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On Reichenbach's argument for scientific realism

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Abstract The aim of this paper is to articulate, discuss in detail and criticise Reichenbach's sophisticated and complex argument for scientific realism. Reichenbach's argument has two parts. The first part aims to show how there can be reasonable belief in unobservable entities, though the truth of claims about them is not given directly in experience. The second part aims to extent the argument of the first part to the case of realism about the external world, conceived of as a world of independently existing entities distinct from sensations. It is argued that the success of the first part depends on a change of perspective, where unobservable entities are viewed as projective complexes vis-à-vis their observable symptoms, or effects. It is also argued that there is an essential difference between the two parts of the argument, which Reichenbach comes (somewhat reluctantly) to accept.

Keywords Scientific realism · Reichenbach · Bayesianism · Base-rate fallacy · Explanation

1 Introduction

There is little doubt that Hans Reichenbach was a scientific realist. A good part of his *Experience and Prediction*, which appeared in 1938, while he was still in the University of Istanbul, aims to articulate an argument for scientific realism. In particular, it

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aims to offer an argument suitable for empiricists, at least the post-positivist empiricists who were critical of a strict verificationist theory of meaning and unpersuaded by the claim that the problem of the reality of an external world of independently existing objects (some of which might well be unobservable) was a pseudo-problem.

Surprisingly, Reichenbach's argument for realism has not attracted a lot of attention. The only notable exceptions are Wesley Salmon and Hilary Putnam. In a series of papers,[1] Salmon insisted that Reichenbach reconciled logical empiricism with scientific realism, but his considered view was that the argument Reichenbach offered in favour of realism is a common-cause argument. In his (2001), Putnam has unravelled some of the nuances of Reichenbach's argument and has rightly stressed a point that is often neglected, viz., that Reichenbach took it that, ultimately, the difference between realism and positivism is a difference between two languages. But Putnam does not explain in sufficient detail how Reichenbach thought the choice between these two languages should be made.

The aim of the present paper is to articulate and discuss in detail Reichenbach's sophisticated and complex argument for scientific realism. The argument presupposes Reichenbach's probability theory of meaning, so Sect. 2 presents the rudiments of this theory. Then, the paper proceeds with a careful reconstruction of Reichenbach's argument. According to this reconstruction, the argument has two parts.

The first part aims to show how there can be reasonable belief in unobservable entities, though the truth of claims about them is not given directly in experience. Reichenbach proceeds in two steps. In the first step (Sect. 3), he aims to secure some common inferential ground between empiricism and realism: there are inferential patterns that are accepted by both empiricists and realists which are such that the reality of unobserved observables is legitimately inferred on the basis of their effects. To this effect, Reichenbach introduces the example of the birds and their shadows. This argument (Sect. 3.1) presupposes a central distinction between reduction and projection, according to which two distinct entities X and Y can be such that X is irreducible to Y (or a set of Ys) and yet Y be a symptom for, a mark for, or the effect of X. In cases such as this, X can be a projective complex of Ys. Claiming some common ground between empiricism and realism renders plausible a second step in Reichenbach's argument (Sect. 4), aiming to show that the inferential patterns that license a transition from an effect to its cause are blind to the observable/unobservable distinction. In other words, the difference between observable and unobservable entities is a difference that makes *no epistemic difference*. The argument Reichenbach offers is illustrated by a modification of the example of the birds and their shadows, the well-known story of the cubic world. An important element of Reichenbach's point of view is that unobservable entities should be understood as projective complexes vis-à-vis their observable symptoms, or effects. But, unlike what Reichenbach seems to think, *this* kind of claim is not licensed by a probabilistic argument; rather it is presupposed by Reichenbach's probabilistic inferential patterns to license belief in unobservable entities.

What exactly is the probabilistic inferential pattern that Reichenbach favours? After flirting with the base-rate fallacy, Reichenbach (Sect. 4.1) endorses a straightforward

[1] Most of them can be found in the posthumously published (2005).

Bayesian inference and relies on prior probabilities of competing hypotheses. Being a frequentist about probabilities, he faces rather significant problems concerning the status of priors, but he (Sect. 4.2) does offer some insightful thoughts as to how prior probabilities can be fixed.

Interestingly, Reichenbach's argumentative strategy has a second part aiming to extent the argument of the first part to the case of realism about the external world, conceived of as a world of independently existing entities (projective complexes) distinct from sensations. Indeed (Sect. 5), he takes it that there is is a formal analogy between the kind of argument employed so far to legitimise belief in unobservables and the argument needed to support the realist conception of the external world. But he seems to realise (somewhat reluctantly) that this kind of move cannot be made. For the reality of an external world is not yet another hypothesis to be confirmed on the basis of evidence and prior probabilities. It is constitutive of a framework—the realist framework—and, as such, its adoption is based on a type of argument different from the type of argument that licenses acceptance of hypotheses within the realist framework.

2 Probability theory of meaning

The kind of empiricism Reichenbach defends in his *Experience and Prediction* is very sophisticated. It is set up in such a way that makes room for ampliative inferences, or for what Reichenbach called "overreaching" inferences. It might be true that all substantive knowledge stems from experience, but the extent and therefore the limits of knowledge depend crucially on the kinds of inferences that are taken to be legitimate. Reichenbach puts all this primarily in terms of his probability theory of meaning (PTM), which allows that statements that are not directly verifiable be meaningful and confirmable on the basis of experience.

