Predictive success, partial truth and skeptical realism

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Abstract

Realists argue that mature theories enjoying predictive success are approximately and partially true, and that the parts of the theory necessary to this success are retained through theory-change and worthy of belief. I examine the paradigmatic case of the novel prediction of a white spot in the shadow of a circular object, drawn from Fresnel's wave theory of light by Poisson in 1819. It reveals two problems in this defence of realism: predictive success needs theoretical idealizations and fictions on the one hand, and may be obtained by using different parts of the same theory on the other hand.

I maintain that these two problems are not limited to the case of the white spot, but common features of predictive success. It shows that the no-miracle argument by itself cannot prove more than a *skeptical realism*, the claim that we cannot know which parts of theories are true. I conclude by examining if Hacking's manipulability arguments can be of any help to go beyond this position.

Introduction: partial truth and predictive indispensability

In its broader understanding, scientific realism is the claim that our scientific theories are *true*, *i.e.* that something about them describe correctly mind-independant realities of nature. While its an old philosophical thesis which has known a handful of different versions, most of today's realists agree that the best argument to defend it has been first proposed by Duhem (Duhem 1914, 37) and reintroduced – on another form – by Putnam (Putnam 1975), Musgrave (Musgrave 1988) and Leplin (Leplin 1997). This argument is the no-miracle argument based on predictive success: it claims that theories which have enjoyed predictive success are *mature*, *i.e.* true, because truth is the only explanation of this success which does not make it a miracle.

Several problems of this argument have been discussed in the literature, including the issue of the inference from predictive success to truth (Stanford 2000). Here, I would like to raise an objection on which nobody has, to my knowledge, ever focused: the realist is in debt for a clear definition of *partial truth*, but the no-miracle argument is not strong enough to give us a criteria to discriminate which parts of a theory are true, and which are mere auxiliaries. This leads to an extremely deflationnist version of realism: *skeptical realism*, the claim that we cannot know towards which parts of approximately true theories we are ontologically committed.

A common objection to scientific realism is the so-called "pessimistic induction" (Laudan 1981) based on past theories, which have enjoyed predictive success, but assumed entities which are now believed to be false. Fresnel's wave theory of light is a strong case in favour of this objection, because it led to a successful novel prediction, while assuming the existence of *optical ether*, which has been proved to be fictional at the end of the XXth century.

The realist's answer is often to reduce its claim from truth to approximately and partial truth. Approximate truth is the claim that a theory is *not far* from truth (which raises the issue of a clear definition of distance to truth). Partial truth is the claim that some aspects of our theories are true, while other have no truth content [1].

One of the line of reasoning to resist the pessimistic induction is to maintain that the true parts of a theory are the one which are necessary to its predictive success *and* that they will be retained if the theory is superseded by a new and more powerful theoretical systematisation. The clearest formulation of this reasoning is the *divide and impera* move proposed by Psillos, one of the only realist to provide an actually realisable procedure to identify the true constituents of a theory:

> How should realists circumscribe the truth-like constituents of past genuinely successful theories? I must first emphasise that we should really focus on the specific successes of certain theories, like the prediction by Fresnel's theory of diffraction that if an opaque disk intercepts the rays emitted by a light source, a bright spot will appear at the centre of its shadow [...]. Then we should ask the question: how were these successes brought about? In particular, which theoretical constituents made essential contribution to them? It is not, generally, the case that *no* theoretical constituents contribute to a theory successes. Similarly, it is not, generally, the case that *all* theoretical constituents contribute (or contribute equally) to the empirical success of a theory. (What, for instance, was the relevant contribution of Newton's claim that the centre of mass of the universe is at absolute rest?) Theoretical constituents which make essential contributions to successes are those that have

[1] Note that approximate and partial truth are two independent claims: one can claim that a theory is approximately true but not partially true, and *vice versa*. But this two claims can also be combined, if one claims that some parts of a theory are just *approximately true* and other false.

> an indispensable role in their generation. They are those which really "fuel the derivation". (Psillos 1999, 110)

One should emphasize that it is very important, for scientific realism, to define precisely partial truth, and that the best way to do it is, as Psillos proposes, to give a procedure to "circumscribe" indispensable theoretical constituent for the predictive success of a theory. Such a procedure may help us not only to defend scientific realism, but also to discriminate amongst different versions of scientific realism, which disagree on the truth-like aspects of theories, such as *entities realism* and *structural realism*.

