Fool's Errand, Devil's Bargain: What Kind of Underdetermination Should We Take Seriously?[1]

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Abstract

Advocates have sought to prove that underdetermination obtains because all theories have empirical equivalents. However, algorithms for generating empirical equivalents simply exchange underdetermination for existing philosophical chestnuts, while the few convincing examples of empirical equivalents will not support the desired sweeping conclusions. Underdetermination does not, however, depend on empirical equivalents: our warrant for current theories is equally undermined by presently unconceived alternatives as well−confirmed by merely all actual evidence, so long as this transient predicament recurs for each theory and body of evidence we consider. The historical record supports the claim that this recurrent, transient underdetermination predicament is our own.

I. Duhem's Challenge

Nearly a century ago, Pierre Duhem forcefully challenged the presumption that even our best−supported theories about inaccessible domains of nature are true, or approximately true, or probably approximately true, or anything of the sort: might there not be, he asked, presently unconsidered alternatives as well or better confirmed by the evidence we have than the theories to which we subscribe on the strength of that very evidence?

> Between two contradictory theorems of geometry there is no room for a third judgment; if one is false the other is necessarily true. Do two hypotheses in physics ever constitute such a strict dilemma? Shall we ever dare to assert that no other hypothesis is imaginable? Light may be a swarm of projectiles, or it may be a vibratory motion whose waves are propagated in a medium; is it forbidden to be anything else at all? (1914/1954, 189−190).[2]

Rightly regarded as a heady doctrine, Duhem's suggestion has enjoyed a dynamic career in the philosophy of science, grounding influential attacks on scientific realism (e.g. Van Fraassen 1980) and finding a place (along with his holism) in Quine's particularly influential general theory of knowledge. But its plausibility is hard to assess: critics have consistently wondered why we should <u>assume</u> that there are always alternative theories equally well−confirmed by the evidence, and why we should let the mere possibility that there <u>might</u> be prevent us from believing the best−confirmed theories we do have.

Challenged to show that underdetermination is more than a speculative possibility, advocates have sought to demonstrate that from any given theory we can (ideally in algorithmic fashion) always produce <u>empirical equivalents</u> to that theory: competitors that make identical empirical predictions and which therefore cannot be distinguished from it by any possible evidence.[3] In the next section I will suggest that these efforts have fallen flat because they are too ambitious in the wrong way: the only senses in which this claim admits of proof or algorithmic demonstration are trivial, while the few convincing examples of genuine empirical equivalents cannot support the sweeping implications that defenders have drawn. In the final section I will suggest that the existence and status of empirical equivalents is neither the only nor the most important issue concerning underdetermination, for there is evidence supporting a distinct underdetermination predicament that poses a more genuinely worrying threat to the warrant for believing even our best scientific theories.

<u>2. Empirical Equivalents and Underdetermination</u>

Algorithms for generating empirical equivalents fall into one of two strategic categories: <u>global</u> algorithms are designed to produce empirical equivalents from absolutely any theory, while <u>local</u> algorithms seek to take advantage of some formal feature(s) of a particular theory to show that an infinite or indefinite number of empirically equivalent rivals to that theory can be produced by varying that feature.

Pursuing the global strategy, Kukla (1996, see also 1993) points out that any theory T supports such all−purpose empirical equivalents as T' (the claim that T's observable consequences are true, but T itself is false), T'' (the claim that the world behaves according to T when observed, but some specific incompatible alternative otherwise), the hypothesis of the Makers (the debatably coherent fantasy that we and our apparently T−governed world are part of an elaborate computer simulation), and the hypothesis of the Manipulators (that our experience is manipulated by powerful beings in such a way as to make it appear that T is true). He devotes his efforts to defending such proposals from the accusation (see Laudan and Leplin 1991, Hoefer and Rosenberg 1994) that they fail to be "real theories" at all.

But Kukla's defense misfires, for whether such farfetched scenarios are real theories or not they amount to no more than a perspicuous presentation of the possibility of radical or Cartesian skepticism.[4] While most contemporary philosophers grant that this possibility is irrefutable, underdetermination was supposed to be a distinct and important problem that arose in the context of scientific theorizing about inaccessible domains of nature even for those who declined the fool's errand of responding to the Cartesian skeptic. Furthermore, even if we grant the holist's claim that our scientific theories are continuous with all of our other beliefs, Cartesian fantasies simply express the possibility of radical skepticism in the holist context, rather than any further problem of underdetermination generated by those holist commitments or otherwise. Thus, the global strategy's farfetched scenarios simply replace our worry about underdetermination with a quite different general (and insoluble) skeptical problem.

