Can van Fraassen's anti-realism give an account of novel predictions of unexpected phenomena?

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Abstract

In this paper it will be addressed the possibility of an account of novel predictions of unexpected phenomena by physical theories within van Fraassen's anti-realism. In particular it will be considered two important cases in modern physics, the prediction of the bending of light in Einstein's theory of gravity, and the prediction of antimatter in quantum electrodynamics. It will be defended that van Fraassen's anti-realism does not seem to enable an account of novel predictions.

1 Introduction

There is a well-known claim that Bas C. van Fraassen's anti-realism does not give an account of novel predictions of unexpected phenomena. In this work I will give a fresh look at this claim by considering novel predictions from a perspective related to intertheoretical relations.

In part 2, I will set the stage for the discussion by addressing Alan Musgrave and van Fraassen's views on the subject. In this section I will develop a characterization of novel predictions by taking into account intertheoretical relations. In part 3, I will present two 'case-studies': the prediction of the bending of light in Einstein's theory of gravity, and the prediction of anti-electrons in quantum electrodynamics. For these particular cases, I will defend the view that we can separate novel predictions in two groups: general and specific novel predictions, which enable to address novel predictions without the need for a detailed account of the relevant intertheoretical relations for each case. In part 4, by taking into account the previous sections, I will defend the view that van Fraassen faces problems to give an account of novel predictions of unexpected phenomena. In particular van Fraassen's anti-realism cannot give an account of novel predictions even in a limited relative way, by relating a general novel prediction made within different theoretical frameworks to a derivation made using a particular theory.

2 On Musgrave and van Fraassen's views

Regarding the predictive success of physical theories, Musgrave stresses the need to distinguish between a theory's capacity to accommodate known phenomena from the prediction of unknown phenomena (Musgrave 1985, 210-11). Thus, Babylonian astronomy was capable of predicting lunar eclipses with a high accuracy, as it did Ptolemaic astronomy (Musgrave 1988, 231). Nowadays we do not consider these as examples of scientific theories, they are seen more as mathematical models that were able to accommodate observed regularities.

A completely different situation occurs, according to Musgrave, when a new theory predicts new (yet not observed an unexpected) phenomena. Musgrave thinks that realism can give a good account of this, and thinks that anti-realist accounts, in particular van Fraassen's, cannot. In Musgrave's view, when van Fraassen addresses this subject in *The Scientific Image*, he only provides a Darwinian metaphor for making sense of maintaining empirically adequate theories: only successful theories survive (van Fraassen 1980, 39-40; Musgrave 1985, 210).[1] Musgrave criticizes this argument, since in his view 'to say that only successful theories are allowed to survive is not to explain why any particular theory is successful' (Musgrave 1985, 210). That is, according to Musgrave, van Fraassen does not give an account of the prediction of unexpected phenomena by a new theory.

In 2006 van Fraassen provided a further remark that is related to the so-called no-miracles argument, while not addressing explicitly the ‘problem’ of novel predictions:[2] 'the success of science is *not a miracle*, because in any theoretical change both the past empirical success retained and new empirical successes *were needed as credentials* for acceptance' (van Fraassen 2006, 298-9). Van Fraassen made this remark in the context of rebuffing structural realism’s claim regarding a possible structural continuity between an old theory and a new superseding theory.[3] To van Fraassen there is no need for a putative structural continuity to cover the continuity in empirical adequacy, it is only necessary that the new theory implies approximately ‘the same predictions for the circumstances in which the older theories were confirmed and found adequately applicable’ (van Fraassen 2006, 298), i.e., the new theory must duplicate the empirical success of the past theory (or equivalently it must retain/accumulate the past empirical knowledge or empirical structure; see van Fraassen 2006, 298-301). I doubt that Musgrave would accept this ‘newer’ remark as providing an account of how new theories are able to predict unexpected phenomena. Van Fraassen's comments give no indication to a possible answer. In fact they can be seen as simply referring to the methodology adopted, according to some, in scientific research, i.e. the matching and superseding of the empirical adequacy of an older theory by a new theory is a *sine qua non* condition for the acceptance of the new theory.

