ARTIFICIAL EXAMPLES OF EMPIRICAL EQUIVALENCE[1]

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Abstract. In this paper I analyze three artificial examples of empirical equivalence: van Fraassen's alternative formulations of Newton's theory, the Poincaré-Reichenbach argument for the conventionality of geometry; and predictively equivalent 'systems of the world'. These examples have received attention in the philosophy of science literature because they are supposed to illustrate the connection between predictive equivalence and underdetermination of theory choice. I conclude that this view is wrong. These examples of empirical equivalence are harmless with respect to the problem of underdetermination.

Keywords: empirical equivalence, underdetermination, van Fraassen, conventionality of geometry, systems of the world.

1. Three sources of empirical equivalence

The problem of empirical equivalence (EE) and underdetermination (UD) of theory choice can be expressed by means of a simple argument. The first premise states that *for any theory T that entails the class of observational consequences O there is another theory T' whose class of observational consequences is also O*. The second premise is that *entailment of evidence is the only epistemically justified criterion for the confirmation of theories*. From these two premises it follows that the objectivity—and maybe even the rationality—of theory choice is threatened. Notice that the universal scope of the first premise implies that the problem holds for science as a whole, in the sense that *all* theories are affected by EE and UD.

EE between theories can be instantiated in four different ways: *i)* by algorithms, *ii)* by accommodating auxiliary hypotheses according to the Duhem-Quine thesis, *iii)* by the regular practice of science, and *iv)* by concrete artificial examples. The universal scope of the first premise of the problem is supported by *i)* and *ii)*. If there exist algorithms that are able to produce EE theories given any theory *T*, or if it is always possible to accommodate evidence by means of manipulation of auxiliary hypotheses, then it follows that EE is a condition that holds for any theory whatsoever. Elsewhere I have argued that neither *i)* nor *ii)* really work as possible sources of EE[2]. In the case of *iii),* Larry Laudan and Jarret Leplin proposed a twofold way out of the problem. First, they claim that EE is a time-indexed feature—in the sense that it is a condition essentially

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relative to a specific state of science and technology—and that it might get broken by future scientific or technological developments. Second, Laudan and Leplin argue that the UD between EE theories can be broken by means of non-consequential empirical evidence—even if the predictive equivalence remains[3].

In this paper I will tackle the remaining source of EE, namely, concrete examples of artificially generated pairs of empirically equivalent theories. These examples are neither the outcome of the application of algorithms, nor obtained by manipulation of auxiliary hypotheses given an actual theory *T*. They are not the result of the practice of real science either. Rather, they have been *cooked up* and exploited by philosophers of science in order to speculate about their epistemological consequences. I will address an examination of three examples of artificially generated EE theories that have received attention in the philosophy of science literature: Bas van Fraassen's alternative formulations of Newton's mechanics; the theories involved in the Poincaré-Reichenbach 'parable'; and the case of predictively equivalent *total theories* or *systems of the world*.

2. Van Fraassen's alternative formulations of Newton's theory

In *The Scientific Image* Bas van Fraassen introduced an argument for his constructive empiricism that involves an example of EE. He presents Newton's theory as a theory about the motion of bodies in space and the forces that determine such motions. The crucial feature that grounds van Fraassen's argument is that Newton's theory is supposed to be committed to the view that physical objects exist in absolute space. Thus, by reference to absolute space the concepts of absolute motion and absolute velocity become meaningful. Then, van Fraassen proposes

> let us call Newton's theory (mechanics and gravitation) TN, and $TN_{(v)}$ the theory TN plus the postulate that the center of gravity of the solar system has constant absolute velocity v. By Newton's own account, he claims empirical adequacy for $TN_{(0)}$; and also that if $TN_{(0)}$ is empirically adequate, then so are all the theories $TN_{(v)}$. (Van Fraassen 1980, p. 46).

Newton's most famous argument for the existence of absolute space is given by the thought experiment of the rotating bucket. In order to make sense of the acceleration of the rotating water in the bucket, the reality of absolute space has to be asserted, Newton argued. Van Fraassen's line of reasoning is that if absolute space exists, as Newton believed, then the concept of absolute motion of objects in space gets defined and so does the concept of absolute velocity. However, since—unlike absolute acceleration—absolute velocity has no observable effects, there are infinitely many predictively equivalent rival formulations of TN, each of them assigning a different specific value to the absolute velocity of the solar system's center of gravity.

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3 (Laudan and Leplin 1991).