If we take Psillos' *divide et impera* move, which rely on indispensable constituents of predictive success to define partial truth, to be a good strategy for realists, then we can assume that most realists are committed to the following indispensability argument:

1. Predictive success should be explained by (approximate) truth and retained in theory change.

2. The constituent H of a scientific theory T is indispensable to its predictive success.

3. Therefore: H is (approximately) true and retained in theory-change.

Naturally, the following issue is: what is it to be indispensable? Here again, Psillos do not avoid the problem:

> When does a theoretical constituent H indispensably contribute to the generation of, say, a successful prediction? Suppose that H together with another set of hypotheses H' (and some auxiliaries A) entails a prediction P. H indispensably contributes to the generation of P if H' and A alone connot yield P and no other available hypothesis H^* which is consistent with H' and A can replace H without loss in the relevant derivation of P.(Psillos 1999, 110)

In other words, H is indispensable if and only if

$$(H, H', A \models P) \wedge [\nexists H^*((H^*, H', A \not\models P)]$$

Once we have define indispensability, it is easy to generalise to the notion of *class of indispensability* of the prediction P, for the theory T. $I_{P,T}$ is the class of indispensability of the prediction of P for the theory T means that

$$I_{P,T} = \{H|(H, H', A \models P) \wedge [\nexists H^*((H^*, H', A \not\models P)]\}$$

This logical formalism should not be used to undermine Psillos' argument by using a logical procedure of elimination of theoretical terms, such as Craig-transform or Ramseyfication:

> Clearly, there are senses in which all theoretical assertions are eliminable, if, for instance, we take the Craig-transform of a theory, or if we "cook-up" a hypothesis H^* by writing P into it. But if we impose some natural epistemic constraints on the potential replacement – if, for instance, we require that the replacement be independently motivated, non *ad hoc*, potentially explanatory, etc. – then it is not certain at all that a suitable replacement can always be found. (Psillos 1999, 110)

However, I will maintain in section 1 and 2 that, even if we restrict ourselves to these "natural epistemic constraints", a careful case-study shows that classes of indispensability are too liberal for the realist and not uniquely defined. I will then raise, in section 3, some skepticism about partial truth and theory-change, and conclude, in section 4, by analysing other defences of scientific realism based on Ian Hacking's manipulability argument.

1 First problem:prediction, fictions and asymptotic idealizations

1.1 The bright spot phenomenon

It seems to me that Psillos' exigence of a careful case-study is a reasonable one, and we shall examine here the paradigmatic case he suggests: Poisson's white spot.

To summarize quickly this coloured episode of the history of science (which has been told in detail and de-romanticized by John Worrall in (Worrall 1989a)), in 1818 Fresnel's *Mémoire sur la diffraction* was presented at the Académie des science of Paris. In the *Rapport sur le Mémoire de M. Fresnel* written by Arago, it is mentioned that one of the member of the jury, Poisson, proved that one of the consequences of Fresnel's should be the presence of a bright spot in the shadow of circular objects. Arago successfully performed the experiment (*cf.* figure 1) and confirmed the novel prediction. As Worrall suggested, this discovery may not have been an important confirmatory weight, but it is often referred (notably by Worrall himself in (Worrall 1989b)) as a "no-miracle" novel prediction.

The reception of this successful prediction, and the reformulation of wave optics by Maxwell, have been extensively studied, but the details of its *derivation* (necessary to circumscribe its indispensability class) have not been sufficiently scrutinised. I shall look

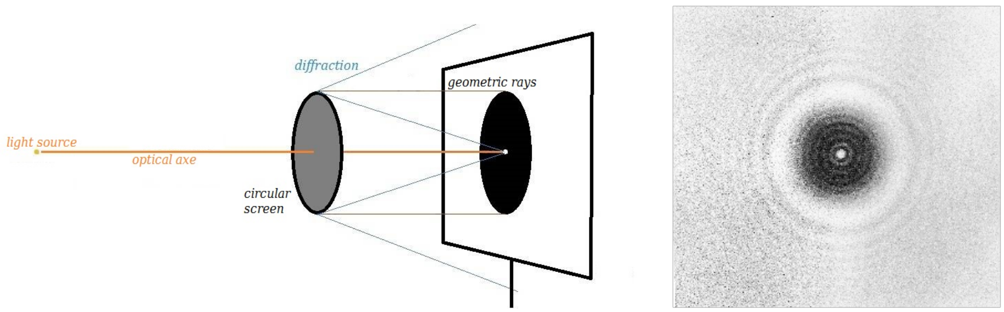

Figure 1: on the left: schematic experimental apparatus of the bright spot. On the right: the shadow of a ball observed in the condition of Fresnel's diffraction. Source: (Moeller 2007, 134)

only to the predictions that have been effectively proposed at that time, which surely satisfy Psillos' "natural epistemic constraints."

