Another Look at Empirical Equivalence and Underdetermination of Theory Choice[1]

Pablo Acuña
Instituto de Filosofía, Pontificia Universidad Católica de Valparaíso
pablo.acuna.l@pucv.cl

Dennis Dieks
Institute for the History and Foundations of Science, Utrecht University
d.dieks@uu.nl

Abstract. In 1991 Larry Laudan and Jarret Leplin proposed a solution for the problem of predictive equivalence and empirical underdetermination. In this paper we argue that, even though Laudan and Leplin's reasoning is essentially correct, their solution should be accurately assessed in order to appreciate its nature and scope. Indeed, Laudan and Leplin's analysis does not succeed in completely removing the problem or, as they say, in refuting the thesis of underdetermination as a consequence of empirical equivalence. Instead, what they show is merely that science possesses tools that *may* eventually lead out of an underdetermination impasse. We apply their argument to a real case of two empirically equivalent theories: Lorentz's ether theory and Einstein's special relativity. This example illustrates the validity of Laudan and Leplin's reasoning, but it also shows the importance of the reassessment we argue for.

Keywords: Empirical equivalence, underdetermination, theory choice, non-empirical virtues, empirical evidence, confirmation, special relativity, Hendrik Lorentz.

1 Introduction

Empirical equivalence of theories is often regarded as a philosophically deeply problematic situation for science, because it would appear to make an evidence-based choice between theories impossible. The problem has received lots of attention in the philosophy of science literature. This is hardly surprising: if the objectivity of theory choice is endangered, perhaps even the very rationality of science might be threatened.

Larry Laudan and Jarret Leplin (1991) have offered a very influential argument designed to neutralize this problem, and they are commonly considered to have achieved their aim. However, in this paper we argue that, even though Laudan and Leplin's reasoning is essentially correct, the question of what exact conclusion can be drawn from it requires an accurate assessment. In particular, it turns out that Laudan and Leplin's argument does not dispel the problems of empirical equivalence and underdetermination from the practice of science. What it does show is that the regular practice of science *may* eventually be able to break a deadlock of underdetermination. In other words, Laudan and Leplin's argument indicates that empirical equivalence and underdetermination are problems about which we can never be certain that they will last: perhaps science will find a way out. The regular practice of science may lead to the breakdown of the empirical equivalence between specific theories and termination of the underdetermination of the choice to be made. However—just as with any other problem of science—that a specific problematic situation will thus be dissolved is not guaranteed and epistemological problems will persist in situations of unresolved underdetermination.

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This paper is divided in six sections. In section two we provide a simple schematic presentation of the problem. In the third section we consider the partial solution that non-empirical or theoretical virtues provide. In section four we analyze Laudan and Leplin's proposal along with the most relevant objections that have been leveled against it. A close consideration of these objections is helpful in order to understand what really has been achieved by their argument. The resulting reassessment, which constitutes the main subject of this paper, is presented in the fifth section. In section six we use an actual example of empirical equivalence between two theories as a test for our arguments: the case of Lorentz's ether theory *vs.* Einstein's special relativity. In section seven we offer some concluding remarks.

2 The problem

Laudan and Leplin present the problem of empirical equivalence (EE) and underdetermination (UD) in the following way: 'By the 1920s, it was widely supposed that a perfectly general proof was available for the thesis that there are always empirically equivalent rivals to any successful theory. Secondly, by the 1940s and 1950s, it was thought that—in large part because of empirical equivalence—theory choice was radically underdetermined by any conceivable evidence' (Laudan and Leplin 1991, 449). As this quote shows, the problem Laudan and Leplin address arises from the threat of universal EE and the general UD of theory choice that is supposed to be its consequence. This UD would lead to deeply problematic consequences—the most immediate one being that the status of the commonly supposed ultimate basis for the acceptance of theories, empirical evidence, gets epistemically eroded:

> The idea that theories can be empirically equivalent, that in fact there are indefinitely many equivalent alternatives to any theory, has wreaked havoc throughout twentieth century philosophy. It motivates many forms of relativism, both ontological and epistemological, by supplying apparently irremediable pluralisms of belief and practice. It animates epistemic skepticism by apparently underwriting the thesis of underdetermination. *In general, the supposed ability to supply an empirically equivalent rival to any theory, however well supported or tested, has been assumed sufficient to undermine our confidence in that theory and to reduce our preference for it to a status epistemically weaker than warranted assent.* (ibid., 450, our emphasis)

A simple schematic presentation of the EE/UD problem as conceived by Laudan and Leplin can be given in terms of a logical argument with two premises. As we will see, Laudan and Leplin's solution consists of two parts, which are best understood as directed against the first and the second premise, respectively. The first premise is that *for any theory T and any body of observational evidence E, there is another theory T′ such that T and T′ are empirically equivalent with respect to E*. The support for this statement is twofold. First, some authors claim that there exist algorithms that are able to generate an empirically equivalent theory *T′* given any theory *T*. Second, there is the Duhem-Quine thesis that a hypothesis can entail observable consequences only with the help of auxiliary assumptions. Thus, one may surmise that conflicting evidence can be accommodated by any hypothesis through the introduction of suitable arrangements in the auxiliary assumptions. Accordingly, one should generally expect that if a hypothesis *H*—along with the class of auxiliary assumptions *A*—entails the observational consequence *e*, there exists another hypothesis *H′* that can also entail *e* by introducing a suitable class of assumptions *A′*[2]. The second premise of the argument says that *only observational statements that can be logically derived from a theory can count as potential empirical evidence to support it*. This is just a formulation of a standard principle of confirmation: an observational report counts as evidence for a certain (deterministic) hypothesis only

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[2] Suppose that the hypotheses *H* and *H′* are rivals, that $(H \wedge A) \rightarrow e$, that $(H' \wedge A) \rightarrow \neg e$, and that *e* is observed—so that *H* is confirmed and *H′* disconfirmed. The Duhem-Quine thesis implies that it is always logically possible to change *A* in a way such that $(H' \wedge A') \rightarrow e$. Therefore, it is always logically possible to create EE between *H* and *H′*.

if it can be derived, explained or predicted from that hypothesis—with the help of auxiliary assumptions, of course[3]. These two premises entail the problematic conclusion. If there is an EE rival to any theory, and if a theory gets empirically confirmed only by means of the observational consequences it entails, then acceptance and rejection of any theory are empirically underdetermined. If this problem were intractable, then the objectivity and even rationality of theory choice in science would come under severe threat.

EE and UD can be understood as a problem in several (connected) contexts. For example, the logical possibility of predictive equivalence is often taken as a challenge for scientific realists—it is an obstacle for the idea that (converging) truth is a goal that science can reasonably pursue. Therefore, it is important to underscore that we will only tackle the problem of EE and UD in the context of the challenge that it poses to the *rationality* and *objectivity* of theory choice. In the following section we will show that non-empirical virtues provide a partial solution to the problem in the sense that they may offer *rational* grounds to make a choice in cases of EE. However, these rational grounds do not suffice to determine an *objective* choice—if non-empirical virtues were the only way to address the problem, a radically pragmatist conception of theory acceptance would result. Fortunately, Laudan and Leplin's arguments show that EE and/or UD can be broken, so the threat against objectivity is countered. But Laudan and Leplin appear to overestimate the scope of their proposal—they present their conclusions in terms of a refutation of the UD thesis as a consequence of EE, so that the problem of theory choice is not really the case. However, as we will discuss, a breakdown of EE and/or UD will only happen under certain conditions, and there is nothing in science or epistemology that guarantees that such conditions will be actually met in all problematic cases[4].

3 A partial solution

A straightforward way out of the problem would be to weaken premise 2 by having recourse to *non-empirical* features of the EE theories involved. If one of the theories is simpler or proves to have more explanatory power than its rival, for example, one has a reason to prefer this theory after all. However, both simplicity and explanatory power are features that are controversial and hard to assess unambiguously. In the case of simplicity, the very definition of the concept is far from clear—it looks like a feature of theories that depends on subjective considerations: *one person's simplicity is another person's complexity*. Moreover, as Mario Bunge states (1961), there are multiple senses in which a theory can be regarded as simple—syntactical, semantic, epistemological and pragmatic—and these different forms of simplicity are not necessarily compatible with each other. Therefore, it is very problematic, at best, whether a theory can be simpler than another in unambiguous, objective terms.

Something similar holds in the case of explanatory power. What a scientific explanation is, or must be, is an open philosophical question. There are well-known arguments for the position that explanation

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[3] In *probabilistic* theories we have to refine the criteria: we should require that the *probability* of the evidence is the same according to the theories in question.

[4] As Laudan and Leplin state, 'A number of deep epistemic implications, roughly collectable under the notion of "underdetermination", have been alleged for empirical equivalence. For instance, it is typical of recent empiricism to hold that evidence bearing on a theory, however broad and supportive, is impotent to single out that theory for acceptance, because of the availability or possibility of equally supported rivals. Instrumentalists argue that the existence of theoretically noncommittal equivalents for theories positing unobservable entities establishes the epistemic impropriety of deep-structure theorizing, and with it the failure of scientific realism. Some pragmatist infer that only nonepistemic dimensions of appraisal are applicable to theories, and that, accordingly, theory endorsement is not exclusive, nor, necessarily, preferential' (ibid., 459-460). In this paper we will deal only with the first and third dimensions of the problem that Laudan and Leplin mention in this quote, not with the second one. For a general assessment of the problem of EE and UD as a problem for the realist, see (Psillos 1999).

is an essentially context-dependent concept. Bas van Fraassen (1980, 134-157), for example, argues that an explanation is an answer to a why-question, so that the degree of explanatory power of a theory depends on the specific why-question that is being asked and on the context of that question. Consequently, different why-questions and different contexts can yield different degrees of explanatory power for the same scientific hypothesis or theory[5].