Two are the principles of PTM. *First*, a proposition is meaningful if it is (physically) possible to determine a degree of probability for it. *Second*, two propositions have the same meaning if they have the same degree of probability on every possible observation. There is an obvious problem, however, with the second condition (noted by Ernst Nagel in his review of *Experience & Prediction*). The statements 'this coin will land heads in the next toss' and 'this coin will land tails in the next toss' are assigned the same probability by every possible observation (if this is a fair coin and a genuinely chancy effect), and yet they have different meaning. Hence, the antecedent of the second condition above cannot be sufficient for sameness of meaning, though it is necessary.[2]

[2] To be more precise, Reichenbach uses the term 'weight' to capture the degree of probability of a proposition (which is, strictly speaking, a concept in the logical theory of probability). 'Weight' is a predicate applicable to propositions and has to do with the degree of certainty with which a proposition is accepted. It therefore varies from utmost uncertainty to highest possible certainty and also varies with our knowledge or ignorance. Verified propositions have weight equal to unity. In §34, Reichenbach associates weights with wagers and we may safely say that the weight assigned to a given proposition is the fair betting quotient that the proposition is true (cf. 1938, p. 319). This way of putting things would amount to admitting that we can meaningfully talk about single-case probabilities. Reichenbach (1938, p. 314) does note that "A weight is what a degree of probability becomes if it is applied to a single case". For him, however, *all* probabilities are frequencies. Hence, he is forced (1938, p. 325) to say that the concept of weight is "a fictional property of

Probability, then, is the new element that Reichenbach brings to the logic of science. The key idea, if you like, is that probabilistic relations can capture the content-increasing, or ampliative, character of scientific inference. They can capture the relation between observations and theoretical hypotheses in a way that respects the mutual independence of both. Observational propositions can be the premises of (probabilistic) inferences to theoretical statements and yet the latter can have excess content over the former. Conversely, theoretical statements can be the premises of inferences to observational propositions even when there is no deductive entailment of them.

With all this in mind, let us proceed with the reconstruction to Reichenbach's argument for scientific realism.

3 Seeking some neutral ground

Empiricists accept the truth of direct propositions, which concern "immediately observable physical facts" (1938, p. 83). But the kinds of entities they accept (or ought to accept) as real are not exhausted by the immediately given. In going beyond the given to the unobserved observable, they clearly engage in ampliative inferences. This means there are non-demonstrative inferential patterns that are accepted by empiricists and realists—at least they are not obviously *denied* by empiricists.

To claim some neutral ground between empiricism and realism, Reichenbach introduces the example of the shadows cast by birds (1938, p. 108). Imagine some birds flying over us. The shadows of the birds are projected along two perpendicular axes (presumably by a set of vertical light-rays from above the birds and another set of horizontal light-rays from the sides of the birds). The birds are then *inferred* to be located at the point where the co-ordinates meet. This is clearly a non-demonstrative inference from the shadows to the birds: the shadows are *marks* of the presence of birds; they could be there without the birds being present and the birds could be there without casting a shadow (that is, without making any marks of their presence). What is important in this example is that there are *two distinct kinds of existents* which are however co-ordinated with each other. The birds are not reducible to shadows; nor talk about birds is exhausted by talk about shadows. It is precisely because of this that we can use the marks (viz., the effects) to infer something about the causes, though the inference is clearly non-deductive.

Significantly, the birds (as well as the shadows) are observable. It is part of the first step of Reichenbach's strategy that their presence can be identified *independently* of their being inferred on the basis of the shadows. But the observability of the birds is the appetiser for the main course that is about to follow. The first step aims to render plausible two thoughts: (a) it is one thing to talk about an entity and quite another to talk about the external symptoms of its presence; and (b) the existence of one type of entity can well be independent of the existence of another, even though the latter can be a (contingent) symptom of the presence of the former. This dual point forms the ground for the legitimacy of an inference from the effects to their causes. The fact

Footnote 2 continued
propositions which we use as an abbreviation for frequency statements"—which means that every weight should be determined, in principle, by a relative frequency.

that the inferred entity is observable turns out to be a, perhaps pleasant, add-on, which does not affect the status of the inference.

3.1 Reduction vs. projection

The philosophical presupposition of the example so far is that there can be distinct kinds of entity which are such that one can be a symptom (or the effect) of the other. To make a case for this presupposition, Reichenbach distinguishes between reduction and projection. Though reduction is introduced as a relation of meaning equivalence between propositions (cf. Reichenbach 1938, p. 95), Reichenbach (1938, p. 99) focuses his attention on a special case of reduction, viz., the relation between a complex and its internal parts, in virtue of which the complex is equivalent to its parts. He moves swiftly from conceptual reduction to ontic reduction because he is interested in cases in which there is reduction of existence: where the existence of an entity is reducible to the existence of others, or where "the complex vanishes with its elements" (1938, p. 114). Constitution, or the whole-part relation, is such a case of ontic reduction. A wall reduces to the set of bricks it is made of (and a certain spatial arrangement of them). It asymmetrically depends on them for its existence: it exists only insofar as the bricks are in place, but it can cease to exist (it can be pulled down) even if none of its constituent bricks is destroyed. We can say that the wall is constituted by (a certain configuration of) the bricks.

Projection is a relation between two distinct types of entity such that one type constitutes a symptom, or an effect, or a mark of the other type. The marks of the presence of an entity (e.g., the sound of steps on the staircase or the footprints on the beach) are 'external elements' of a distinct entity, a means to infer the presence of something other than them. Reichenbach contrasts them to 'internal elements' that are the constituents (or the parts) of a type of entity. A type of entity, then, may well be reducible to its internal elements (constituents) but it is only projected to its external elements (symptoms; effects). In projection, there is no asymmetric dependence. It is not the case that if the external elements (the marks) cease to exist, the projective complex ceases to exist too.

