The Anti-Realist Explanation for Science's Success: Semantics, Method and Attitude

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Abstract

Antirealist explanations for the success of science have been widely discussed up to today and have received several formulations. This makes it rather complex to assess them all. The objective of this paper is to help understand and assess the proposal of an anti-realist explanation for science's success. I show the core assumptions contained in the several anti-realist explanations, how they relate to each other, and which background assumptions are required in order to warrant each position. I argue that, since the many anti-realist explanations are only plausible when maintained conjointly, there is essentially only one anti-realist account to science's success: scientists find successful (and even fertile) theories because they use methods of theory-selection and theory-construction that preserve only successful theories; the fact that these theories prove themselves successful will not be mysterious if it is conjointly assumed that false theories are often empirically successful. This explanation relies on a semantic and a methodological view concerning the probabilistic relation between success and truth, and also on an epistemic stance regarding the limits of explanatory reasoning. The crucial divergence between realist and antirealist accounts of science's success lies in how probable they assess the possibility of a theory to be false and empirically successful. Since the stale-mate between realist and antirealist explanations results from a prior disagreement about the probabilistic connection between success and truth (or the underdetermination thesis), the challenge raised by the antirealist explanation to realism becomes equivalent to the traditional charge that the no-miracles argument is circular.

Keywords: *scientific realism, no-miracles argument, constructive empiricism, van Fraassen*

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1 Introduction

Hillary Putnam has famously stated that "realism is the only philosophy that does not make the success of science into a miracle" [31, p. 73]. Similarly, Smart argues that it would be necessary to postulate a kind of cosmic coincidence in order to believe that everything we know behaves *as if* atoms exist and yet they don't [35]. Nearly half a century has passed and the No-Miracles Argument (NMA) still remains as the main argument for scientific realism. Nowadays, his canonical formulation receives the form of an inference to the best explanation:

(1) Science is an extraordinarily successful enterprise.

(2) The best explanation for the success of science (and maybe the only plausible one) assumes that mature scientific theories are approximately true.

(3) Therefore, mature scientific theories are approximately true.

This is a very general formulation of the argument. It does not specify a lot of things: in what sense science is successful? What kind of explanation is provided by truth? What theories should we consider as "mature" ones? Why should we trust inferences to the best explanation? One may elaborate many other worries [cf. 43]. Yet, these vague premises constitute the explanatory core of the argument and have been developed in several versions [e.g. 1, 2, 4, 11, 20, 22, 23, 25, 26, 28, 30, 33, 38]. My purpose in this paper is to assess one general criticism that attacks the argument directly in its core: antirealists claim that there is an alternative explanation for the success of science which does not require a realist commitment to scientific ontology. Although realists may still reply that the realist explanation is somehow *better*, this is a thin ground to sustain the argument. This is mainly because inferences to the best explanation require more than a hypothesis being the best explanation for the data. For one thing, it's required that "the best" is good enough. For another, if there are two good enough explanations available, it may seem wrong to neglect one simply because the other is better. If we can think about two probable causes for an event, we can't refute one merely by stating that the other is a little more likely. If one judges that the testifier has 60% probability of being truthful, then one should not infer that the testifier has 0% probability of being lying because being truthful is a better explanation; one should keep a probability distribution of 60% and 40% until external reasons are given to reassess the situation. This is

why inferences to the best explanation are not simply comparative, they are eliminative reasoning, requiring reasons to eliminate all but one of the plausible explanations [cf. 21]. In parallel, it is usually assumed that *if* there is a *plausible* antirealist explanation for the success of science, then we should not infer (3) unless we have a reason to reject the anti-realist explanation or to re-assess its plausibility. But things are not crystal clear because realists still disagree about the exact nature of inference to the best explanation [e.g. compare 21, 29]. Still, this much is clear: the more plausible one takes to be the anti-realist explanation for the success of science, the less plausible becomes the NMA's conclusion.

Antirealist explanations for the success of science have been widely discussed up to today and have also received several formulations. This makes it rather complex to assess them all. The task is made worst by the recurrent stale-mate created by them: wherever an anti-realist explanation is proposed, we can find a realist replying that it is not a legitimate explanation, and the antirealist agree to disagree. It can be rather difficult to assess who has the burden of proof in these situations. The objective of this paper is to help understand and assess the anti-realist explanation for the success of science. I try to clarify this stale-mate by showing the core assumptions contained in the several anti-realist explanations, how they relate to each other, and which background assumptions are required in order to warrant each side of the debate.

As mentioned, several antirealist alternatives have been proposed. I'll show that all these proposals share some core commitments, so that we can use these commitments to assess the general proposal of an antirealist account of science's success. The most influential antirealist accounts are (i) the evolutionary explanation, (ii) the empirical adequacy explanation, and (iii) the empiricist stance. In section 2, I elaborate each one of these core ideas and show how the wide variety of antirealist explanations can be understood as ramifications of them. Then, in sections 3–5, I argue that although these antirealist explanations are not intrinsically implausible, their plausibility is directly connected to a broader antirealist background: the claim that the (subjective) probability of successful theory being true is low, which can be associated to the thesis of the underdetermination of theory-choice by data. This is a relevant result because it shows that the antirealist explanations are considerably less trivial than what is suggested by their proponents. The antirealist explanation is never based upon a previous defense of the underdetermination thesis. But it should be. The antirealist explanation only makes

sense within a deeper antirealist background of assumptions. So, can antirealists provide a plausible alternative explanation to science's success? The answer depends on which fundamental assumptions and epistemic stances one adopts. As a result, the challenge raised by antirealist explanations becomes precisely the same challenge raised by the charge of circularity against the NMA. And by invoking a permissivist conception of rationality [cf. 38] or a conservativist epistemology [cf. 30], we can appreciate that it is not a matter of determining who has the burden of proof; it is a matter of understanding two fundamentally departed worldviews, trying to develop a productive dialogue between them.