These remarks already illustrate that non-empirical features will often be pragmatic and context-dependent, so that they cannot be invoked in order to make an entirely objective, epistemically compelling choice between EE theories. It is true that pragmatic considerations regarding simplicity and/or explanatory power can provide plausible reasons to prefer one of the theories, and in this sense they may lead to dissolution of the problem with respect to the *rationality* of theory choice—even if empirical evidence cannot be invoked to determine a decision, pragmatic aspects could be used to make a rationally grounded choice. However, the *objectivity* of theory choice cannot be rescued in this way. Because of their pragmatic nature, non-empirical virtues like simplicity and explanatory power are context dependent. Even if it were possible to state completely unambiguously that a theory possesses more explanatory power than its rival in a certain context, there might well be other contexts in which the rival is simpler or explains better. In this case we would have a situation in which there are good pragmatic reasons supporting both theories, but since they are rivals we cannot accept them both at the same time[6]. The limitation of this kind of pragmatic solution is therefore that, even though pragmatic non-empirical features may provide us with plausible reasons to favor one of the theories, they are not enough to provide a fully objective and uniquely determined choice—the opposite choice could be rational as well. But, fortunately, we can do better than this. As we will see below, there are arguments showing that, in spite of EE, *empirical evidence* can be invoked in order to find a way out of the problem—so that a fully objective and uniquely determined choice can be made after all.

These remarks should not be taken to imply that non-empirical virtues and empirical evidence are two completely unrelated concepts—we are not arguing that non-empirical virtues must necessarily be *merely* pragmatic, subjective and context-dependent, and totally unconnected to empirical evidence. It is possible to conceive of these non-empirical virtues as ultimately grounded in an evidential basis—so that *theoretical* or *aesthetic* features would be a better label than *non-empirical*. James McAllister (1989) has offered an interesting account of such features along this line (see also Dawid 2013). McAllister argues that 'indicators of beauty' that can work as relevant criteria for theory choice are based on meta-inductions on the aesthetic aspects of empirically successful theories of the past. That is, the criteria that define theoretical virtues may be indirectly based on the empirical success of theories:

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[5] See also (De Regt and Dieks 2005). There it is argued that 'scientific understanding', and *a fortiori* 'explanation', are pragmatic, context-dependent features. A phenomenon *P* is *understood* if there is an *intelligible* theory about *P*; and a theory *T* is intelligible if scientists are able to recognize qualitatively characteristic consequences of *T* without performing exact calculations. Different 'conceptual toolkits' can work as sources of intelligibility for a theory—visualization, causal explanations and unifications. The crucial point is that none of these explanatory virtues can be asserted as necessary or sufficient in order to obtain intelligibility for a theory; rather, which tools can provide intelligibility depends on contextual features: 'there is no universal tool for understanding, but a variety of 'toolkits', containing particular tools for particular situations. Which tools scientists have at their disposal, depends on the (historical, social, and/or disciplinary) context in which they find themselves. This context-dependence is typical of a meso-level nature, i.e., it is the scientific community that determines what tools are available and which skills are required to achieve understanding' (De Regt and Dieks 2005, 158).

[6] For constructive empiricists it is possible to accept both theories at the same time. Since they are not committed to the non-empirical content of the theories, they can accept both as empirically adequate and make a pragmatic preference if the context so requires. This stance only works if we are willing to accept that empirical adequacy is enough; that is, if empirical adequacy is the basic and sufficient feature that we should expect from a theory in order to accept it. The cost would be to quit to demands for *understanding* from scientific theories, for example. We think that a more general solution is available. There are arguments that show that a way out is possible regardless of whether one is a constructive empiricist, a realist, or what have you.

> A community selects its aesthetic canon at a certain date from amongst the aesthetic features of all past theories by weighting each feature proportionally to the degree of empirical success scored to that date by all the theories which have appeared to embody it. The community's aesthetic canon is then composed of the set of such mutually consistent features which have gained the greatest weighting. This is a clearly inductive procedure: as a theory demonstrates empirical success its aesthetic features will gain proportionate weight within the canon which is to serve in the evaluation of current theories, while conversely the aesthetic features of a theory which suffers a streak of empirical failures will win a progressively lesser weighting in theory-reference. (McAllister 1989, 39).

This account of aesthetic virtues might look as a way to justify them as a full solution to our problem—they might count as objective evidence after all. However, on closer inspection it appears that this possible connection between empirical evidence and theoretical-aesthetic features is not enough for considering the latter as a source for objective decisions in cases of EE. First, if so-called theoretical virtues are inductively linked to the empirical success of past theories, they are *ipso facto* epistemically subordinated to empirical evidence when it comes to theory choice—if (dis)confirming evidence goes against the prevailing canons of theoretical beauty it is clear that the former will be more important[7]. Second, we agree with McAllister when he emphasizes that 'indicators of truth', the evidential criteria which are decisive for justified theory choice are determined by the basic goals of science. Thus, indicators of truth are of a different character and are much more stable than indicators of beauty[8]. Actual, present empirical success remains the ultimate criterion for theory choice even after scientific revolutions, whereas the canons of theoretical beauty are governed by past performance and are intrinsically related to a specific state of science and to individual credos of scientists. Therefore, 'indicators of beauty' are not likely to provide objective and uniquely determined choices in cases of EE and UD: 'The hope that indicators of beauty will defeat the threat of underdetermination is incidentally revealed illusory: any decision on aesthetic grounds between empirically equivalent theories will in general be perceived as valid only within the paradigm then current and cannot hence be considered definitive' (ibid., 44)[9]. In other words, that theoretical virtues may derive from meta-induction on empirically successful theories does not imply that there will be a uniquely defined canon of beauty for all scientists. Moreover, within a single canon of beauty different theoretical features will usually play a role, and it may happen that in a pair of EE theories one of them scores better than its rival according to one feature, but that the

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[7] 'T. H. Huxley's aphorism about 'the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers' aptly describes the lag of aesthetic appreciation behind empirical assessment. The perceived beauty of a hypothesis is a function of the observational success of anteceding theories aesthetically similar to it; the novel fact appears as yet ugly because unassimilated within a theory of which the aesthetic qualities have been sufficiently weighted by the community. In time the community's indicators of beauty will evolve to render the theory erected about the new fact a structure of sovereign beauty and the disproven hypothesis merely passé' (ibid., 39-40).

[8] 'Metarationalism is clearly responsible for the genesis of indicators of truth because their inclusion among the desiderata of theories derives entirely from the a priori definition of the goal of science, the complete and true explanatory account of the universe. The requirements of internal consistency or predictive accuracy are prized not because they have previously been witnessed to accompany verisimilitude but because they are the elements of an explication of that very concept: indicators of truth appear in other terms to provide not a mere ampliative connotation but rather an analytic definition of truthlikeness. It remains of course possible for indicators of truth to be inductively learned by a scientific community but this is irrelevant to the a priori logical status of such criteria' (ibid., 38). In order to retain neutrality regarding the realism-antirealism schism, we can replace 'indicators of truth' for 'indicators of empirical success'.

[9] Furthermore, in times of scientific crisis there is no unique canon of beauty (if there ever is). A good example is given by the four-dimensional formulation of special relativity by Hermann Minkowski. Some scientists (such as Sommerfeld and Laue) considered the chrono-geometric formulation as expressing aesthetic virtues (based on simplicity, mainly), whereas others (e.g., P. Frank, at least for some time) considered it as expressing an empirical flaw (given the loss of intuitive visualizability involved). See (Illy 1981) and (Walter 2010).

situation is inverted according to a different feature. There is no clear and objective ranking of importance to classify the different theoretical features within one single canon. Scientists' and philosophers' ranking of aesthetic virtues connects to their individual epistemological and metaphysical commitments, with the consequence that canons of theoretical virtues are not fully objective—even if they are meta-inductively grounded.

Apart from the objectivity problem, another obstacle for meta-inductively supported theoretical virtues as a full solution of the problem of EE and UD comes from the nature of the inductive argument itself. As McAllister stresses, the meta-induction at issue is strictly *Humean*, in the sense that it is based upon mere past correlation, without any guarantee of empirical success *now*:

> The present account of indicators of beauty is intended to resemble the Humean explanation of the origin of notions of cause: just as Hume believed the inductive apprehension of causal links to be unsupportable by nomological data but a nonetheless ineluctable product of a driven mind, aesthetic canons in science boast no systematic relation to truth but spring from the psychological concerns of scientists. Neither Hume's account nor this concludes that notions thus formed are of no value: as causal links are a convenience of the Humean life, so indicators of beauty may here aid theory-construction and choice. It is, however, important to remember the contingent nature of concepts generated by Humean inductions and to avoid attributing to them any necessity. (ibid., 40-41)

The Humean nature of the induction implies that even though non-empirical features can provide reasonable grounds for making a choice, a decision based on this kind of features runs the risk of contradicting empirical evidence. If we face a case of EE and UD, and if we choose one of the theories in terms of theoretical virtues, it is still possible, as we will see below, that the future development of science may break the EE and/or the UD favoring—in terms of empirical evidence—the theory we rejected; and this is so even if the rejected theory is inferior in all contexts and in all aspects with respect to theoretical features. It is true that theory acceptance on the basis of empirical evidence is also risky. Any well-confirmed theory might prove empirically wrong in the long run. But a choice based on theoretical features implies a risk in the sense that further development of science could demonstrate that, according to the *ultimate* criterion of theory choice—empirical evidence—our choice is wrong. This risk can be avoided only if an epistemological inherent connection between theoretical virtues and empirical success were provided. However, if possible at all, an argument like this would necessary rely on very strong—and quite likely very doubtful—metaphysical presumptions.

