



Running Mice and Successful Theories: The Limitations of a Classical Analogy

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Abstract

Bas van Fraassen's Darwinian explanation for the success of science has sparked four decades of discussion, with scientific realists and antirealists alike using biologically inspired reasoning to support their points of view. Based on critical engagement with van Fraassen's proposal itself and later contributions by Stathis Psillos and K. Brad Wray, we claim that central arguments on both sides of this controversy suffer from an insufficient understanding of Darwinism and its underlying biological concepts. Adding the necessary biological background turns out to subvert the argumentative force of viewing the success of scientific theories as analogous to the behaviour of biotic entities. In conclusion, we sketch more productive ways of viewing the relationship between biology and scientific realism.

Keywords Scientific realism · Darwinism · Success of science · Antirealism

1 Introduction

There is a long tradition of exploring structural parallels between the historical development of scientific knowledge and the evolution of life on earth. Kuhn (1962) famously came up with a phylogenetic tree of science, Popper (1972) wrote a whole book about the evolutionary perspective on scientific knowledge and Hull (1974) and Sober (1993) have worked extensively on science as a phenomenon of cultural evolution. This paper takes a critical look at one particular strand of this tradition, which was initiated by van Fraassen (1980) and has since then been seeking to draw lessons from biological evolution for the debate on scientific realism.

In a brief but famous passage, van Fraassen (1980, 39–40) sketched a Darwinian argument against the scientific realist's claim that the only satisfactory explanation for the amazing success of science is the (approximate) truth of scientific theories. Instead of invoking

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approximate truth, van Fraassen suggested that the success of science can be explained by the Darwinian selection process that drives the development of scientific theories. Van Fraassen's Darwinian¹ explanation struck a chord within the realism debate. It has received wide opposition from realists (Boyd 1985; Kitcher 1993; Leplin 1997; Lipton 1991; Musgrave 1988; Park 2014; Psillos 1999, 2020) and a somewhat mixed response from fellow non-realists, some of whom have joined the realists in criticising van Fraassen's proposal (Kukla 1996; Stanford 2000, 2020), while others have developed responses to such criticism (Wray 2007, 2010, 2018, 2020).

Our aim in this paper is to demonstrate that central arguments on both sides of this controversy suffer from an insufficient understanding of Darwinism and its underlying biological concepts. The arguments seek to support philosophical claims by ways of reasoning which seem to be rooted in biology because they specifically address Darwinism and evolutionary concepts such as species, selection, survival, the genotype etc. But as we will show, these roots are too superficial, and a proper look at actual biological reasoning turns out to invalidate the arguments that were built upon them.

To keep the scope of our investigation manageable, we focus on van Fraassen's Darwinian explanation for the success of science, leaving aside other applications of Darwinism within the scientific realism debate. In particular, we cannot do full justice to the various points of contact between scientific realism and what is known as *evolutionary epistemology*. For example, we will not discuss attempts to draw support for scientific realism from the resources of evolutionary epistemology (see de Regt (1998) for a critical review; notice also van Fraassen's own dismissive attitude towards evolutionary epistemology (van Fraassen 1985, 260–263)). Likewise, we will not discuss general evolutionary arguments *against* scientific realism (see de Ray (2022) and references therein). Finally, we will not be concerned with what evolutionary epistemology can tell us about the aim of science (Rowbottom 2010).² This issue is irrelevant for our purpose, because discussing explanations for the success of science (as we will do in this paper) is entirely compatible with avoiding any talk about the aim of science (as Rowbottom (2014) advocates).

We will proceed as follows: To warm up and to eliminate possible sources of confusion for the subsequent discussion, Sect. 2 will be dedicated to clarifying some questionable points in van Fraassen's original proposal, some of which have gone unnoticed in the debate so far. We will then (in Sect. 3) turn to one of the most prominent realist responses to van Fraassen (due to Lipton and Psillos) and show how it rests on assumptions that are unjustified from a biological point of view. Things do not look much better on the antirealist side, as Sect. 4 will show: Wray's attempt to defend van Fraassen against realist criticism also

¹ Some authors prefer the term "selectionist explanation", which might be viewed as having a broader meaning than "Darwinian explanation". While the term "selectionist" most commonly refers to natural selection which is ultimately Darwinian, there is indeed a discussion as to what degree explanations of such phenomena as cultural or scientific evolution by selection should still be referred to as Darwinian (Hull 2001). As we will briefly discuss in Sect. 5, taking non-natural selection into account would constitute a step forward in this debate, but in the present paper we are concerned with those authors who use natural selection and other biological concepts to support their points of view.

² A superficial glance might suggest that this article is directly relevant for our discussion, because it starts by quoting the same passage from van Fraassen (1980) with which we are concerned in what follows. As it turns out, however, van Fraassen's Darwinian explanation plays no role in Rowbottom's subsequent discussion. This is actually not so surprising, given that the quoted passage doesn't speak about the aim of science at all. It is therefore somewhat misleading that Rowbottom (2010, 209) counts van Fraassen among those who "have used evolutionary analogies to defend their views on the aim of science".

turns out to pay insufficient attention to the details of evolutionary biology. This will lead us to the conclusion (in Sect. 5) that the analogy between biological phenomena and scientific theories has, despite four decades of extensive debate, so far failed to give us much insight concerning the explanationist defense of scientific realism. We therefore suggest that the debate should either be discontinued or at least be thoroughly reoriented.