According to van Fraassen, this entails a problem for the realist. The realist is committed to the view that only *one* of these alternative formulations is the true theory, but the realist's choice cannot be determined on evidential grounds[4]. For the constructive empiricist, van Fraassen argues, there is no such problem. In his/her case there is no commitment to the truth of the theory, but only to its empirical adequacy. Therefore, for the constructive empiricist it is enough to accept the empirical content of the theory as empirically adequate and assume a dodging attitude with respect to its non-empirical content—including the value for the absolute velocity of the solar system, of course. In other words, the empirical equivalence of the alternative formulations of Newton's theory does not necessarily put the constructive empiricist in the position of having to make a choice[5].

A systematic consideration of van Fraassen's challenge shows that the real problem is not EE. It is true that Newton endorsed absolute space and that his preferred alternative was $TN_{(0)}$. However, rather than a case of EE, what is behind van Fraassen's example is a situation where there is a superfluous hypothesis within TN. A hypothesis is superfluous if it is not logically relevant for the derivation of any empirical consequences of the theory it forms a part of; and a hypothesis being superfluous is a strong indication that it represents nothing physical—an ontologically empty hypothesis, we could say. Therefore, the fact that the predictive equivalence between van Fraassen's alternative formulations is grounded on the stipulation of a specific value for a superfluous parameter—absolute velocity—indicates that we have a problem with the foundations of $TN_{(v)}$, rather than a genuine problem of EE.

The problem of the superfluity of the concept of absolute velocity in Newton's theory has actually been solved and, *a fortiori*, the specious problem of EE gets dissolved. The key concept is a structure known as *neo-Newtonian space-time*[6]. The basic elements of this structure are event-locations—the spatiotemporal locations where physical events (can) occur. A temporal separation—that can be zero—is defined for all pairs of event-locations, and this is an absolute relation in the sense that it is not relative to particular frames of reference, states of motion, etc. A class of simultaneous event-locations—those for which their temporal separation is zero—forms a *space*[7], and the structure of each space is that of Euclidean three-dimensional space.

The feature that differentiates Newtonian absolute space and neo-Newtonian space-time is the way in which the spaces are connected or 'glued-together'. In absolute Newtonian space points conserve their

[4] From the viewpoint of the semantic conception of scientific theories, that van Fraassen endorses, the realist is committed to the view that *only one of the models that satisfy $TN_{(v)}$ correctly represents the world*. In the case of Newton, that model is given by $TN_{(0)}$, though the absolute velocity of the solar system is not a phenomenon.

[5] In semantic terms, the constructive empiricist stance is that to accept $TN_{(v)}$ as empirically adequate means that $TN_{(v)}$ *has a model which is empirically adequate*, i.e., it possesses an empirical substructure isomorphic to all phenomena. Making a choice is possible for a constructive empiricist, and he/she could do it based on pragmatic features of one of the alternative formulations. However, such a choice does not have an epistemic import, according to van Fraassen's view.

[6] Neo-Newtonian space-time is the result of the work of P. Frank in 1909, and E. Cartan and K, Friedrich in the 1920s. For a technical exposition of neo-Newtonian space-time and references to the seminal works of Frank, Cartan and Friedrichs, see (Havas 1964). For simpler expositions see (Sklar 1974), and (Stein 1970).

[7] In neo-Newtonian space-time simultaneity is an equivalence relation: every event is simultaneous with itself, if a is simultaneous with b then b is simultaneous with a, and if a is simultaneous with b and b is simultaneous with c then a is simultaneous with c. Therefore, it is possible to divide the class of all events in equivalence classes under the relation of simultaneity—classes that have no members in common and that taken together exhaust the class of all events.

spatial identity through time, and it is thus meaningful to ask whether a certain point or event-location at time t_1 is identical with some point or event-location at time t_2. In neo-Newtonian space-time this question makes no sense, since the notion of spatial coincidence is only defined for simultaneous event-locations.

This difference in structure has a straightforward effect on the way that velocity is defined in each case. In neo-Newtonian space-time it is coherent to ask for the velocity of a particle between two events in its history, but only if we are talking about its velocity with respect to some particular object or frame of reference—we can ask if the distance of the particle with respect to another object or frame is the same as its distance to that same object or frame at an earlier time, of course. But since absolute spatial coincidence through time is not defined, the concept of 'absolute velocity' is meaningless in neo-Newtonian space-time. Since points or event-locations do not conserve their identity through time, we cannot ask if the distance of an object with respect to a certain point in space at time t_2 has changed, or not, with respect to the distance between the object and that same point at an earlier time t_1.