4 Laudan and Leplin's solution

Larry Laudan and Jarret Leplin's influential paper (1991) consists in a careful analysis of the validity of the two premises explained in section 2. We will present their criticisms of the premises in turn.

4.1 The first premise

4.1.1 EE, observability and auxiliary assumptions

Laudan and Leplin affirm that three non-controversial theses regarding the nature of evidential confirmation imply that EE is not a universal feature of theories in the sense of the first premise. The first of these theses focuses on *the variability of the range of the observable*: 'any circumscription of the range of the observable phenomena is relative to the state of scientific knowledge and the technological resources available for observation and detection' (Laudan and Leplin 1991, 451). Whether an entity or process described by a theory qualifies as observable or not depends not only on the meaning of the corresponding term. Observability also crucially depends on the available experimental methods and instruments at a certain stage of scientific development[10]. The second thesis is *the need for auxiliaries in prediction*: 'theoretical hypotheses typically require supplementation by auxiliary or collateral information for the derivation of observable consequences' (ibid., 452). This *Duhemian* statement is so widely known and accepted that it does not require further comments. The third thesis concerns *the instability of auxiliary assumptions*: 'auxiliary information providing premises for the derivation of observational consequences from theory is unstable in two respects: it is defeasible and it is augmentable' (ibid.). As a consequence of scientific progress the class of auxiliary assumptions which are suitable for the derivation of observational consequences from theoretical hypotheses may get enlarged by the introduction of new well-confirmed theoretical hypotheses or newly discovered facts, or it may get reduced through the rejection of theoretical hypotheses which were previously accepted.

The effect of these three non-controversial theses on our problem is clear. If what is observable is variable and depends on current background knowledge, and if the class of auxiliary assumptions that are available for the derivation of observational consequences is also variable and background knowledge-dependent, then the class of observable consequences of any theory is relative to a particular state of scientific knowledge. Therefore, EE between two theories is a feature that is relative to a certain state of scientific knowledge as well: 'Any determination of the empirical consequence class of a theory must be relativized to a particular state of science. We infer that empirical equivalence itself must be so relativized, and, accordingly, that any finding of empirical equivalence is both contextual and defeasible' (ibid., 454).

The upshot is that if two theories completely coincide in their predictions now, it does not follow that they are essentially EE, for further development of science could break the equivalence and, *a fortiori*, the empirical UD of the choice to be made. However, Andre Kukla (1993, 1996) has offered the following natural criticism. We can accept that two theories (T_1, A_t) and (T_2, A_t)—where A stands for the auxiliary assumptions—can be considered as EE only relative to a time t. However, 'there is nothing in the argument that would force me to give up the view that every indexed theory has empirically equivalent rivals with the same index' (Kukla 1996, 142). Although the first premise of the problematic argument has been relativized with respect to time, it remains universal in scope, for even if the EE between T_1 and T_2 were broken in T_2's favor at time t'—by means of the new set of auxiliary assumptions $A_{t'}$—, at t' there will be a theory (T_3, $A_{t'}$) which is EE with (T_2, $A_{t'}$)—and so on for any future t. As Kukla puts it, 'the point is that we know that, whatever our future opinion about auxiliaries will be, there will be timeless rivals to any theory under those auxiliaries' (ibid.).

This implies that even though EE is a time-indexed relation between two given theories, theory choice will be empirically underdetermined for any value of t. The crucial point in Kukla's revival of UD is the universal scope of the EE premise—it is supposed to hold *for any* theory. Kukla argues for this universal scope on the basis of the existence of algorithms that provide an alternative EE theory for any input theory. If algorithms like this indeed exist and are effective, it certainly follows that any theory has a time-indexed EE rival. Therefore, Laudan and Leplin have to show that such algorithms are ineffective.

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[10] Laudan and Leplin acknowledge that van Fraassen would not accept this thesis. However, they claim that 'we reject [van Fraassen's] implicit assumption that conditions of observability are fixed by physiology. Once it is decided what is to count as observing, physiology may determine what is observable. But physiology does not impose or delimit our concept of observation. We could possess the relevant physiological apparatus without possessing a concept of observation at all. The concept we do possess could perfectly well incorporate technological means of detection. In fact, the concept of observation has changed with science, and even to state that the (theory-independent) facts determine what is observable, van Fraassen must use a concept of observation that implicitly appeals to a state of science and technology' (Laudan and Leplin 1991, 452).

4.1.2 Algorithms and theoreticity

Two logical results that have been very relevant in connection with the problem of EE are Craig's theorem and Ramsey's sentence. These results were originally interpreted by logical positivists as showing that theoretical terms are unnecessary in scientific theories[11]. Based on this interpretation, they can be understood as providing algorithms that given an input theory *T* deliver an empirically equivalent theory *T′* in which the theoretical conceptual baggage of *T* has been excised. Kukla has proposed an algorithm that precisely describes the logical maneuver involved: 'for any theory *T*, construct the rival *T** that asserts the world to be observationally exactly as if *T* were true, but denies the existence of the theoretical entities posed by *T*' (Kukla 1993, 4). Laudan and Leplin dismiss algorithms like this because the output they produce is not really a genuine rival to *T*, but simply an *instrumentalized* version of *T*—in the case of Kukla's algorithm we could call it the *antirealist* version of *T*. *T** does not include the theoretical terms of *T*, but since it is a logical consequence of *T*, and since the theoretical terms are crucial for the derivation of the observational consequences of *T*, *T** is *parasitic* on *T*:

> The algorithm does not produce a rival representation of the world from which the same empirical phenomena may be explained and predicted. On the contrary, a theory's instrumentalized version posits nothing not posited by the theory, and its explanations, if any, of empirical phenomena deducible from it are wholly parasitic on the theory's own explanations. A theory's instrumentalized version cannot be a rival to it, because it is a logical consequence of the theory and it is bound to be endorsed by anyone endorsing the theory. (Laudan and Leplin 1991, 456-457)[12]

Kukla complains that if parasitism is used against these algorithmic structures, then a principle that is actually applied in scientific practice gets violated (Kukla 1996, 149-150). Following Daniel Dennett, he states that the instrumentalist view of intentional psychology is accepted by the relevant community because of its predictive power, but the ontology of the theory is not believed to be true because it is incompatible with physicalism. Therefore, if Laudan and Leplin reject the use of such a structure in terms of the parasitic nature of *T′*, they would be denying an accepted practice in real science.

But this argument clearly rests on a misunderstanding. Laudan and Leplin are not saying that the outcome *T′* of the algorithm must be dismissed from the outset—as a pseudo-theory—because it is a parasitic theory with respect to *T*. Their point is that the parasitic reference of *T′* to *T* means that *T′* is not a genuine *rival* to *T*, *T′* is simply the *instrumentalized* or *antirealist* version of *T*. The difference between *T* and *T′* boils down only to the epistemic stance one takes towards the very same theory. What Kukla shows is only that, according to Dennett, in the case of psychology the *instrumentalist* attitude with respect to intentional psychology is more appropriate than the realist one.

Kukla's defense of algorithms does not stop here though. He mentions yet another candidate for an EE-algorithm that does not fall prey to the parasitism rejection: take a theory *T* with class *O* of observational consequences, and construct from it the theory *T′*, which states that *T* is true for the world under initial conditions in which it is being observed, but that also says that when nobody is observing the

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[11] For a detailed explanation of the Ramsey sentence and Craig's theorem, and of why both failed to accomplish the logical positivist goal, see (Suppe 1974, 27-35).

[12] John Norton provides a similar reason to dismiss Kukla's algorithm. Even if we accept that *T* and *T** have the same empirical consequences, it is clear that the theoretical terms and entities in *T* are necessary for the derivation of such consequences for *both* theories—for the theoretical terms are required to derive the empirical consequences of *T**, but they are denied in the latter theory (see the example of intentional psychology below). Therefore, by negating those terms and entities *T** gets gratuitously impoverished: 'If we assume that the algorithm is applied to a well-formulated theory *T* whose theoretical structure is essential to *T*'s generation of observational consequences, then the construction of *T′* [Kukla's *T**] amounts to a gratuitous impoverishment of theory *T*, the denial of structures that are essential to the derivation of observational consequences that are well confirmed by them' (Norton 2008, 39-40).

universe behaves according to the laws of *T**—where *T** is any theory which is incompatible with *T*. It is clear, Kukla asserts, that *T* and *T′* are EE rivals (1993, 4-5). This example is enough, he argues, to prove that there exist algorithmic procedures that are capable of producing non-parasitic, predictively equivalent propositional structures. Therefore, if they are going to be rejected, their outputs must be shown to be pseudo-theories on the basis of *theoreticity* criteria[13].

From Laudan and Leplin's response to Kukla's challenge (1993) three such criteria can be extracted: non-superfluity, plausibility and testability. A hypothesis is superfluous if it could be dispensed with in the theory it belongs to without any loss of empirical content, i.e., if it does not contribute to deriving any observational consequences. The algorithm candidate that Kukla proposes includes the postulation of a hypothesis like this: that the laws of nature are intermittent and depend on the presence/absence of observers—by definition, the consequences of *T** do not belong to the class of *observable* consequences of the algorithmic theory *T′*, so the intermittency hypothesis is empirically superfluous.