2 The Antirealist Explanations for the Success of Science

There are many antirealist accounts for the success of science, but they can be gathered around three main ideas: (i) the *evolutionary explanation* states that scientific theories are successful because they have been subjected to a rigorous process of theoretical selection where only the successful theories survive; (ii) the explanation by *empirical adequacy* states that scientific theories are successful because they are empirically adequate, period; and (iii) the *empiricist stance* claims that science is successful and there is no mystery in that, thereby rejecting the existence of any explanatory demand on the fact that science is successful. For each one of this core proposals, we can find a wider group of antirealist accounts that can be seen as ramifications of them. But focusing on the core ideas instead of the ramifications will allow us to organize the discussion and misspell the common misunderstandings surrounding each account. Accordingly, I will proceed by analyzing each core idea, while briefly pointing to their ramifications. We can begin by looking at the formulations of *evolutionary explanation*:

> 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. [42, p. 40].
>
> Science is successful, to the extent it is successful, because scientific theories result from a winnowing process which is arguably more robust and more discriminating than other techniques we have found for checking our empirical conjectures about the physical world. [18, p. 101].

> [...] the best explanation for the success of science is the fact that unsuccessful theories have been abandoned. The methods of science enable us to determine which of the theories we have developed so far are the most successful. And scientists respond to the assessment of theories accordingly, abandoning those that do not measure up. But employing these methods will not necessarily lead scientists to accept a true or approximately true theory. Only if scientists have developed a true theory would the methods of testing lead them to choose it. [43, Ch. 1].
> [...] the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a Permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar. [13, pp. 172-3].

According to the evolutionary explanation, the success of science can be explained by looking at the methodological process by which scientific theories are developed: theories are rigorously tested and re-articulated (or even abandoned and replaced) in order to match the experimental results. At the end of this process, it is expected for the remaining theories to be empirically successful because they have been selected to be so. The explanation can be deepened by developing the Darwinian analogy and analyzing (i) which factors in science serve as constraints for the survival of theories; and (ii) which properties of a theory are relevant to its survival in this habitat. Presumably, (i) will include a set of methodological rules M that prescribe the acceptance of theories with the highest degree of predictive success (expressed in terms of quantity, variety, and precision of correct predictions) and explanatory success (expressed in terms of theoretical virtues, puzzle solving and understanding-related capacities); and (ii) will include a list of the semantic or relational properties of theories that are relevant according to M's criteria. Thus, the fact that theories have the properties included in (ii) (such as theoretical simplicity, high degree of scope, coherence with background theories and

with known facts, and so on) are due to their adaptation to the environment created by the norms of (i) that only preserve theories with such properties.

A common realist reply states that the Darwinian explanation is unable to explain the occurrence of novel predictions [e.g. 38, p. 40]. The reasoning goes as follows: If the success of a T theory is understood as affirming the coherence (or adequacy) of T with an observed set of evidence *E1*, we can explain the coherence of T with *E1* through the fact that T was selected precisely because it has such coherence. When, however, we include novel predictions on T's success and observe T's coherence with a new evidence *E2* distinct from *E1*, then the Darwinian explanation fails because *E2* goes beyond the initial criterion by which T was selected.

But, at least at first glance, the evolutionary explanation can easily be adapted to account for novel predictions. Realists invoke in their favor the existence of novel predictions as a common event of scientific activity. According to them, theories who do not produce novel predictions (after some period of time and resources) tend to be abandoned in exchange of theories who do it [e.g. 19, 28]. If so, then the realization of novel predictions must be included in the constrains of evolutionary selection that operates in the scientific activity. Fertile theories are preserved in the scientific agenda, and stagnant research programs undergo theoretical revision [vide 17]. Therefore, the fact that theory T made novel predictions can still be explained by the fact that T was the theory selected by a process whose selection-criterion consisted precisely in the ability to originate novel predictions [cf. 12].

Now let's put the evolutionary explanation aside for a moment. The second main antirealist account is the deflationary *explanation by empirical adequacy*. One rationale for the realist explanation is the idea that if a given theory is true, then its consequences will be equally true, and therefore the predictions and explanations derived from this theory will succeed. Realists appeal to the notion of approximate truth, but they typically assume that the implication from truth to success still exists in approximately true theories, although in a more fallible way (although Laudan problematizes this [cf. 18]). In probabilistic terms this will mean that the probability of an approximately true theory to be successful will be considered a very high probability: $Prob(S|T) \gg Prob(\neg S|T)$. But if this probabilistic relationship is sufficient for the approximate truth of a theory to explain its success, then it is natural to wonder whether there are other semantic properties that maintain this same probabilis-

tic relationship to empirical success. To explain the success of a theory, one needs only to stipulate some semantic property that implies the adequacy of the theory's predictions and of its observable content, since this will make the theory successful in relation to observable phenomena. Following this clue, some authors have proposed antirealist explanations for the success science based upon semantic-related properties:

> Surrealist Explanation: "[...] an antirealist could explain the success of science in terms of the notion of as-if-true. The idea is that a scientific theory is successful because the world operates as if it were true, i.e., because the observable phenomena are as if the theory were true" [7] ([See also 13]).
> Instrumentalist Explanation: "if it is the instrumental success of science that we think wants explaining, then it seems that we require nothing more than the instrumental reliability of science in order to carry the explanation off. Indeed, anything more than that would be doing no explanatory work" [7, p. 153] ([See also 5, p. 95]).
> Explanation by Empirical Adequacy: "[...] the antirealist may claim that the empirical adequacy of a theory explains its success and that is that" [16, p. 186].
> Explanation by Predictive Similarity: "[...] the success of a given false theory in a particular domain is explained by the fact that its predictions are (sufficiently) close to those made by the true theoretical account of the relevant domain" [36, p. 275].

Although these proposals are sometimes treated as distinct [e.g. 27, 36, p. 173], the common point between them is to explain a theory's success regarding the observed phenomena by appealing to a property that asserts a general compatibility between the theory and the observable phenomena: the success of a theory can be explained by its empirical adequacy, instrumental reliability, predictive utility, 'as-if-truth' with observable phenomena, and so on. Thus, it is possible to reduce the assessment of all these proposals by turning to the core idea that the success of a theory is explainable by the truthfulness of its observable consequences, that is, by its empirical adequacy.

For many authors, this is a satisfactory explanation of science's success. Moti Mizrahi, for example, is quite enthusiastic in arguing that we cannot chose between the realist and the empirical adequacy explanations, because both have the same testable predictions. Furthermore,

since the antirealist explanation is more parsimonious, then it is preferable to commit only to it, thereby suspending our judgment about the truth of "extra-empirical" parts of theories, i.e. their claims about unobservable entities [24]. Arthur Fine and Andre Kukla do not judge the explanation by empirical adequacy as satisfactory, but nonetheless they claim that it places a theoretical burden for the no-miracles argument: If both explanations imply the success of theories, why would the realist explanation be more satisfactory than the explanation by empirical adequacy? [6, 7, 15].