Even though 'absolute position' and 'absolute velocity' are undefined, the concept of 'absolute acceleration' is well defined in neo-Newtonian space-time, but this definition does not require reference to absolute space. First we need to introduce the three-place relation of 'being inertial' between three non-simultaneous event-locations a, b and c. The relation holds if there is a possible path for a particle such that three events in its history are located at a, b and c; and if the particle is at rest in an inertial frame—a frame in which no inertial forces act upon any physical system at rest in it. More generally, a collection of events conforms an inertial class of events if they are all locations of events in the history of some particle that moves free of forces, a particle that moves *inertially*.

We can now explain the absolute acceleration of a particle along a time interval. Take the particle at the beginning of the interval and find an inertial frame in which the particle is at rest. At the end of the interval we find the new inertial frame in which the particle is at rest. Then we find the relative velocity of the second frame with respect to the first one at the end of the interval. Even though there is no such thing as the absolute velocity of the first inertial frame, we do know that, by definition, its velocity—with respect to any other inertial frame—has not *changed* throughout the interval. Therefore, the relative velocity of the second frame with respect to the first one gives us the absolute change of velocity throughout the interval, since the particle was at rest with respect to the first frame at the initial instant, and at rest with respect to the second frame at the end. We take this absolute change of velocity and divide it by the time separation between the initial and final event-locations and we obtain the absolute acceleration of the particle over the interval—to obtain the instant absolute acceleration we simply integrate over time. That is, absolute acceleration, within the context of a neo-Newtonian space-time, is defined not as relative to absolute space, but as relative to any inertial frame[8].

[8] Notice that the formulation of Newton's theory in terms of neo-Newtonian space-time does not imply a rejection of a substantivalist position. Space-time can still be postulated as the *arena* in which physical events occur, and the substantivalist can still argue that such arena possesses an independent existence, not reducible to relations between physical objects. See (Earman 1970).

Now we can go back to van Fraassen's challenge. As I mentioned above, the formulation of Newtonian mechanics in terms of neo-Newtonian space-time can be understood as the solution for an unease about its foundations—the superfluous concept of absolute velocity. That is, the example that van Fraassen offers is not a genuine case of EE between rival theories. The problem is simply that the presence of the superfluous parameter v in TN *manifested* in that alternative, apparently incompatible formulations could be given. Neo-Newtonian space-time solves this problem. It allows a more satisfactory formulation of TN in which the superfluous parameter has been swept away, so that there is no EE arising from different values assigned to v. In other words, the EE equivalence between van Fraassen's formulations was not the sickness, but just a symptom. Therefore, van Fraassen's challenge cannot be fruitfully used in order to extract conclusions related to the problem of EE and UD[9]. These remarks, of course, do not intend a refutation of constructive empiricism. The point is only that this particular example has no relevant consequences regarding the problem of EE and UD.

3. The Poincaré-Reichenbach argument

In *Science and Hypothesis*, Henri Poincaré introduced an argument for the conventionality of geometry that has been considered as an example of EE. He designed a 'parable' in which a universe given by a Euclidean two-dimensional disk is inhabited by flatlanders-physicists. The temperature on the disk is given by $T(R^2 - r^2)$, where R is the radius of the disk and r is the distance of the location considered to the center of the disk—therefore, the temperature at the center of the disk is TR^2 and at the edge it is $0°$ absolute. The inhabitants of this world are equipped with measuring rods that contract uniformly with diminishing temperatures, and all such rods have length 0 when their temperature is $0°$. The two-dimensional physicists proceed to measure distances in the disk with their rods in order to determine the geometry of their world; but they assume, falsely, that the length of their rods remains invariant upon transport—the flatlanders themselves also contract with diminishing temperature. Accordingly, the result they obtain is that they live in a Lobachevskian plane of infinite extent. For example, they measure that the ratio of a circumference to its radius is always greater than 2π. They obtain the same result by using measurements performed with light rays, for their universe is characterized by a refraction index $1/(R^2 - r^2)$; but they falsely assume that light beams travel along geodesics in their world, and that the index of refraction of vacuum is everywhere the same.

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[9] The reader might complain that since the alternative formulations of $TN_{(v)}$ are based on a theory that forms part of 'real' physics means that van Fraassen's argument is a case in which EE is supposed to arise from the actual practice of science, not an artificial example. However, notice that a choice between formulations of $TN_{(v)}$ was never an issue for the scientific community, there never was a scientific debate about what is the correct value of v. What did happen was a debate concerning the meaningfulness of v—Leibniz's arguments in the *Leibniz-Clarke correspondence*, for example. This debate was not grounded on a problem of EE and UD of theory choice, it was (is) a debate about the ontology of space. This is yet another indication that van Fraassen is exploiting a problem with the foundations of Newton's theory in order to create a (specious) artificial case of EE.