1.2 Fresnel's proof

The only "proof" of the prediction of the white spot that Fresnel gives, in a note following his *Mémoire*, can be quite surprising for the modern reader, but it is perfectly sound. Fresnel begins by building a geometrical model of the incident wave when it is diffracted by the circular object[2]:

> Let's divide the surface of the incident wave into an indefinite sequence of rings. [...] Each of which contains the same number of little rings of the same area, their rays differing only by half a wave. (Fresnel 1866, 368)

The mention of "absolute speed" refers to the particles of ether, believed to be moved by the propagation of light, before returning to a state of absolute rest. In Fresnel's theory of light, the intensity of light is defined as proportional to the square of the absolute velocity of these ethereal particles. In Fresnel's model, the vibration of ethereal particles are "destroyed" by interferences with the rays of the surrounding rings, which have been diffracted with a difference of half an oscillation.

> Therefore, we can consider all the rays coming from a ring as destroyed by half of the vibrations of the adjacent rings, except for the rays of the ring on the edge of the screen and of the extreme ring, which keep half of their absolute speed. But the rays of the extreme ring can be considered as nothing,

[2]I'm responsible for the translation of all Fresnel's quotations

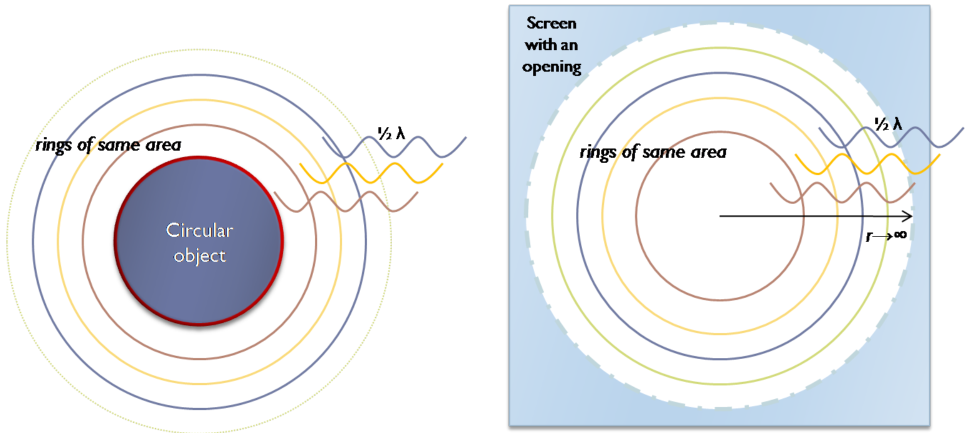

Figure 2: on the left, the circular object case. Each ring around the disk has the same area and contains rays differing from half an oscillation with the rays of the surrounding rings. On the right, the circular aperture case. Similarly, inside the aperture, each ring has the same area.

> because of their large obliquity; so that only half of the vibrations of the rays of the ring contiguous to the screen. (Fresnel 1866, 368)

Here appear a theoretical idealization necessary to Fresnel's proof: far away parts of the incident wave "can be considered as nothing", *i.e.* are not diffracted by the circular object and do not contribute to the bright spot. This idealization is essential because it allows Fresnel a comparison with another theoretical model built to compute the intensity of light diffracted by a circular aperture:

> Now, this ring has the same area than a small ring inside an indefinitely large opening. In that case, the center of the shadow of a circular screen must be illuminated as if there was no screen.

The two situation are equivalent, and identical (for the center of the shadow or of the light on the screen) to the absence of any obstacle (see figure 2 for an illustration). It is a real prediction by any standard, because it use no fact already known to fix free parameters of the model, and could have led to the theoretical discovery of the bright spot, if Poisson had not did it before.

What is indispensable in this prediction ?