2 The Intentional Mouse

As part of his critique of scientific realism, van Fraassen (1980, 39–40) briefly discusses what he calls “the Ultimate Argument”, which is now better known as the No-Miracles Argument. He construes it as the realist’s response to a “demand for a scientific explanation of the success of science” (van Fraassen 1980, 39). The explanation given by the realist in response to this demand is in terms of approximate truth, understood as “the ‘adequacy’ of the theory to its objects, a kind of mirroring of the structure of things by the structure of ideas” (van Fraassen 1980, 39). It is in this context that van Fraassen introduces Darwinism to the debate of scientific realism by offering an elegant and seemingly simple alternative explanation for the success of science:

I would like to point out that science is a biological phenomenon, an activity by one kind of organism which facilitates its interaction with the environment. And this makes me think that a very different kind of scientific explanation is required.

I can best make the point by contrasting two accounts of the mouse who runs from its enemy, the cat. St. Augustine already remarked on this phenomenon, and provided an intentional explanation: the mouse *perceives that* the cat is its enemy, hence the mouse runs. What is postulated here is the ‘adequacy’ of the mouse’s thought to the order of nature: the relation of enmity is correctly reflected in his mind. But the Darwinist says: Do not ask why the *mouse* runs from its enemy. Species which did not cope with their natural enemies no longer exist. That is why there are only ones who do.

In just the same way, I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive – the ones which *in fact* latched on to actual regularities in nature (van Fraassen 1980, 39–40).

It is probably fair to say that the importance of this argument within van Fraassen’s work is rather limited. Immediately after the passage just quoted, he turns from the critique of scientific realism to the development of his own proposal (constructive empiricism), which makes no further reference to the Darwinian account of scientific success. Nor does he seem to have taken part in the extensive debate that was initiated by this passage (except for a brief comment on Boyd (1985) in van Fraassen (1985, 282–283)).

Due to this mismatch between the comparably small importance attributed to this passage by van Fraassen himself and the considerable impact it had on the subsequent debate, we will not be very concerned with the question of what exactly van Fraassen meant, but rather with what later commentators have taken his argument to be. And even with respect to this latter issue, we do not aspire to completeness (see Wray (2018, 158–171) for a fairly

comprehensive review). Instead, we concentrate on those parts of the debate which are specifically affected by what we claim to be an illegitimate use of biological concepts, regardless of whether such a use is to be blamed on van Fraassen or on his interpreters.

In order to decide whether a certain use of biological concepts is legitimate or not, one needs to be clear about the kind of argument within which they appear. We will address this issue at the end of the present section. Before that, we briefly discuss a few points that are somewhat peripheral to our main issue but still merit highlighting, because they constitute possible sources of confusion which, to the best of our knowledge, have not yet been adequately recognized in the debate so far. As we will see in Sect. 4, at least some of these points have indeed led to confusion in the recent literature.

First, one should not be misled by the fact that van Fraassen uses a *predator-prey interaction* (the cat preying on the mouse) to introduce his argument. This could lead to confusion, because when he subsequently talks about scientific theories, the focus is on a *competitive interaction* (theory competing with theory), which is a rather different kind of interaction. For example, a predator-prey interaction usually increases in intensity with decreasing relatedness (i.e. increasing genetic distance) while a competitive interaction increases in intensity with increasing relatedness. Since competition seems to be what van Fraassen is really after, what matters for the analogy is not so much the presence of a predator but simply of some selective constraint (a notion to which we will come back in the next section) that shapes the competition. The notion that competition is more important than predation can also be seen in Wray's (2010) defence of van Fraassen's argument, which relies heavily on the claim that scientific success is a *relative* notion, such that when we call a theory successful, what we mean is really just that it is *more* successful than its competitors.

Another point that requires clarification is van Fraassen's remark that the Darwinist allegedly seeks to dissuade us from asking *why* the mouse runs from its enemy. Kitcher (1993, 156) has already pointed out that this amounts to a rather shallow level of Darwinism. Indeed, a foundation of Darwinism is adaptationism and adaptationism is nothing but asking such why-questions. We would now like to reinforce this point by showing that there is even a sense in which the kind of intentional explanation that van Fraassen attributes to St. Augustine fits into Darwinian thinking. The Darwinist is reverse engineering (*sensu* Dennett (1995) the results of the blind process of evolution by natural selection by asking why-questions regarding present organisms, as if there had been an invisible designer at work. This is the fundamental strategy of a Darwinist and often the only one that permits significant steps of inference about the blind, algorithmic process of natural selection. Dennett (1987, Chs. 7, 8)) famously referred to this as 'the intentional stance', but there is some controversy about its proper target. What matters for our discussion is not Dennett's intentional stance towards the process of natural selection itself ('reading mother nature's mind'), but the intentional stance towards individual organisms that Okasha advocates in response to Dennett: "Treating an evolved organism as akin to an agent with a goal, and conceiving of its traits as means by which it tries to achieve its goal, does real work in biology; and where evolved behaviour is at issue, this fits naturally with making the organism the subject of intentional attributions and applying the organism-as-rational-agent heuristic. There *is* a pattern in nature that this helps us to discern, namely the organismic unity and integration that such attributions presuppose; and this is not captured by talk of Darwinian function alone" (Okasha 2018, 41). Of course, an antirealist can always treat such intentional attributions as nothing but useful fictions, but if Okasha is right, then adaptationist reasoning cannot do without them. The

intentional stance therefore still has an indispensable place within Darwinian biology, hence nothing should prevent the Darwinist from asking why the mouse runs from its enemy.

Further need for clarification concerns the relation between species and individuals. In his example, van Fraassen first refers to “the mouse” as an individual, because it is the individual mouse that is running from the cat, not the species. But immediately after this example, van Fraassen translates the “mouse” into the “species” which does not cope with its environment and ceases to exist. This raises slippery issues with the species concept and the levels of selection which we will not have space here to explore further (for more information see Futuyma (1998, 350), Okasha (2006) and Lennox (2019)). To avoid these issues, we assume here what is uncontroversial: The mechanism of natural selection acts on individuals. And this mechanism explains the appearance of design features shared within groups of individuals called adaptations, an example of which is the mouse’s disposition to run from the cat.