The example of the birds is meant to illustrate the difference between reduction and projection. The relation between the birds and the shadows is projective and not reductive. In a sense, this is *so* obvious that needs no arguing. Still, Reichenbach offers two reasons. *First*, the bird-propositions (that is, propositions that refer to birds) are not equivalent to shadow-propositions (that is, propositions that refer to shadows). This is so because one cannot deductively infer one from the other. On the contrary, the connection between bird-propositions and shadow-propositions, like all inferential connections between causes and effects, is ampliative—and in particular, probabilistic. *Second*, in the example at hand, "there is no reduction of existence" (1938, p. 109), viz., there are two distinct types of entity; the birds have independent existence over the marks. In the end, of course, the two reasons are one and the same. The non-equivalence implies distinct existence and conversely; and this is underwritten by the fact that the relation between claims about birds and claims about shadows is non-deductive.

The very opening of the distinction between reduction and projection makes plausible the following thought: observable entities are symptoms (or effects) of unobservable ones (that is, unobservable entities are projective complexes, whose external elements are observable entities). This implies a certain inversion of the way empiricists view things. The unobservables become legitimate because, *qua* projective complexes, they are distinct existences whose external elements are observable entities.

On the face of it, reduction and projection need not be in conflict provided they are performed on different bases. One and the same entity can be a projective complex vis-à-vis its external elements *and* a reductive complex vis-à-vis its internal elements. A bird, for instance, is a projective complex vis-à-vis its shadow and can be taken to be a reductive complex vis-à-vis its cells and molecules. The operative relations are clearly distinct. If reduction is, at least typically, constitution, projection is, at least typically, causation. There is a problem, however. Reichenbach (1938, p. 114) has claimed that in the case of reduction it is possible to define an entity in such a way that it "vanishes with its elements". If the very same entity is a projective complex vis-à-vis a set of external elements, it is a distinct existence, whose reality is defeasibly inferred (by means of a probabilistic inference) from the external elements. It seems that *the very same entity* is real and independently existing and unreal (or less real, so to speak) and dependently existing. Besides, if reduction, *qua* conceptual relation, is such that the equivalence between the reductive complex and the reductive basis is ascertainable a priori (as Reichenbach suggests it is the case; cf. 1938, pp. 95 and 98–99), it follows that if macroscopic entities are reductive complexes of microscopic constituents, this must be knowable a priori—which would be absurd for an empiricist.

Reichenbach does address this problem, but quite later on in the book and after he has completed his argument for realism. What he says, however, is very instructive and relevant to his argument. He (1938, p. 216) introduces the concept of "internal projection", which—in effect—amounts to an a posteriori theoretical identification. A table is a collection of atoms—current physics tells us. Atoms are the constituents of the table. They fix the properties of the table, whatever they are. But the table is *not*, strictly speaking, a reductive complex vis-à-vis its constituent atoms because the relations that exist between the atoms and the table (in virtue of which the properties of the table are fixed by the properties of the atoms) can be known only a posteriori and by means of probabilistic inferences. In other words, the table is not a reductive complex of atoms because there are no deductive inferential relations (and hence no equivalence) between propositions about the table and propositions about its constituent atoms. The relation between the table and its constitutive atoms is projection (since it is captured by probabilistic inferences), but it is an *internal* projection (since, there are not two distinct existences: the table *and* the atoms). An internal projection, then, is a relation between an entity and its constituents which is such that (a) the entity is not something distinct from its constituents (for instance, there is the already noted asymmetric dependence between them) and (b) the constitutive relations between the entity and its constituents are knowable a posteriori. An internal projection, in other words, is a kind of reduction "which is ascertained by probability inferences, not by definition" (1938, p. 216).

The notion of internal projection brings together reduction *and* projection. Though Reichenbach would not put it this way, ontically (but not conceptually) it is a reduction whilst epistemically it is a projection. Reichenbach, then, was wrong to identify, at least initially, two senses of reduction: an ontic relation of constitution *and* a deductive inferential relation, by means of which all truths about the reductive complex are deducible from truths about the reductive basis. These are not the same sense of reduction—at least not necessarily. Running them together obscured the idea of internal projection. It obscured, at least initially, an important subsequent element in Reichenbach's argument for realism, viz., that apart from being independently existing entities (*qua* projective complexes), whose external elements are observable entities, unobservable entities are the constituents of observable entities (that is, observable entities are reductive complexes, whose internal projective elements are unobservable entities).

4 Reaching outside the C-world

The example of the birds and their shadows has made plausible the view that there are inferential patterns (*from* external marks *to* projective complexes) that are shared between empiricists and realists. But in the example so far there is a two-way independent access to the marks *and* the causes: the birds are, after all, observable. One may naturally wonder: how can we proceed if there is only one-way independent access to the marks? How can we possibly infer the existence of something *distinct* from the marks? Something we cannot have an independent epistemic access to? More generally: how exactly do ampliative inferences generate the excess content attributed to the theoretical propositions of scientific theories?

Reichenbach's answer to this question is motivated by a modification of the example of the birds and their shadows—the example of the cubical world (C-world) (1938, §14). In this story, the inhabitants of the C-world are confined within a huge cube, whose walls are made of white cloth. It is soon observed that there are shadows dancing around the walls. Unbeknownst to the inhabitants, these are the shadows of birds flying outside the cubical world. A "friendly ghost" (a benevolent demon?) has set up a complex set of mirrors that project shadows of the birds on the walls. The birds (causes of the shadows) are, by hypothesis, unobservable—in fact, the laws of nature are presumed to be such that the birds cannot be seen. Is it possible for the inhabitants to come legitimately to believe that there are birds outside the C-world?