1. The definition of light rays as waves and the (implicit) use of Huygens principle (diffraction and interference)

2. The relation $I \propto |v|^2$ (the square of the absolute velocity of ethereal particle)

3. The idealization of a perfectly circular object (Galilean idealization).
4. The idealization of an indefinitely large opening divided in same areas rings (asymptotic idealization)

The indispensable element (1) is fundamental, because it is exactly what realist would like to keep: the idea that light has wave behaviour. But the other elements are problematic.

The definition (2) is problematic because it seems to indicate that a reference to the absolute speed of ethereal particle are indispensable to this novel prediction. We will see that it is not the case.

Idealization (3) is a mathematical simplification. MacMullin called this kind of idealizations, "Galilean idealizations" (McMullin 1985), and argued that they can be "de-idealized", *i.e.* that they are mere practical simplification which can be eliminated with a more complete mathematical treatment of the physical system.

But even if we grant that to MacMullin (which is not sure, because the mathematics to compute a more complex case might not have been available at the beginning of the XIXth century), assumption (4) is of a different nature. It is what Batterman has called an "asymptotic idealizations" (Batterman 2010). Contrary to Galilean idealizations, this kind of idealizations is necessary to the explanation and prediction of phenomena, they do not represent anything physical (they are genuine fictions), and there is no hope to de-idealized them.

False assumptions and idealizations seem indispensable to the predictive success of Fresnel's theory. Indispensability is too liberal to be a definition of partial truth: if we trust the indispensability argument, it leads us to identify fictional simplifications and idealization as true parts of the theory.

1.3 Central and auxiliary hypotheses

Psillos would probably answer that definition (2) and idealizations (3) and (4) are *not* fundamental hypotheses of Fresnel's theory but auxiliaries hypotheses (belonging to the class symbolized by A in his formalisation). But such an answer is not very satisfying.

If we presuppose that there is a difference between central and auxiliaries hypotheses of a theory, and if we use this distinction to discriminate between true and false parts of a theory, we have just postpone the problem and not solve it. The realist has now to provide an independent criterion to distinguish central and auxiliaries hypotheses.

Clearly, the realist cannot define fundamental hypotheses as the ones we believe to be true and are still part of our present scientific image: it wouldn't be an independent but an *ad hoc* criteria.

A more subtle argument would be to qualify idealizations (3) and (4) as auxiliaries hypotheses because they are not part of Fresnel's optics. They seem to describe the experimental apparatus in question, and do not appear in the formulation of the theory. This option may help get rid of the innocuous idealization (3), but not of idealization (4). This one is in fact equivalent to the idea that the parts of an incident wave at infinite are not diffracted by obstacles. This is an essential assumption to apply Huygens Principle to real physical system, and deduce from it patterns of diffraction. It should be considered as a theoretical hypothesis as much as Huygens' Principle or Fresnel's integrals.

1.4 Poisson's proof

However, when philosophers talk about the prediction of the bright spot, they do not refer to this proof by Fresnel, but to Poisson's proof. We do not have documents clearly testifying how Poisson did it, but what is clear is that he started with Fresnel's integrals for the amplitude u of a wave of light, and apply it to the circular screen case (*cf.* figure 3):

$$u = A2\pi\rho_0 \int_{\sqrt{(a^2+\rho_0^2)}}^{\infty} (\frac{1}{\rho^2})e^{ik2\rho}d\rho \tag{1}$$

Where u is the amplitude of the wave at the center of the shadow, A the amplitude of the incident wave at the source, ρ, ρ_0 and a dimensions of the apparatus. Here is the derivation given in a classical textbook of optics (Moeller 2007, 133). Integration by parts of (1) yields:

$$u = \left[(\frac{1}{\rho^2})(\frac{1}{2ik})e^{ik2\rho} \right]_{\sqrt{(a^2+\rho_0^2)}}^{\infty} + (1/ik) \int_{\sqrt{(a^2+\rho_0^2)}}^{\infty} (\frac{1}{\rho^3})e^{ik2\rho}d\rho \tag{2}$$

Neglecting the right part as null

$$u = \left[(\frac{1}{\rho^2})(\frac{1}{2ik})e^{ik2\rho} \right]_{\sqrt{(a^2+\rho_0^2)}}^{\infty} \tag{3}$$

To compute the intensity, we use the relation between amplitude and intensity:

$$u^2 = I = (A^2 \frac{\rho_0^2}{(a^2+\rho_0^2)})(\frac{\lambda^2}{4}) \tag{4}$$

$$I \propto \frac{\lambda^2}{4} \tag{5}$$

This is what was supposed to be proved because it corresponds to the intensity in the absence of any obstacle.