Now, the analogous design feature in the case of scientific theories is their empirical success, their “latching on to actual regularities in nature”. This has tempted some commentators to reimport a certain notion of success into the biological side of van Fraassen’s analogy, as follows: in their view, the explanandum is not a certain design feature of the mouse (such as its response to the cat) but its success in the Darwinian struggle for life, in other words, its fitness. One problem with this is that Darwinian fitness has the two components of survival and reproduction, and it is unclear how this is supposed to map onto the notion of success of a scientific theory. More importantly, viewing success (in the sense of fitness) as the explanandum undermines the explanatory force of Darwinism in this context: “You do not explain why (say) the mouse is (biologically) successful by saying that if it had not been it would have been eliminated” (Musgrave 1988, 242). Or similarly: “The simple practice of noting that members of enduring species had high fitness (healthy propensities to survive and reproduce) is close to the caricature view of Darwinism as ‘tautologous,’ explaining survival in terms of itself” (Kitcher 1993, 156). If this were van Fraassen’s explanatory project, it would be a very questionable one indeed. It is therefore important to distinguish the notion of success that constitutes the explanandum in the case of scientific theories (namely, their capacity to generate accurate predictions and explanations) from the kind of success commonly associated with Darwinian processes (survival and reproduction).

This brings us to the most fundamental interpretive question concerning van Fraassen’s Darwinian argument: what kind of argument is it exactly? In the title of our paper, we speak of an *analogy* between mice and theories (and features thereof), but van Fraassen’s remark that “science is a biological phenomenon” might indicate that he has something stronger in mind. According to this stronger reading, Darwinian evolution would not only provide us with an analogy for certain features of scientific theories, but with a direct (scientific) explanation of these features. This reading rests on the implicit assumption that cultural evolution is viable, that the application of a Darwinian framework to inorganic units such as scientific theories actually works. We do not need to enter the discussion about the validity of this assumption (we will return to it in Sect. 5), but merely note that even among those who grant it, there is quite some controversy about how theories of cultural evolution should be fashioned (Lewens 2023). What matters for our discussion is that the debate on van Fraassen’s Darwinian argument does not make any reference to these theories. This suggests that van Fraassen has in general not been taken to advocate an explanation of science’s success in terms of cultural evolution, but something more indirectly connected to Darwinism, usually

designated as “analogy”. This still leaves open many different interpretive options, not only due to the many different ways in which analogies are used in science and philosophy (see Bartha (2022) for an overview), but also due to different possible characterisations of the domains between which the analogy is supposed to hold. On a very general level, analogical reasoning seeks to infer features of a *target domain* (in our case: some set of scientific theories) from features of a *source domain* (in our case: some set of biotic entities). The main interpretive controversy for our purpose revolves around the role of biology in the description of the source domain.

We are thus confronted with a spectrum of options on how seriously we should take the reference to Darwinism in van Fraassen’s argument. On one end of the spectrum is the strong view discussed above, according to which Darwinism is not just an analogy but a direct scientific explanation for the success of science.³ On the other end, one could take a very weak view, according to which Darwinism is nothing more than a suggestive image that helps to illuminate the phenomenon of science’s success.⁴ If that was the correct interpretation of van Fraassen’s argument, then much of our discussion in this paper would be beside the point, because it would then not matter that the ‘Darwinism’ discussed by van Fraassen’s critics and defenders has little to do with Darwinism in actual biology. After all, philosophers routinely build their arguments on fictitious scenarios without any claim to scientific accuracy. This, however, would strike us as a disingenuous use of the label “Darwinism”, illegitimately claiming its scientific respectability for a story made up by philosophers with little or no regard for the notion’s actual scientific use. Our paper is therefore based on the working assumption that those who discuss a Darwinian explanation for the success of science generally treat Darwinism as more than a fictitious image and should therefore pay proper attention to the details of actual biology.

It is important to stress that our dissatisfaction with what we just described as the “weak view” is not based on alleged mismatches between the target domain and the source domain. Again, this would be to miss the point of analogical reasoning, which always works by comparing domains that differ in *some* respects. (After all, Darwin himself was using an analogy when he introduced the term ‘natural selection’. The analogy between the activity of breeders and the course of nature is legitimate despite the fact that nature does not ‘select’ in the same sense as breeders do.) Our criticism rather concerns the proper description of the source domain itself. Since the source domain in the present case consists of biotic entities (organisms, genes, adaptive traits etc.), the weak view is mistaken in holding that biological details do not matter for making inferences from the source domain to the target domain. As we will show in the following Sections, the problem is not that there are some differences between the two domains (this is so with any analogy), but that if the source domain is

³ A seemingly similar classification into “strong view” and “weak view” can be found in Renzi and Napolitano (2011, 6–7). However, what they classify are evolutionary epistemologies of theories directed at the process of scientific change, whereas here, we focus on Darwinian explanations for the success of science.

⁴ Notice that neither end of the spectrum requires a realist attitude towards Darwinism. Even what we called the strong view is compatible with antirealism, because it only involves the claim that Darwinism is *explanatory*, not that it is (approximately) *true*. Acknowledging this difference invalidates Park’s (2017) criticism of the use of evolutionary theory on both sides of the realism debate. Park criticizes the realist’s argument as circular and the antirealist’s argument as self-defeating, but neither of these criticisms is justified: the arguments in question do not presuppose the truth of evolutionary theory, but only its explanatory power, which is not disputed by realists nor antirealists.

scientifically misdescribed, then the analogy misfires by making inferences about the target domain based on features which aren't there in the source domain to begin with.