Instead of defending any of those explanations, one might simply refuse to join the "let's explain the success of science" game. The third antirealist account – the *empirical stance* – does not postulate a theoretical explanation for the success of science, but challenges whether it needs an explanation to begin with. To reject inferring the realist explanation, one can simply deny that the success of science demands an explanation. If it is reasonable to accept truth (or empirical adequacy) as a semantic property that has no further explanatory demand, then why can't we straightforwardly deflate the demand of explaining science's success? Keep in mind that the reasonableness of attributing an explanatory demand to a fact varies with the *epistemic attitude* one adopts. Some antirealists simply do not share the realist intuition that the success of science would be a "miracle" if theories were false, and behind this divergence there is a difference of attitude regarding how easily we should infer the speculations we make to explain facts. Van Fraassen [38], for example, defends the *Empirical Stance* as (very roughly) *the attitude of distrusting explanations by postulation* (specially metaphysical ones) while trusting perceptual evidence and the rationality of science. By adopting a more skeptical epistemic attitude about explanatory reasoning, the antirealist rejects that the success of science has an explanatory demand in any sense capable of warranting abductive inferences (though it may have in other senses, such as pragmatic or heuristic ones). Although this is not an *explanation* for science's success, it is a framework that antirealists can adopt in order to *account* for it, in the sense that it recognizes the success of science and removes any normative tension between it and the antirealist view.

Van Fraassen [42, p. 25] argues that a satisfactory theory will always have to take some facts as basic and unexplained, or it will fall into an infinite explanatory chain (if not circular). And since we must break the explanatory chain at some point and stop inferring speculations, then what prevent us from breaking the explanatory chain right

at the start? Perhaps we should simply accept that empirical success is a more ordinary and less mysterious property than realism suggests, and that it requires no further explanation. This intuition is reinforced if we remember that the history of science is populated (to some discussable extent) by historically abandoned theories which have been empirically successful for long periods of time. This fact makes manifest how ordinary it is for a theory to be empirically successful regardless of its truth.[1] Once again, this is not an antirealist *explanation* for the success of science, since it does not postulate a new hypothesis as *explanans*. But in a broader sense, this is an epistemic attitude that the antirealist can adopt to *account* for the success of science.

3 A Realist Reply: Do We Have an Antirealist Explanation, or Is It the Circularity Charge All Over Again?

I have presented the three main antirealist proposals to account for the success of science: the evolutionary explanation; the deflationary explanation by empirical adequacy; and the empiricist attitude applied to the success of science (which is not an explanation). Are these proposals satisfactory? Can they block the NMA's inference by offering opposing accounts to the realist explanation? This depends on which theoretical background (or epistemic attitudes) we adopt. More specifically, I believe that the plausibility of antirealist explanations presupposes a version of the thesis of underdetermination of theory-choice by evidence. If this is correct, then the antirealist explanation of science's success is far more contentious than its proponents made it sound: it does not offer an hypothesis that can compete in its own with the realist explanation; rather, it offers an hypothesis that, together with a broader antirealist background, warrant rejecting the realist explanation. As a result, the challenge raised by antirealist explanations becomes precisely the same challenge as the charge of circularity: the defense of the NMA presupposes an optimistic view about the connection between success and truth, just as the plausibility of antirealist accounts for the success of science presuppose an antirealist view of this same connection (expressed by the thesis of underdetermination). To defend this, I propose the following general argument:

(1) Antirealist Explanations Presuppose an Empirical Stance: The plausibility of antirealist explanations for the success of science crucially depends on deflating the explanatory demand of empirical adequacy (or of the success of science).

(2) The Empirical Stance Presupposes Rejecting a Positive Connection from Success to Truth: We are only justified in deflating the explanatory demand of empirical adequacy (or of the success of science) if we attribute a high probability for an empirically adequate (or a successful theory) to be false.

∴(3) Antirealist Explanations Presuppose Rejecting a Positive Connection from Success to Truth: The plausibility of the antirealist explanation for the success of science crucially depends on whether we attribute a high (subjective) probability for an empirically adequate (or a successful theory) to be false.

The argument has two key components. The first one asserts that all of the mentioned antirealist explanations for the success of science will have to rely on a sort of empiricist attitude when considering the empirical success of theories. Antirealist explanations are frequently presented and analyzed in isolation, and I take this to be a widespread mistake. The three core ideas for an antirealist account of science's success should be seen as three elements of a single and unified antirealist account. But if this is right, then the antirealist explanation will require an empiricist stance as a key element to maintain its plausibility. This key role of the empirical stance will be deeply discussed in section 4. The second part of my argument accepts the relevance of the empirical stance to the discussion, and offers a criterion to distinguish between situations where it is reasonable to deflate explanatory demands and situations where it is not. This is the discussion we should have if we are to assess the plausibility of applying an empiricist attitude to the success of science. The criterion I propose is that we can deflate an explanatory demand if and only if we can attribute a minimal probability for the occurrence of the *explanadum* happening without the *explananda*. This is spelled out in section 5. The final result is that the plausibility of accepting that theories are empirically successful (or empirically adequate) without a commitment to realism will depend on our previous assessment of the probabilities of a successful theory to be false. This result is far from ending the discussion of what is the correct explanation for science's success, but it is relevant for two reasons. First, it shows that the antirealist explanations are deeply connected between themselves and a version of the underdetermination thesis. Second, it shows that the challenge raised to the NMA by anti-realist explanations is equivalent to the traditional charge that the NMA is circular because it employs an abductive reasoning. Aside from the circularity challenge to the NMA, there is no distinct challenge brought about by antirealist explanations.

4 The Key Role of The Empiricist Stance

Thesis (1) claims that antirealist explanations for the success of science presuppose a deflationary stance. Since there are three core proposals for antirealist accounts, this analysis will have to address each one of the three. My position is that these three antirealist explanations must be understood as three complementary aspects of a single antirealist account, such that the antirealist explanation for the success of science relies simultaneously on three points: the evolutionary explanation as an explanation for how scientists get epistemic access to successful theories; the explanation by empirical adequacy as an explanation for the empirical success enjoyed by scientific theories; and the proposal to deflate the explanatory demand of empirical adequacy is required as an epistemic stance accounting for why we cannot infer a theory's truth from its empirical adequacy and/or success. I show that if any of these three components is rejected, then the antirealist account will either lose its explanatory value or lose its antirealist character. Thus, I believe in (1) because of the following trilemma:

(1a): There are three proposals of antirealist explanations for the success of science: the evolutionary explanation; the explanation by empirical adequacy; and the empiricist stance of deflating explanatory demands.