At first sight, it seems that we are in a much better place than with Fresnel's proof,

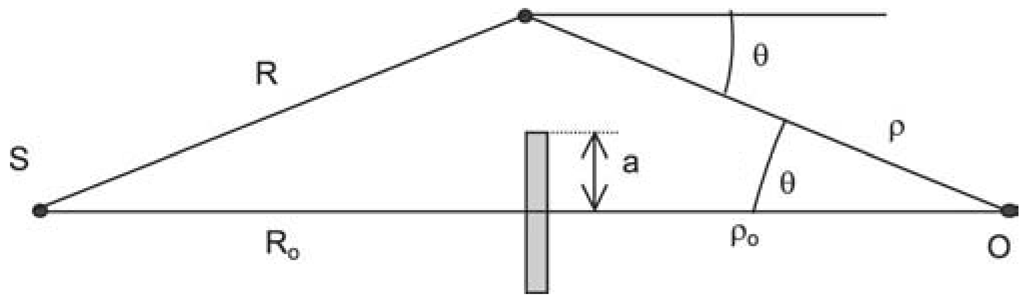

Figure 3: Application of Fresnel's integral to the circular screen case. Source: (Moeller 2007, 133)

because no reference to aethereal particles or whatsoever is needed. To compute the intensity at the center of the shadow form Fresnel's integrals, all the physicist needs is a mathematical relation between intensity and another magnitude. This seems to plead in favour of epistemic structural realism: we may not know anything more of this magnitude than its relation to the observable quantity I and its relations to other theoretical constituents through Fresnel's equation, but we do know that these relations are true [3].

But it is not all that is needed to derive this prediction. What are its indispensable theoretical constituents?

1. Huygens principle and Fresnel's integrals
2. The definition of $I = u^2$
3. The idealization of a perfectly circular object
4. The mathematical simplification of the integration by parts between (2) and (3)
5. The idealization of rays infinitely far from the circular object with a null contribution, necessary to resolve (3)

While idealization (3) is still innocuous for realist's claims, idealizations and mathematical simplifications (4) and (5) are asymptotic idealizations, as dangerous for the realist's concern than they were in the case of Fresnel's proof[4].

[3] This seems to me what is argued in (Worrall 1989b)

[4] Moreover, we know Fresnel's integrals to be true only if $\frac{a^2}{\rho_0 \lambda} 1$ Otherwise, if $\frac{a^2}{\rho_0 \lambda} \ll 1$ it is a case of Fraunhoffer diffraction. This prediction seem then to gives the flank to a cartwrightian criticism of the truth of Fresnel's integral, based on the *ceteris paribus* clause necessary to its application.

2 Second problem : predictive indispensability is not uniquely defined

The fact that two predictions of the bright spot phenomenon are possible in the framework of wave optics raises another problem for the realist: which part of this theory should be considered as indispensable and therefore true if we have two different way of predicting the same phenomena?

The problem is important, because, as we have seen, Fresnel "propose[s] to give the simplest solution to this problem [the intensity at the center of a circular shadow] *without using the integrals which helped me in the preceding Mémoire to compute other diffraction phenomena.*" (Fresnel 1866, 368)

Now, Fresnel's integral are exactly what are said to be fundamental constituents of his theory, constituents which have been retained through scientific change and still believed to be true. Does it mean that the *divida et impero* move leads to the disqualification of Fresnel's equation as true parts of his theory?

I examine in this section several possible answers to this problem.

2.1 Answer 1: minimize the problem

A realist could argue that the bright spot prediction is just a single case, and that most of predictive successes do not have several proofs.

I think it is quite the opposite: given a theory, it is frequent that several derivations of the same predictions are possible, just as several proofs of a theorem are possible.

And if an important result, such as a successful novel prediction, can be obtained by only one way, we should expect scientists to find other ways to prove it. It is often regarded as a virtue of a theory, because if there are several ways that lead to the same result, it proves that it is a *natural consequence* of the theory, and that this prediction is *robust.*

We often lack the ability to see these multiple paths leading to the same prediction, because, often, only specialists know how to derive correctly a specific result. But if you look closely at a scientific field you know in detail, probably many example will come to your head. Here are some famous examples in the field of astronomy and astrophysics:

- The prediction of Kepler's laws from Newton's Dynamics can be made "traditionally" (see (Tennant 2012) for an example) or by using Noether famous theorems.
- The prediction that the Moon will escape Earth's gravitational field can be made with two different sets of premisses using different frames of reference.