Reichenbach points out that there in a sense in which the inhabitants of the C-world are in the same epistemic situation as those involved in the initial example of the birds. To bring this out, he introduces a local Copernicus who uses a telescope and finds out that the marks on the walls fall under regular patterns: the movements on the side wall are co-ordinated with movements on the top wall.[3] The local Copernicus is pictured to engage in an inference from the marks on the walls to their causes and to *infer*

[3] Reichenbach stacks his deck here a bit, since Copernicus is supposed to figure out that the dots on the walls have the shape of animals—of *birds* really—which seems to illicitly assume that there are birds within the c-world too and that people know a lot about them. But let us leave this to one side.

the existence of unobservable birds as causes of the marks on the walls. Here is how Reichenbach (1938, p. 118) puts it:

> He [the local Copernicus] will maintain that the strange correspondence between the two shades of one pair cannot be a matter or chance but that these two shades are nothing but effects caused by one individual thing situated outside the cube within free space. He calls these things 'birds' and says that these are animals flying outside the cube, different from the shadow-figures, having an existence of their own, and that the black spots are nothing but shadows.

Let us try to dig a bit deeper into the way the local Copernicus reasoned, since Reichenbach does not say much about it. Here is how I would put the matter. He observes the patterns on the walls and he forms a hypothesis that purports to explain them. This is a causal hypothesis: it posits a cause of the observed pattern in virtue of which this pattern is rendered intelligible. Copernicus, in other words, posits an entity (better: a type of entity) that brings some causal-nomological order in the world-view of the inhabitants of the C-world. Instead of taking a certain pattern as a brute fact, he offers an explanation of it—an explanation by postulation (of unobservable entities). As is stressed in the quotation above, the call for explanation is motivated by the thought that surprising coincidences should not be attributed to chance—there must be a reason for them to hold and hence an explanation. To my mind, this is clear case of what has been called inference to the best explanation (IBE).

This claim might not amount to much—since IBE needs articulation. But the general point that needs to be driven home is this. Recall the question we (and Reichenbach) faced above: how can the existence of something *distinct* from the marks, something to which there is no independent epistemic access, be inferred? The answer is that explanatory reasoning does precisely this: it generates hypotheses with excess content over the observations that probe them. The projective complex (the birds), which has independent and distinct existence, is not strictly speaking the product of probabilistic reasoning. Rather, it is first posited as the best (causal) explanation of some marks or effects and *then* the issue is raised as to how probable it is relative to these marks. A probabilistic connection does hold between an explanatory hypothesis and some evidence for it, but it is *not* this probabilistic connection that generates the excess content; rather, the probabilistic connection suggests that ampliative hypotheses can be confirmed and hence that the excess content they possess in virtue of the fact that they are explanatory hypotheses can be legitimately accepted.

So: the excess content is generated by the explanatory connection there is between the projective complex and its external elements. The relation of projection is an explanatory relation: it relates two distinct existences. I am not sure Reichenbach saw this point very clearly, though, as we are about to see, the way he went on to develop his argument in step 2 shows some appreciation of it.

This way of viewing things explains why the observability of the birds does not matter to their being posited as causes of the shadows (or the dots). Both in step 1 and the step 2 of Reichenbach's argument a physical entity is posited as a projective complex and its reality is accepted on the basis of the claim that it causes (and hence it

causally explains) some observable events. It makes no difference to this function of the posited entity whether it is observable (as in step 1) or unobservable (as in step 2).

4.1 The base-rate fallacy (and how Reichenbach avoids it)

After presenting the brief summary of Copernicus's reasoning that we saw in the last quotation, Reichenbach went on to claim that the hypothesis of the local Copernicus is "highly probable" when "judged from the facts observed". For, as he put it, it is "highly improbable that the strange coincidences observed for one pair of dots are an effect of pure chance" (1938, p. 120). Faced with improbable coincidences, he added, scientists will not believe that they are a matter of chance but instead they will look for a causal explanation (or for "causal connection", as he put it).

This suggests that Reichenbach saw the argument in step 2 as a straightforward probabilistic argument with a highly likely conclusion. But then the argument seems to be open to the charge that it commits the base-rate fallacy. Here is a brief reminder of the fallacy (introduced by the standard example in the literature, known as the Harvard Medical School test).

Harvard medical school test

A test for a disease has two outcomes, 'positive' (+) and 'negative' (−). Let a subject S take the test and let H be the hypothesis that S has the disease and −H the hypothesis that S doesn't. The test is highly reliable: it has zero *false negative* rate: the likelihood that S tested negative given that S does have the disease is zero (i.e., prob(−/H) = 0). The test also has a very small *false positive* rate: the likelihood that S is tested positive though S doesn't have the disease is, say, 5% (prob(+/−H) = .05). S tests positive. What is the probability that S has the disease given the positive test? That is, what is the posterior probability prob(H/+)?

Given only information about the likelihoods prob(+/H) and prob(+/−H), the question above is indeterminate. This is so because there is some crucial information *missing*: we are not given the incidence rate (base-rate) of the disease in the population. If this incidence rate is very low, e.g., if only 1 person in 1,000 has the disease, it is very *unlikely* that S has the disease even though S tested positive: prob(H/+) would be less than .02. For prob(H/+) to be high, it must be the case that prob(H) be not too small. But if prob(H) is low, it can dominate over a high likelihood of true positives and lead to a very low posterior probability prob(H/+).

Reichenbach has invited us to compare the likelihoods of two competing hypotheses, viz., H: the existence of unobservable birds; and not-H: there are no birds outside the cubical world (and hence that the observed coincidences are a matter of chance). More generally put, Reichenbach's argument so far is this: there is an effect *e* (the strange coincidences) observed; *e* would be very unlikely if not-H were the case, but *e* would be very likely if H were the case; hence, H is very likely (or much more likely than not-H). Indeed, he says quite clearly: "Reflections like this would incline the physicists to believe in the hypothesis of Copernicus …" (1938, p. 121). But this kind

of argument commits the base-rate fallacy. The likelihoods are not enough to fix the posterior probability of H, let alone to make it high.