The parable also tells us that one particularly smart and revolutionary scientist in the disk comes up with the correct theory about the geometry of their world. Even though they are not able to observe effects of the temperature gradient $(R^2 - r^2)$ and of the refraction index $1/(R^2 - r^2)$, our brilliant physicist notices that, by assuming the reality of such unobservable features, the result is that the geometry of their universe is that of a finite Euclidean disk. The scientific community on the disk does not have the resources to make an evidentially based decision between the theories, and Poincaré's point is that the only way they can determine a specific geometry for their world is in terms of a *convention*. Poincaré also states that in our three-dimensional world we are, in principle, in the same situation. Empirically equivalent theories of our world that differ in the geometry they pose are analogously attainable. Therefore, the geometry of the physical world is a matter of convention also for us.

Two remarks can be made at this point about Poincaré's argument. First, it is clear that it is not an argument directly aiming to extract conclusions about the problem of EE and UD; but an argument concerning the epistemology of geometry. This feature indicates that if we are going to take it as a concrete example of EE and UD some provisos must be introduced. Second, it is also clear that the example of empirically equivalent theories it considers is of a peculiar kind. The theories are not about the 'real' physical world. The universe of the flat disk is a mental construction and, as such, it can be arranged and manipulated so that it totally complies with the description given by each of the theories. The world described by the theories is an *ad hoc* world. But this feature of the argument suggests that the example of EE involved is not a very serious or threatening one. The choice between the theories is underdetermined because the whole situation can be conceptually manipulated in the required way.

Hans Reichenbach, in *The Philosophy of Space and Time*, introduced a sort of generalization of the argument. He presented it as a theorem showing that from any space-time theory about the *real* physical world it is possible to obtain an alternative theory which is predictively equivalent but that assigns a different geometry:

> Mathematics proves that every geometry of the Riemannian kind can be mapped upon another of the same kind. In the language of physics this means the following:
> *Theorem* θ: 'Given a geometry G' to which the measuring instruments conform, we can imagine a universal force F which affects the instruments in such a way that the actual geometry is an arbitrary geometry G, while the observed deviation from G is due to a universal deformation of the measuring instruments'. (Reichenbach 1958, pp. 32-3)[10].

Under this formulation, the argument for the conventionality of geometry has a more substantial upshot on the problem of EE and UD. Reichenbach claims that the parable that Poincaré introduced can be effectively applied to 'real' space-time theories. For example, it could be stated that general relativity is empirically equivalent to a Newtonian-like theory of gravitation in which the curvature of space-time is

[10] A *universal force*, roughly speaking, is a force that acts equally on all physical objects and that it cannot be shielded against. A *differential force*, on the contrary, can be shielded against and does not act equally on all physical objects. See (Reichenbach 1958, §6).

replaced by the action of a universal force. This complies with the first remark I made above regarding Poincaré's parable. Under Reichenbach's formulation, the argument for the conventionality of geometry can, in principle, be considered as an instance of EE involving theories about *our* world.

However, we still need to be precise about in what sense this argument, that primarily concerns the epistemology of geometry, affects the problem of EE and UD. For this purpose it is useful to take a look at what exactly Reichenbach is arguing for. The conventionalist stance he defends is weaker than Poincaré's. According to Reichenbach, what is a matter of convention regarding geometry are not, bottom line, the geometric features of the physical world, but the specific 'language' in which those features are expressed. This argument relies on the concept of *coordinative definition*, that is, arbitrary definitions that settle units of measurement and which ground the particular conceptual systems that underlie physical theories:

> Physical knowledge is characterized by the fact that concepts are not only defined by other concepts, but are also coordinated to real objects. This coordination cannot be replaced by an explanation of meanings, it simply states that *this concept* is coordinated to *this particular thing*. In general this coordination is not arbitrary. Since the concepts are interconnected by testable relations, the coordination may be verified as true or false, if the requirement of uniqueness is added, i.e., the rule that the same concept must always denote the same object. The method of physics consists in establishing the uniqueness of this coordination, as Schlick has clearly shown. But certain preliminary coordinations must be determined before the method of coordination can be carried any further; these first coordinations are therefore definitions which we shall call *coordinative definitions*. They are *arbitrary*, like all definitions; on their choice depends the conceptual system which develops with the progress of science.
> Wherever metrical relations are to be established, the use of coordinative definitions is conspicuous. If a distance is to be measured, the unit of length has to be determined beforehand by definition. This definition is a coordinative definition. (Reichenbach 1958, pp. 14-5).