- The prediction of the recession of galaxies depending on their distance is a consequence of Einstein's General Relativity, but it may be predicted as an effect of the metric (De Sitter effect) or by the assumption of an expanding space-time.

2.2 Answer 2: combine the classes of indispensability

Let's call $I_{P,T}$ (respectively $I'_{P,T}$) the class of indispensability of Fresnel's (respectively Poisson's) proof. One could argue that only the *common* elements of these two classes are *really* indispensable to the prediction of the bright spot, and that the real class of indispensability of this prediction (say, $\mathbf{I}_{P,T}$) should be: $\mathbf{I}_{P,T} = I_{P,T} \cap I'_{P,T}$. The problem is that this drastic view is even more deflationary than the one proposed in (Saatsi 2005)), and yet include problematic constituents:

1. Something about intensity being proportional to the light wave amplitude
2. Galilean idealizations
3. Asymptotic idealizations

It seems to me that no realist would like to go down this road.

2.3 Answer 3: give privilege to Poisson's proof

A more subtle answer could be to argue that the existence of two independent ways of predictiong the bright spot does not threat the indispensability of Fresnel's integrals.

Indeed, in Psillos' terminology, to prove the dispensability of these integrals, we should find another hypothesis H^* which entail the prediction of the bright spot P *with the help of the same hypotheses* H' *and* A. Yet, in Fresnel's proof we do not find *exactly* the same hypotheses and auxiliaries than in Poisson's proof.

But this is just to postpone the problem. Because the question now becomes: why should we trust more Poisson's proof than Fresnel's proof to indicate us which parts of Fresnel's theory is true?

Fresnel's integrals are retained in theory-change (deducible from Maxwell's equations and Green's theorem), but we cannot appeal to that fact in order to prove that the privilege should be given to Poisson's proof. It would again be an *ad hoc* manoeuvre, because what we try to prove is precisely that theoretical elements which are indispensable to predictive success are the one which are retained in theory-change and worthy of belief.

It is difficult to find an independent criterion which give the precedence to Poisson's proof. It is not more precise, nor shorter, nor simpler than Fresnel's proof.

However, one line of reasoning is still open to the realist, but to my knowledge it has never been defended. It could be argued that Poisson's proof is superior to Fresnel's one, because it starts from Fresnel's integrals and that these equations are responsible for the rest of the predictive capacity of Fresnel's optics.

In that case, the criteria for partial truth is not predictive indispensability, but predictive centrality; and it implies a new procedure to define partial truth, which includes not only specific case-study but the definition and examination of the notion of the predictive capacity of a theory.

2.4 Partial conclusion: the path from theory to data

The two problems listed in the preceding sections have the same root: prediction-making is not a uniquely logically defined process.

Most philosophers acknowledge that picturing the relation between a theory and a predicted fact as a mere deduction from general laws and initial conditions, or as direct consequences of this theory, is an oversimplification.

Yet we still speak of "novel predictions", "predictive capacity" and "predictive indispensability" as if it was the case.

In reality, prediction is as much a business of deducing and computing as a tack of model-building with a theoretical toolbox, by choosing the right auxiliary hypotheses, boundary conditions and approximations. To my knowledge, one of the only philosopher who has focused on that problem is Ronald Laymon, who called the process of prediction-making "the hierarchical counterfactual road from theory to data" (Laymon 1982), because these auxiliary hypotheses and boundary conditions could be formulated, for him, as counterfactual statements.

If we change our way of picturing prediction-making, the two problems seem obvious. The indispensability class of a prediction almost always includes idealizations and fictions amongst its auxiliaries hypotheses, because these hypotheses are stated as counterfactual conditions. And indispensability classes are not uniquely defined, because choosing different auxiliary hypotheses in a theoretical toolbox may lead to different proofs of the same prediction.

3 Skeptical realism

My criticism against indispensability arguments must not be understood as a blunt rejection of scientific realism. It is only an examination of where we can go with the no-miracle argument based on predictive success. It leads to distinguish to kinds of use of predictive successes:

1. The first one is the use of the no-miracle argument to circumscribe a set of "mature" theories which can escape the pessimistic induction.