In order to avoid the fallacy, we need to take into account prior probabilities.[4] The form of the argument, then, would be like this:

(A)
prob(e/H) is high.
prob(e/−H) is very low.
e is the case.
prob(H) is not very low.
Therefore, prob(H/e) is high.

(A) is not fallacious. In fact, even with relatively low prob(H), the posterior probability prob(H/e) can be quite high. In any case, assuming prior probabilities, the degrees of confirmation of hypothesis in light of the evidence becomes quite definite.

Reichenbach agonises a lot (and over several pages) about how different likelihoods could be attributed to the competing hypotheses, but in the end he rescues his argument from falling prey to the base-rate fallacy by admitting prior probabilities. He is clearly aware that the probabilistic inference he has in mind requires another element, viz., the prior probabilities of the competing hypotheses. Curiously, however, he relegates this important point to a brief and obscure footnote (under the pretentious heading "Remark for the mathematician") (1938, p. 124). There he notes that the probabilistic inference he has in mind relies on "Bayes's rule" (which, however, he never states). All he says is that one can use Bayes's rule to specify the posterior ("backward) probability of a hypothesis given the evidence as a function of the likelihood ("forward probability") and the "initial probability" of a hypothesis. More importantly, different prior probabilities make a difference to the posterior probabilities of competing hypotheses, even if the likelihoods are equal. Towards the end of the book, Reichenbach (1938, p. 390) notes that Bayes's theorem is a rule "for inferring from given observations the probabilities of their causes".

Surprisingly little is said about the status of prior probabilities: "It is these initial probabilities that are involved in the reflections of the physicist about causal connections" (1938, p. 124, ft. 4). What he has in mind is this. The two competing hypotheses H (the existence of unobservable birds) and not-H (the observed coincidences are a matter of chance) can legitimately be given different prior probabilities on the basis of analogy and past experience. Even if a persistent positivist contrived a hypothesis such that a strange coincidence is the outcome of a (strange) causal law that did not involve projective complexes, one could point to the fact that in many other similar cases where strange coincidences were present, there had been a causal connection among them that involved projective complexes (simply put, there had been a common cause) (cf. 1938, p. 123).

[4] One can always adopt likelihoodism, which uses the *likelihood ratio* to capture the strength by which the evidence supports a hypothesis over another, but it does not issue in judgements as to what the probability of a hypothesis in light of the evidence is (cf. Sober 2002). But this is clearly not the way Reichenbach proceeded.

4.2 Prior probabilities to the rescue

By bringing prior probabilities into play, Reichenbach is able to show that hypotheses about unobservables are confirmable on the basis of the evidence—provided of course they are allowed some non-zero initial probability. Besides, he is able to argue that the difference between forming beliefs about observables and forming beliefs about the unobservable on the basis of the evidence is one of degree. Both kinds of belief are ampliative; they concern projective complexes; their degree of confirmation is based on the same type of probabilistic reasoning. So probabilistic (Bayesian) inference is overreaching: it allows the justification of hypotheses (by showing how they are confirmed by the evidence) irrespective of whether or not their content is observationally accessible. For him, this type of probabilistic inference "is the basic method of the knowledge of nature" (1938, p. 127) and this is so for everyone—that is, positivists too have to rely on it (as the first step of the argument has shown).

There is, of course, the issue of the status of prior probabilities. Reichenbach comes back to this issue quite late in the book (§30) and treats them as initial weights (or posits), and hence as estimates of how likely a hypothesis is (prior to the evidence). The overall tone of Reichenbach's discussion (as well as his frequentist theory of probability) suggests that for him the assignment of prior probabilities to competing hypotheses is not a matter of subjective preference. But it is not quite clear how they are fixed. The first reaction of the critics of the book was to claim that Reichenbach leaves us in the dark. Eleanor Bisbee (of the neighbouring American College of Istanbul) noted in a review of *Experience and Prediction* (1938, p. 365):

> Dr. Reichenbach does not hesitate in the least to make a philosophy of gambling. His object is to find out how to gamble well. Every decision about a specific instance is a 'posit' of a possible outcome based on the highest known probability for similar cases. The trick is to choose a class of cases to which the similarities are significant. In passing, it may be noted that if instinctive appraisals are admitted, it seems as though a factor in the weight might be wishful thinking, which the author does not discuss. Presumably, his reply to this would be that clear knowledge of the probability basis of decisions would be the best check on that tendency.

To be sure, the already noted reliance on analogy and past experience might also help Reichenbach to draw some connections between prior probabilities and relative frequencies. For at least there could be some pool of similar cases, from which an estimate of a weight of a new case could be made. But of course, a lot more would have to be said about the similarities among theories in virtue of which prior probabilities could be specified.

Generally, Reichenbach's conception of probabilities as limiting relative frequencies creates a number of problems. For one, it is not clear how relative frequencies can be specified for advanced hypotheses, viz., hypotheses for which analogy and past experience cannot be relied upon. For another, it appears that treating the prior probabilities of hypotheses concerning unobservables as relative frequencies would require independent access to these unobservable entities so that success frequencies

are specified. But (a) there is no such independent access; and (b) this kind of requirement would remove the attraction of Reichenbach's second step, since it would imply that probabilistic inference requires independent epistemic access to the entities whose existence is supported by the probabilistic inference.

Though the criticism against Reichenbach's frequentism is telling, the important point of the second step of the argument for realism is two-fold: (a) probabilistic inferences rely on prior probabilities and (b) by capturing explanatory relations of projection, they are blind to the observable/unobservable distinction.