Some putative examples of empirical equivalents that are not algorithmically generated are nonetheless skeptical fantasies, like the notorious prospect of a continuously shrinking universe with compensatory changes in physical constants that make this state of affairs undetectable to us (i.e. theories we describe as making unmotivated and wildly implausible assumptions about nature). The point is that some judgments of prior plausibility are required in order to escape radical or Cartesian skepticism in the first place, and we are no less entitled to these resources in a scientific context than any other.[5] Thus, if Cartesian fantasies are the only evidence for underdetermination, then there is no distinctive problem to worry about aside from the (irrefutable but less interesting) possibility of radical skepticism.

Similar considerations apply to the demand that we consider the 'Craigian reduction' of a theory (i.e. a statement of that theory's observable consequences) as a competitor when trying to assess the plausible threat of underdetermination. Perhaps even Craigian reductions are "real theories", but the underdetermination worry was that there might be too many different accounts of the remote and inaccessible features of nature equally well−confirmed by the evidence, not simply that there are (as we already knew) multiple options for beliefs about the world that the evidence leaves us free to accept (cf. Van Fraassen 1980). Agnosticism about any and all accounts of the inaccessible aspects of nature is always possible, but is defensible only if the underdetermination of theory by evidence (or some other ground for suspicion about all theories) has already been independently established. It is not enough that the epistemically modest choice to believe all and only a theory's claims about observable phenomena is always left open by the evidence: for that matter, so is choosing to believe nothing at all.

By contrast to the global, the local algorithmic strategy eschews skeptical fantasies and seeks instead to take advantage of formal or systematic features of some particular theory to construct an algorithm for building an infinite or indefinite number of serious and genuinely scientific empirical equivalents to that very theory. Consider the now−famous example of TN(0): Newtonian mechanics and gravitational theory along with the claim that the universe is at rest in absolute space. We can generate any number of empirical equivalents to this theory of the form TN(v), where v ascribes some constant absolute velocity to the universe.

But such empirical equivalents prove too little. The sensible realist will reply that it is pathological to describe the alternatives here as a range of competing theories making identical predictions about the observable phenomena: rather, she will insist, we have just a single theory to which is being conjoined varying factual claims about the world for which that very theory (together with auxiliary assumptions accepted at the time[6]) implies that we cannot have any empirical evidence. Of course, it is not always trivial to determine which elements of a proposed theory are otiose by its own lights and those of the auxiliaries we accept, but the sensible realist will counsel realism only about those theoretical claims (whatever they are) that our theories themselves imply are amenable to empirical investigation. This realism should no more extend to the conjunction of Newtonian theory with claims about the absolute velocity of the universe than with claims about the absolute velocity of God.

Another way to see this point is to note that empirical equivalents of the TN(v) variety pose no threat to the approximate truth of our theories: if the realist believes TN(0) when one of the various TN(v) obtains, most of her theoretical beliefs about the relevant domain will be straightforwardly true. Thus, empirical equivalents of the TN(v) variety show at most that we would have been unjustified in taking any stand on the constant absolute velocity of the universe, not in accepting the other theoretical claims of Newton's theory.

Our response to the local algorithmic strategy, like the global, applies equally well to some famous non−algorithmic examples. John Earman suggests (drawing on results from Clark Glymour and David Malament), for example, that underdetermination threatens because "even idealized observers who live forever may be unable to empirically distinguish hypotheses about global topological features of some of the cosmological models allowed by Einstein's field equations for gravitation" (1993, 31).

But such claims about global topology−−concerning, for example, the compactness of space (as determined relative to some canonical foliation of spacetime)−−are simply factual claims about the world for which the General Theory of Relativity itself (again, in conjunction with the accepted auxiliary hypotheses) suggests that we are (or may be) unable to acquire evidence. But there is again surely something pathological about the claim that hooking one or another such claim to the General Theory of Relativity produces genuinely distinct, empirically indistinguishable theories: once again, the sensible realist will surely counsel realism only about those aspects of well−confirmed theories that <u>those theories themselves</u> (given accepted auxiliaries) hold to be empirically significant.