In Musgrave's view there are still some loss-ends in framing beyond an intuitive level a relevant notion of novel prediction: 'Difficulties remain, of course, not least that of making precise the intuitive distinction between known effects and novel predictions' (Musgrave 1985, 211). Van Fraassen develops a related point by picking up William Whewell's notion of independence: the prediction of unexpected phenomena can be taken to be independent support for a new theory. In van Fraassen's view the notion of independence must be related to the existing theories and experimental knowledge at the time the new theory was being developed; accordingly, 'the old charge of arbitrariness lurks nearby, of course, for this is to some extent a matter of historical accident' (van

1 Let us recall that in van Fraassen’s view only empirical adequacy (i.e. saving the phenomena) counts as criteria for acceptance of a theory (see, e.g., van Fraassen 1980, 12).

2 Musgrave and van Fraassen's arguments are made in the context of the so-called no-miracles argument. This is taken to be a positive general argument that says that only realism 'doesn't make the success of science a miracle' (Putnam 1975, 73). Arguments against and in favor of this argument have been made. According, for example, to Howson (2000, 52-4), Lipton (2005, 1267), and Magnus and Callender (2004, 323-29) the argument rests on a fallacy. However this view is made regarding only a probabilistic version of the argument. Another way of presenting the argument is as an inference to the best explanation. Here too we face a possible problem, now of circularity (Magnus and Callender 2004, 330-31; Newman 2010, 112). Again maybe this only shows that the argument should not be presented as an inference to the best explanation. A more sympathetic view regarding the no-miracles argument was given in Schurz (2009) defending a restricted form of the argument based on a proposed structural correspondence between a superseded theory and a new theory. I will not address here directly the no-miracles argument, only the question about van Fraassen's anti-realist eventual account of novel predictions.

3 It seems that van Fraassen sees the relation between the 'new' and 'old' theory in a way that has similarities with some realist's views in terms of theory reduction: the 'new' theory is completely superseding the 'old' theory (even if just in terms of empirical structure). For a different view on intertheoretical relations see Darrigol (2008).

Fraassen 1985, 267). This might create a doubt about to what point (or in what sense) are the prediction of unknown phenomena novel in a philosophically relevant way, or simply dependent of an historical cultural context that sees them that way and possibly, because of this, philosophically the prediction of unexpected phenomena might not be so important after all?

In my view the situation is not one of relativism or historical contingency as these previous views might led us to think; *in terms of intertheoretical relations it is possible to determine with clarity what are novel predictions*, which we can regard as constituting independent support for a new theory. Musgrave himself called the attention, in a later work, to the possibility of making a clear distinction between known predictions and novel predictions: 'a predicted fact is a novel fact for a theory if it was not used to construct that theory' (Musgrave 1988, 232). I do not think that this is the best option for framing a meaningful notion of novel prediction. In fact, is it not possible that there are predicted facts (of a new and old theories), not used in the construction of the new theory, that are not novel predictions? We would need to make an exhaustive list of all 'predicted facts' to disentangle this matter.

I think that a characterization of novel predictions based on comparing 'new' and 'old' theories (i.e. on intertheoretical relations) is less ambiguous, even if this characterization is to be applied case by case, after clarifying what are the relevant theories or theoretical approaches for each case. *This view agrees with van Fraassen's comment that we must take into account existing theories, but there is no historical contingency here. It is not important if a particular novel prediction is an historical accident; what matters is that in physics there are novel predictions, and this is not an historical contingency.*

In this way, I take the prediction of the bending of light in Einstein's gravitation theory to be a novel prediction (not known and not expected according to Newton's gravitation theory; the 'old' theory in this case); another example is the novel prediction of anti-electrons made in quantum electrodynamics (unexpected according to relativity theory and classical electrodynamics; the 'old' theories in this case). Now, van Fraassen seems to be facing a problem. Are not these novel predictions taken to be independent support for the corresponding physical theory? I think they are. They are not part of the phenomena saved by the older theory, neither they are part of the phenomena not saved by the old theory and expected to be saved by the new theory, i.e. they are an (eventual) independent support for the new theory. The theory is predicting unexpected regularities in nature. It will be empirically adequate only if observation and/or experimentation agree with the theory's novel predictions.

3 The distinction between general and specific novel predictions

3.1 Two case-studies: the bending of light and antimatter

Two well-known novel predictions in 20th century physics are the bending of light due to gravity and the ‘existence’ of antimatter.