It would be unfair, however, to Reichenbach not to say something in his defence, since at the very end of his book (and in a way seemingly unrelated to the argument for realism) he pointed to what I think is the right general attitude about what kind of considerations play a role in fixing initial probabilities.

Discussing the issue of weights attributed to scientific theories (which cannot be so easily equated with relative frequencies), he drew an all-important distinction between two levels of probability ascriptions (cf. 1938, pp. 397–398). When we try to specify the degree of confirmation of a scientific theory, that is when we look at how the evidence supports a certain theory, we can proceed at two distinct levels. At the first (or ground) level, we look into the specific information concerning the theory at hand: its predictions, its likelihood, and its initial probability, which might reflect an initial plausibility. We then calculate its degree of confirmation. We can however move to a second level and, as Reichenbach (1938, p. 397) put it, "consider the theory as a sociological phenomenon and (…) count the number of successful theories produced by mankind". It is obvious that at this higher level, we are interested in the base-rate of truth among scientific theories. The relevant prior probability then assigned to a scientific theory is the prior probability that it is true given that it belongs to a pool of theories with certain characteristics. We don't quite have this kind of statistical information. But had we had it, it would be a frequentist prior probability. Reichenbach was overly optimistic that this kind of information might become available. The key point, however, is that these two levels of determination of the (prior) probability of a theory need not (and as a rule will not) be the same. The kind of information that can be employed in the determination of the prior probability of a specific theory (assuming relevant background knowledge etc.) will be much more detailed and specific than the kind of information that can be employed at the second level—where information from the history of science and the past performance of scientific theories will be pertinent.

Reichenbach thought that *both* kinds of consideration should be taken into account in fixing the probabilities of theories. He also suggested that there may be reason to trust second level probabilities more than first level one (and conversely). For, instance, there might be domains of inquiry where the truth is harder to get than others; or where theories have had a greater falsity rate.[5]

[5] In my (2009, Chap. 4), I have drawn a similar distinction between *first-order evidence* in favour of a scientific theory and *second-order evidence* coming from the past record of scientific theories and/or from meta-theoretical (philosophical) considerations that have to do with the reliability of scientific methodology. I take it that when we think about scientific theories and what they assume about the world we need to balance both kinds of evidence.

5 The problem of the external world

If we were to recap the key point so far, I would say the following. The crucial element of Reichenbach's argument for scientific realism, at least in the way I think it ought to be going, is that unobservable entities are seen as projective complexes that are detected by means of their effects, symptoms or marks. This kind of move is *not* the outcome of probabilistic reasoning, but of explanatory reasoning: the relation of projection is an explanatory relation between two distinct existences in virtue of which the first causally explains the second and the second becomes a mark of the first. Having this kind of relation in place (which cuts through the observable/unobservable distinction), probabilistic reasoning (with an ineliminable role assigned to prior probabilities) can yield definite degrees of confirmation of ampliative hypotheses concerning these projective complexes and underwrite their warranted acceptance.

It turns out that Reichenbach has had a more ambitious aim in mind. He attempts to generalise the lessons drawn from the second step to the realism-positivism debate in general. So there is a second part (and a *third* step) in his strategy. His initial thought (1938, §15) is that the very question of the existence of the external world of physical things (what he (1938, p. 139) calls "the realistic conception of the world") as distinct and independent from sense-impressions can be settled along the lines of the argument of the second step (see 1938, p. 154). After all, there are two rival hypotheses. One, favoured by Reichenbach, is that the physical objects of the external world are independently existing projective complexes, with the impressions being external elements of them—that is, signs or marks or effects of their presence. The rival (positivist) hypothesis is that physical objects are reduced to impressions—they are reductive complexes and as such, they are equivalent to collections of sense-impressions. It is crucial to Reichenbach's argument that the realist claim (the projective hypothesis) is not equivalent to the positivist claim (viz., the reductive hypothesis). That they are not, in particular that the realist hypothesis has excess content over the positivist, is licensed by PTM. If the two hypotheses are not equivalent, there is, after all, a *problem* of the existence of the external world.

PTM opens up a space for the problem of the external world to be a *genuine* problem and not merely a pseudo-problem as many of Reichenbach's contemporaries would have it. By the same token however, he must offer a *genuine* solution to it. It is tempting then to think that the required genuine solution is simply an extension of the argument for the reality unobservable entities—sketched above. In particular, it is tempting to equate the shadows on the walls of the C-world with impressions and the birds that cause them from the outside with external physical objects (cf. 1938, p. 154).

Note that Reichenbach's contemplated move rests on the assumption that the very same method that is employed in science to accept hypotheses (probabilistic inference) can be employed in defence of realism as a philosophical position. There is, however, a difference between the argument of step 2 for the reality of unobservables and the argument of step 3 for the reality of independently existing physical objects (the external world). In step 2, the argument takes place *within* the framework of (independently existing) physical objects. Both the shadows (marks on the walls of the C-world) and the birds (outside the C-world) are distinct and independently existing physical objects. It's just that the former are observable while the latter

are unobservable and Reichenbach's forcefully made point was that this is a difference that makes *no* epistemic difference. In particular, the (unobservable) birds are projective complexes vis-à-vis the shadows on the wall. But the very idea of their being projective (as opposed to reductive) complexes requires that they exist as physical things independently of their external symptoms.

In generalising the argument to the realist conception of the world, there is a movement from (dependently existing) impressions to (independently existing) physical objects. Reichenbach (1938, p. 143) thinks that what matters is that in both steps 2 and 3, the relevant relation is projection and not reduction. But a committed reductive positivist would have objected that even if the argument in step 2 were to go through, the argument in step 3 generates new content out of nothing: it introduces an altogether different type of entity.