Now it becomes clear why I said that Reichenbach's conventionalist view is a 'weak' one. What is at stake in the EE between theory $T = F + G$ and $T' = F' + G'$—where F denotes the set of forces that affect physical objects according to T, and F' is that same set plus a universal force f' that accounts for the deviation from geometry G according to T'—is only a divergence regarding the particular coordinative definitions that are presupposed by the theories. That is, we are in a situation analogous to a decision concerning whether Lionel Messi's height is 1,69 meters or 5 feet and 7 inches. In the case of Poincaré's disk, there are two different coordinative definitions at stake: one states that distances measured by rods have to be corrected according to a certain law, whereas in the other the measuring rods are rigid bodies that always express correct distances. Reichenbach's view on the conventionality of geometry is 'linguistic', we could say. T and T' are two versions of the same theory expressed in different geometrical *languages*. To state that T is truer or more correct than T', or vice versa, is analogous to say that 'meter' is a more correct unit of measurement than 'foot'[11].

[11] Reichenbach also argues that the *default* language is the geometry in which universal forces are set to the zero value. If we do so, then the question regarding the specific geometry of the physical world becomes really meaningful, not only a matter of linguistic definitions: 'The forces which we called universal are often characterized as forces *preserving coincidences*; all objects are assumed to be deformed in a way that the spatial relations of adjacent bodies remain unchanged. […] It has been correctly said that such forces are not demonstrable, and it has been correctly inferred that they have to be set equal to zero if

If Reichenbach is right, then the case of EE between T and T' that the argument involves is a harmless one. The choice between the theories is just a matter of the language we pick to express the same physical theory. Under Reichenbach's view the conventionality of geometry has no special upshot on the problem of EE and UD as defined above. It is true that the choice between T and T' can be done only in terms of pragmatic considerations such as simplicity—empirical evidence, by definition, cannot settle the case. However, this is not a scientific or epistemological problem at all, for the choice does not involve incompatible rivals that differ in the way they describe the world. If we follow Reichenbach's line of thought, a genuine case of EE and UD would happen only if the theories involved postulate incompatible geometrical features for the world *provided that in both theories the universal forces are set to the zero value*. There is nothing in Reichenbach's argument to believe that this cannot happen, but it does not involve any example of this kind either.

This easy way out of the problem works only if Reichenbach is right, of course. His position regarding the epistemology of geometry is, clearly, quite close to the verificationist criterion of meaning endorsed by most of logical positivists. As it is known, this criterion has been shown to be untenable, and Reichenbach's view of the meaning of geometrical statements as reducible to coordinative definitions falls prey, *mutatis mutandis*, to the typical objections that have been leveled against logical positivistic semantics. That is, there are good reasons to think that Reichenbach's position is wrong, and, *a fortiori*, that the case of EE involved in his argument might be a relevant example with respect to the problem of UD of theory choice.

However, it turns out that even if we consider the case of $T = F + G$ vs. $T' = F' + G'$ as a genuine case of EE, this does not necessarily imply that we are dealing with a case of UD. The reason is given by the evidential status of the 'universal forces'. We can understand Reichenbach's theorem as stating that space-time theories can have alternative empirically equivalent formulations by means of universal forces, and we can assume—unlike Reichenbach—that such alternatives are *genuine* rivals. However, that there exists an EE rival that postulates the reality of universal forces is not, *ipso facto*, an indication that the choice to be made is underdetermined by the empirical evidence. All 'real' physical theories that invoke forces as the cause for dynamical effects postulate these forces as associated to *observable* effects; but the universal forces involved in Reichenbach's arguments are not at all like these 'typical' forces. They are, in principle, not associated to any empirically detectible effect. The reality of usual, differential forces in physical theories is