Kukla replies that if non-superfluity is to be considered as a criterion of theoreticity it would follow that 'normal' theories like *T* should also be rejected, for they contain the equally superfluous hypothesis that the laws of nature continue to hold when nobody is looking. But this argument is unconvincing. If *T* is a theory in which the *content* of the laws of nature postulated is such that they hold regardless of whether anyone is looking, then the 'continuity' of the laws of nature is not an *extra* hypothesis in *T*, it is just a feature of its laws. That is, *according to the laws of T* the behavior of the world is not affected by the presence/absence of observers. In the case of Kukla's *T′* two situations are possible: its laws establish a connection between observation and the behavior of the world, or the 'intermittency' of the laws of nature is an extra, unexplained hypothesis. In the second case, it is indeed the case that the 'intermittency hypothesis' is superfluous—also untestable—and therefore the theory has problems of theoreticity.

The other possibility for Kukla's *T′*—that it includes laws which connect observation and the behavior of the world—is interesting because it allows us to clarify another criterion of theoreticity. If the laws in *T** and in *T′* explain 'the inconsistency of the behavior of the world', then whether or not *T′* can be accepted as genuinely scientific will depend on the *kind* of explanation *T** provides:

> Provisions that fly in the face of what we have good empirical reason to assume must claim some offsetting rationale if they are to be admitted as part of a theory. It would be different if the course of nature were known to exhibit such vast and mysterious ruptures or bifurcations as *T′* envisions, if natural law did not exhibit isometry, at least. One might then be willing to entertain wild, unexplained and unconfirmable scenarios as genuine possibilities. But the world is not known to be like that. (Laudan and Leplin 1993, 14)

Though Laudan and Leplin do not dub this feature, it seems that 'plausibility' fits. In order to be considered as *genuinely scientific*, a hypothesis must possess a minimum degree of plausibility—which is normally judged on the basis of a background of empirically well-confirmed knowledge. This requirement must, of course, not be made so strict as to demand complete consistency between new hypotheses and background knowledge. Hypotheses that 'fly in the face of what we have good empirical reason to believe' have formed a part of successful science. But even those revolutionary hypotheses must be

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[13] 'It seems to me that the whole philosophical dispute between the received-viewers and Laudan and Leplin comes down to the issue of distinguishing genuine theoretical competitors from logico-semantic tricks. Laudan and Leplin represent the issue as being concerned with the existence or nonexistence of empirical equivalents. But it is evident, both from my example as well from the example they reject in a footnote, that there *do* exist empirically equivalent propositions to any theory. The only question is whether these structures fail to satisfy some *additional* criteria for genuine theoreticity. The received-viewers are satisfied with their examples of empirical equivalence. The burden is on Laudan and Leplin to explain why empirical equivalence isn't enough' (Kukla 1993, 5).

given a minimum of plausibility. In our context this means that Kukla's *T′* will be genuinely scientific if the hypotheses in *T** that explain the 'inconsistent behavior of the world' by connecting observation with the course of nature possess a measure of plausibility, in the sense of some (perhaps indirect) empirical support. Since it is clear that the algorithm to produce *T′* does not contain any indication of how to obtain that minimum degree of plausibility—it is rather unlikely that *any* algorithm could do so, given the connection between plausibility and scientific creativity and ingenuity—it follows that it is nothing but a promissory note for an algorithm[14].

One final requirement of theoreticity we would like to address is given by the 'testability' of hypotheses. This feature can also be used to disregard possible algorithms. If algorithms produce theories that contain superfluous additional hypotheses, in the sense that they do not participate in the entailment of observational consequences, these hypotheses will be untestable:

> Because the purpose of theorizing is, at least in part, to gain predictive control over the subject matter under investigation, a theory must, at least in principle, be open to test. A 'propositional structure' that is not even in principle confirmable, that could not logically be an object of epistemic evaluation, is not a theory; for it could not in principle impart understanding nor advance practical interests. (Laudan and Leplin 1993, 13)

Superfluity, implausibility and untestability are thus features that can be coherently defined and justifiably invoked in order to dismiss hypotheses as unscientific. The demand for testable, non-superfluous and plausible hypotheses and/or theories is justified by basic goals of science. We will not deal with a detailed consideration of what these goals are, but both testability and non-superfluity are requirements which are grounded in the aim of achieving *empirical* knowledge and of excluding metaphysical-unfalsifiable elements from scientific theorizing. On the other hand, the demand for plausibility relies on the aim of achieving explanations of natural phenomena that make them intelligible[15] in the sense of making them fit in with general empirically based background knowledge.

It must be underscored that even though theoreticity conditions can be considered as *a priori* in the sense that they work as pre-given constraints on theories in order to make them genuinely scientific, whether or not a given hypothesis or theory is testable, superfluous or plausible, is not something to be determined *a priori*. A hypothesis is testable if, along with other assumptions, observable consequences can be derived from it; and a hypothesis is non-superfluous if it is required for the derivation of observational consequences of a theory. But the class of auxiliary assumptions available for the derivation of observational consequences changes with time. Therefore, it is possible that a hypothesis which is non-testable and superfluous in a given state of science may become testable and non-superfluous with the introduction of new auxiliary assumptions. In the case of 'plausibility', this property is typically grounded in background scientific knowledge. Consequently, a hypothesis that is completely implausible with respect to a certain stage of the development of scientific knowledge might become plausible

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[14] It is still possible to weaken the algorithm and take it just as stating that *T′* asserts that *T* holds when we are observing, but it does not hold when nobody is looking. As a *theory*, this would be way too bizarre to be considered as genuinely scientific. However, the weakened algorithm can still be taken as an instance of the evil-genius argument—as an instance of the fact that, from a logical point of view, there are many hypotheses consistent with the information of our senses but that deny them as providing reliable information about reality. But in this case the algorithm is no longer a problem of the philosophy of science, but of metaphysics.

[15] We consider, unlike constructive empiricists, that explanation and understanding are essential aspects of science—see (De Regt and Dieks 2005).

enough with the acceptance of new theories. Hypotheses are not superfluous, untestable and/or implausible in themselves, but with respect to a concrete state of scientific knowledge[16].

Summarizing, Laudan and Leplin's treatment of the first premise of the problematic argument shows that *i)* EE is an intrinsically time-indexed feature; and that *ii)* theoreticity constraints imply that there are no automatic algorithms capable of producing an EE rival given any theory *T*[17]. Therefore, the problem that arises from EE and UD is not necessarily *universal*. It is not true that *for any* theory T there is *eo ipso* an EE rival *T′*, for the algorithms that were proposed to support this view are ineffective[18]. Moreover, EE and UD, if present, are not necessarily *everlasting* features, for the development of science might be such that the EE between theories gets broken.

However, Laudan and Leplin have not disproved the possibility of time-indexed EE, so that a corresponding time-indexed UD of the choice between them is still possible. Although algorithms may not work, it is still possible that a genuinely scientific EE rival might be formulated after all. Moreover, that EE is essentially time-indexed does not logically *imply* that further development of science will *surely* break the equivalence. These remarks are crucial for the reassessment of Laudan and Leplin's solution that we will argue for below.

4.2 The second premise

4.2.1 Evidence and entailment

The second part of Laudan and Leplin's argument is directed against the second premise of the problem, namely, that *only observational statements that can be logically derived from a theory can count as empirical evidence to support it*. Laudan and Leplin claim that this statement is an overly simplified and inaccurate view of the dynamics of evidential confirmation. According to them, a correct assessment of the nature of evidence and confirmation shows that 'significant evidential support may be provided a theory by results that are not empirical consequences of the theory' (1991, 460). If theories can obtain evidential

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[16] It is important to emphasize that theoreticity constraints serve as a tool for blocking algorithms that automatically yield EE theories; the main point in this subsection is to discuss the first premise of our problem, that given any theory *T* there is an EE rival *T′*. The universal scope of this premise crucially depended on the effectiveness of algorithms. But theoreticity requirements preclude that their outputs may be considered as genuinely scientific hypotheses or theories. When it comes to EE between *genuine* scientific theories these basic theoreticity requirements are fulfilled by the theories involved, by definition—otherwise the theories would not be genuinely scientific—, so they cannot function as criteria that provide a way out of the choice problem. These remarks prevent a possible objection. The reader might complain that in section 3 non-empirical virtues were dismissed as a full solution of the problem because of their context-dependency, but now another context-dependent feature, theoreticity, is being used as a part of the defended solution. However, as we just mentioned, theoreticity constraints block *algorithms* and so undermine the first premise of the problem. We are not using theoreticity as a criterion to make a choice between EE 'real life' theories. For example, even if the degree of plausibility of a certain hypothesis or theory may not be objectively addressed in some cases of 'real-life' science, in the case of 'algorithmic theories' it is clear that the algorithms involved do not include any receipt to provide their outputs with the mentioned property.

[17] More precisely, it has not been *demonstrated* that algorithms of this kind cannot exist. However, it is extremely unlikely—given the non-*a priori* character of the theoreticity requirements—that an algorithmic procedure could include a recipe for obtaining plausible hypotheses. In 'real life' science plausibility for a new hypothesis is usually originated in scientists' creativity and ingenuity, so it is difficult to see how an algorithm could contain a receipt for this property to be included in their output.