Excluding the weak view from consideration and noting that the strong view has not been taken in the debate so far suggests that the proper view of van Fraassen's argument for our purpose lies somewhere in between these two extremes. It shares with the weak view the appeal to analogical reasoning, but differs from it by taking at least some of the biological aspects of the source domain seriously when making inferences (by analogy) about the target domain. This does not preclude the use of some fictitious elements, because fictions frequently perform indispensable work in science (Suárez 2009). What it means is that the scientific status of analogies or models involving fictions needs to be assessed on a case-by-case basis, in order to see whether or not their fictitious elements compromise their scientific value (Bokulich 2012). In the rest of this paper, we will perform this task for two important cases, showing that the assumptions underlying the analogy between biotic entities and scientific theories do indeed fall short of the standards of serious biological modelling.

3 A Gene for Truth

Psillos (1999, 96–97) provides a defence of the realist explanation for the success of science against van Fraassen's Darwinian explanation. Psillos argues that van Fraassen's Darwinian explanation for the success of scientific theories is in some sense a phenotypic explanation, an explanation referring to the phenotype of a scientific theory. He calls this phenotype "empirical success" and claims that the phenotypic explanation is compatible with a genotypic, realist explanation, where the genotype is shared by all theories that have the phenotype "empirical success". Psillos calls this genotype "approximate truth". He then argues that the genotypic realist explanation is not only compatible with the Darwinist phenotypic explanation but even preferable because it explains on a deeper level what all empirically successful theories have in common and (with reference to Lipton (1991, 170)) why empirically successful theories continue to be successful in the future.⁵ In his view, the genotypic explanation is preferable to the anti-realist Darwinian explanation because it "tells a story about the deeper common traits in virtue of which the selected theories are empirically successful" (Psillos 1999, 96). Psillos illustrates his line of reasoning with an example taken from Lipton (1991, 170): The red hair of a group of people can be explained with their membership in a club for red-haired people. The membership criteria are selecting for red hair. According to Psillos, this does not, however, explain why one particular individual has

⁵ While we are here only concerned with Psillos's claim that the realist explanation is *preferable* to the Darwinian one, his claim that the two explanations are *compatible* with each other has been intensely debated in the recent literature. Several authors have responded to Wray's (2018, 171–174) rejection of the compatibility claim by reiterating (in their own terms) Psillos's point that the two explanations are compatible because they result from differing explanatory demands: Where Psillos contrasts a 'phenotypic' explanation with a 'genotypic' one, Stanford (2020) contrasts a 'SURVIVE'-explanation with a 'WHAT FEATURES'-explanation. In the same vein, Lee (2021) holds that the selectionist seeks 'phylogenetic and/or adaptive-significance explanations' in contrast to the 'mechanical and/or ontogenic explanations' sought by the realist. Finally, Shech (2021) contrasts the selectionist's 'pragmatic-doxastic explanatory project' with the realist's 'logical-ontic explanatory project'. See also Wray (2021) for a response to Shech (2021) and Park (2022) for a response to Lee (2021).

red hair. Psillos argues that, to explain the red hair of one particular individual we would have to look at its genotype.⁶

It can hardly be denied that in this example, the selectionist (“phenotypical”) explanation is unsatisfactory and calls for a deeper explanation. But Psillos needs more: the deeper explanation has to be of a certain type, namely it has to be in terms of a specific feature (the genotype) shared by all red-haired individuals. Otherwise, the example would not support Psillos’s claim that in the (allegedly analogous) case of successful theories, approximate truth provides this type of explanation: “every theory which possesses a specific phenotype, i.e. it is empirically successful, also possesses a specific genotype, i.e. approximate truth, which accounts for this phenotype” (Psillos 1999, 96). Let us therefore investigate whether actual genotypic explanations of certain phenotypes really work that way.

A first thing to note is that the relationship between the genotype, the phenotype and its selective environment is given by Darwinism and is not an optional detail of Darwinism. The relationship is an interaction (x) which can be formalised in the following way: genotype (g) x environment (e) = phenotype (p) (see Baye et al. (2011) for a review). So if Psillos asks “Can Darwin help?” (which is the title of the Section we are discussing here), then this question necessarily invokes the relationship $g \times e = p$.

With this in mind, let us stick with the red hair example for the moment and look more closely at how the genotype is supposed to explain the red-hair phenotype in a way that the membership of the red-hair club does not, as Psillos claims. Is the red-hair phenotype determined by the red-hair genotype? No. The red-hair phenotype is only correlated with a unique genotype and not strictly determined by it. Given a group of red-haired people, ca. 80% of those people have the genotype for red-hair, while ca. 20% have a different genotype. In addition, given a group of people with a different hair colour than red, a certain percentage of it will have a genotype for red hair (Beaumont et al. 2007; Sulem et al. 2007). Hence, if the approximate truth of theories really worked in analogy to the genotype for red hair, it would not give us “an explanation in terms of some underlying feature which all successful theories share in common” (Psillos 1999, 96). If anything, the analogy would support two points that antirealists have been pressing since Laudan (1981, Sects. 4 and 5): that some successful theories are not even approximately true,⁷ and that we should not expect all approximately true theories (if there are any) to be successful.

A possible response to this critique might run as follows: Agreed, the red-hair example doesn’t work, because the genotype-phenotype correlation is just not strong enough in this case. But couldn’t this be remedied by simply choosing a better example, involving a phenotypic trait invariably produced by a single gene? The answer is no, with reference to the relationship $g \times e = p$ introduced above. Even if 100% of the genetic information was available, this would not be sufficient to predict the phenotype with certainty. The reason for this is that genotype-phenotype maps are never perfect correlations. Even if some of these correlations are strong, the genotype will inescapably interact with the environment during ontogeny, the latter of which constitutes an indispensable piece of information to predict the phenotype.