According to Newtonian gravitation there is no bending of light due to gravity and there is no reason to expect such phenomenon. It is a novel prediction of Einstein’s theory of gravity which as been confirmed in 1919 (see, e.g., d'Inverno 1992, 199-201). Looking closely into the derivation of this prediction within Einstein’s theory of gravity we see that it is already derived using the so-called parametrized post-Newtonian (PPN) formalism, which contains the post-Newtonian approximation of different metric theories of gravity as a special case:[4] by choosing a particular set of parameters the PPN formalism gives the post-Newtonian limit for different theories of gravity (see, e.g., Misner, Thorne, and Wheeler 1973, 1069). In fact the bending of light is a very general prediction that can even be derived without a new theory of gravity, by considering a relativistic kinematical principle (the equivalence principle) that relates phenomena in a constant gravitational field observed from a inertial reference frame to phenomena observed in a constantly accelerated reference frame. With this approach it is already possible to predict the bending of light (even if this approach only predicts half of the deflection predicted by Einstein's theory of gravity; see, e.g., Callahan 2000, 191-195). The situation is then the following:

1) The bending of light can be predicted in the post-Newtonian approximation, not being necessary to take fully into account Einstein's field equations.

2) It can be derived by other rival theories (using an equivalent PPN approximation).

3) It can be derived by a general principle (the equivalence principle) defined within the 1905 theory of relativity.

We see that there is an 'atmosphere of generality' in all this; apparently it is not a prediction that comes out only of Einstein's field equations. I see the possibility for two excluding options here:[5]

1) Maintain the view that the bending of light is a novel prediction of Einstein's theory of gravity.

2) Do not regard the bending of light as a novel prediction of Einstein's theory of gravity, but as a prediction already made within the *different* relativity theory framework.

A realist would possibly choose option 1, since it would open the door to the possibility of showing that only Einstein's theory of gravity gives a consistent derivation of the bending of light, and that the equivalence principle derivation could in fact be seen as an approximation, or better a work in progress, that only makes sense from the perspective of the later developed theory. With this option the realist would possibly only have to confront the eventuality of the underdetermination of Einstein's theory of gravity (see, e.g., Lyre and Eynck 2003), which he might intent to undermine by defending a structural realism that might not be affect by the possible underdetermination of the theory.[6] The realist would certainly reject option 2 because it would open the door for a strong form of underdetermination in which *different* theoretical frameworks give the same novel predictions.

An anti-realist would reject option 1 or at least consider that even for structural

4 We can apply the post-Newtonian approximation to the case of a weak gravitational field and slow motions; this enables the description of gravitational phenomena in terms of second-order (post-Newtonian) corrections to the (first-order) Newtonian treatment.

5 I will not consider the case of alternative theories of gravity since, in the present time, there are no convincing rivals to Einstein's theory of gravity (see, e.g., Will 2006).

6 The possible underdetermination of physical theories is usually considered an important argument against realism (see, e.g., Ladyman 1998); there are authors that consider that structural realism avoids any possible underdetermination problem (see, e.g., French and Ladyman 2003; Cao 2003; Worrall 2009; Bain 2009).

realism there still would be a serious case of underdetermination (see, e.g., Lyre 2009). The best option for the anti-realist seems to be 2 since, as mentioned, it opens the door for a strong underdetermination of novel predictions between non-reducible theoretical frameworks. This could be presented as a negative argument against the realist claim that considers novel predictions as evidence for realism, but by itself is not a positive argument showing how, for example, van Fraassen's anti-realism can make sense of novel predictions.

Both the realist and the anti-realist would have to provide a conclusive argument to show the theoretical continuity or discontinuity between the theory of relativity and Einstein's theory of gravity (i.e. they must address questions regarding intertheoretical relations). There is an alternative to having to choose between options 1 and 2 that enables a correct characterization of the situation by stressing the *de facto* situation that there is a different degree of application of theories in novel predictions of unknown phenomena.

Instead of thinking in terms of novel predictions of one or the other theory one can think in terms of general novel predictions and specific novel predictions. In this way the bending of light can be seen as a general prediction that is made by Einstein's theory of gravity and that can also be made by the theory of relativity and a kinematical principle; and, for example, the prediction of black holes can be seen as a specific prediction of Einstein's theory of gravity since it arises only by consider an exact solution of Einstein's field equations (see, e.g., Ludvigsen 1999, 134-155). It is the general novel predictions that bring the possibility of a strong underdetermination of novel predictions.

To show that this way of addressing novel predictions in terms of general or specific predictions can be applied more generally let us consider another example where the differentiation makes sense.