(1b): The plausibility of the evolutionary explanation depends on jointly assuming a semantic explanation for the success of theories (which can be either the explanation by empirical adequacy or a more radical stance of deflating the explanatory demand of theories' successes).

(1c): The plausibility of the antirealist explanation by empirical adequacy depends on accepting to deflate the explanatory demand for empirical adequacy.

(1d): The proposal of accepting to deflate the success of science trivially depends on accepting to deflate the success of science.

∴(1): Antirealist Explanations presuppose an Empiricist Stance: The plausibility of antirealist explanations for the success of science crucially depends on accepting to deflate the explanatory demand for empirical adequacy (or for the success of science).

At the origin of the trilemma, we offer a choice between the three core proposals of antirealist accounts. (1a) claims that these are the only

three proposals, based on the fact that all the antirealist explanations for science's success (proposed until now) can be essentially reduced to one of this core ideas, as stated in section 2. If a different and innovative antirealist explanation is proposed, then it will have to be considered separately. Until then, the trilemma states that, regardless of which antirealist explanation we endorse, we will eventually have to rely on an empiricist stance that rejects certain key explanatory demands as illegitimate. This occurs because of the justificatory dependencies that exist between each antirealist proposal, as I will show.

Let's begin by assessing (1b), which is focused on the evolutionary explanation. As we have seen, the evolutionary explanation states that scientific theories are successful because scientists abandon unsuccessful theories. Now, a typical realist reply is that the evolutionary explanation and the NMA don't conflict with each other because they are explaining different things: the evolutionary explanation explains how scientists gained *epistemic access* to successful theories (i.e. why successful theories exist within the range of theories known by current scientists), but it does not explain why these theories (considered individually as semantic entities) are empirically successful unlike the abandoned ones. Thus, it is sometimes said that the evolutionary explanation "misses the *explanandum*" ([cf. 18, p. 92]; [25, 28, p. 96-7]; [21, p. 170]).

At this point, it is important to have a sharper understanding of what exactly realism explains in the NMA. In fact, the NMA is focused at two different aspects of science's success: the empirical success enjoyed by scientific theories; and the methodological success of scientists' theory-choices. The major discussion of the NMA emphasizes the first, treating the success of science as a property of scientific *theories* taken as abstract semantic entities [e.g. 2, 4, 11, 20, 22, 23, 25, 26, 28, 31, 33, 35, 44]. This theoretical success is typically understood as the predictive and explanatory success of any mature theory (including its capacity to motivate novel predictions). Truth can be seen as an explanation of theoretical success in so far as a (approximately) true theory is expected to imply (probably) true predictions. But alternatively, some versions of the NMA emphasize science's *methodological* success, understood mainly as scientist's successes on theory-choices, such as which theories to accept, abandon, or further investigate [e.g. 1, 2, 3, 26, 28, 30, 32]. In this version, the success of science is understood partially as a property of *scientists*, namely, the fact that scientists have epistemic access to incredibly empirically-successful theories. It is not easy to find theories that satisfy the rigorous scientific standards of predictive and explanatory success,

especially when we include the realization of *novel* predictions in those standards. So how can scientists consistently find such theories? By having methodologies of theory-building and theory-choice that in the long run (but not so long) reliably conduct to an approximately true theory. These methods include all sorts of evidential standards widespread in scientific practice, including explanatory ones. So, while the first version of the NMA concluded solely that successful theories are (approximately) true, this version offers a reason to believe in the reliability of science's methodological practices, including inferences to the best explanation. This methodological version of the NMA immediately raises circularity issues, since it looks as a sort of "meta-abduction", but this is a bullet that these realists are ready to bite.

It must clear that the two versions of the NMA are complementary. On one hand, saying that a theory (considered as an abstract semantic entity) is true wouldn't be a plausible explanation for its empirical success if we could not explain how scientists as human beings have found and identified true theories at such matters. The realist explanation for the success of science cannot imply that scientists recurrently found true theories by luck and without any relevant methodology fueling these achievements. On the other hand, it would not make sense to say that current scientific methodology is reliable to find approximately true theories if we do not assume that theories of mature sciences are approximately true. It would be tantamount as saying that a reliable methodology has nothing but failed all the time for really long time. So, as a whole, the semantic and the methodological versions of the NMA must be understood as combined to offer a wide explanation for the success of science accounting both for its methodological and semantic aspects.

Having in mind that the NMA has *conjointly* a semantic and a methodological *explanandum*, let's return to the evolutionary explanation. In parallel, the evolutionary explanation dialogues directly with the methodological dimension of the no-miracles argument, explaining the success of theory-choices in providing epistemic access to more successful theories. This clarifies that the problem with the evolutionary explanation is not simply that it "misses the *explanadum*". To state this would be to assume that the *only explanandum* of the NMA was the success of theories. But realists must *also* explain (and do explain) the methodological success of how scientists obtain successful theories. Therefore, the evolutionary explanation targets a legitimate *explanandum*. Notwithstanding, the objection points to an important problem:

the evolutionary explanation is *incomplete* until it is complemented by an antirealist explanation in the semantic dimension of the no-miracles argument, the empirical success of theories.

To fully understand this explanatory incompleteness, two remarks are in order. The first one is to notice that if we try to apply the evolutionary explanation strictly to the empirical success of theories then the explanation would not be satisfactory. For while the application of rigorous selection methods is a relevant condition for revealing the success of theories, it is also a relevant condition for revealing its failures: submitting a theory to rigorous tests does not guarantee its success; the tests may refute it. Therefore, the evolutionary explanation does not illuminate why theories are successful rather than unsuccessful [cf. 27, p. 7]. And to address this point and dialogue with the semantic version of the NMA, the antirealist explanation needs to be complemented with some claim on how empirical success relates to the truth/falsity of a theory.