Could one simply include these additional pieces of information so as to construct a more complete explanation that could then be viewed as analogous to the realist’s explana-

⁶ We invite those who think that this example is outdated and plays no role in the contemporary debate to consult Psillos (2020, 23).

⁷ This claim is obviously related to what is known as the pessimistic meta induction, but it is more defensible, since it does not involve the claim that *most* (or even all) successful theories fail to be approximately true.

tion and be contrasted with the more shallow phenotypic explanation?⁸ Note that in this case, the explanatory role of approximate truth could no longer be viewed as analogous to the explanatory role of the genotype by itself, because there would be cases in which the phenotype can be explained even if the required genotype is absent. Rather, some complex combination of genetic and environmental factors would have to be identified as the underlying feature accounting for a certain phenotype, analogous to the role approximate truth plays in the realist's explanation for empirical success. This is a welcome move towards a more adequate treatment of biological explanations, but as we will now show, it threatens to undermine the argumentative force of Psillos's contrast between phenotypic and genotypic explanation.

In Lipton's red-hair story, club membership plays the role of a selective constraint, which is assumed not to interact with the genotype for red hair. This means that the selective environment of club membership cannot participate in the Darwinian relationship $g \times e = p$ and can thus not act on the phenotype and interact with the genotype in a way that affects the number of copies of a genotype in a population and the realisation of a genotypic disposition during ontogeny.⁹ In this respect, club membership is fundamentally different from Darwinian selection.

This becomes important when we follow Psillos's invitation to ask for a deeper explanation of red-hairedness and ask why there is a genotype for red hair at all. The genotype of an individual under selection serves two purposes. It provides the individual with a predisposition to realise a certain phenotype and it provides the individual with a replicator for this predisposition. To understand what this means we need to move just a little bit closer to actual red-hair genotype biology. The genotype for red hair contains the predisposition for the following phenotype: red hair, fair skin, freckles, skin cancer. Red hair is thus just one conspicuous symptom of a composed phenotype. But for the history of the genotype the symptoms 'skin cancer' and 'fair skin' happen to be most relevant because only these symptoms interact with the environment in a way that affects relative fitness of the individual phenotype and the corresponding genotype. A genotype for red hair will only leave copies of this predisposition in an environment with low UV radiation. It can only be successful in an environment where the disposition for skin cancer is realised with a low probability. And why then would a genotype for red-hair spread through the population in the first place? Because the synthesis of the essential vitamin D3 which depends on UV radiation is much more efficient in individuals with the fair skin-type of red-haired people. This gives red-haired people an advantage in an environment with low UV radiation and this advantage settles down as an increase in the number of copies of the red-hair genotype in the population. With this deeper information at hand, we can now take a geographic map with latitude and cloudy days per year superimposed and not only observe, but also predict, where clubs of red-haired people are likely to thrive and where they will lack possible members. We can also use this information to design manipulative experiments with model organisms to assess the degree to which both genotype and environment contribute to the realization of a certain phenotype (e.g. expose mice with the "red-hair" genotype to high and low UV radiation). The details are much more complicated (see for example Sulem et al. (2007) and Parra (2007)) but this simplified version should make the point clear that the genotype only gains some real explanatory depth in connection with the environmental constraints. Disconnecting the genotype from the

⁸ We owe this suggestion to an anonymous referee.

⁹ Another class of $g \times e$ interactions are epigenetic effects (e.g. methylation), which touches on a controversy not deemed relevant here.

environment means disconnecting the genotype from its history, which is crucial information for explaining the success of any phenotype. This casts doubts on Psillos's separation of a genotypic explanation from a phenotypic one and on his claim that the former is deeper than the latter. It could even be argued to the contrary, that the selective constraint of UV radiation offers more explanatory depth than the genotype for red hair. UV radiation existed long before the genotype for red hair existed and the selective constraint of UV radiation is much more ubiquitous and covers much more ground in terms of explaining different aspects of phenotypes (attractant for insects, photosystem in plants, pigmentation in fungi and animals etc.).

One might object that we have taken the red-hair analogy too far and burdened it with a kind of biological seriousness that Lipton and Psillos never claimed for it, and that the heavy weapons of evolutionary biology are really missing the point of the realist. In particular, it should have been obvious from the start that a story involving only one phenotypic trait of one particular species cannot do full justice to a phenomenon as complex as the success of scientific theories. But let us not forget that it is Psillos himself (following Lipton) who explicitly refers to the "specific genotype, i.e. approximate truth" and insists that a different story "most likely genetic" (Psillos 1999, 96) should be told about hair coloration. So while the appeal to biology is clearly traceable to Psillos, the simplistic invocation of a single genotype might not be the only way to go beyond the antirealist's phenotypic explanation. Let us therefore end this section by briefly exploring another way in which genotype-phenotype interaction might illuminate the success of science.

Looking for a phenotypic trait that might serve as a good analogy for the trait of empirical success, one needs to take into account that empirical success occurs in a wide variety of scientific theories in different scientific disciplines. This leads to the idea of convergent evolution.¹⁰ Natural selection relies on regularities, some more fundamental than others, which generate mechanisms designed to work well in environments exhibiting those regularities within the range of available variation. If the selective constraint is a fundamental constraint, we are warranted to see this constraint mirrored in the design of our individuals under selection over and over again. In other words, fundamental constraints create gravitation toward the "Good Moves in Design Space" (Dennett 1995, 306). The eye is an example for such convergence: a solution to a fundamental constraint that has arisen independently several times and at different points in time in phylogenetic history. The idea is that wherever there is light, evolution will yield some sort of an eye with a genotype for such predisposition (Futuyma and Kirkpatrick 2018, 47). Likewise, the success of scientific theories could be viewed as an example for convergence, in the sense that wherever there is some regularity in the world, scientific development will yield some sort of empirically successful theory. Yet the problem is that this shift toward the selective constraints comes with the cost of having to let go of the idea of a 'genotypic' explanation, because a convergent trait is realised via a whole range of different genotypes, some more closely related than others. Again, the biological analogy does not support Psillos's search for "some underlying feature which all successful theories share in common" (Psillos 1999, 96).