[18] Notice that theoreticity constraints also block the holist Duhem-Quine thesis as providing support for the universal scope of the first premise of the problem. As Adolf Grünbam showed, the Duhem-Quine thesis 'nor other logical considerations can guarantee the deducibility of *O′* [the class of observational consequences] from an *explanans* constituted by the conjunction of *H* and some *non-trivial* revised set *A′* of the auxiliary assumptions which is logically compatible with *A* under the hypothesis *H*' (Grünbaum 1960, 77). Suppose rival hypotheses *H* and *H′* are given, and suppose that a *crucial experiment* to test them favors *H′*. The Duhem-Quine thesis implies that it is always *logically* possible to save *H* by arranging the set of auxiliary assumptions *A* and replacing it by *A′*, so that the outcome of the experiment could be accommodated. In that case, we could always have a case of EE between *H* and *H′*. Grünbaum shows that this *logical* feature is not enough to prove that there will be a suitable *A′* of *non-trivial* assumptions for *H* to accommodate the observations. In our context, we could simply replace *non-trivial assumptions* for *assumptions that accomplish theoreticity constraints.*

support from empirical facts which do not belong to the class of their observational consequences, then, in the context of our problem, 'the relative degree of evidential support for theories is not fixed by their empirical equivalence' (ibid). Therefore, the fact that two theories are EE does not imply that the choice to be made between them is empirically underdetermined.

Laudan and Leplin illustrate their point by means of a scheme of empirical confirmation: if a theory T entails two logically independent theoretical hypotheses H_1 and H_2, and if in turn these hypotheses entail the classes of observational consequences E_1 and E_2, respectively, then the truth of any member of E_1 will support H_1 and *also* H_2, even though H_2 does not entail any statement in E_1 (the same holds, *mutatis mutandis*, for the truth of the statements in E_2) (ibid., 461-462). This scheme implies that the class of the observational consequences a theory entails is not identical with the class of observational statements that can confirm that theory. In turn, this last remark implies that the EE of two theories is not a sufficient condition for the UD of the choice between them—and thus a way out of the problem becomes available:

> Theoretical hypotheses H_1 and H_2 are empirically equivalent but conceptually distinct. H_1, but not H_2, is derivable from a more general theory T, which also entails another hypothesis H. An empirical consequence e of H is obtained. e supports H and thereby T. Thus, e provides evidential warrant for H_1, of which it is not a consequence, without affecting the credentials of H_2. (ibid., 464)

Laudan and Leplin's attack on the second premise of our problem can be complemented by remarks made by Richard Boyd (1973). As Boyd points out, given two EE theories, their different inter-theoretical connections with background knowledge may be invoked to make a decision based on evidential grounds. Given EE theories, the background knowledge available might be such that it is, or becomes, at odds with essential hypotheses in one of the theories, but completely coherent with the other one. The friction between the rest of the well-confirmed theories that constitute the background knowledge and the core-structure of one of the EE theories can count as indirect empirical evidence to reject the latter.

Boyd explains his point by an example. He takes the famous Poincaré-Reichenbach argument for the conventionality of geometry as an instance of two EE theories. F & G is a theory which asserts that the world is governed by a class of forces F, and that its spatial features are described by a geometry G. F' & G' is a rival theory asserting that the world is governed by the class of forces F'—a class containing all the forces in F plus a universal force f'—and that its spatial features are described by the geometry G'. The theories are EE; however,

> even though "F & G" and "F' & G'" have the same observational consequences (in the light of currently accepted theories), they are not equally supported or disconfirmed by any possible experimental evidence. Indeed, *nothing* could count as experimental evidence for "F'& G'" in the light of current knowledge. This is so because the force f' required by F' is dramatically unlike those forces about which we know—for instance, it fails to arise as the resultant of fields generating in matter or in the motions of matter. Therefore, it is, in the light of current knowledge, highly implausible that such a force f' exists.
> Furthermore, this estimate of the implausibility of "F' & G'" reflects *experimental* evidence against "F' & G'", even though this theory has no falsified observational consequences. (Boyd 1973, 7-8)

Boyd's (adapted) view[19] is a good complement to Laudan and Leplin's argument because it relies on similar grounds. First, that inter-theoretic relationships can count as indirect evidence to accept or reject

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[19] Boyd's own position is that, in a case of EE between T and T', the compliance of T with the form of causal explanations present in empirically successful theories in background knowledge counts as an indicator for the truth of T that is lacking

theories shows that the class of statements which are confirmationally relevant for a theory does not reduce to the class of its observational consequences, just as Laudan and Leplin claim. Second, the development of background knowledge over time is crucially relevant for Laudan and Leplin and also for Boyd's view. Suppose that *T* and *T′* are EE and at the time of their formulation equally coherent with the rest of background knowledge. However, new well-confirmed theories might be deeply at odds with *T′*, but coherent with *T*. This feature is an evidential reason to choose *T*. Even though *T* and *T′* remain EE, the UD of the choice has been broken by inter-theoretical connections. That is, just as EE, UD is a time-indexed feature[20].

Laudan and Leplin's view on the second premise, as complemented by Boyd, seems quite compelling. However, it is useful and fruitful to consider some criticisms that have been put forward against it in order to appreciate what has been really achieved.

4.2.2 Okasha's objection: Hempel's problem

Samir Okasha (1997) objected that Laudan and Leplin's argument falls prey to a problem that Hempel had already noticed in 1945. Okasha correctly claims that the epistemic support for Laudan and Leplin's argument is given by the following—*prima facie* very plausible—two principles: *i)* if evidence confirms a hypothesis, then it also confirms any statement that entails the hypothesis; and *ii)* if evidence confirms a hypothesis, then it also confirms any statement that is entailed by the hypothesis. Hempel (1945, 103-104) labeled these two principles as the *converse consequence condition* and the *special consequence condition,* respectively[21], and Okasha reminds us that Hempel had also noticed that a simultaneous commitment to these principles leads to a problem:

> The absurdity that results is this: every statement confirms any other one. For consider any statement *S*. Every statement confirms itself, so *S* confirms *S*. By converse consequence, *S* confirms (*S* & *T*), since (S & T)$\rightarrow S$. By special consequence, *S* confirms *T*, since (S & T)$\rightarrow T$. This result holds for arbitrary *T*, and must

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in *T′*–for the explanations in *T′* do not have the mentioned form. The principle of confirmation just defended weakens Boyd's original position in the sense that it is detached from any realist commitments (Boyd considers the problem of EE and UD as a threat to the realist), and at the same time generalizes it in the sense that possible friction with background knowledge is not given only by divergence from the canonical form of *causal* explanations.

[20] The epistemic justification of the principle we have extracted from Boyd's argument is a very basic goal of science: mutual consistency between accepted theories. Suppose that *T* and *T′* are EE, that *T* is consistent with another well-confirmed theory *P*, and that *T′* is at odds with it. The evidential support for *P* counts as empirical evidence against *T′ granted that we agree that consistency between the theories we accept is a basic principle of science*. If we want that our theories are mutually consistent, then Boyd's argument should be taken as a principle in the dynamics of empirical confirmation. This is a very plausible stance of course: if we aspire to obtain knowledge of reality by means of scientific theories, it is clear that if the set of scientific theories we accept were inconsistent, we would hardly call such set 'knowledge'. Suppose that in a certain domain of physics theory *T* is introduced and that all of its predictions are confirmed, that in a different domain theory *P* is proposed and all its predictions are confirmed, and that *P* and *T* are incompatible. This situation, of course, would be taken as a serious problem for science, and it would be expected that endeavors in order to show that one of the theories must be given up would be undertaken by scientists.

[21] As Okasha asserts (1997, 254), Laudan and Leplin's argument can be schematized this way:

i) H_1 and H_2 are EE
ii) $T \Rightarrow H_1$
iii) $T \nRightarrow H_2$
iv) $T \Rightarrow H$
v) $H \Rightarrow e$
vi) $H_1 \nRightarrow e$,
vii) $H_2 \nRightarrow e$,
viii) e,

therefore; *ix) e* confirms *T* (this requires the *converse consequence condition*), and then *x) e* confirms H_1 (this requires the *special consequence condition*); but *e* does not confirm H_2.

> therefore be regarded as a *reduction ad absurdum* of the simultaneous use of the special and converse consequence conditions. (Okasha 1997, 253)

Okasha is certainly right in that Laudan and Leplin endorse both the special and converse condition. However, his criticism is not enough to threaten their argument. Hempel's problem comes up if we understand the dynamics of confirmation only as an abstract exercise in logical entailment relations. However, if we consider *theoreticity* conditions—more specifically, testability and non-superfluity—the problem does not automatically arise. In Okasha's reconstruction, T cannot be *any* arbitrary statement: it has to be testable and non-superfluous, i.e., it must be relevant for the derivation of at least some of the statements in the class of observational consequences derived from (S & T). If the only extra statement that can be derived from (S & T)—with respect to the ones derivable from S alone—is T, then (S & T) will not be considered a genuine theory.

One could still argue that the example of the problem that Hempel himself offered cannot be dismissed in this way (1945, 104-105). He took the theory T to be (H_1 & H_2)—where H_1 is 'all the ravens are black' and H_2 is 'Hooke's law'. The class of observational consequences of T is O_T, which is defined as (O_1 & O_2), where O_1 is the class of observational consequences of H_1 and O_2 is the corresponding class of H_2. Since H_1 is relevant for the derivation of O_1 and H_2 is relevant for the entailment of O_2, both hypotheses are testable and non-superfluous. In other words, the problem is now that any evidence confirming a theory or hypothesis could be used to confirm any other *scientific* hypothesis.

It is clear that such a maneuver is against good sense, and it would be certainly dismissed in scientific practice. The reason is, again, theoreticity. Two different hypotheses or theories can be fruitfully conjoined in order to form one single theory only if by so doing new observational consequences can be derived, or if by so doing unexplained phenomena become explained by the new theory—consequences and phenomena that could not be predicted or explained by means of any of the conjoined theories alone. Simply put, the resulting theory must be more than the mere sum of its parts. This should be adopted as a principle, otherwise cosmologists could simply conjoin string theory with genetics and then claim that the discovery of a new gen confirms that space-time has eleven dimensions.