The second remark to be noticed is that the evolutionary explanation is fully compatible with realism. The fact that scientists employ rigorous methods of theory-selection is compatible with both the epistemic reliability of those methods and the truth of successful theories, along with all the premises of the NMA. Thus, if the evolutionary explanation is intended to be an *antirealist* one, it must be supplemented with a position that rejects any intimate connection between theoretical success and truth as assumed by realists (or at least, with a position that rejects that we can know this connection). As a result, there is a relationship of codependency between, on the one hand, the evolutionary explanation, and on the other hand, either the explanation by empirical adequacy or a more radical empiricist stance. Just as the realist explanation jointly encompasses two dimensions of science's success (the success of theories and the success of theory-choice), so must the antirealist account do in order to fully eliminate the explanatory demands that fuel the NMA. In the methodological dimension, the evolutionary explanation is relevant to understand how scientists have gained access to successful theories. And in the semantic dimension, the explanation by empirical adequacy (or a more radical deflationary attitude regarding the success of theories) is needed to express how the antirealist comprehends the relation between empirical success and truth.

The evolutionary explanation is generally presented by antirealists as independent from the explanation by empirical adequacy, against (1c) [e.g. 42, 40, 43, chpt. 9]. So, I believe it is worth making (1b) very clear.

The codependency between the methodological and semantic explanations can be captured more precisely through the notion of *explanatory independence*: neither explanation is explicatively independent of the other; i.e. neither *holds its explanatory value if we assume the other to be false*. On the one hand, if we assume that the evolutionary explanation is false, then by assuming the explanation by empirical adequacy, we will have no antirealist explanation of how scientists have gained access to empirically adequate (or successful) theories. Thus, we end up with a position that makes a crucial dimension of the success of science into a mystery, as if scientists have come up with incredible complex and successful theories out of nowhere. On the other hand, if we assume that the explanation by empirical adequacy is unsatisfactory and that the deflationary attitude is unreasonable, then the evolutionary explanation helps us understanding how scientists arrived at successful theories, but at the semantic level we have no indication as to why these specific theories are consistently fertile and empirically successful in contrast with unsuccessful abandoned theories, ending up with an explanatory demand that we accept as legitimate and have nothing but realism to account for. In this sense, the two antirealist accounts have no explanatory independence because each one is plausible only if we tacitly assume the other. The evolutionary explanation and an empirical stance (applied either to empirical adequacy or directly to theories' successes) complement each other offering a general antirealist account for the semantic and methodological dimensions of the success of science. This is what underlies the statement of (1b).

So, in order for the antirealist account to explain the success of science, it needs an explanation for the success of theories within the semantic dimension of the NMA. What about the explanation by empirical adequacy, targeted by (1c)? As we've seen, it states that theories are successful because they are empirically adequate, period. This surely gets the *explanandum* right. But here too, the traditional realist reply is to claim that the provided explanation is compatible with the realist account. Leplin, for example, states that "considered as an explanation of what is observed or of how a theory can predict what is observed, empirical adequacy is itself a property that calls for an explanation" [19, p. 23]. If, in a first moment, the empirical success of a theory can be explained by its empirical adequacy, in a second moment, the fact that the theory is empirically adequate can be explained as following from the fact that the theory is true. Hence there is a chain of facts in explanatory relation: the theory is successful; the theory is empirically adequate; the

theory is true.

Given Leplin's proposal, however, antirealists straightly deny the need to follow the explanatory chain to the end, suggesting that we should stop in the explanation by empirical adequacy. Ladyman, for example, rejects the need to explain empirical adequacy. When faced with Leplin's statement above-quoted, he claims that it "needs to be argued rather than merely stated. . . " [16, p. 186] ([see also 14, pp. 303-5]). In this sense, he claims that "the empirical adequacy of a theory explains its success *and that is that*" [16, p. 186, emphasis added].

This leaves the debate in a standstill: Realists claim that the realist hypothesis is the only explanation for the success of science, since they assume that the explanation by empirical adequacy is not by itself an independent explanation capable of rivaling with the realist one; at the same time, antirealists deny that the realist explanation is the only one, since they assume that the antirealist explanation is by itself a rival alternative. There is a divergence about how far we need to go looking for explanations, and it is not obvious who has the burden of proof here. To move on, one must clarify: Is there a real explanatory demand in a theory being empirically adequate? That is, if we accept that a theory is empirically adequate, do we need to rationally seek an explanation for it and, in the absence of any other satisfactory explanation, infer the realist explanation as the correct one?

I believe this much is clear: at this point, the explanation by empirical adequacy becomes crucially dependent on applying a deflationary stance (such as the empiricist stance) to the empirical adequacy of theories (*vide* 1c). For the explanation to be properly antirealistic, one must assume that empirical adequacy does not provide (as far as we know) abductive warrant for a theory's truth, which implies the attitude of deflating the explanatory demand of empirical adequacy. This is what underlies (1c). By recognizing that the explanation by empirical adequacy requires the application of an empiricist stance to the empirical adequacy of theories, we can unfreeze the stale-mate and focus the discussion on the reasonability of adopting an empiricist stance.

Thus, the trilemma stated by (1a)–(1d) expresses a good reason to claim (1): the plausibility of antirealist explanations crucially depends on deflating the explanatory demand created by empirical adequacy (or, more radically, by the empirical success of theories). And from then on, we find the core point of dispute: is the empirical adequacy of a theory a property that demands extra explanation, allowing to infer a theory's truth, or is it an ordinary property that has no additional explanatory

demand? Can we say the same about the empirical success of a theory? When is someone justified to deflate an explanatory demand?

5 *Can We Apply the Empiricist Stance to the Success of Science?*

According to thesis (2), the second key component of my general argument, deflating the explanatory demand for empirically adequate theories is tantamount to claiming that there is a high (subjective) probability for an empirically adequate (or successful) theory to be false. To sustain this thesis, I begin by briefly clarifying what is involved in the act of deflating explanatory demands, and then I propose a general theory about the conditions which warrant deflating explanatory demands. This general theory, when applied to the discussion of explaining the success of science, will imply the thesis (2). And with that, we can have a better understanding of the stale-mate generated by the antirealist explanation.