In sum, this realist response to van Fraassen's Darwinian explanation for the success of scientific theories relies on unrealistic assumptions regarding the nature of genotypic explanations and the inertness of environmental constraints. Questioning these assumptions thus

¹⁰ There is a discussion of „convergent realism" in Renzi and Napolitano (2011, 108–110) related to our example. The idea is that regularities in nature constrain scientific change in a way that it appears to be directed toward truth.

seems to support the selectionist argument against scientific realism, in line with what Wray (2010) has claimed. However, this is not the end of the story. As we will see in the next Section, a closer look at actual Darwinian thinking cuts both ways and also undermines some parts of Wray's case against realism.

4 The Illusion of Persistence

One of Lipton's and Psillos's arguments against van Fraassen that we have not yet discussed is their claim that Darwinism cannot explain why theories that have been predictively successful in the past should be predictively successful in the future. What will concern us in this Section is Wray's response to this demand for explanation:

I believe that such a demand can be readily met, provided we take the comparison with natural selection seriously. In the biological world, it does not surprise us when a species that has survived until now continues to survive in the future. Given its past success, we are apt to be surprised if it *does not* continue to survive, unless we are aware of changes in the environment or the arrival of new competitors that would alter the species' prospects of survival (Wray 2018, 169).

Before we address the main issue, let us briefly note that Wray's account inherits two possible sources of confusion from van Fraassen (and from the subsequent discussion), to which we have already drawn attention in Sect. 2: First, talking about species survival potentially raises controversial issues regarding the concept of species and the levels of selection. It is not entirely clear whether Wray indeed treats the species as the unit of selection (which would be problematic in light of what we said in Sect. 2); he might be able to formulate his argument also in terms of organism selection. We can therefore neglect these issues in the present context, because it doesn't matter for Wray's argument what the exact target of selection is. Second, Wray's explanandum is not a phenotypic trait of some organism, but *survival*, one of the two Darwinian fitness components (the other being reproduction). To short-circuit Darwinism in this way is just a slight variation of the near-tautological level of explanation criticised by Musgrave and Kitcher. The criticism is echoed by Wray's recent critics, who claim that his proposed explanation is either no explanation of the relevant phenomenon at all (Psillos 2020; Shech 2021) or at least not one that competes with the realist's, because it actually responds to a different explanatory demand (Stanford 2020; Lee 2021).

Now to the main point: Wray makes a claim about what is to be expected regarding the survival of species, given the workings of natural selection. Survival is the norm, he says, and deviations from it are due to environmental change or the arrival of new competitors. But is it appropriate to neglect environmental change and new competitors when assessing expectations about the fate of species? That depends on the timescale on which the phenomenon is examined. The longer the period one considers, the less appropriate such neglect becomes and, correspondingly, the more surprised should we be about a species' survival. This concerns (i) absence of extinction and (ii) absence of speciation.

- (i) It goes against human intuition that species go extinct because few have observed an actual extinction event (the death of the last individual of a species). The zoo keeper of

the last female Sumatra rhino might become one of those few. For the pre-Darwinian observer the persistence of species has been a reliable experience and one that matched the prevailing image that some deity has created and sustains all living things. Today we know, however, that 99% of all species that have ever existed on this planet are now extinct, that the ratio between speciation and species extinction has changed several times in phylogenetic history and that we are currently entering a phase of human induced mass extinction which may surpass even the greatest of the five past mass extinction events with an estimated one in four species currently at risk of extinction (Diaz et al. 2019).

- (ii) Besides the absence of extinction, the survival of a species also requires the absence of speciation. There are two epistemic foundations for the intuition that speciation is somewhat unusual: Stasis in the fossil record and the notorious indiscernibility of gradual evolutionary change to the human eye. The following example may help to illustrate this: It takes an estimated 20,000 years for natural selection to evolve a mouse into an animal the size of a cat. This rate is much too slow for an evolutionary biologist to observe in the field even with the most exact measurements possible. And the rate is much too fast to be detected by dating the fossil record which has a resolution down to about 100,000 years (see Dawkins (1986, 242)). Any morphology ending up in the geological archive must per definition exhibit some degree of stasis as this is a precondition for its discovery. Dennett makes the point clear: “The ‘discovery’ that all species exhibit stasis much of the time is like the discovery that all droughts last longer than a week” (Dennett 1995, 293). Evolution typically is a very slow process compared with the life-expectancy of humans. Darwin had not observed a single species evolve when he published *On the origin of species* (Darwin 1859). Today, though, there are hundreds of examples where evolution (both genetic and phenotypic) has been observed directly (Futuyma and Kirkpatrick 2018, 56) and even speciation has been well documented for some living organisms (a famous example can be found in Grant and Grant (2009)). In sum, the few examples of species that can be found throughout the fossil record and are alive today do not overturn the fact that most species have either changed or gone extinct (Futuyma and Kirkpatrick 2018, 533, 544).

Hence, whether we regard species survival or species disappearance (by extinction or speciation) as the norm that doesn’t call for an explanation depends on the timescale considered. The lack of surprise about continued survival emphasized by Wray only obtains if the issue is considered on a relatively short timescale. But if Wray (2018, 169) really seeks to “take the comparison with natural selection seriously”, then he should take into account “Darwinian” periods of time, that is, periods long enough for processes of evolution by natural selection to become visible. And once this is done, it *does* become progressively surprising when a species that has survived until now continues to survive in the future. In the long run, species are in no way guaranteed to survive or persist unchanged and likewise are theories not guaranteed to continue to make accurate predictions. Somewhat ironically, then, Wray’s supposedly Darwinian response to the realist’s demand relies on restricting attention to a non-Darwinian timescale.

