With Miracles at the Limit… a functional-externalist and perspectival account of scientific progress

By
Francisco Javier Hernández Gutiérrez

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Supervisor: Maria Kronfeldner

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ABSTRACT

Scientific progress is a widely acknowledged phenomenon in the history of science. However, the assessment of whether and when it happens remains a subject of controversy. In this thesis, I address the problem of assessing scientific progress and propose an account that elucidates its nature and implications. I examine contemporary approaches to progress, including what are known as the semantic, epistemic, functional-internalist or problem-solving, and noetic approaches. Building upon these discussions, I introduce a novel functional-externalist and perspectival account of progress. According to this account, scientific progress is characterized by the minimization of “success miracles,” which are unlikely, unreliably supported claims and *ad hoc* postulates that are necessary to justify the formulation of a theory in observance of the empirical evidence available to a scientific community at a given time. By addressing the problem of progress, this thesis aims to contribute to a deeper understanding of scientific advancement and its evaluation.

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To the ancestors.

Matétera ba

Pam+pariyutsi

Tlazohcamati miyac

Muchas gracias

Thank you very much

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INTRODUCTION

What is scientific progress? Is there such a thing, and can we tell whether and when it happens? It is common among experts and laypeople alike to recognize science as the paradigm example of a progressive human activity. That is, if there is progress in at least one domain of human endeavours, this must include scientific progress. However, the belief in progress is not uncontroversial nor universally held, as many have challenged the prospect of there even being scientific progress to begin with. Yet, in order to resolve this controversy, one must first consider the following question: 'how to *assess* whether there has been scientific progress at any given time?' I call this the 'problem of progress'. In this thesis, I introduce a proposal for an account that answers this question and explains what, if anything, scientific progress is and implies.

The problem of progress reached analytic philosophy with the rise of interest in the diachronic study of scientific theories (i.e. the study of the development and evolution of theories through time), as briefly explained by Ilkka Niiniluoto (2019) in his *Stanford Encyclopedia of Philosophy* entry on 'Scientific Progress'. Before this, analytic philosophers of science had more generally embarked in the project of applying modern logic to the analysis of the structure of scientific theories. However, this approach failed to take into account the historicity and evolution of these theories. Among the first major contributions to this diachronic shift were Karl Popper's *The Logic of Scientific Discovery* (1934/1959) and *Conjectures and Refutations* (1963). Popper argued that science progresses through the introduction of bold and risky claims that remain unfalsified for a period of time. He called this (boldness and riskiness) the 'informativeness' of a theory, which determines the degree to which it can be falsified. In other words, the more informative a theory is, the more falsifiable it is; and the more falsifiable it is, the more progressive it will be as long as it remains unfalsified. Highly informative, unfalsified theories are more progressive because they get closer to the truth, or so he thought.

Famously, after Popper, Thomas Kuhn (1962/1970) problematized this notion of progress by challenging the ideal of science progressing towards the truth. Kuhn described the history of science as a series of successive paradigms with shared commitments to the same core beliefs and values. He distinguished between three stages in scientific practice. ‘Normal science’, which was identified with the solution of puzzles, ‘crises’, or the encounter of ‘anomalies’ (i.e. unsolved puzzles), and ‘scientific revolutions’ that would enable new solutions to these unsolved puzzles. Kuhn sought to prove that the ideal of science progressing towards the truth was by itself unable to explain this history of successive paradigms.

Popper’s student, Imre Lakatos (1978/1989) tried to reconcile these views with what he called ‘research programmes’. These are constituted by theories containing the same “hard core” of unquestioned assumptions surrounded by a “protective belt” of theoretical claims that can be modified on the basis of principles implicit in these assumptions in order to accomodate new discoveries in the midst of falsifying evidence. According to Lakatos, theories are not evaluated in isolation, but rather whole research programmes are assessed as ‘progressive’ or “degenerating” (I prefer to use ‘regressive’). In a progressive research programme, theories lead to new discoveries, contrary to regressive research programmes where “theories are fabricated only in order *to* accommodate known facts” by modifying the claims belonging to the protective belt (p. 5).