This analysis suggests that the local strategy, like the global, actually trades underdetermination in for a distinct and long−standing philosophical problem, this time in the theory of confirmation: if true empirical consequences of a theory are all that matters to its confirmation, then evidence E confirming theory T will equally well confirm theory T+C (where C is some further claim that does not affect T's empirical import), thus offering spurious confirmation to claim C. Thus, each algorithmic strategy manages to provide <u>proof</u> of the underdetermination predicament only by <u>transforming</u> the problem into one venerable philosophical chestnut or another.

In retrospect, perhaps efforts to <u>prove</u> that underdetermination obtains should always have struck us as overreaching, for such proof requires what is surely unattainable: a formal, algorithmic procedure for generating serious and genuinely distinct scientific alternatives to any given hypothesis that are equally well−supported by the evidence, just the kind of hard work it takes scientists years, decades and sometimes careers to accomplish. Perhaps it is small wonder, then, that <u>algorithms</u> for producing empirical equivalents ring hollow, or that realists have begun to demand that underdetermination doomsayers actually produce genuinely scientific empirical equivalents genuinely distinct from existing theories before they are willing to take rumors of their existence seriously (see Kitcher 1993, Leplin 1997).

Are there, then, any convincing examples of empirical equivalents which are neither skeptical fantasies nor trivial variations on a single theory? I believe that there are, although plausible cases involve pairs (rather than infinite or indefinite collections) of empirical equivalents and are not generated algorithmically. Perhaps the most convincing is the only further example Earman offers in support of the underdetermination predicament: "TN (*sans* absolute space)...opposed by a theory which eschews gravitational force in favor of a non−flat affine connection and which predicts exactly the same particle orbits as TN for gravitationally interacting particles" (1993, 31; see also Glymour 1977). Neither of these theories is a skeptical fantasy, nor are they plausibly construed as the same theory to which additional claims unsupportable by any evidence have been tacked on: treating gravitational attraction as a fundamental force seems substantially different from treating it as manifesting the curvature of spacetime.[7] There are other plausible (albeit more controversial) cases too, such as Special Relativity versus Lorentzian Mechanics (controversial because it might be thought a skeptical fantasy to suggest, as Lorentzian Mechanics does, that objects, including our measuring devices, expand or contract when in motion relative to absolute space), or Bohmian hidden variable versus standard Von Neumann−Dirac formulations of Quantum Mechanics (controversial because it is not clear that we understand Quantum Mechanics well enough to say convincingly what formulations of it count as genuinely different theories).[8]

But just what moral is it appropriate to draw from the existence of one or even a small number of such cases? Do they provide a sufficient warrant for concluding that serious empirical equivalence is a ubiquitous phenomenon? Surely not. Examples of empirical equivalents are supposed to ground underdetermination worries by suggesting that with a little work and ingenuity, empirical equivalents to virtually any of our best−confirmed scientific theories could be likewise generated from a presumed infinite space of alternatives. But the profound difficulties encountered in trying develop even one or a few convincing examples of <u>nonskeptical</u> and <u>genuinely distinct</u> empirical equivalents might sensibly be seen to support just the opposite conclusion: that nontrivial empirical equivalents are actually quite difficult to come by and that our few hard−won examples therefore offer no reason to suppose that an infinite space of nonskeptical and genuinely distinct empirical equivalents looms over each of our scientific theories.

Furthermore, convincing examples of empirical equivalents are nearly always found in the physical sciences and (as Laudan and Leplin (1991, 459) rightly point out) typically invoke the relativity of motion in one guise or another. This pattern might well lead us to suspect that something about the characteristic formal or mathematical structure of some contemporary physical theories makes it possible to introduce novel elements into their fundamental ontologies and/or mechanics whose impact on those theories' empirical consequences can be eliminated by integrated sets of compensating adjustments elsewhere, and that it would therefore be a <u>mistake</u> to generalize from this atypical sample to the whole of scientific inquiry: contemporary biologists and philosophers of biology, for example, haven't the slightest idea how they might go about constructing even one (substantively distinct, nonskeptical) empirical equivalent to the modern synthesis of Darwinian evolutionary theory and Mendelian genetics. We might reasonably doubt, then, that what can (sometimes) be done for physical theories can probably be done for all or most scientific theories, or even that it can <u>ever</u> be done for any theories besides those with the characteristic formal and/or mathematical structure (if such there be) of contemporary physical theories.