As it is well-known, Paul Dirac, using his relativistic wave equation, predicted the 'existence' of antimatter, which was an unexpected phenomena according to classical theories and was confirmed in the early thirties (see, e.g., Schweber 1994, 66-9). Like in the case of the bending of light, we are facing here an 'atmosphere of generality' since this is not a specific prediction of quantum electrodynamics. In reality already the so-called Klein-Gordon equation predicted antimatter.[7] Dirac himself noticed that the prediction of antimatter followed from any relativistic wave equation (for charged particles), due to the fact that the relativistic energy-momentum equation has a quadratic form (Dirac 1930, 360). In this way, antimatter is not a novel prediction specific to quantum electrodynamics.

From a realist perspective this situation might seem to be unproblematic. According to Steven Weinberg, local quantum field theory (LQFT), with its prediction of negative and positive charged quanta – and quantum electrodynamics can be seen as a particular instantiation of LQFT –, is the only theoretical approach that satisfies three assumptions: the principle of Lorentz invariance (i.e. the theory of relativity), the quantum formalism, and the so-called cluster decomposition principle (Weinberg 1999). Jonathan Bain uses this result, even if it is not a strict theorem, to defend the impossibility of underdetermination of LQFT (Bain, 1999).

7 This equation was first developed by E. Schrödinger when looking for a wave equation to describe quantum phenomena. Using this wave equation Schrödinger determined the energy levels for the hydrogen atom, and arrived at a result that was not in agreement with Sommerfeld's result for the hydrogen spectrum (obtained within the so-called old quantum theory), and so he drooped it (Kragh 1981, 32-7). It was only in 1935 that the equation reappeared when it was shown that it could give a physical description of Mesons (Kragh 1984, 1031).

From an anti-realist perspective one might try to overturn the realist position by stressing the fact that more than a result that follows imperatively from a (quantum field) *theory*, it is a prediction that is made previous to quantum field *theory* proper (as the prediction of the bending of light can be made previous to Einstein's theory of gravity) by a *quantization* of any (*relativistic*) charged classical field, like for example the complex scalar field (see, e.g., Bogoliubov and Shirkov 1959, 35-8).

Again, like in the previous case, each option would need a clear account of intertheoretical relations, in this case between the theory of relativity, classical electrodynamics, quantum theory, and quantum electrodynamics. A simpler option is to frame the discussion in terms of general or specific novel predictions (which does not involve engaging into details of intertheoretical relations). Antimatter can be seen as a general prediction made for any (charged) quantum field, while, for example, the light-light scattering can be seen as a specific prediction of quantum electrodynamics that is not expected according to classical electrodynamics (Schweber 1962, 558-9).[8]

Summing up the results so far: the two novel predictions (the bending of light and antimatter) are both general predictions not specific to Einstein's theory of gravity and quantum electrodynamics. They can arise in one case from the theory of relativity and a kinematical principle (previous to any new theory of gravity), and in the other case from the quantization of any relativistic wave equation for charged matter. By considering general novel predictions we have a new perspective from which to discuss realist and anti-realist positions regarding novel predictions. One example is the possibility of a strong underdetermination of general predictions. However this is a negative argument against realism not a positive argument that makes sense of novel predictions from an anti-realist position. We will see in section 4 that *framing the discussion in terms of general novel predictions will help in revealing the weakness of van Fraassen's position*.

3.2 Kirchhoff's prediction of light diffraction as a general prediction

Saatsi and Vickers (2011) presented a case-study that in their view might undermine certain realist positions; it is based on Kirchhoff's derivation of a diffraction equation, within an ether paradigm of propagation of (scalar) light waves, for the case of light incident on a screen with a small aperture. This equation predicts – in similar lines to Fresnel's novel prediction (when compared with Newton's corpuscular theory of light) of a bright spot in the centre of the shadow of a small circular disk – a diffraction pattern in the intensity of light on the other side of the screen. Saatsi and Vickers call the attention to the fact that Kirchhoff's derivation relied on physically wrong assumptions (from the point of view of Maxwell's theory), which are even mathematically inconsistent:

> it turns out that Kirchhoff's derivation turns on crucial assumptions regarding the *amplitude* of light waves that (i) *differ considerably from the actual situation* (as described by Maxwell's equations, for example) in various respects, and (ii) as a matter of fact are *inconsistent* (Saatsi and Vickers 2011, 30);
>
> the 'success-fuelling' assumptions are radical wrong (Saatsi and Vickers 2011, 31).

8 In this paper I will not be making a case for a throughout characterization of novel predictions in physics in terms of general or specific novel predictions. I am only considering that it is meaningful for the two important cases being considered here (i.e. we can give clear-cut examples of general or specific novel predictions), and that this characterization is useful when approaching the issue of novel predictions by considering intertheoretical relations.