Maybe this was too quick, however. We surely do not want to accuse Wray’s argument of relying on pre-Darwinian intuitions, and we do acknowledge that there may be reasonably long periods in evolutionary history for which it is appropriate to neglect changes in the

environment and therefore not to expect the extinction of any species. After all, the cases of extinction mentioned under (i) above are standardly viewed as prompted by changes in the environment.

In response, we first note that for some cases, extinction risk can be shown to increase even in the absence of significant change in the environment (Melbourne and Hastings 2008). If such an increase is sufficiently high, a species' continued survival might again end up surprising us, contrary to what Wray supposes. Additionally and more importantly, recall that the lack of surprise postulated by Wray not only requires neglecting changes in the environment but also neglecting "the arrival of new competitors" (Wray 2018, 169). This becomes problematic when we turn our attention from extinction to speciation, the second way in which a species can disappear. Since speciation by definition involves the arrival of a new species, any episode in which the survival of a species ends in this way can be regarded as due to the arrival of a new competitor. Does Wray really want to exclude *by fiat* all such episodes from consideration when reasoning about the prospects of a species' survival? To the extent that he does, the lack of surprise for which he argues would seem to become a matter of *a priori* reasoning, and certainly not an instance of "taking the comparison with natural selection seriously" (Wray 2018, 169).

At this point, an anonymous referee has suggested the following charitable interpretation of Wray's argument: While it is true that species may become extinct due to new competitors emerging from random variation, there is no analogous phenomenon of random variation for scientific theories. So, for the purposes of the analogy, we can ignore this phenomenon. In other words, speciation through random variation just isn't one of the details that is important to Wray's analogy. However, random variation is not an optional detail of Darwinism; it is (beside heredity and selection) one of the three necessary and sufficient conditions for Darwinian evolution to take place (Lewontin 1970). But even if we agreed to construe the analogy in such a pseudo-Darwinian way, *some* kind of variation would have to be admitted for scientific theories as well, in order for science to develop at all. And with variation of any kind, the arrival of new competitors is inevitable, so the problem discussed above persists.

Finally, let us take a closer look at a case in which even Wray admits that we might be surprised at a species' survival, namely if there *is* a significant change in the environment. The realist will point out that this corresponds to what her demand for explanation was particularly concerned with in the first place: scientific theories' *novel* predictive success, where novelty can be spelled out in terms of a theory's continued success when confronted with a "changed environment" consisting of new possibilities for applications and severe testing ("new" in the sense that the theory wasn't originally designed to deal with them). Wray (2018, 170) responds to this by resorting to a well-known weapon in the antirealist's arsenal: reference to historical examples of *false* theories enjoying novel predictive success. Without entering the extensive debate surrounding such examples, we can exhaust the logical space by distinguishing two cases: Should it turn out that there is a viable realist strategy to deal with these examples, then Wray's attempt to counter the realist's demand for explanation is undermined. If, on the other hand, none of the realist strategies succeeds, then the realist is already in trouble anyway, independently of any Darwinian considerations. In either case, we reach the same conclusion as the one recently advanced by Stanford (2020): van Fraassen's Darwinian explanation, in spite of Wray's efforts to defend it, does not lend any independent support to antirealism.

5 Conclusion and Outlook

Regardless of how one estimates the argumentative strength of van Fraassen's Darwinian proposal, its suggestive force is undeniable and impressively evidenced by the ongoing debate it has sparked. This makes it all the more important to critically assess the systematic import of the proposal and the subsequent debate. If what we argued in the previous sections is correct, then this import is rather limited, because central arguments on both sides of the debate do not stand up to biologically informed scrutiny.

This is not to say that the debate must inevitably remain philosophically sterile. As we emphasized at the end of Sect. 2, there are different ways of interpreting the role of Darwinism (or biology in general) in van Fraassen's argument (and in the ensuing debate), and some of them are not vulnerable to our criticism. On the one hand, one need not follow Psillos, whom we criticized in Sect. 3 for responding to van Fraassen by appealing to a notion of "genotypical explanation" that doesn't make sufficient contact with the biological notion of "genotype". If one takes what we called the "weak view" (see Sect. 2), one can respond to van Fraassen on a purely philosophical level, without any pretence of taking the biological aspects of the analogy seriously. One step in this direction is taken by Leplin (1997, 9), cited approvingly by Stanford (2020), whose illustrative example, tellingly, does not deal with biology, but with tennis. A similar strategy could be employed by antirealists in response to our criticism in Sect. 4. This would mean to abandon the comparison of theory selection with natural selection and to concentrate on the (non-natural) selection mechanisms that are in place in the scientific community. If these were spelled out in sufficient detail, they might also furnish an explanation of the continued success of scientific theories, thus obviating the realist's explanation in terms of approximate truth.

