in light of the objections above.

[61]Psillos attempts to show those claims that were 'essential' to 'key predictions' have been retained across theory change. The criterion he presents for determining what he calls the 'essentiality' of a postulate is very elaborate (1999, p. 110). However, he himself does not appear to take that criterion seriously. Nor will I here. I have pointed out elsewhere that he does nothing to show that retained theories fit his criterion or that rejected theories fail to fit his criterion. In fact, after introducing his criterion, in the 36 some odd pages where he is addressing the historical argument, he never employs or even mentions that criterion again. Moreover, his criterion is not suitable to the purposes for which he has invoked it — namely, determining which constituents are *responsible* for the prediction; it is too vague to be applicable; and it does not appear to be epistemically motivated. In fact, he inadvertently replaces it with what turns out to be an altogether different criterion for realism: we should be realists about the constituents to which scientists are (or at least express themselves in print as being) committed.

[62]Though a deployment realist, Psillos can admit this for his purposes, as he is attempting to show that the constituents were not *essential*, which he defines using a much stricter condition than the mere *use* of a constituent. See footnote above.

[63]As detailed by Hutchison in (1981a).

[64]Notably the richness of Rankine's theory is not exhausted by its temporally novel and use-novel success. For instance, Rankine's was a unifying theory, exhibiting a significant degree of breadth, reconciling seemingly disparate phenomena. Hutchison writes, 'unlike earlier proponents of this general view, Rankine extended the hypothesis to cover the structure of the luminiferous aether as well as that of ordinary matter' (1981a, p. 4).

[65]It leads for instance to Dalton's conclusion that water is HO rather than H_2O.

[66]I have borrowed Newcomb's quote from Adolf Grünbaum ([1976], quoted on p. 356). Grünbaum is concerned with a different issue, however, namely, *ad hoc* hypotheses in science and their implication for Popper's system.

67For instance, Aristotelian cosmology may well be seen to predict, in a use-novel sense, the rise of certain modern objects that appear to be fire driven — e.g., hot air balloons, bottle rockets, the space shuttle, etc.: the fire seeks to return to its natural place, and as its quantity is greater than the quantity of earthly matter in the given object, it pushes that object upward.

[68]I am interested in rendering MS more robust. I consider promising the possibility of conjoining it to variants of the taxonomical realism of Duhem (1906) and Carrier (1991, 1993) and/or the structural realism of Poincaré (1902) and Worrall (1989a).