Once again, the second premise of the problem relies on an oversimplified conception of the dynamics of empirical evidence. Logical entailment of an observational statement by a hypothesis is not a necessary condition for that statement to confirm the hypothesis. Inter-theoretical relations are also crucial features to be considered.

4.2.3 Bangu's objection: encompassing theories

Sorin Bangu (2006) introduced yet another objection against Laudan and Leplin's argument which is also illuminating. We saw above that a way out of the EE and UD problem can be found if there is a well-confirmed general theory T that encompasses only H_1 in the EE pair—the evidential support that e gives to T, although neither H_1 nor H_2 entails e, flows to H_1 but not to H_2. Bangu claims that this does not work, for the possibility of yet another general theory capable to encompass H_2 has not been ruled out—and this alternative general theory may be also supported by the same evidence e:

> The supporter of underdetermination can reply that nothing rules out the possibility that another theory T^* exists, such that $T^* \rightarrow H_2$ [H_2 being the other member in the EE pair]. Moreover, it is possible that T^* is supported by evidence e as well […].
>
> The only constraint imposed on the relation between T and T^* is that they behave differently with respect to H: T^* entails it, while T does not. What evidence supports each of these theories is another matter. So, can two different theories, each entailing different hypotheses, be supported by the same evidence? This is trivially true. (Bangu 2006, 273-274)

If a theory such as T^* were given, then the evidence e would also flow to H_2, and the UD of the choice between H_1 and H_2 would come up once again. However, Bangu overlooks one further constraint on T^* for the UD of the choice to be restored: it is required that T and T^* are also EE—the evidence supporting each of the theories *is* relevant. Otherwise the case between H_1 and H_2 could be settled by means of the different evidential support between T and T^*. If T^* is a theory with more evidential support than T, then we should choose H_2.

Bangu's objection is correct, but it does not undermine Laudan and Leplin's argument. If there is a theory T which encompasses H_1 but not H_2, and if there is no theory such as T^*, the evidence e does break the UD. It is important to emphasize that Bangu's argument is not based on algorithms, for he has not shown that given *any* theory such as T there is a theory such as T^*. Actually, the joint actual existence of T and T^* is a rather unlikely situation. EE between theories is not a common feature in science—scientists look for better theories, not for equivalent ones. Moreover, most of the times it is a very difficult task to come up with *one* empirically successful theory with respect to a certain domain of natural phenomena, and Bangu's reply requires not only one pair of EE theories, but two pairs.

However, Bangu's argument is clarifying with regard to the nature and scope of Laudan and Leplin's solution. Their argument, as complemented by Boyd's, is that UD is a contingent feature—even if two theories are EE non-consequential evidence that could be available might provide an evidentially justified reason to make a choice. Bangu's argument implies that the eventual breakdown of UD may be undone again by contingent scientific developments. He has effectively established that even if UD is broken à la Laudan and Leplin, this UD breakdown need not be a definitive resolution of the choice problem. So although Laudan and Leplin have shown that there are ways in which the underdetermination problem can be overcome, they have not shown that the problem cannot happen at all or that it cannot return.

5 Reassessing Laudan and Leplin

Laudan and Leplin's attack on the first premise of our problem shows that EE is a time-indexed feature and that there is no guarantee that it is universal in scope. Theoreticity conditions block the effectiveness of algorithms to produce EE rivals, and the development of science can be such that variation in the class of available auxiliary hypotheses may break the EE between two theories. Based on this analysis they conclude that

> This contextuality [time-index] shows that determinations of empirical equivalence are not a purely formal, a priori matter, but must defer, in part, to scientific practice. It undercuts any formalistic program to delimit the scope of scientific knowledge by reason of empirical equivalence, thereby *defeating the epistemically otiose morals that empirical equivalence has been made to serve.* (Laudan and Leplin 1991, 454, our emphasis)

Their attack on the second premise shows that the class of observational statements that can count as evidence for a theory is not limited to its observational consequences. The UD between two EE rivals can be broken by subsuming one of the theories in the EE pair under a more general and well-confirmed theory, whose particular evidential support flows to the encompassed theory but not to the non-encompassed rival. In addition we have Boyd's argument: the inter-theoretic connections of a theory can work as indirect evidential (dis)confirmation. A theory in an EE pair could be rejected if it contains essential parts which are (or become) at odds with respect to the background knowledge—whereas its rival is (or remains) coherent with it. Accordingly, Laudan and Leplin draw the following conclusion:

> Results that test a theory and results that are obtainable as empirical consequences of a theory constitute partially nonoverlapping sets. Being an empirical consequence of a theory is neither necessary nor sufficient to qualify a statement as providing evidential support for the theory. Because of this, it is illegitimate to infer from the empirical equivalence of theories that they will fare equally in the face of any possible or conceivable evidence. *The thesis of underdetermination, at least in so far as it is founded on presumptions about the possibility of empirical equivalence for theories—or 'systems of the world'*[22]*—stands refuted*'. (ibid., 466; our emphasis)

In order to attain an accurate evaluation of the implications of EE and UD, these conclusions must be carefully assessed. Laudan and Leplin address the EE/UD problem in its universal and general form: the threat that EE holds for *all* scientific theories and that, consequently, UD infects *all* theory choice. As we have seen, their arguments are effective in denying the generality of the problem—it is not true that for all theories there is an EE rival, and EE is not a sufficient condition for UD. However, the conclusions that the epistemological morals derived from EE are *otiose* and that the thesis of UD as founded on presumptions of EE stands *refuted*, can be understood as implying that EE and UD are epistemologically idle issues[23]. So understood, these conclusions do not provide a reliable analysis of the seriousness of EE and UD. First, even if EE is a time-indexed feature and theoreticity requirements can block algorithms, that two theories may be EE remains a possible scenario, for a genuinely scientific EE rival to a given theory might be formulated after all. Besides, further development of science and consequent variation of the available auxiliary hypotheses might break the EE, but it is also possible that this will not happen. Second, it is true that a more general theory could break the UD by means of 'transferring' empirical evidence. However, there is nothing in science or in epistemology that assures that such a theory will be actually available. Moreover, Bangu has shown that there is nothing in science or in epistemology that precludes that an alternative general theory that restores the UD may be formulated. Recourse to inter-theoretic connections is not a guaranteed way out either. It is possible that if a pair of EE is given both theories are equally coherent with respect to background knowledge—further development of science could be at odds with one of the theories in the pair, but nothing can assure that either.

That two genuinely scientific theories are EE (with respect to a given state of science) remains a possible situation. If (also with respect to a given state of science) it happens to be the case that both theories stand on equal footing in terms of compatibility with background knowledge, and if there are no general theories to encompass them, it does follow that the choice between the theories is underdetermined by empirical evidence. That is, in a case like this—a case whose possibility Laudan and Leplin's arguments do not deny—the morals of EE are not epistemically otiose, for UD would indeed follow from EE. The problem of UD as a consequence of EE has not been dispelled.

From Laudan and Leplin's proposal it can be accepted that the problem of UD, just as EE, is a time-indexed feature, and this is certainly an important clarification. But that the problem may disappear with time does not imply that there is no problem at all. EE leading to UD *can* happen in science, and that there is a solution à la Laudan and Leplin is not guaranteed in any specific case. What has been achieved is a demonstration that strategies and methods typically used in science *might* be effective in overcoming the problem if it comes up. In other words, Laudan and Leplin's proposal does not prove that EE and UD do not constitute a problem, though it certainly clarifies that we are dealing with a

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[22] In this passage Laudan and Leplin cannot be using the term 'systems of the world' in its canonical meaning. See (Hoefer and Rosenberg 1994).

[23] Actually, even if we interpret Laudan and Leplin's argument as directed only against the universality and generality of the problem, they do not even mention that a remaining 'local' problem of EE and UD still stands—nor that this remaining problem has important epistemic dimensions, of course.

problem that science may solve. The tools of scientific practice that could solve it do not come with a guaranteed success certificate. But, after all, this is a feature that all the problems that science is to solve share.

In conclusion, that EE and UD constitute a problem that science may solve in individual cases does not mean that we are dealing with an epistemologically idle situation. Actually, the very fact that the solution of the problem is not very different from the solution of other scientific problems implies that important epistemic features are involved. That the scientific solution is contingent, in the sense that the conditions required for the breakdown of EE and/or UD might or might not obtain, means that recalcitrant cases of UD of theory choice are possible. Moreover, elaborating on Bangu's objection and on the contingency of Boyd's solution, we note that a problem of EE and UD that gets eventually solved might become problematic again—variations in well confirmed background knowledge or the introduction of further suitable encompassing theories might reintroduce the problem. It is clear that a situation like this would pose urgent questions concerning the epistemological status of theory acceptance[24].

6 An example

Between 1886 and 1904 the Dutch physicist Hendrik Antoon Lorentz developed a theory with the aim of solving the conflict between experiments and Maxwellian electrodynamics that arose during the second half of the 19th century. In order to achieve that aim, Lorentz's theory introduced several radical modifications to the Newtonian framework of physics. The main elements of his theory were *i)* a set of coordinate transformations under which Maxwell's equations remain invariant—the famous *Lorentz-transformations*; *ii)* the concept of 'local time'—the time that is experimentally determined in a frame of reference in motion with respect to the ether as opposed to the 'real time' that is measured only in the ether-rest frame—; *iii)* the Lorentz-Fitzgerald length-contraction hypothesis that affects bodies in motion through the ether; and *iv)* a model of the electron in which this elementary charged particle was subject to the Lorentz-Fitzgerald contraction and its mass was velocity-dependent[25]. Lorentz's theory was formulated against the backdrop of a basic ontology of an immobile ether pervading all of space, and of electrons, the ultimate (charged) particles of nature whose mass was of entirely electromagnetic origin—the mass of electrons, and *a fortiori* of all of matter, was due to the electron's electromagnetic field interaction with the ether[26].