The publication of Yafeng Shan’s (2022a) recent essay collection on *New Philosophical Perspectives on Scientific Progress* is proof of the lively interest that this problem has received in recent years. This volume includes the most prominent contemporary approaches to progress, which are labeled (in the literature) as ‘semantic’, ‘epistemic’, ‘functional-internalist (or problem-solving)’, and ‘noetic’. I review each of these approaches and introduce, in response, what I call a ‘functional-externalist and perspectival account of progress’.

According to this account, science progresses by minimizing the amount of (what I call) ‘success miracles’ relative to a scientific perspective at a given time. These are unlikely, unreliably supported claims and *ad hoc* postulates that are necessary to justify a theory in light of what I call the ‘empirical import’. This stands for all the relevant pieces of evidence available to a scientific community at a given time. I restrict my thesis to theory change in physics, by focusing on the example of the change from classical to relativistic mechanics in the early 20th Century (also discussed by Popper, Kuhn and Lakatos[1]). ‘Progress with respect to theory change’ is not the only kind of progress in physics (as explained by Olivier Darrigol (2022)), let alone in science. However, I still decided to focus on this phenomenon as a good starting point for my analysis of progress.

The thesis is organized as follows: in chapter 1, I provide an exposition of the main problems that are relevant to the problem of progress and derive from them the desiderata for a good account of scientific progress with respect to theory change. In chapter 2, I evaluate the state-of-the-art with respect to these desiderata. Finally, in chapter 3, I introduce my proposal for an account of progress and evaluate it with respect to the same desiderata.

[1] Popper and Lakatos were primarily interested in the shift from Newtonian to Einsteinian theories of gravitation; whereas Kuhn paid closer attention to the paradigm shift from classical mechanics to the special theory of relativity.

CHAPTER 1: PROBLEMS AND DESIDERATA

In this chapter, I provide an exposition of the main problems that are relevant to the problem of progress and derive from them a set of desiderata for a good account of scientific progress.

Problems of progress

The first problem is the **pessimistic meta-induction**, which states that the history of science has been marked by many theory changes, such that from the standpoint of the present, most past theories have been abandoned. Many of these theories were abandoned because they were falsified. Yet, they were very successful for a time. Thus, it seems that, since most past successful theories were eventually falsified, we have *prima facie* no reason to claim that present successful theories will remain unfalsified in the future.

The second problem is what I call the **problem of uncertainty**. This is derived from the previous problem in the sense that, since we have *prima facie* no reason to claim that present successful theories will remain unfalsified in the future, there is no way of telling whether these theories are true. And, if there is no way of telling whether present successful theories are true, then there is *prima facie* no way of telling whether any theory is true.

The third problem arises from attempts to respond to the previous two problems. It is derived from what is commonly known as the 'no-miracles argument'. This is usually presented as an abductive argument (or inference to the best explanation) in support of a realist position in light of the various skeptical and anti-realist arguments (as those introduced by the problems above). It was formulated by Hilary Putnam (1975), who stated that "scientific realism is the only philosophy that doesn't make the success of science a miracle" (p. 73). It states that unless our most successful theories are, in some way or another, tracking the truth, their success would be a 'huge miracle'. Since miracles are considered aversive to scientific explanation, philosophers would rather look for an alternative explanation of this success. I call this the **problem of non-miraculous success**. This is relevant to the problem of progress because whatever progress is, it must involve the successful application of scientific theories, since this success is in part what makes people recognize science as a paradigm example of a progressive activity.

The fourth problem is the **problem of incommensurability**. This is a problem for the comparison of incompatible theories, such as classical mechanics and special relativity. As Kuhn (1960/1972) argued, there is a sense in which trying to understand past successful theories from the standpoint of current successful theories involves a significant conceptual change, by which one loses the *common measure*, according to which one can objectively evaluate the validity of the superseded theory. In the context of the problem of progress, this means that without a common measure, it becomes difficult to assess whether a theory change is progressive or not.