The case for empirical equivalents, then, will simply not support the intoxicating morals that proponents hoped to draw: algorithms generate skeptical fantasies or trivial variants, while one or a few convincing examples, all dearly purchased and drawn from a single domain of scientific theorizing, are unable to support the sweeping conclusion that there are likely empirical equivalents to most theories in most domains of scientific inquiry. Scientists or philosophers concerned with a particular theory should surely consider whether that theory has serious empirical equivalents, but the sensible realist will insist that the existence of even a few such cases provides sufficient warrant only for suspending belief between those empirical equivalents we discover, or at most for withholding belief from a theory that shares the distinctive characteristics of theories in that single domain from which convincing examples can be drawn.

<u>III. Recurrent, Transient Underdetermination and a New Induction over the History of Science:</u>

Notwithstanding the amount of ink spilled over the issue, however, the lack of any convincing case for the widespread existence of genuine empirical equivalents simply does not settle the question of how seriously we should regard the threat of underdetermination. To see why, notice that Duhem's original formulation of the concern (Section I) does not at all depend upon the possibility of empirical equivalents indistinguishable by all <u>possible</u> evidence from one another: he worries instead whether we should "ever dare to assert that no other hypothesis is imaginable" and whether light is "forbidden to by anything else at all" besides the alternatives we have considered. That is, Duhem worries that there might be alternative hypotheses <u>we have not yet even</u>

imagined or entertained that are nonetheless equally well−confirmed by all of the evidence we actually have in hand. Following Sklar (1975), we might describe this as a transient underdetermination predicament; that is, one that could be resolved by accumulating further empirical evidence.

But even a transient underdetermination predicament suffices to undermine any justification for believing in our scientific theories so long as it is recurrent: that is, so long as this predicament arises for each theory we consider and each finite body of evidence we manage to generate in support of it. Thus, scientifically serious alternatives that are merely as well−confirmed by all the actual (rather than all possible) evidence we have as the theories we embrace on the strength of that evidence are sufficient to undermine the warrant for those theories, so long as we have some reason to think that there is (probably) at least one such alternative available whenever we face a decision about whether or not to believe a given theory on the strength of a given body of evidence.

Thus, advocates of underdetermination have struck a Devil's bargain in hitching their case so closely to empirical equivalents. Finding empirical equivalents was, after all, simply the most promising strategy for trying to prove that underdetermination obtains, but the connection between the two issues has become so firmly established that both the most influential (and ostensibly general) recent attack on underdetermination (Laudan and Leplin 1991) and its most influential recent defense (Earman 1993) proceed solely by addressing the existence and status of putative empirical equivalents.[9] Little−noticed in the crossfire is the fact that a recurrent, transient underdetermination predicament serves just as well to defeat any warrant for believing in the truth of even our best scientific theories.

A notable exception is Sklar (1981), which is indeed concerned with the threat of presently unimagined alternatives to our theories. But Sklar seems to regard it as simply obvious in light of our imaginative limitations (or perhaps these and "reflection upon historical scientific experience" (18)) that there is invariably a "vast array" (17) of equally well−confirmed, serious alternatives to any given theory, just the claim with which scientific realists wish to take issue (see Kitcher 1993, Leplin 1997). Elsewhere (1975, 30) Sklar simply supposes without defense that even those who are skeptical of empirical equivalence "are likely to admit that transient underdetermination is a fact of epistemic life". Sklar is in good company here,[10] but the crucial question remains whether we have some reason to believe that we occupy a general predicament of recurrent, transient underdetermination or not. In what follows I will try to flesh out the suggestion that the historical record of scientific inquiry provides evidence that this is indeed our epistemic predicament, but I will suggest that the historical record contradicts Sklar's further suggestion (1982) that this threat can be substantiated only for fundamental physical or cosmological theories.