Lorentz's eventual theory was empirically successful and was capable of providing an explanation for all of the main physical puzzles of the time: stellar aberration, Fresnel's coefficient, Fizeau's experiment, why massive solid objects can move through the ether without disturbing it, and (the negative results of) the experiments designed to find optical effects caused by the motion of the earth through the ether—the most famous of them being the Michelson-Morley experiment[27].

In 1905 Albert Einstein published his first paper on the special theory of relativity. Its two principles—that all inertial frames of reference are physically equivalent, and that the speed of light is constant and independent of the state of motion of its source—along with his revolutionary conception of simultaneity and the nature of time, allowed him to explain and predict everything that Lorentz's theory explained and predicted.

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[24] The case of Bohmian mechanics—originally introduced in the early 50's— and standard quantum mechanics seems to be an example of a case of EE and UD that cannot be readily solved *à la* Laudan and Leplin (and Boyd). In this case, thus, the epistemic underpinning of the reasons for the dominant position of the latter theory is a subject for philosophical discussion. We defer an analysis of this case for a future paper.

[25] For an excellent presentation and analysis of Lorentz's theory see (Janssen 1995, chapter 3).

[26] For an analysis of the physical and epistemological framework of Lorentz's theory see (McCormmach 1970).

[27] For the scientific context and motivations of Lorentz's invention of his theory, see (Janssen and Stachel 2004).

Therefore, Lorentz's theory is usually considered as EE to Einstein's special relativity. This is correct, although some caution is needed. Lorentz's theory of 1904 is not yet strictly empirically equivalent to Einstein's theory. For example, as Michel Janssen (2003) has shown, the theories predict a different outcome for the Trouton experiment. The full equivalence of the theories is obtained only if several crucial amendments and contributions introduced by Henri Poincaré are taken on board[28].

On the other hand, the reason why the theories were *rivals* is that special relativity claims that we live in a space-time determined by a *Minkowskian metric*—so that the time coordinates of events and the spatial dimensions of objects are essentially relative to the frames in which they are measured—whereas Lorentz's theory says that we live in a *Newtonian* space-time described by a *Euclidean metric* plus *absolute time*, but in which certain dynamical effects—caused by the interaction between matter and the fields propagating in the ether—*deceive* observers who move with respect to the ether and make time and length measurements, so that the world looks Minkowskian to them. In other words, the theories assign different chrono-geometrical structures to the space-time we inhabit.

Therefore, in 1906 physics was facing an instance of our problem[29]: there were two predictively equivalent theories and the choice between them was underdetermined by empirical data. We know that in the end Einstein's theory won the competition. The historical course of events that led to this victory was rather complex, though. For example, between 1906 and 1909 the scientific community often spoke about the *Lorentz-Einstein theory*, as the difference between the two rival theories had not been generally recognized. Clarification concerning this rivalry had to wait until Minkowski's groundbreaking work (1908). A couple of years later, around 1911, the expression *Lorentz-Einstein theory* disappeared from the vocabulary of physics, and special relativity was adopted as the main-stream theory[30].

Some non-empirical features of Einstein's theory have undoubtedly been historically relevant for its victory; especially its comparative simplicity as a theory based on only two principles (though from the beginning there have also been complaints about relativity's lack of understandability as judged from an intuitive viewpoint)[31]. In the philosophy of science literature one can also find several arguments for explaining and justifying why special relativity defeated Lorentz's theory that invoke non-empirical features as the decisive factor. The accusation of ad-hocness leveled against the length-contraction hypothesis is a well-known older example. More recently, Michel Janssen (1995; 2002a; 2002b; Balashov and Janssen 2003; 2009) has proposed and defended an argument of this kind. In a nutshell, Janssen states that special relativity must be (and was) preferred because of its explanatory superiority. He argues that in Lorentz's theory the fact that all laws of physics are Lorentz-invariant remains an unexplained coincidence—in special relativity, however, this feature gets naturally explained by the structure of Minkowski space-time.

Notwithstanding the plausibility of the analysis by Janssen and of other arguments based on non-empirical features (we defer a detailed evaluation of Janssen's interesting views to another paper), in section 3 we have argued that even though features relating to theoretical virtues can provide good

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[28] Poincaré's contributions and corrections on Lorentz's work were the following: *i)* he showed that 'local time' was not a mere mathematical tool, as Lorentz originally claimed, for it was connected to observable effects in the behavior of moving clocks; *ii)* the introduction of a *fictitious fluid* in the ether that carried an amount of electromagnetic momentum; *iii)* the introduction of the *Poincare-pressure* which kept the moving electron stable and precluded its explosion due to Coulomb forces; *iv)* he corrected Lorentz's expressions for the transformation of velocities and charge density between moving frames; and *v)* he showed that the Lorentz's transformations form a *group*, and by so doing he showed that they are fully symmetric. Only with these amendments Lorentz's theory becomes completely predictively equivalent to special relativity. See (Darrigol 1995).

[29] Poincare published his *On the Dynamics of the Electron*—the work where he introduced the amendments and developments of Lorentz's theory that make it predictively equivalent to special relativity—in 1906.

[30] For a historical treatment of the formulation and early reception of special relativity see (Miller 1981).

[31] See (Brush 1999).

pragmatic reasons to prefer one theory over another in a case of EE, they are not able to solve the problem in a fully objective and uniquely determined way. We shall now show for the specific case of Lorentz vs. Einstein how it can be assessed from the empirical side, using non-consequential empirical evidence, and how in this way the decision in favor of special relativity can be objectively and uniquely justified.

One of the main reasons why Lorentz's theory actually got rejected and special relativity accepted is in fact an instance of the non-consequential confirmation solution of our problem. In 1899 Max Planck introduced the concept of the *quantum of energy* in order to derive the correct law for black-body radiation. Before Planck's trailblazing work, the radiation spectrum of black-bodies had been an intractable problem for classic electrodynamics and thermodynamics. In 1905, a few months before his paper on special relativity appeared, Einstein himself published his famous work on the *light-quantum hypothesis*—a hypothesis that relied on Planck's concept. The relevance of the rise of quantum physics for our subject is that the quantum hypothesis was soon acknowledged to be deeply at odds with the foundations of classic electrodynamics. In a nutshell, the problem was that classic electrodynamics and thermodynamics predict that an accelerated electron must emit radiation of all wavelengths in a continuous range of energy, whereas the quantum hypothesis postulated emission of determinate, specific wavelengths in a discrete spectrum. Since Lorentz's ether theory and his electron theory were essentially built upon the very core of classic electrodynamics, namely, Maxwell's equations, the groundbreaking new physics of the quantum led physicists to gradually abandon this classical framework. The problem got even deeper with Bohr's first contributions to a quantum theory of the structure of the atom: the fruitfulness of the quantum hypothesis and its conflict with classical electrodynamics were central features in Bohr's work.

As R. McCormmach (1970, 486-487) and A. J. Kox (2013) report, Hendrik Lorentz himself played a central role in the recognition of the incompatibility of quantum physics and the core of classic electrodynamics. Between 1900 and 1903 he tackled the task of finding the dependence of the energy density in a blackbody as a function of its temperature and the wavelength of the radiation. The formula he derived on the basis of electrodynamics applied to thermodynamics—essentially the Rayleigh-Jeans law—worked in the long-wavelength part of the spectrum of emission, but failed in the short wavelength part of the spectrum[32]. Although he first took this result as very promising, he soon realized that there was a deep conflict between the quantum hypothesis and the results of experiments, on one side, and the core of electrodynamics on the other:

> In 1908 Lorentz came out in support of Planck's theory; it was then that he emphasized the profound antithesis between the quantum hypothesis and the electron theory. At a mathematical congress in Rome that year Lorentz spoke on Planck's and James Jeans' theories of blackbody radiation. His object was to prove that the union of the electron theory with Hamilton's equations of motion and J. W. Gibbs' statistics leads inescapably to Jeans' radiation law, which, like his own of 1903, agrees with experience only in the case of long wavelengths. He said that the alternative, Planck's theory, demands far-reaching changes in electron theory. He pointed out that this is easily seen, since an accelerated electron should emit rays of all wavelengths, a result incompatible with the hypothesis of energy elements whose magnitude depends on wavelength. At the time of his lecture he had not yet decided between the two theories. Wien, however, called his attention to experiments showing that for short wavelengths a body emits much less light in

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[32] 'Lorentz pointed out that his black-body formula agrees with the long wavelength limit of the quantum formula that Planck had derived in 1900, a coincidence which struck him as highly remarkable considering the widely different assumptions in the two cases. It was characteristic of Lorentz to spell out what was incomplete in his work and what was still unknown; he stressed that his theory is valid only for long wavelengths and that Planck's applies to the whole spectrum. So it was Lorentz, an originator of the electron theory, who first intimated the possible limits of the theory. Starting from the electron theory and from a mechanism appropriate to the theory, he arrived at the limiting case of the radiation law; and he did not see how to extend his theory to Planck's general case.' (McCormmach 1970, 486-487).