On the other hand, antirealists like Wray might strengthen their case by moving towards the "strong view" (as described in Sect. 2), according to which Darwinism *directly* (and not just by analogy) explains the success of science. This, however, would require working out a theory of cultural evolution in sufficient detail to successfully describe (and, ideally, even predict) the development of scientific theories. The starting point for any theory of cultural evolution is the premise that evolution by natural selection is substrate neutral, meaning that it can in principle act on biotic entities (organisms) and abiotic ones (e.g. scientific theories) alike. Support for this premise comes from striking similarities between the evolution of organic and inorganic entities, an example of which was already mentioned by Darwin himself: "The formation of different languages and of distinct species, and the proofs that both have been developed through a gradual process, are curiously the same" (Darwin 1871, 59). However, cultural entities differ from biotic ones as their replication is not based on the language of DNA and shows the potential for fast paced transmission owing to the possibility of "going viral" through horizontal spread, which is rare in genetic evolution. And then there is this potential for human induced non-random variation as a target for selection that is not well understood (Futuyma and Kirkpatrick 2018, 567). As a result, there are currently various strategies to build a comprehensive theory of cultural evolution, differing mainly in the degree to which the cultural entity under scrutiny (e.g. scientific theory) is viewed as an independent replicator or as an entity within a co-evolution framework of gene and culture (Lewens 2023). We do not place any bet on which of the above mentioned strategies will prevail or on whether it will at all be possible to work out a detailed theory of cultural evolution for scientific theories in the near future, but if it is, it might save Wray's account

from our criticism in Sect. 4, for the following reason: Such an explanation of the continued success of previously successful scientific theories would no longer depend on *comparing* scientific theories to biotic species as products of evolution (a comparison which, as we saw in Sect. 4, does not support the conclusion Wray seeks to draw from it), but on viewing scientific theories *themselves* as products of evolution in the precise sense to be provided by the hypothesized theory of cultural evolution.

To stick our neck out a little further on cultural evolutionary theory, we give here one specific (and widely discussed) example to illustrate what we think is a useful starting point for a Darwinian analogy with scientific theories for both the realist and the anti-realist. As an instance of analogous reasoning, this illustration is situated somewhere between the ‘strong view’ and the ‘weak view’ we discussed in Sect. 2. The example concerns the human artefact of Polynesian canoes, which in our analogy, just like scientific theories, have been crafted by humans but their success depends on their environmental constraints where “[e]volution takes care of the quality control” (Dennett 2013, 275). The canoe example has first been noted by the French philosopher Chartier: “Every boat is copied from another boat. [...] Let’s reason as follows in the manner of Darwin. It is clear that a very badly made boat will end up at the bottom after one or two voyages, and thus never be copied. [...] One could then say, with complete rigor, that it is the sea herself who fashions the boats, choosing those which function and destroying the others” (Chartier 1908, 41, 42) (translation from Rogers and Ehrlich 2008, 3417). It seems natural to think that in analogy, boat design corresponds to scientific theories, the prediction of which is tested against the physical world, the waves and the wind in the case of the canoes. Just as the best boat designs get passed on because they regularly return to shore, while boats with a less suitable design do not, so the best scientific theories are retained because they repeatedly make accurate predictions, while others do not and are therefore eliminated. Note that the analogy’s usefulness does not depend on there being a perfect match between the source domain and the target domain. For example, all canoes serve a practical purpose (transport of people and goods), while some theories may not have any practical applications. But even these theories are (at least indirectly) subject to empirical testing and to the process of elimination analogous to the sinking of canoes.

This example gets rid of many of the ambiguities and problems that hamper van Fraassen’s analogy of running mice and successful theories as discussed in this paper. Scientific theories are highly designed human artefacts which, just like the Polynesian canoes, have functional features which proof their success by being tested against the environment—by coming back to shore (boats) or by making accurate predictions (scientific theories). The analogy is clearly Darwinian as it allows for testable predictions which rely on population biological reasoning which Rogers and Ehrlich (2008) have quantified by looking at functional and symbolic design features. With reference to Psillos and the club of red-haired people, the example of canoe design does not distract us with a possible genetic explanation as there are no genes as a substrate for canoe copying. Rather the design of canoes invites us to think about convergence and fundamental selective constraints as suggested in Sect. 3. To the extent that Darwinian research (of the type performed by Rogers and Ehrlich) spells out in which sense ‘the sea herself [...] fashions the boats’, realists may exploit this to show how reality itself (and not just our instrumental preferences) is reflected in our successful scientific theories. And with regard to Wray, there is no survival of the species issue here, nor any ambiguity about relevant time scales as we are talking about cultural artefacts (boats and theories) which have both been crafted by humans. There is even a sense in which

the canoe analogy offers a useful contribution to a recent attempt to distinguish between Wray's anti-realist selectionist explanation for the success of science and the original by van Fraassen. Van der Merwe (2023) argues that for Wray, selection mainly takes place in the interaction between theories and the scientist (intrinsic), while for van Fraassen the selective interaction is a theory's test with the physical world (extrinsic). He then argues that Wray's intrinsic view would make the selectionist explanation for scientific success indistinguishable from a selectionist explanation of artwork-success, while in reality, the faddish nature of artwork-selection "is not what we witness in science" (van der Merwe 2023, 12). The canoe analogy sheds light on this debate, because both kinds of selection are operative in the evolution of canoe design features: Functional features are those tested against the waves and the wind, while the symbolic features, the artwork on the boats, is tested only against "mimetic" or "aesthetic norms" (van der Merwe 2023, 12) of the artist community. Rogers and Ehrlich (2008) found that functional features were more conserved and showed significant slower rates of evolution than symbolic traits. Van der Merwe could, for example, use this analogy to support his claim that the relative stability of scientific theory selection in comparison to artwork selection stems from what he calls an (empirical) "testability norm", to which science (unlike art) subscribes (van der Merwe 2023, 9). This is the kind of biologically informed analogical reasoning that we think may be fruitful for both sides of the realism debate. The debate might hence benefit from the application of Darwinian ideas even in the absence of a full-blown theory of cultural evolution.

In conclusion, we encourage those who wish to continue this debate to either acknowledge that biology does not really play a role in it or to take the challenge of providing a truly Darwinian account of the cultural phenomenon of science seriously. A middle way between these two extremes may also be viable, by using analogies between scientific theories and other cultural artefacts amenable to Darwinian analysis, such as the canoe example just discussed. What we urge philosophers *not* to do is to continue debating scientific realism by relying on superficial (but scientifically sounding) analogies to biology, which on closer inspection turn out to be subverted by biological knowledge itself.

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