While it is difficult to acquire convincing evidence regarding the likely existence of presently unconceived theories, of course, the history of scientific inquiry is itself an important source of evidence regarding the extent to which recurrent, transient underdetermination is our actual epistemic predicament rather than a speculative possibility. We have, it seems, throughout the history of scientific inquiry and in virtually every scientific field, occupied an epistemic position in which we could conceive of only one or a few theories that were well−confirmed by the available evidence, while the subsequent history of inquiry has routinely (if not invariably) revealed additional possibilities as well−confirmed by the evidence then available as those we were inclined to accept on the strength of that evidence.[11] For example, in the historical progression from Aristotelian to Cartesian to Newtonian to contemporary mechanics, the evidence available at the time each earlier theory was accepted offered equally strong support to each of the (then−unimagined) later alternatives. The same pattern would seem to obtain in the historical progression from elemental to early corpuscularian chemistry to Stahl's phlogiston theory to Lavoisier's oxygen chemistry to Daltonian atomic and contemporary physical chemistry, from various versions of preformationism to epigenetic theories of embryology, from the caloric theory of heat to later and ultimately contemporary thermodynamic theories, from effluvial theories of electricity and magnetism to theories of the electromagnetic ether and contemporary electromagnetism, from humoral imbalance to miasmatic to contagion and ultimately germ theories of disease, from 18th Century corpuscular theories of light to 19th Century wave theories to the contemporary quantum mechanical conception, from Hippocrates's pangenesis to Darwin's blending theory of inheritance (and his own 'gemmule' version of pangenesis) to Weismann's germ−plasm theory, Mendelian and contemporary molecular genetics, from Cuvier's theory of functionally integrated and necessarily static biological species or Lamark's combination of spontaneous generation and need−driven evolutionary progressivism to Darwinian evolutionary theory, and so on in a seemingly endless array of theories, the evidence for each of which ultimately turned out to support one or more unimagined competitors just as well. Thus, the history of scientific inquiry offers a straightforward inductive rationale for thinking that there typically <u>are</u> alternatives to our best theories equally well−confirmed by the evidence, even when we are unable to conceive of them at the time.[12]

We cannot respond to these examples by noting that theories in the same general family or category as a later alternative (say atomism) sometimes <u>had</u> already been entertained by the time of an earlier theory's exclusive dominance, for our confidence in the truth of our present theories cannot survive an inductive rationale for thinking that present evidence likely supports a presently−unconceived, detailed <u>version</u> of a theory from an existing alternative family or type just as well as it supports the present alternative we accept on the strength of that evidence.

It will surely be objected, however, that in at least some of these cases changes in the accepted auxiliary hypotheses were required before the alternatives could rightly be regarded as equally well−confirmed by the available evidence as the accepted theory. This is so, but the objection misses the point that in such cases the needed alternative auxiliary hypotheses (often unconceived at the time) are typically <u>themselves</u> ones for which the available evidence provided equally compelling support. In other words, the <u>totality</u> of evidence available at the time of an earlier theory's acceptance typically offers equally compelling support for the <u>combination</u> of a later accepted alternative to that theory together with the requisite changes in auxiliary hypotheses that would later be accepted. And surely such a combination must be regarded as a scientifically serious alternative possibility, rather than a mere skeptical fantasy, for it is ultimately accepted by some actual scientific community. This points out one further respect in which the history of science offers support for the prospect of recurrent, transient underdetermination: serious alternative hypotheses <u>need</u> not be unconceived of at the time, for our judgment that a particular existing hypothesis is scientifically implausible must sometimes be reevaluated in light of changes (perhaps themselves unconceived at the time) in our <u>other</u> beliefs, as in the famous cases of Avery's hypothesis that DNA is the hereditary material and Wegener's theory of continental drift. What the history of inquiry reveals, then, is a general pattern of our exclusive commitments to theories being undermined in time, <u>whether</u> by the creativity of scientists in generating previously unconceived (and equally well−confirmed) hypotheses or by previously dismissed hypotheses attaining scientific respectability.

This line of evidence will no doubt disappoint some advocates of underdetermination for it falls well short of the sort of proof of the underdetermination predicament that advocates have traditionally sought. Furthermore, unlike constructing empirical equivalents, it does not allow us to say just which actual theories are underdetermined by the evidence, nor anything about what the (unconceived) competitors to present theories look like. But the advocates have erred, I suggest, in focusing exclusively on the kind of underdetermination they can hope to prove obtains, rather than the kind that actually threatens the confirmation of even our best scientific theories. The historical record provides only evidence rather than proof of underdetermination (and in these brief remarks I have been unable to do more than suggest that this is indeed the verdict of the historical record), but it has the virtue of being evidence in favor of the sort of serious, distinctive and genuine underdetermination predicament that is well worth our taking seriously.