So, let's begin by briefly clarifying: What is an explanatory demand, and what exactly does it mean to deflate it? At least in this context, we are discussing whether or not there is an epistemic need to postulate and infer an explanation for a phenomenon (a theory's empirical adequacy, or a theory's empirical success). Thus, deflating an explanatory demand is essentially an inferential decision. The focus of the discussion is sometimes misplaced on the question of whether empirical adequacy is an ordinary property or an extraordinary property to be found only in true theories. This suggests that an explanatory demand consists on whether the analyzed phenomenon is surprising or not, and suggests that what is being discussed is how surprising it would be to find an empirically adequate theory to be false. The connection between surprise and explanation is not entirely unfounded. Hempel [10, pp. 430-33] and Friedman [8, pp. 9–11], for example, have already considered that the act of explaining could consist in offering considerations to eliminate the surprising aspect of a phenomenon by turning it into a familiar one. A magic trick is surprising until someone explains us how it is done, thereby eliminating the "surprise" component. However, the theory of explanation as surprise-elimination is known to be problematic, since familiar phenomena can be just as unexplained as surprising phenomena. My car engine has been making a strange rumble for years, and that no longer surprises me at all, although I still don't understand why it rambles [the example is from 21, p. 25]. This elucidates that an explanatory demand

does not consist in the surprising or unlikely character of a phenomenon, but in the fact that a reasonable explanation seems unavailable to us. Similarly, when antirealists speak of deflating explanatory demands, this is a statement that there is no need to search and infer any additional hypotheses in order to explain the phenomenon at hand. The absence of this inferential need assumes that the facts already known are sufficient to explain the phenomenon, and so there is no need to postulate new facts.

Bearing in mind that deflating an explanation is an inferential decision, the relevant question then becomes: Are we in a situation where we should rationally infer an explanation for empirical adequacy? Most proponents of the empirical adequacy explanation simply state that it has no additional explanatory demand, or that the burden is with realists [e.g. 14, 16]. But Van Fraassen provides two reasons to motivate the idea that it is reasonable to deflate explanatory demands. These reasons show that our epistemic attitude cannot be to assume that everything must be explained. The first reason is what Lipton calls the "Why-Regress": we can ask "why?" endless times about anything, just as curious children can (infinitely) do. But we do not want to develop an infinite explanatory chain, so we will have to accept at some point that it is reasonable to assume a hypothesis for which we have no further explanation [21, 42, chpt. 2 sect. 4]. The second reason is that there are paradigmatic cases where we deflate explanatory demands and this seems epistemically healthy. Van Fraassen mentions the example where two people casually meet at the supermarket by simple coincidence. Someone overly motivated by romanticism could explain this coincidence by claiming that there is a mystical connection between these two people, such that they were pre-destined to meet. But it is entirely reasonable to reject this explanation on the ground that it was a plain coincidence. We can name each person's individual causes for going to the supermarket, but we do not need any metaphysical explanation above that. Moreover, even in scientific practice, we have cases such as the traditional one from Newtonian theory, where gravitational force was initially postulated without further explanation of its nature [42, chpt. 2]. In this case, even if a metaphysical explanation were suggested, scientific rigor would suggest not to compromise the theory with a purely speculative metaphysical explanation. Scenarios like this remind us that we often accept hypotheses for which we have no further explanation. Moreover, they suggest that sometimes the epistemically healthy attitude is to slow down our abductive practice, refusing to infer an explanation even when

it is the only or best available one.

Realists typically concede that we should not *always* infer an additional explanation for the facts. Of course, we accept many facts for which we have no further explanation. But on the other hand, we have paradigmatic cases where we do make inferences by the best explanation. Footprints are good evidence that an animal crossed my garden because this is the best explanation to the footprints. Imagine a crime suspect claiming in the court that his fingerprints on the murder weapon simply "need not to be explained." Until the suspect gives us better reasons, it looks as if he is asking us simply to ignore the evidence and be irrational. This poses a theoretical challenge: What distinguishes scenarios where it is reasonable to deflate explanations from the ones where it is reasonable (and even rationally mandatory) to infer the best explanation?

A common way of distinguishing between the two kinds of scenarios would be to point out that in situations of reasonable inference there is a good explanation *available*. In this direction, realists often suggest that we should automatically infer the truth of a theory simply because it is a good explanation for its empirical adequacy, and we know no other. Samuel Schindler, for example, rejects the proposal to deflate an explanatory demand, stating only that:

> "Seeking to block a demand for explanation when a good explanation is available is as irrational as insisting that no explanation is needed for why the sun rises every day in the morning and sets every day in the evening, when we clearly possess a good explanation for this in terms of the earth's daily rotation in its orbits around the sun." [34, p. 27].

Schindler's answer is not entirely satisfactory, as there are cases where we have an explanation available but it is still unreasonable to infer it, as in the example of the two people who meet at the supermarket by coincidence. More generally, conspiracy theories often offer precise and illuminating explanations for facts that we have no other explanation available except than claiming that they are accidental coincidences without common ground.

Realists can deal with these cases by claiming that we have an explanation available, but not a *good* one. To determine if an explanation is good, we evaluate not only whether it explains the facts, but also its plausibility in terms of theoretical virtues and prior probability. Thus, we need to keep in mind that in order to infer a hypothesis via abduction, it is not enough for it to adequately explain the facts, but also needed to consider the existence of alternative explanations and to consider the

how likely it is that the explained phenomenon could occur in the absence of the postulated explanation. We can accept the meeting in the supermarket as a simple coincidence, because if we assume that there is no romantic connection between the two people, it is still plausible to think that their paths crossed by mere chance. And this holds true until the encounters surpass the rarity of an acceptable coincidence. For example, if the two persons met 100 days in a row in everywhere they went, we can no longer believe that the case is a simple coincidence. It became too unlikely to be a coincidence, and it has to have a common cause. In contrast, in order to reasonably accept that the suspect's fingerprints on the murder weapon "need not be explained," we need a minimally plausible hypothesis showing how the fingerprints could be there without the suspect being guilty (or at least to believe in the existence of such a hypothesis). That is, there must be a good (subjective) probability of the phenomenon occurring in the absence of the hypothesis that the suspect is guilty.