2. The second one is the use of the no-miracle argument to circumscribe, through an indispensability argument, a set of true aspects or constituents in these mature theories.

The two problems raised in the precedent sections show that this second use may not be legitimate. They do not attack the soundness of the first use. It means that the no-miracle argument can grant approximate truth to theories which have enjoyed predictive success and retained past theories successes, but is not able to determine which aspects of these theories are true or what is their distance to truth.

This is the position I have called "skeptical realism", in the hope that the name does not already coins one of the many versions of scientific realism[5]. It is actually not so much a new version of realism than a way to look at the realist/anti-realist debate, and search for a neutralist middle ground.

Another independent argument supports skeptical realism: the unpredictability of the evolution of science.

For realists, predictive success is retained (as much as possible) and increased through theory-change, and the aspects indispensable to predictive successes are retained and worthy of beliefs.

But one of the implicit premise of this argument does not hold: a new theory can enjoy the predictive success of a past one without retaining the same constituents. If different ways to predict a same phenomenon are available in the old theory, surely the new one can propose its own prediction, which will include different constituents than the previous theory. Predictive capacity may increase through scientific revolutions without continuity of structures, properties or entities.

A realist may argue that even if the constituents of the new theory are different from the constituents of the old theory, they might not be completely different, which explains why the old theory enjoyed predictive success. Explaining the success of past theories is, indeed, a requirement for any new theory. But it does not mean that the constituents of new theories could not be completely different, and even contradictory, with the constituents of the old theory.

If we could determine which aspects of theories are true and are to be retained in future theories, then theory-change could be partially predictable. But, if some laws seem to predict fairly technological evolutions (such as Moore law), no law or theory has ever been able to predict scientific change. As stated by the physicist Steven Weinberg:

> It is foolhardy to assume that one knows even the terms in which a future final theory will be formulated. Richard Feynman once complained that jour-

[5] If it so, "holistic realism" could be another denotation, but it seem to imply that *all* constituent of a theory are true.

> nalists ask about future theories in terms of the ultimate particle of matter or the final unification of all the forces, although in fact we have no idea whether these are the right questions. (Weinberg 1992)

It is possible, and common, to be confident in the objectivity of modern science theories – as Weinberg certainly is! – and yet to acknowledge ignorance about what are going to be the terms of future theories and their metaphysics implications. Skeptical realism tries to capture this stance, frequent among scientists.

Skeptical realism has some deep metaphysical implications, because ontologies of scientific theories should be seen as hypothetical and revisable theories granting existence to theoretical theories which may turn out to be rejected in future theories. But I will not explore these consequence here for lack of time and space.

4 Conclusion: manipulability and entities realism

As said before, skeptical realism is not supposed to be the final word on scientific realism, but the final frontier of the no-miracle argument. If one want to assert the existence of certain entities or structure, it requires another argument.

One possibility, chosen for example by Anjan Chakravartty (Chakravartty 2007, 45), is to use the "manipulability argument" of Ian Hacking:

> Experimenting on an entity does not commit you to believing that it exists. Only manipulating an entity, in order to experiment on something else, need do that. (Hacking 1983, 263)

This argument is independent from the no-miracle argument, and implies belief into entities. It is a good way out from skeptical realism, at least for two reasons.

The first one is explicit (but often misunderstood): only the entities used as *tools*, and not the objects of experiment, are believed to be true, because manipulating a tool requires to believe in its causal powers. If one believes in the causal power of a class of entities, they are "no longer ways of organizing our thought or saving the phenomena that have been observed. They are ways of creating phenomena in some other domain of nature." (Hacking 1983, 263) So, the manipulability argument is also an argument for the belief in the causal power of entities.

The second reason why Hacking's argument is a good one, is that the belief in entities implied by manipulability is not dependant from a theoretical paradigm, and therefore insensible to their incommensurability.

> Even people in a team, who work on different parts of the same large experiment, may hold different and mutually incompatible accounts of electrons.

> [...] You may choose someone with a foreign training, and whose talk is nigh incommensurable with yours, just to get people who can produce the effects you want. (Hacking 1983, 264)

Manipulability argument operating at the experimental level, it selects entities which are not likely to be forgotten or denied in the next paradigm-shift. Unlike the no-miracle argument, it gives you both truth of entities *and* their resistance to theory-change.

However, it should be emphasized that the manipulability argument is a double-edged sword, and does not come for free.