From this, in my view wrong, assessment of the situation Saatsi and Vickers go on to conclude that realists have a problem since the predictive success of the theory seems to depend on wrong assumptions (Saatsi and Vickers 2011, 42).[9]

As Saatsi and Vickers called the attention to, Kirchhoff's equation can be derived from a set of consistent assumptions within the theoretical framework where Kirchhoff develops his approach, that of a scalar wave theory based on the idea of an ether taken to be the bearer of light waves (Saatsi and Vickers 2011, 39). They go on to consider these two approaches, Kirchhoff's original one and the newer consistent approach, as different theories (Saatsi and Vickers 2011, 39), while they call the attention that these are not fundamental theories (Saatsi and Vickers 2011, 36). I would instead prefer to call them different models developed within the same theoretical framework.[10] By calling the models theories, Saatsi and Vickers can claim that we are facing a case of underdetermination.[11] Even conceding to Saatsi and Vickers that the two identical equations developed in the ether paradigm can be considered as two diffraction *theories*, it is still the case that, contrary to Saatsi and Vickers' claim, Kirchhoff's equation does not depend *crucially* on his wrong assumptions:

> all of the possible ways in which Kirchhoff's assumption (A1) differs from the truth (according to Maxwell's equations), it just so happens that the difference has negligible effect (Saatsi and Vickers 2011, 41).

That is, mathematically Kirchhoff's wrong assumptions do no affect the final form of the equation. Accordingly, Saatsi and Vickers consider that 'due to the nature of diffraction there is a many-to-one mapping, so to speak, from amplitude-distribution-at-the-aperture to diffraction patterns' (Saatsi and Vickers 2011, 43).

I think it is possible to relate this characterization of the situation regarding Kirchhoff's 'theory' to the one presented in section 3.1 regarding the bending of light and antimatter. From my perspective what Saatsi and Vickers call limited or local underdetermination – in the context of regarding as a theory Kirchhoff's derivation of an equation for the particular case of light incident on a screen with an aperture – can be rephrased by saying that Kirchhoff's prediction of a diffraction pattern is a general prediction made within an ether theoretical framework, and that (eventually) it can be made a case for a strong underdetermination of novel predictions due to this (since that, even if historically not exact, we can see Kirchhoff's result as part of the novel predictions made within the wave theoretical description of light in relation to the 'older' corpuscular theory of light). In my view this is where the (possible) cash-value for anti-realists is, not in the fact that Kirchhoff deduces his equation in part from wrong

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9 It is important to notice that Saatsi and Vickers are presenting a negative argument that might undermine (to some extent) realist positions. They do not explore the possibility of an anti-realist account of Kirchhoff's 'novel' prediction.

10 Looking into the details of both derivations, the main difference is that Kirchhoff makes a very idealized (and mathematically inconsistent) assumption regarding the amplitude of the light waves at the aperture. He considers that the screen does not perturb the light waves passing through the aperture, which is wrong from the point of view of Maxwell's theory (Saatsi and Vickers 2011, 34-6). A consistent formulation of Kirchhoff's approach is possible by making a more 'correct' assumption regarding the amplitude of the light waves at the aperture by taking into account the scattering of light off the edges of the aperture (Saatsi and Vickers 2011, 39).

11 Saatsi refers to it as 'local underdetermination' (Saatsi, 2010, 4) or 'limited underdetermination' (Saatsi and Vickers 2011, 43). I will defend below that, independently of regarding Kirchhoff's equation as a theory or not, we can relate Kirchhoff's prediction to what I called general predictions. In this way, when adopting an anti-realist position, it might be possible (with the appropriate argumentation in terms of intertheoretical relations that Saatsi does not provide) to defend the possibility of what I called strong underdetermination of novel predictions.

assumptions.

4 The problems related to van Fraassen's account of empirical success and intertheoretical relations

As we have seen, according to van Fraassen, a new successful theory is only related to the old theory in the most minimal way of duplicating the empirical structure of the old theory, i.e. it must be empirically adequate where the old theory was. This means that according to van Fraassen there is no requirement of theoretical continuityy between the theories (van Fraassen 2006, 299). In his words, 'the relation is much looser, but nevertheless very demanding. The difference is that it grants conceptual autonomy to the new theory, which is allowed to re-describe nature entirely in its own terms' (van Fraassen 2006, 299). If this is the case then anti-realists have an argument to claim the existence of a strong underdetermination of novel predictions, since it would not be possible to make sense of a novel prediction made within different theoretical frameworks by relating the different derivations to the derivation made with a particular theory. It does not give an anti-realist account of novel predictions but it would show that realists are not better off.