Reichenbach (1938, pp. 136–138) tries to block this objection by noting that positing physical objects as distinct from impressions would result in an image of the world in which causal laws are homogeneous. That is, the very same causal laws would hold irrespective of whether or not anyone perceived anything. The positivist image, on the other hand, exhausted as it is by sense-impressions, requires strange and unhomogeneous causal laws. In particular, it would require causal laws that ensure continuity among sense impressions even when no-one observes anything. Reichenbach's idea is that an image of the world based only on sense-impressions would have to mimic the fact that physical objects exist continuously, and in particular unperceived, and to achieve this it would need to posit two distinct and co-ordinated sets of causal laws: those that hold when someone perceives something and those that hold when no-one perceives anything.

All this may well be right. But the fact remains that these considerations concerning causality and homogeneity can at most influence the prior probabilities of the two competing hypotheses—if they are indeed seen as theoretical hypotheses. For the argument for the reality of the external world to be of the same type as the argument for the reality of unobservable entities advanced in step 2, Reichenbach needs to rely on initial weights—as it became clear in the development of the argument in step 2. Here, for one, is where probabilities as frequencies are in their worst shape. What can plausibly be the reference class for which relative frequencies are determined? In his review of *Experience and Prediction*, Ernest Nagel (1938, p. 271) was quick to pick on this:

> How can a conception of probability, which takes its stand firmly upon interpreting probabilities as relative frequencies in empirical sequences, be made to apply intelligibly to a domain inaccessible to the requisite material investigation? Universes, with or without external worlds, are not so plentiful as blackberries; and not even Reichenbach's ingenuity can make plausible the assumption that a statistical view of probability is relevant to solving such a problem.

Perhaps, we should think of prior probabilities as based on plausibility considerations and analogy. The prior probability, one might say, of an external world distinct from impressions is significantly higher than the prior probability of a world of impressions because the former is such that it has simpler causal laws, it is more unified etc.

But there is a problem. The very idea of assigning prior probabilities to the realist hypothesis and to the positivist hypothesis requires that there is another framework in place in which these two hypotheses are compared in terms of plausibility and the rest. This is clearly what happens in step 2 of the argument. There, the framework of physical things, *qua* independently existing projective complexes, is *already in place*. The issue then is to assign prior probabilities to the hypothesis that the shadows are caused by unobservable birds and the rival hypothesis that the co-ordination of the shadows is a coincidence. This can be done and hence, we can legitimately attach a degree of confirmation to the hypothesis that the shadows are caused by unobservable birds, without, as Reichenbach put it, digging a hole on the wall of the C-world (cf. 1938, p. 149). But in trying to extend the argument to the problem of the external world (step 3) at stake is the reality of the very framework of physical things, *qua* independently existing projective complexes vs its unreality. And there is simply no further framework in which this issue can be examined and in which the two rival hypotheses can be assigned different prior probabilities on the basis of their respective initial plausibilities.

Here is another way to state the same problem. Reichenbach's PTM requires the realist framework and cannot be a proof of it. PTM requires probabilistic relations between distinct types of entity, viz. the causes and the effects, or the projective complexes and their external elements, or the external physical objects and sense-impressions. Hence PTM cannot prove the distinctness of these types of entity; it presupposes it. What PTM does is to allow ampliative inferences between the marks and the projective complexes, after both have been admitted. In particular, as argued in Sect. 4, PTM does not yield the reality of the projective complexes. Based on the claim that talk about projective complexes has excess content over talk about their marks, PTM presupposes their existence and shows how there can be probabilistic relations between them and hence evidence for them, irrespective of their status vis-à-vis observability.

5.1 A perspectival approach to reality

Interestingly, Reichenbach comes close to accepting all this—and hence to denying (or neutralizing) the third step of the argument in the form he did present it. It seems he is aware that the strategy of the first part of his argument in favour of unobservables cannot be generalised to the problem of realism in general. After all, the realist conception of the world is *not* a hypothesis at the same level as the hypothesis of birds outside the walls of the C-world. The choice of an overall framework (say an egocentric framework, where things are reduced to classes of impressions or a realist framework, where impressions are merely effects of independently existing objects) cannot be simply a matter of probability and confirmation of two competing hypothesis.

Reichenbach (1938, §17) put the point in terms of languages. Ultimately, the problem of the external world is a problem of choosing a certain language (a language that allows us to talk about independently existing physical things) as opposed to another one (an "egocentric" language, as he put it). And choices of language are not factual but based on decisions (cf. 1938, p. 145). Reichenbach goes on to stress that the

choice is, ultimately, between two different conception of meaning: his own PTM and a verificationist one. This choice does not answer to truth and falsity.

The general problem of realism is then divided into two components—the first is the adoption of a language (framework; theory of meaning) as the result of a free (that is non-dictated by evidence or a priori considerations) decision; the second is the investigation of the adopted language (framework) by looking into its fruits. This consequentialist move is, for Reichenbach, a way to justify the choice of the language (framework)—especially by showing, in a comparative fashion, that one language is better suited than another to achieve certain aims or to satisfy certain desiderata (cf. 1938, pp. 146–147). This last move suggests that the original decision to accept a certain framework (the realist one that Reichenbach favours) is *not* arbitrary, though unforced by facts or reason.