My general thesis here, therefore, is that *the act of deflating an explanatory demand* (i.e. refusing to infer an available good explanation) *is reasonable only if it is plausible to accept* (*there is a high subjective probability*) *that the phenomenon occurs in the absence of the postulated explanation.* The argument in favor of this thesis is simply that it offers the most adequate description of our abductive inferential practice, especially bearing in mind the paradigmatic situations of abduction, such as the examples of finding foot-prints in the garden or finger-prints in the murder weapon, and the paradigmatic situations of deflating explanatory demands, such as the examples of coincidental meetings or of conspiracy theories. By defending this normative thesis on the basis of its explanatory role for our epistemic practices, I include myself among those who are happy to commit the naturalistic fallacy, believing that we are capable of justifying and improving our inferential practices by describing them, reflecting on our inferential rules in order to account for the rationality of our inferential acts [9]. After the historicist turn in philosophy of science, I believe that this is not only the correct but the orthodox way to proceed at this meta-methodological level of the discussion [cf. 32]. Perhaps anti-realists may resist this general thesis, since it creates an unwelcome burden for the act of deflating an explanatory demand. In this case, if he agrees with the meta-methodological framework of methodological naturalism, then he'll have to show us how to make the anti-realist explanation plausible in the scenarios that violate this general thesis, and what other inferential rule can better account

for the contextual plausibility of deflating explanatory demands. If he doesn't agree with the meta-methodological framework, then we must focus the discussion on this meta-level.

If we accept this general thesis, then we can more clearly analyze whether or not there is a legitimate explanatory demand behind the empirical adequacy. To determine whether empirical adequacy has an additional explanatory demand, we must question the prior probability for a theory to be empirically adequate in the absence of the realist explanation, that is, being radically false at its unobservable content. Here it is useful to invoke again the notion of explanatory independence. For the antirealist explanation to compete with the realist explanation and block the NMA, it must have explanatory independence from the realist explanation, i.e. it must remain a plausible explanation even when we assume the realist hypothesis as false. So how probable is that a false theory could be empirically adequate?

Certainly, realists and antirealists will have different perspectives on this issue. This reveals that the impasse over the antirealist explanation stems from a deeper disagreement over the probability of an empirically adequate theory to be false. Since the realist will assume the background probabilities to be $Prob(\neg T|EA) \ll Prob(T|EA)$, where '$EA$' indicates that a theory is empirically adequate and 'T' indicates a theory's approximate truth, it will follow that the antirealist explanation does not have independent plausibility. So, it becomes intuitive that empirical adequacy is a property that calls for explanation, because an empirically adequate theory to be false is an anomaly in the background probabilities of a realist belief system. On the other hand, if the antirealist assumes the inverse background probabilities, where $Prob(\neg T|EA) \gg Prob(T|EA)$, then the explanation by empirical adequacy will look to him as an independently plausible competitor.

So, to determine whether or not empirical adequacy contains a legitimate explanatory demand, we need to estimate the probability of $Prob(T|AE)$. But beyond the explanation by empirical adequacy, antirealists also proposed to adopt an empirical stance and directly deflate the need to explain the success of science. Here, likewise, to determine whether the empirical success of a theory has any explanatory demands, we will need to determine the prior probability of how ordinary it is for a false theory to achieve such a degree of empirical success. A realist perspective will assume that it is really odd (a "miracle") for a false theory to achieve a specified degree of empirical success, such that $Prob(\neg T\&S) \ll Prob(T\&S)$. If this assumption is accepted, then the

success of a theory will allow us to infer its probable truth. To block such an inference, the antirealist's claim that "empirical success has no explanatory demand" must be interpreted as implying a rejection of the assumption that $Prob(\neg T\&S) \ll Prob(T\&S)$. The antirealist perspective will therefore be based on a probability assignment closer to $Prob(\neg T\&S) \gg Prob(T\&S)$.

Finally, notice that the assumption of $Prob(\neg T\&S) \gg Prob(T\&S)$ is a version of the traditional thesis of Underdetermination of Theory-Choice by Data (UTD), according to which for any set of evidence (or at least to a given set of available evidence) there are many incompatible theories well-confirmed by this evidence. The equivalence between the UTD thesis and the probabilistic connection between success and truth arises if we assume that true theories will be successful when tested, because then $Prob(\neg T\&S) \gg Prob(T\&S)$ will imply that for each true theory there will be a greater number of false theories that will also succeed in the face of evidence. In these terms, the UTD thesis and the probabilistic connection between success and truth constitute general principles that will determine whether the correct epistemic attitude towards the scientific evidence is to accept the truth of successful theories or to assume that other successful theories exist to account for this same evidence (even if we have not conceived them yet). We can express this by contrasting the two arguments:

Non-Miracles Argument:		Argument by the UTD:
$S(t)$	VS	$S(t)$
$Prob(S\&T) \gg Prob(S\&\neg T)$		$Prob(S\&T) \ll Prob(S\&\neg T)$
$\therefore Prob(Tt)$ is high		$\therefore Prob(\neg Tt)$ is high

Comparison with the UTD thesis makes it clear that we can understand the antirealist explanation as indicating the existence of alternative causes for scientific explanations (i.e. at the local level of the NMA), even if we are still unaware of what those causes are. This gives a new perspective on the stalemate between realist and antirealist explanations. Rather than both being seen as two concrete explanations or as two possible causal descriptions for the same set of phenomena, it becomes more appropriate to view them as two perspectives on the abductive capabilities of scientists. On the one hand, the realist explanation is committed to the fact that by considering a range of causal explanations and electing one as the best explanation, the scientific community will find the objectively better explanation. On the other hand, the antirealist perspective suggests that even after considering a wide range of explanations and choosing one as the best, there will still be (as a mat-

ter of fact) other better explanations that have not yet been considered. What the antirealist explanation really suggests is that scientific analysis chose only the best of explanations within a bad lot that did not include the true explanation, if this analysis was at all intended to find the best explanation according to epistemic criteria (instead of trying to construct phenomena that promote private corporative interests, for example, or that violate the value-neutrality in another vicious way).