First of all, as quoted before, when Hacking talks about the manipulation of electrons in order to experiment on something else, he talks about the "creation of phenomena", and it is no metaphor. For Hacking, an experimental effect "does not exist outside of certain kinds of apparatus. [...] The effect can only be embodied by such devices." (Hacking 1983, 226)

This is precisely why manipulation has the power to makes us believe in something we did not believed before: if one does not think that the phenomenon did not existed before your intervention, she or he has no reason to believe that it is a true entity with true causal power. If the phenomenon is just "revealed" by the apparatus, then the belief in the causal powers of the entities used as tool are not required. The idea that phenomena are created in laboratories, originating in the works of Bachelard (Bachelard 1938, 72) under the name of "*phenoménotechnique*", is an interesting one, but it is often rejected by most philosophers of science, and especially realists.

Secondly, the manipulability argument can make us believe only in *experimental entities*, and not *theoretical entities*. It cannot be used to prove the approximate truth of a theory, or of some of its constituents, which are not used in laboratories. One might wonder, then, what become of observational sciences such as archaeology, geomorphology, astrophysics, cosmology, and so on[6]. Moreover, most of the times, scientific realism means realism toward scientific *theories*, but Hacking's argument is directed only toward scientific experimentation.

If one wants to marry the no-miracle argument with the manipulability argument, she or he should be aware of these points, because they may be strong grounds for divorce.

Références

Bachelard, G. (1938). *La formation de l'esprit scientifique : contribution à une psychanalyse de la connaissance objective*. Vrin.

[6]Note that Hacking closes its book on "a certain scepticism, about, say, black holes", even if he admits that "the experimental argument for realism does not say that only experimenter's object exist." (Hacking 1983, 275).

Batterman, R. (2010). On the explanatory role of mathematics in empirical science. *The British journal for the Philosophy of Science 61*(1), 1–25.

Chakravartty, A. (2007). *A Metaphysics for Scientific Realism : Knowing the Unobservable*. Cambridge University Press.

Duhem, P. (1914). *La Théorie physique : son objet, sa structure*. Vrin.

Fresnel, A. (1866). *Oeuvres complÃ¨tes d'Augustin Fresnel*. Paris : Imprimerie ImpÃ©riale.

Hacking, I. (1983, November). *Representing and Intervening : Introductory Topics in the Philosophy of Natural Science* (First Edition ed.). Cambridge University Press.

Laudan, L. (1981). A confutation of convergent realism. *Philosophy of Science 48*(1), 19–ÂÂ49.

Laymon, R. (1982). Scientific realism and the hierarchical counterfactual path from data to theory. In *PSA : proceedings of the biennial meeting of the Philosophy of Science Association*, pp. 107–121.

Leplin, J. (1997). *A Novel Defense of Scientific Realism*. Oxford University Press.

McMullin, E. (1985). Galilean idealization. *Studies in History and Philosophy of Science Part A 16*(3), 247–273.

Moeller, K. (2007). *Optics : Learning by Computing, with Examples Using Maple, MathCad, Matlab, Mathematica, and Maple*. Undergraduate texts in contemporary physics. Springer.

Musgrave, A. (1988). The ultimate argument for scientific realism. In *Relativism and realism in science*, pp. 229–252. Springer.

Psillos, S. (1999). *Scientific realism : How science tracks truth*. Routledge.

Putnam, H. (1975). *Philosophical Papers : Mathematics, matter and method*. CUP Archive.

Saatsi, J. (2005). Reconsidering the Fresnel-ÂÂMaxwell theory shift : How the realist can have her cake and EAT it too. *Studies in History and Philosophy of Science Part A 36*(3), 509–ÂÂ538.

Stanford, K. (2000). An antirealist explanation of the success of science. *Philosophy of Science*, 266–284.

Tennant, N. (2012). The logical structure of scientific explanation and prediction : Planetary orbits in a sun's gravitational field. *Studia Logica*.

Weinberg, S. (1992). *Dreams of a final theory*. Pantheon Books.

Worrall, J. (1989a). Fresnel, poisson and the white spot : the role of successful predictions in the acceptance of scientific theories. In D. Gooding, T. Pinch, and S. Schaffer (Eds.), *The Uses of Experiment, Studies in the natural sciences*, pp. 135–157. Cambridge University Press.

Worrall, J. (1989b). Structural realism : The best of both worlds ? *Dialectica 43*(1-2), 99–ÂÂ124.