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the question concerning the structure of space is to be meaningful. It follows from the foregoing considerations that this is a *necessary* but not a *sufficient* condition. Forces *destroying coincidences* must also be set equal to zero, if they satisfy the properties of the universal forces […]; only then is the problem of geometry uniquely determined. […]
We can define such forces as equal to zero because a force is no absolute datum. When does a force *exist*? By force we understand something which is responsible for a *geometrical change*. If a measuring rod is shorter at one point than at another, we interpret this contraction as the effect of a force. The existence of a force is therefore dependent on the coordinative definition of geometry. If we say: actually a geometry G applies, but we measure a geometry G', we define at the same time a force F which causes the difference between G and G'. The geometry G constitutes the zero point for the magnitude of a force. If we find that there result several geometries G' according as the material of the measuring instrument varies, F is a differential force; in this case we gauge the effect of F upon the different materials in such a way that all G' can be reduced to a common G. If we find, however, that there is only one G' for all materials, F is a universal force. In this case we can renounce the distinction between G and G', i.e., we can identify the zero point with G', thus setting F equal to zero. This is the result that our definition of the rigid body achieves' (Reichenbach 1958, pp. 27-8).

evidentially supported by the observable effects they cause, but this is not the case with universal ones. That is, in the case of $T' = F' + G'$ there is a hypothesis which is not, in principle, evidentially warranted. Therefore, we can conclude that $T = F + G$ possesses a higher degree of evidential support than T'. As Richard Boyd states it:

> 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 [universal] force f' required by F' [the class of the forces postulated by our T'] is dramatically unlike those forces about which we now know—for instance, it fails to arise as the resultant of fields originating in matter or in the motions of matter. Therefore, it is, in the light of current knowledge, highly implausible that such a force as f' exists.
> Furthermore, this estimate of the implausibility of "F'&G'" reflects *experimental* evidence against "F'&G'", even though this theory has no falsifies observational consequences. (Boyd 1973, pp. 7-8)[12].

Boyd's passage is illuminating in two respects. First, it is not only the unobservability of a universal force what makes it bizarre and lacking evidential support. It is also a very implausible concept, in the sense that it is not alike at all to usual forces in another crucial respect: there is nothing in Reichenbach's theorem to let us know about its physical underpinning. Usual forces have a source, for example—typically charges and massive objects—; but what is the source of universal forces? Second, the quote underscores that the problematic nature of universal forces is not just a matter of theoretical uneasiness. Universal forces are bizarre not only from the point of view of formal *a priori* or pragmatic considerations. The difficulties with them are also based on lack of *empirical evidence* to support their reality. Let me clarify this point with yet another quote, this time from a paper by John Norton:

> I must note that the notion of a universal force, as a genuine, physical force, is an extremely odd one. They are constructed in such a way as to make verification of their existence impossible in principle. The appropriate response to them seems to me not to say that we must fix their value by definition. Rather we should just ignore them and for exactly the sorts of reasons that motivated the logical positivists in introducing verificationism. Universal forces seem to me exactly like the fairies at the bottom of my garden. We can never see these fairies when we look for them because they always hide on the other side of the tree. I do not take them seriously exactly because their properties so conveniently conspire to make the fairies undetectable in principle. Similarly I cannot take the genuine physical existence of universal forces seriously. Thus to say that the values of the universal force field must be set by definition has about as much relevance to geometry as saying the colors of the wings of these fairies must be set by definition has to the ecology of my garden. (Norton 1994, p. 165)[13].

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[12] Kyle Stanford offers a similar account of the matter: 'While Eddington, Reichenbach. Schlick and others have famously agreed that general relativity is empirically equivalent to a Newtonian gravitational theory with compensating 'universal forces', the Newtonian variant has never been given a precise mathematical formulation (the talk of universal forces is invariably left as a promissory note), and it is not at all clear that it can be given one (David Malament has made this point to me in conversation). The 'forces' in question would have to act in ways no ordinary forces act (including gravitation) or any forces could act insofar as they bear even a family resemblance to ordinary ones; in the end, such 'forces' are no better than 'phantom effects' and we are left just with another skeptical fantasy. At a minimum, defenders of this example have not done the work needed to show that we are faced with a credible case of non-skeptical empirical equivalence' (Stanford 2001, p. S6, footnote 6).

[13] In his paper Norton introduces this comment only as a sort of side-remark: 'As an aside from my main argument…' (*ibidem*). His main goal is to disprove the conventionality of geometry, and his main argument is that universal forces finally

I totally agree with Norton's view regarding universal forces[14]. However, this stance, I think, should not be taken as an ultimate rejection of them as a possible part of scientific theories. Hypotheses are not testable or untestable in *a priori* terms. For example, new available auxiliary hypotheses could be conjoined to a certain untestable hypothesis and turn it into a testable one. I see no reason why this might not happen in the case of universal forces. That is, *so far as we know*, there is not empirical evidence for the reality of such forces, but future findings might provide good reasons to postulate them in physical theories. A future theory could include observable effects that, at least indirectly, support the reality of a universal force.