I have two reasons to doubt the strength of van Fraassen's position. Let us consider the first one. As already mentioned, van Fraassen thinks one can frame empirical success of past theories simply by calling the attention to the fact that they give approximately the same predictions as the new theories (van Fraassen 2006, 298). In fact van Fraassen goes to the point of saying that the empirical success of old theories, 'consisted in their success of fitting the data, the deliverances of experimental and observational experience' (van Fraassen 2006, 303). This is simply not the case because we know that, contrary for example to Babylonian astronomy, physical theories actually predict unexpected phenomena they were not intended to save. An old theory was in a time a new theory that made novel predictions that went beyond the experimental and observational experience available. Here is then the first problem for van Fraassen:

Problem 1: it seems that van Fraassen does not offer a convincing characterization of physical theories since, for example, it is not able to distinguish between Babylonian astronomy (without any novel prediction beyond the data it was intended to save) and Newton's theory of gravity (that for example 'predicted' a yet-unknown planet, Neptune; see, e.g., Hirst 1946).

Be it in his Darwinian account, or in his more recent views on empirical success, van Fraassen does not come face to face with the problem of novel predictions, only indirectly when addressing the issue of the independent support for a new theory. As mentioned above, this might be due to a confusing view, mixing the possible historical contingency of particular novel predictions with the fact that there are novel predictions (van Fraassen 1985, 267), i.e. that in physical theories, 'saving the phenomena' goes beyond the phenomena they were originally intended to save.

The second problem derives from van Fraassen's account of intertheoretical relations in terms of a loose relation between theories without any continuity at the theoretical level (van Fraassen 2006, 300).[12]

12 For example, according to van Fraassen 'there is no sense in which the models of Newtonian physics can in general be embedded in the models of [the special theory of relativity]. The transformation groups are too different' (van Fraassen 2006, 299).

If one accepts van Fraassen's account of intertheoretical relations we cannot even give a relative answer regarding a novel prediction made within a particular ('older') theoretical approach in terms of the relation that this might have to, for example, a superseding theory. That it, *at the same time that van Fraassen's views about intertheoretical relations bring the possibility of a strong underdetermination of novel predictions, it also makes impossible to van Fraassen to give a positive relative answer about novel predictions by relating a general novel prediction made within different theoretical frameworks to a derivation made using a particular theory*. So the second problem with van Fraassen views is:

Problem 2: How to understand a general novel prediction from the perspective of, for example, a superseding theory, since according to van Fraassen there is no continuity at a theoretical level?

This in not an irrelevant question, since when adopting van Fraassen's account one is unable, for example, to understand the prediction of the bending of light within the theory of relativity by reference to Einstein's theory of gravity; also one cannot make sense of the proliferation of predictions of antimatter. This is a strong evidence for the impossibility of giving an answer in absolute terms (for example by addressing the novelty of the bending of light within Einstein's theory of gravity).

We see that without going into the realist/anti-realist debate of what is the right view on intertheoretical relations for each of the cases that we have considered, *the characterization of novel predictions in terms of general or specific novel predictions gives us a valuable perspective from which to address van Fraassen's views: van Fraassen cannot provide an account of a general novel prediction by resort to a particular theory*.

The situation we face then is that it is not just that novel predictions seems to be merely blind luck from the perspective of van Fraassen's anti-realism, since as we have seen it is missing an account of them in van Fraassen's views on empirical success; it is a blind luck that repeats itself in several cases in the same way: it is not just that a particular prediction, that of the binding of light for example, is a mystery; the case is that, according to van Fraassen, the same novel prediction poops out from totally unrelated theoretical approaches without any reason for that.

5 Conclusion

According to the view developed in this paper, there are good reasons to doubt that van Fraassen's anti-realism might come up with an account of novel predictions. Van Fraassen's views on empirical success lack an account of novel predictions. Also, according to van Fraassen, in theoretical change there is only continuity in the most minimal sense of retaining and superseding past empirical knowledge. This makes it impossible to make sense of a general novel prediction by resort to a derivation made within a particular theory. That is, van Fraassen's anti-realism cannot make sense of novel predictions even in a limited relative way. Thus, I consider doubtful that van Fraassen's anti-realism might come out with a philosophical account of the *de facto* situation that physical theories actually predict unexpected phenomena in nature.

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