This kind of consequentialism fits well with Reichenbach's overall approach to epistemology. He took it that the critical task of epistemology is to separate the factual from the conventional—a remnant of his Kantian heritage. The conventional element amounts to a decision to adopt a framework. Yet, it is not enough to point out that the choice of a framework does not answer to truth or falsity. Part of the critical task of epistemology is to examine what kinds of consequences follow from the adoption (the *unforced* adoption) of a certain convention. Reichenbach insisted that though the choice of a framework is based on an unforced decision, this decision entails others—what he (1938, p. 13) called "entailed decisions"—which, therefore, are far from arbitrary in that one is no longer free not to adopt them if one has already chosen the framework. By examining these 'entailed decisions' certain judgements can be made about the consequences of adopting a certain framework, their plausibility and their fruitfulness.

As noted already, a case discussed by Reichenbach in some detail is the choice between an egocentric framework, in which objects do not exist while unperceived, and a realist one. Even if it is a matter of unforced decision to adopt an egocentric framework, one entailed decision that follows this is the adoption of strange and unhomogeneous causal laws. These entailed decisions may be contestable, or implausible, on independent grounds and this counts against the framework that implies them. The very presence of entailed decisions helps to build, as Reichenbach (1938, p. 15) put it, "a dam" against "extreme conventionalism".

This way of putting things neutralises the alleged similarity between steps 2 and 3. The argument of step 2 is not consequentialist. The bird hypothesis is better supported than the shadows-hypothesis. Conversely, step 3, unlike step 2, need not (in fact, it cannot) rely on prior probabilities.

Having said this, Reichenbach did not clearly and forcefully uncouple the arguments of step 2 and 3. As noted already, the real problem is that we cannot talk of the probability of a framework as a whole, especially since the very idea of assigning probabilities to competing hypotheses *within* a framework (as in the second step of the strategy) requires that the framework is already in place.[6]

[6] This is a point made by Feigl (1950, p. 54). For more on Feigl's argument for realism, see my (2010).

Reichenbach characterised theoretical entities (like atoms) "illata", meaning inferred entities. He (1938, p. 212) contrasted them to both concreta (which he took them to be immediately accessible to observations) and abstracta. Concreta are real beyond any doubt—they are immediately existent, as he put it. Illata, to be sure, "have an existence of their own" (1938, p. 212). But it seems that Reichenbach puts a premium on concreta because they are epistemically accessible and their knowledge is not probabilistic (nor inferential). On the other hand, he admitted that a possible basis for the construction of the world (a clear allusion to Carnap's *Aufbau*) is the elementary particles posited by scientific theories, which are illata from the point of view of how their reality can be ascertained. He went as far as to claim that everything there is is a reductive complex of illata (as the atomic theory of physics implies) (cf. 1938, p. 215)—where, of course, the reductive relation he had now in mind was ontic: what we have already seen him calling *internal projection*. Seen from this point of view, only illata have objective existence. The world of observable things, the world of concreta, is taken to be a "substitute world", "not the world as it is—objectively speaking" (1938, p. 220). The manifest image of the world is essentially false (cf. 1938, p. 221).

To be more precise, Reichenbach had a perspectival approach to reality. He did use the notion of perspective and thought there is no perspective-free view of reality (cf. 1938, p. 221). This is perhaps a permanent loan from Kant, that Reichenbach kept even when he abandoned other key elements of his early Kantianism. Frameworks, then, can be seen as perspectives on reality. Occasionally, Reichenbach calls them "descriptional" frames by means of which we view the world (1938, p. 221). The manifest image of concreta then, is just one perspective on reality; it is "one-sided": it reveals us only what is tuned to our perceptual capacities; and yet, "it shows some essential features of the world" (1938, p. 225). The realist framework of illata is yet another perspective on reality. It is a more objective perspective, since it is not anthropocentric (it is cosmological, as Feigl would put it). But it is still a perspective on reality. The task of epistemology then is to reveal these perspectives and to combine them. As Reichenbach (1938, p. 225) put it:

> We wander through the world, from perspective to perspective, carrying our own subjective horizon with us; it is by a kind of intellectual integration of subjective views that we succeed in constructing a total view of the world, the consistent expansion of which entitles us to ever increasing claims of objectivity.

6 Concluding thoughts

Reichenbach's argument for scientific realism (steps 1 and 2 in the argument) is an argument *within* the realist framework (the realist conception of the world) and not an argument *for* it. It presupposes, rather than proves, that there are projective complexes that cause certain observable phenomena. Given this presupposition, Reichenbach's argument shows that hypotheses about unobservable entities are confirmable and confirmed on the basis of the evidence—provided that some non-zero initial weight is ascribed to them. The role of the realist framework is precisely to allow ascriptions

of non-zero initial weights to hypotheses about unobservable entities. Differently put, the very fact that probabilities are assigned to hypotheses about unobservable entities requires the prior adoption of a realist framework within which such hypotheses are formulable and evaluable.

It's a different matter, of course, how prior probabilities are to be assigned after the framework is adopted. Here, Reichenbach's interpretation of probability as limiting relative frequency might betray him, since as Feigl (1950) pointed out, it would require some estimation of the actual success of inferences from observable entities to hypotheses concerning unobservable entities—and, clearly, there is no independent assessment of the latter. But of course, the important point is not whether prior probabilities are relative frequencies but rather that (a) they are necessary and (b) their ascription requires a prior adoption of the realist framework.

Seen in this light, the attraction of Reichenbach's argument for scientific realism is in the thought that the very issue of observability of an entity is spurious. An entity is posited for explanatory reasons and the probabilistic inferential pattern by means of which some degree of belief in its existence is specified is blind to whether or not this entity is (un)observable. Here is where an appeal to common causes is important. Not as a distinct inferential pattern, but by way of reminding us that the relevant reference class for assigning prior probabilities to hypotheses should be inferences from correlations to common causes (that is, that, by and large, such correlations admit of further explanation by reference to third factors), *irrespective of whether the common causes are observable or not*.

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