It is important to underscore that these remarks hold for universal forces as such, that is, independently of their involvement in Reichenbach's argument. Actually, it seems that this particular argument requires, by definition, that universal forces are not related to any observable effects. The EE between T and T' seems to have as a condition that the universal forces are totally undetectable. However, as I just mentioned—and putting Reichenbach's argument aside—, there might be possible physical theories in which universal forces do relate to observable features. At least this possibility has not been disproven.

The answer to the question of whether Reichenbach's argument involves a challenging case of EE is thus negative. The reason is that, so far as we know, there is no evidential support for the reality of universal forces. Therefore, even though we could concede that Reichenbach's example involves genuine EE and rivalry, this does not mean that we are facing a case of UD, for the theory in which universal forces are absent has more evidence in its favor than its rival. Moreover, the fact that Reichenbach's argument requires that the universal forces involved are totally undetectable suggests that this particular example cannot provide a case of UD, no matter what particular form these forces take within the theory they are a part of. If universal forces are to have any special consequences with respect to EE and UD, it will not be through Reichenbach's example.

4. 'Total theories' or 'systems of the world'

The last case of EE in terms of artificial examples I will address is given by 'total theories' or 'systems of the world'. Such theories are defined by providing an account of all possible phenomena, past present and future, in opposition to regular 'local' theories that hold for a determinate realm of appearances:

> The thesis of underdetermination of theory choice by evidence is about empirically adequate total science; it is a thesis about what Quine calls 'systems of the world'—theories that comprehensively account for all

reduce to 'correction-terms' in suitable gauge transformations that preserve the physical meaning of invariants in general covariant formulations of space-time theories. This implies that the underlying metric in the space-time theories involved is not affected at all by the introduction of universal forces. As Norton himself explicitly acknowledges, this is a refutation of a strong version of the conventionalist thesis. This argument leaves the weak 'linguistic-definitional' version untouched—which is Reichenbach's stance—though Norton states that such a version is trivial.

[14] Reichenbach's followers could reply that, since they define force as something which is responsible for a geometrical change, and therefore it essentially depends on the coordinative definitions underlying a physical geometry (see footnote 11 above), then the reality of universal forces is also a matter of convention—and their introduction becomes justified. But this answer only shifts the problem. The usual physical meaning of 'force' is much more substantial than a mere stipulation about the presence or absence of geometrical changes. Reichenbach's conventional definition of force is quite debatable.

> observations—past, present and future. It is a thesis about theories that entail all and only the true observational conditionals, all the empirical regularities already confirmed by observation and experiment. (Hoefer and Rosenberg 1994, p. 594).

As I mentioned in section 1, Laudan and Leplin introduced an argument intending to show that EE and UD is a surmountable problem in the case of usual local theories. They state that EE between theories is a contingent, time-indexed feature, in the sense that further development of science and technology might break this condition—new available auxiliary hypotheses might lead to diverging predictions, for example. Besides, even if EE remains, the UD of the choice might get broken anyway: if only one of the theories can be encompassed in a more general one, then the evidential support of the latter flows to the encompassed theory but not to its predictively equivalent rival—and thus the evidential tie gets broken.

Hoefer and Rosenberg accept this solution[15], but they correctly affirm that it cannot work in the case of total theories—that's why they state that the problem of EE and UD is a problem only for total theories. Since Laudan and Leplin's argument makes essential reference to background science, that is, to *other* theories, if we are dealing with systems of the world such other theories are, by definition, not available. All possible auxiliary hypotheses are included in the EE total theories involved, and there cannot be more general theories in which to encompass any system of the world. Therefore, EE in the case of total theories seems to pose a special challenge.

I think that it is true that if a pair of predictively equivalent theories of this kind were given, then the UD involved could not be overcome. However, we do not need to worry about this example of EE either. Even though the very definition of a system of the world precludes that UD could be broken in terms of empirical evidence if EE is given, this definition is problematic in the sense that there is no way for us to know whether a specific theory counts as a system of the world or not.

There are several ontological and epistemological difficulties with the concept. First, if we are going to take systems of the world seriously, it would have to be shown that the world admits a description by a theory like that. This question involves a metaphysical issue of course: is the set of all natural phenomena regular and coherent enough as to be describable in terms of one single theoretical framework? Second—even if we take for granted that this is possible—is human science capable to provide an alternative, rival, predictively equivalent system of the world? If we discard algorithms and bizarre, parasitic theories this sounds like an extremely unlikely scenario.