> proportion to its absorbing power than that predicted by Jeans' theory. This proves, Lorentz said in a note appended to the published version of his talk, that any theory that bases itself on the electron theory and the equipartition theorem has to be profoundly revised. Later in that year he elaborated that note: he had 'long hoped', he confessed, 'that it would be possible to escape the universal applicability of that theorem [equipartition] by combining electron theory and kinetic theory'. He added, 'this hope has not been fulfilled'. He was now ready to concede that the interaction of matter and ether takes place by means of vibrating charged particles to which Gibbs' statistics, for unknown reasons, are inapplicable.
> Lorentz thus accepted the quantum theory as the only theory capable of explaining the complete spectrum of black-body radiation, while at the same time regarding it as very incompletely understood in its connection with the other branches of physics and in particular with electron theory. (ibid., 487)

The divide between electrodynamics and quantum physics that Lorentz thus helped to clarify became more and more important for the scientific community in the following years, and it was one of the main reasons that led to the abandonment of the electromagnetic world-view program—of which Lorentz's ether theory was a fundamental part—leaving the path open for Einstein's special relativity to be accepted:

> The sense of the first Solvay Congress in 1911 was that the electron theory was incompatible with quanta and that it could not be made compatible without far reaching reform. The Congress and especially its published proceedings went far to redefine the fundamental problems for fundamental physical theory. Niels Bohr's doctoral dissertation in 1911 was a reformulation of Lorentz's theory of metals on more general principles. In his dissertation Bohr pointed to persuasive evidence of the ultimate incompetence of mechanics and electrodynamics on the molecular level. His 1913 quantum theory of atoms and molecules, which gave sharp focus to the quantum problems and intimated their enormous fruitfulness, was based on the explicit denial of the validity of ordinary mechanics and the classical electron theory in the atomic domain. Lorentz's theory continued to be worked on, but its concepts were increasingly recognized as unsuited for the basic reconstruction of physical theory demanded by the quantum hypothesis. (ibid., 488)

Einstein's special relativity, though predictively equivalent to Lorentz's theory, did not rely on any electrodynamic assumptions about the ultimate nature of matter. Consequently, it was not at odds with quantum physics. As Minkowski's seminal contributions clarified, special relativity was a theory grounded on the kinematics of the four-dimensional continuum (Minkowski space-time), and this made superfluous any specific assumptions about dynamics. This difference between Einstein's and Lorentz's theory was crucial for the abandonment of the latter and the acceptance of the former.

Considering that quantum physics became more and more important during the first two decades of the 20th century, it was only natural that, given our two predictively equivalent theories, the one which was not at odds with quantum theory was to be accepted. Indeed, as already mentioned, by 1909 the expression *Lorentz-Einstein theory* began to disappear from the physicists' vocabulary. Minkowski's work had shown that they were two theories of a different nature, and by 1911 the rise of quantum mechanics turned the balance decidedly in Einstein's favor:

> Why did the electromagnetic program run out of power? […] More important was the competition from other theories that were either opposed to the electromagnetic view or threatened to make it superfluous. Although the theory of relativity was sometimes confused with Lorentz's electron theory or claimed to be compatible with the electromagnetic worldview, about 1912 it was evident that Einstein's theory was of a very different kind. It merely had nothing to say about the structure of electrons and with the increasing recognition of the relativistic point of view, this question—a few years earlier considered to be essential—greatly changed in status. To many physicists it became a pseudo-question. As the rise of relativity made life difficult for electromagnetic enthusiasts, so did the rise of quantum theory. Around 1908, Planck reached the conclusion that there was a fundamental conflict between quantum theory and the electron theory, and he was cautiously supported by Lorentz and other experts. It seemed that there was no way

> to derive the blackbody spectrum on a purely electromagnetic basis. As quantum theory became more and more important, electron theory became less and less important. The worst thing that can happen to a proclaimed revolution is that it is not needed. (Kragh 1999, 115)

Clearly, this situation is an instance of one of the ways out of the UD problem explained in section 4. As mentioned there, inter-theoretical relations of theories with background knowledge can count as (non-consequential) empirical evidence for or against a theory, and this evidence may be able to break the UD between two EE theories. This is exactly what happened in the Einstein *vs.* Lorentz case. The rise of quantum physics implied that new background knowledge became irreconcilable with the core of Lorentz's theory, whereas this friction did not occur in the case of Einstein's special relativity. Therefore, the emergence of a new overarching conceptual framework, urged by empirical results, resulted in indirect empirical evidence against Lorentz's theory, whereas Einstein's remained unharmed. In spite of the predictive equivalence, the empirical UD of the choice got broken.

There is another reason to prefer special relativity over Lorentz's theory that exemplifies one of the ways out of the problem that Laudan and Leplin propose. In 1916 Einstein finished his general theory of relativity. In this gravitational theory, the Minkowski space-time described by special relativity becomes a special case. General relativity tells us that Minkowski space-time depicts a flat spatiotemporal region without mass-energy, or an infinitesimal part of a curved space-time that does contain mass-energy—just as in Gauss geometry an infinitesimal region of a curved surface approximates a Euclidean tangent plane. That is, special relativity becomes a special case of the general theory, both in the mathematical and the physical sense.

Lorentz's theory—in spite of its predictive equivalence with respect to special relativity—does not fit within general relativity. As mentioned above, it claims that the physical world, in its spatiotemporal features, has the structure of Newtonian space-time. Therefore, even if we take Lorentz's theory as holding for a local region of a global space-time, it is in conflict with the structure of general relativity: Einstein's gravitational theory claims that empty space-times or local regions of space-times containing matter have a Minkowskian metric. Despite the mathematical and empirical equivalence between Lorentz's theory and special relativity, the former cannot be understood as a special case of general relativity; the theories are incompatible.

As a result, special relativity possesses greater empirical support than Lorentz's ether theory. General relativity entails predictions that special relativity cannot entail on its own—it allows a satisfactory description of the motion of the perihelion of Mercury, and it predicts that light gravitates and 'bends', for example. However, since the special theory is a special case of the general one, the empirical support for the latter *flows* to the former: the perihelion of Mercury and the light-bending effect are empirical evidence for the Minkowskian metric of a local region of space-time. On the other hand, since Lorentz's theory is incompatible with general relativity and thus cannot be included in it, the empirical support of Einstein's gravitational theory cannot flow to Lorentz's ether theory[33].

This is obviously an instance of Laudan and Leplin's argument explained in section 4.2. Given two EE theories, H_1 and H_2, such that only H_1 can be encompassed by a more general theory T, the independent empirical evidence for T flows to H_1 but not to H_2—and thus the UD of the choice gets broken. In our example H_1 corresponds to special relativity, H_2 to Lorentz's theory, and T to general relativity.

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[33] Unlike the relation between Lorentz's theory and quantum physics, the connection between special relativity and general relativity was not *historically* relevant. General relativity got completed in 1916—and empirically tested in 1919—and by then special relativity was already generally accepted by the scientific community, whereas Lorentz's theory had been put aside. Our claim that the connection between special and general relativity grounds a reason to choose Einstein's theory instead of Lorentz's is thus only conceptual, not historical.

7 Concluding remarks

Laudan and Leplin's argument certainly constitutes an important step towards an accurate evaluation of the status of the problem of EE and UD. That algorithms to produce EE are not effective and that both EE and UD are conditions which are time-indexed indicate that the problem is not as universal and pernicious as sometimes believed. However, their argument does not show that the morals that can be extracted from EE are epistemically otiose or that the thesis of UD as a consequence of EE stands refuted—to use their own terminology. Scenarios of EE and UD, though time-indexed, remain possible. As we have explained, when the problem arises, normal scientific practice may eventually lead out of the impasse. But as in any other scientific problem, that a solution will be found is not guaranteed. In our judgment, then, an accurate evaluation of the situation is that there *is* a problem of EE and UD. In any specific situation it is a contingent problem, which may be dissolved by scientific progress; but the problem still has interesting epistemological repercussions.

The case of special relativity *vs.* Lorentz's theory illustrates that the way out of the problem of EE and UD is actually a part of scientific practice. This example also shows that the reassessment of the solution we propose is in principle correct. Two features of the Lorentz *vs.* Einstein case clarify and illustrate this further. First, there was a period in which the choice between Einstein's theory and Lorentz's was indeed underdetermined. Before it became generally recognized that classic electrodynamics—and *a fortiori* Lorentz's theory—was incompatible with the results of quantum physics there were no empirically grounded reasons to make a choice. Therefore, the Einstein *vs.* Lorentz case demonstrates that EE and UD can actually happen in science, and that a problem of evidence-based choice may arise.

Second, the incompatibility between quantum physics and classic electrodynamics is a contingent feature of science. Quantum physics could have been compatible with Lorentz's theory and not with Einstein's—in that case the choice would have been the opposite—, it could have been compatible with both of them, or it could have been never formulated, and in these two cases the UD would not have been removed. In other words, that the further development of science was going to break the UD was not assured from the outset. In the case of general relativity something similar holds. That Einstein's gravitational theory was to encompass special relativity, but not Lorentz's theory, was not assured in advance. Moreover, and here we follow Bangu, Lorentz or another scientist might have formulated an alternative encompassing theory that could have restored the UD situation.

This leads us to the conclusion that even though EE and UD are not universal and necessarily everlasting problems affecting all theories at all times, the EE/UD problem can nevertheless be a practical reality. When the problem arises, science, in principle, might find a breakthrough; but it also might not. If with respect to a certain stage of the development of science we have a pair of EE theories for which inter-theoretical connections are not able to break the UD of the choice to be made, there still is the problematical situation that we can only have recourse to non-empirical features in order to justify a preference. As stated above, such features are not able to assure a uniquely determined and evidence-based decision. Nevertheless, we are entitled to keep the hope that future scientific developments will provide a way out, based on empirical evidence.

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