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Notes

[1] [Acknowledgments withheld.]
[2] I will not be concerned with Duhem's holism, which grounds the distinct worry that theory choice is underdetermined because any theory may be retained if we make suitable changes to the set of auxiliary assumptions we accept. The worries are connected, of course, for such changes might also be presently unconceived, but the force of the worry Duhem articulates here does not depend on accepting holism.
[3] For Earman (1993), the crucial sense of empirical equivalence (his EI_3) obtains between two hypotheses just in case two worlds in which those two hypotheses are respectively true need not be distinguished by some piece of empirical evidence. This difference will not matter for our purposes.
[4] Kukla sometimes appreciates this point (1996, 158), but not how it undermines his case (see below).
[5] The need for such judgments will not alone evade underdetermination, however, for the prior plausibility of electrons, phlogiston or curved spacetime is simply not on par with that of Cartesian Evil Demons (cf. Van Fraassen 1980, 36).   I suspect that this difference is what is really at issue in the (misleading) claim that some scenarios are too farfetched to constitute "real theories" at all (e.g. Leplin and Laudan 1993, 11).
[6] Of course, the set of accepted auxiliary assumptions may change over time, defeating the claim of empirical equivalence (a central point in Laudan and Leplin's (1991) attack on underdetermination), but here we are concerned with what to make of the prospect of theories that are empirically equivalent given (or 'indexed to', see Kukla 1993) a particular set of auxiliary assumptions, or alternatively, with empirically equivalent "global theories" or "systems of the world" (see Hoefer and Rosenberg 1994).
[7] It is worth noting, however, the serious difficulties Sklar (1982) points out facing any realist notion of theoretical equivalence distinct from empirical equivalence itself.
[8] While Eddington, Reichenbach, Schlick and others have famously agreed that General Relativity is empirically equivalent to a Newtonian gravitational theory with compensating "universal forces", the example is questionable because the Newtonian variant has never been given a precise mathematical formulation (the talk of universal forces is invariably left as a promissory note), and it is not at all clear that it can be given one. (David Malament has made this point to me in conversation.) The "forces" in question would have to act in ways no ordinary forces act (including gravitation), or any forces could act insofar as they bear even a family resemblance to ordinary ones. In the end, the "forces" are no better than "phantom effects" and we are left with just another skeptical fantasy. At a minimum, defenders of this example have not done the work needed to show that we are faced with a credible case of nonskeptical empirical equivalence. Were this example to be accepted as genuine, however, it would not greatly affect the status of my conclusions (see below).
[9] Kukla (1993, 5−6) accuses Laudan and Leplin of presuming that the case for underdetermination rests upon empirical equivalents alone. Leplin and Laudan (1993, 16) deny this, but insist that their joining of the two doctrines was "not capricious", for "the philosophers whose derogations of the epistemic enterprise we have been concerned to redress (e.g., Quine and Rorty)...come to [the underdetermination thesis]...through [their belief in empirical equivalents]". And Kukla's own later defense (1996) describes "the" argument from underdetermination as proceeding simply from the premise that all theories have empirical equivalents.
[10] Quine's classic (1975, 313), for example, so often cited as providing evidence for underdetermination, only blusters: "Surely there are alternative hypothetical substructures that would surface in the same observable ways".
[11] Of course, a theory need not explain or accommodate all the existing data in order to be well−confirmed: evidential anomalies are allowed. The point is that we have repeatedly been able to conceive of only a single theory that was well−supported by all of the available evidence when there were indeed alternative possibilities equally well−supported by that evidence according to the same standards of confirmation. Nor does this argument ignore the phenomenon of explanatory losses in the transition from an earlier theory to a later one: a theory need not explain everything that a competitor explains in order to be as well−supported by the totality of available evidence: the theories may simply have different evidential anomalies.
[12] By contrast, the Disastrous Induction over the History of Science notes simply that past, successful theories have turned out to be false and suggests that we have no reason to think that present successful theories will not suffer the same fate (see Laudan 1981).