It could be argued that the possibility of a total theories-EE scenario has not been disproven, and that this is enough to take the problem seriously. We can concede this, but the problems with the concept of a system of the world do not end here. Recall that the definition involves the property of being empirically adequate for all possible phenomena, past, present and future; but how in the world could we know that a certain

[15] For a critical reassessment of Laudan and Leplin's argument see (Acuña and Dieks 2013). There we argue that even though their argument does provide a possible way out of the problem, it is not a *guaranteed* solution. The solution that Laudan and Leplin propose essentially depends on the *contingent* development of science, and such a development might not be as required for the solution to be instantiated. New auxiliary hypotheses and new general theories might not be capable of breaking either EE or UD, for example.

(total) theory will be empirically adequate with respect to all future phenomena? Notice that the problem is not that we cannot know whether a certain total theory is true (or empirically adequate) or not; the problem is that since we can never know that a certain theory is empirically adequate with respect to future phenomena implies that we cannot know whether a certain theory is really a system of the world. That is, the very definition of the concept at issue precludes us to know that any candidate-theory is really a total one or not.

Analogously, we cannot know whether a certain theory has all possible phenomena under its scope. It is true that by its form and content a certain theory can claim to be valid in a total way—for all possible phenomena—but the fact that a certain theory intends to be a total one does not necessarily mean that it is. Our world is not like the universe in Poincaré's parable, we cannot accommodate it in a way such that it complies with our theoretical framework. There might always be realms of phenomena that are not accounted for in a theory, even if such a theory intends to be a system of the world. For example, assume that we are facing a case of EE between two total theories. In spite of what the theories say, nothing precludes the possibility that new kinds of phenomena—that have never been observed before and that cannot be accounted for by any of the theories involved—get detected. This already shows that we can never know if the theories involved are total or not. Besides, if such unexpected phenomena are indeed detected, then the problem of EE and UD at issue could be solved à la Laudan and Leplin—the auxiliary hypotheses provided by a new theory that explains the unexpected phenomena could break the predictive equivalence, for example.

The upshot of these remarks for the problem of EE and UD is clear. It is true that if two total theories are EE then the UD of the choice would be a big problem[16]. However, from the point of view of human scientific knowledge, the very concept of a system of the world is problematic. It is impossible to know whether a certain theory qualifies as a total one. At most, philosophers can speculate about their epistemological and/or metaphysical consequences on a high level of abstraction, but total theories do not present a serious case of EE and UD in the context of the philosophy *of science*. The situation is thus analogous to Descartes' evil-genius argument. It is an interesting and serious issue in metaphysics and general epistemology, but it does not have any particular or relevant consequences for the philosophy of science[17].

[16] If one of the theories includes implausible universal forces, for example, the alternative theory might be better supported by evidence in spite of the EE. That is, the EE between systems of the world would be a big problem granted that both the theories are genuinely scientific and have solid foundations.

[17] Samir Okasha has offered an objection to the cogency of the very concept of a total theory, but along a different line of reasoning. He claims that since the theoretical-observational distinction is not absolute, but context-dependent—a certain term in a theory counts as theoretical, but the same term in a different theory can count as observational—neither the observational content nor the theoretical apparatus of a system of the world can be defined: "If we are even to understand this suggestion [that EE between two total theories leads to UD], let alone endorse it, we must have a criterion for deciding which side of the divide an arbitrarily chosen statement falls on. But such a criterion is precisely what the minimal, context-relative theory/data distinction does not give us. If that distinction is all we have to go on, we can get no grip on what it means for our 'global theory' to be underdetermined by the 'empirical data', nor indeed on what a 'global theory' is even supposed to be." (Okasha 2002, p. 318).

5. Summary and conclusion

I have considered three examples of artificial examples of EE that have received attention in the philosophy of science literature insofar as they are supposed to imply UD. We have seen that, rightly assessed, none of these examples really entails a problem regarding UD of theory choice. They might be interesting for other reasons—van Fraassen's $TN_{(v)}$ and Reichenbach's argument were originally introduced with a different aim—but they are harmless with respect to the problem that occupies us here. Elsewhere[18] I have argued that neither algorithms nor the Duhem-Quine thesis can be used as sources of problematic EE. This means that the only case where EE and UD can imply a serious problem is in the case of actual scientific theories. However, in scenarios like this Laudan and Leplin's argument offers a possible, contingent way out[19].

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[18] (Acuña and Dieks 2013).
[19] See (Laudan and Leplin 1991).

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