This result is also in harmony with discussions about the base rate fallacy. Those who claim that the NMA commits the base rate fallacy [e.g. 22] argue that, in order to determine the final probability of a successful theory being true $Prob(T|S)$, we have to consider the prior probability of a true theory being successful $Prob(S|T)$ and the prior probability of a theory being successful $Prob(S)$. But that's not enough. It's also crucial to consider the prior probability of a theory being true $Prob(T)$. This is easily seen if we invoke Bayes theorem: $Prob(T|S).Prob(S) = Prob(S|T).Prob(T)$. So, in order to sustain the NMA without fallaciously neglecting a relevant parameter, realists need to assume a favorable $Prob(T)$, and that begs the question against anti-realism. The above discussion concluded that the core disagreement between realist and anti-realist explanations is about the probability of theory being successful (or empirically adequate) without being true $Prob(S\&\neg T)$. Invoking Bayes theorem, we can also see that $Prob(S\&T) = Prob(S|T).Prob(T)$. So, the conflict between the NMA and the UTD thesis, or the conflict between realist and anti-realist explanations, is accompanied by a conflict in the estimated values for $Prob(S|T).Prob(T)$. Since $Prob(S|T)$ is rarely challenged by critics of the NMA, it's safe to presume that the conflict is on the prior probability of a theory being true $Prob(T)$. And so, the results are in harmony. I believe this also clarifies the relevance of the base rate fallacy for the debate. Saying that the NMA "begs the question because it assumes a favorable $Prob(T)$" is only one side of the story. Because on the other side, the plausibility of an anti-realist explanation for the success of science requires a favorable $Prob(S\&\neg T)$, which implies a favorable $Prob(\neg T)$. So, it's not only the defense of realism that begs the question. It's that, once again, we have a stale-mate between two opposing epistemic frameworks.

6 Conclusion

Let me recap the two central points of this paper. Firstly, I sought to clarify how the various antirealist explanations for the success of science converge into one general antirealist account. In this account, scientists find successful (and even fertile) theories by using methods of theory-selection and theory-construction that preserve only theories that remain successful. The fact that these theories prove themselves fertile and successful will not be mysterious (nor will it have any additional explanatory demand) if it is conjointly assumed that false theories are often empirically successful and theoretically fertile. Thus, at the semantic dimension, the account assumes a probabilistic distribution of $Prob(S\&T) \ll Prob(S\&\neg T)$. This assumption makes plausible the methodological idea that, by rejecting unsuccessful theories in theory choice and theory construction, one is able to find successful theories although they are not approximately true. This same idea also gives warrant to the application of a deflationary epistemic attitude to the success of science (or to a theory's empirical adequacy), in harmony with Van Fraassen's empiricist stance. Thus, the antirealist explanation can be seen as a wide account encompassing science's semantics and methodology, as well as expressing a deflationary epistemic attitude towards empirical success (or empirical adequacy).

Secondly, I argued that the crucial divergence between realist and antirealist accounts of science's success lies in how each one previously understands the relationship between a theory's success and truth. The plausibility of the antirealist account will depend on one's previous disposition to apply a deflationary epistemic attitude towards a theory's empirical success (or empirical adequacy). But, reflecting on situations of our epistemic life, it seems that our common epistemic decisions of whether or not to dismiss an explanatory demand are guided by the norm: it is reasonable for someone S to dismiss an explanatory demand if and only if S attributes is a high subjective probability to the occurrence of the explanandum in the absence of the explananda. If we apply this norm to the fact that science is successful, it follows that the plausibility of adopting a deflationary epistemic attitude will depend on whether we endorse $Prob(S\&T) \ll Prob(S\&\neg T)$ or $Prob(S\&T) \gg Prob(S\&\neg T)$. And if we apply this norm to the fact that scientific mature theories are empirically adequate, it follows that the plausibility of adopting a deflationary epistemic attitude will depend on whether we endorse $Prob(EA\&T) \ll Prob(EA\&\neg T)$ or $Prob(EA\&T) \gg Prob(EA\&\neg T)$. In this context, a deflationary attitude expresses the view that empirical

success (or adequacy) is obtained by false theories with a relatively acceptable frequency, such that there is no mystery in false theories proving themselves successful (and even fertile) in various areas of science.

Since the stale-mate between realist and antirealist explanations results from a prior disagreement about the probabilistic connection between success and truth (or the UTD thesis), the challenge raised by the antirealist explanation to the no-miracles argument becomes equivalent to the problem of offering a previous or independent justification to the connection between success and truth. Now this is precisely the challenge posed by the traditional charge of circularity, since it is this connection that the NMA attempts to endorse. Thus, insofar as a solution to the problem of circularity is offered by showing how to defend the success-truth connection [e.g. 1, 2, 3, 26, 28, 30, 32], the antirealist explanation will pose no further argumentative challenge. Still, the antirealist explanation shows how an antirealist who does not share the theoretical background of realism is capable of accounting for the success of science: by being committed to a prior rejection of the connection between success and truth, or to the thesis of underdetermination of theory-choice by data. Furthermore, if one endorses a permissivist conception of rationality [cf. 38] or a conservativist epistemology [cf. 30], the stale-mate between realist and antirealist accounts of science's success can be seen as a divergence of two worldviews departed in their fundamental methodological claims. That being so, it's futile to discuss who has the burden of proof in the stale-mate. The best we can do is try to develop a productive dialogue that recognizes the background differences of both positions.

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Notes

1 As a reviewer pointed out, since Van Fraassen endorses the evolutionary explanation, it may seem incoherent for him to claim that the success of science does not demand explanation. For those who are interested in the exegetics and want to know what is Van Fraassen's definitive view, some remarks are in order. First of all, it's crucial to notice that Van Fraassen defends a voluntarist epistemology according to which "any truly coherent position is rational." [39, p. 277]. Although this view is only explicitly defended on Van Fraassen's later work [cf. 38, p. 129]; [40, p. 157, 357]; [41], it was already assumed on *The Scientific Image* (for a detailed defense of this exegetical claim, see [37]). Accordingly, Van Fraassen's main interest in there is to defend constructive empiricism as a coherent and rationally possible view. A main part of this project is showing that scientific realism is not rationally *mandatory*. And for this purpose, Van Fraassen presents different ways to coherently reply realist arguments. The evolutionary explanation is one of them. A deflationary attitude is another. According to voluntarism, both options are rationally permissible, and their adoption will be determined by what epistemic stance one decides to adopt. Which path does Van Fraassen prefer? In his "The False Hopes of Traditional Epistemology", he states that the success of science is the combined result of courage, luck, and improved techniques of logic and pure math [cf. 39, p. 275]. So, I think he endorses the evolutionary explanation indeed. Nonetheless, he invokes the deflationary stance as a way of arguing that principles of abdutive reasoning (such as Salmon's principle of common cause) are not rationally *compelling* [cf. 42, pp. 23-34].

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