The fifth problem is the **problem of theory-ladenness**. This indicates that all empirical observations are theory-laden in the sense that they involve a set of theoretical background assumptions. As Nora M. Boyd and James Bogen (2021) have argued, one needs to explain how these assumptions provide an objective basis for scientific reasoning. This is especially problematic for accounts that aim to reduce progress to successful predictions, since any empirical observation will have to assume some theoretical background that cannot be justified by prediction or observation alone, without falling into a justificatory circle.

The sixth problem is known as the 'Duhem-Quine thesis' (attributed to Pierre Duhem (1954) and Willard Van Orman Quine (1951, 1986))[2]. I prefer the label **problem of underdetermination of theory by evidence** (also used in the literature). This applies the previous problem to the explanation of failed predictions. It states that no failed prediction can

[2] Roger Ariew (1984) has argued that Duhem's thesis is not the same as Quine's, since Duhem was concerned with separability (according to which, theoretical propositions cannot be put to empirical tests in isolation), from which falsifiability is derived; whereas Quine introduced it to defend a kind of *confirmational holism* (i.e. the view that no individual proposition can be confirmed or disconfirmed by empirical tests, but rather only whole theories can be put to such tests).

be explained in isolation, since some background assumptions will always influence our decision in determining what caused this failure. For example, in the 1800s, it was established that light is a wave. Since waves are disturbances in some medium, physicists started wondering what this medium was in the case of light. In other words, ‘through which medium does light propagate?’ Prior to 1887, most electrodynamic theories postulated the existence of a *luminiferous aether* pervading all space, through which these waves propagated. Famously, that year, the Michelson-Morley experiment (named after physicists Albert A. Michelson and Edward W. Morley) was performed as an attempt to prove the existence of this aether. The experiment compared the round-trip speed of light travelling in perpendicular directions (one of which faced resistance by the aether), expecting that *if a luminiferous aether existed*, *the light waves should arrive at slightly different times*. The results were null, as the expected *aether effect* was undetected (even with slight modifications to the experimental setup). This meant that the postulation of the aether was unjustified. This supported the dismissal of the *aether postulate* and paved the way for the change to relativistic mechanics (that made no use of this postulate). But, what explained the decision to dismiss this postulate? What if instead of interpreting it as proof that it should be dismissed, physicists interpreted it as proof that ‘the aether is being dragged by the Earth, such that the velocity of the aether relative to the Earth is zero, and thus has no detectable effect on the propagation of light’ (as some in fact proposed). In accordance with the Duhem-Quine thesis, these decisions had to be influenced by some theoretical background assumptions. Any account of progress with respect to theory change must take this into account.

The seventh problem is the **problem of justification**. This was introduced by Alexander Bird (2007, 2022) in a series of thought experiments aimed at showing that accidental discoveries are intuitively unscientific. He argued that science is supposed to be “well-founded,” rather than grounded on accidental discoveries. This criteria of well-foundedness is understood as the requirement for some kind of justification for any theoretical claims. Since only well-founded claims are thought to contribute to progress, any account of progress must identify the criteria for determining whether these claims are well-founded.

Lastly, the eighth problem is what I call the **normative problem (**i.e. the problem of the aim of science). This is related to the problem of progress because, as Bas van Fraassen (1980) has explained, the aim of a discipline "determines what counts as success" (p. 8), and this success is (at least partly) what makes science progressive. Hence, one must determine what this aim is, in order to determine what progress in the discipline entails. Darrell Rowbottom (2014) criticized this way of thinking about progress because, according to him, it introduces unnecessary teleological discourse. In his response to van Fraassen's analogy between chess and science (used to explain that the aim of science should not be identified with the aims of individual scientists, but rather with the *aim of the discipline*), Rowbottom argued that while in the case of chess one can easily identify the aim (of winning by checkmate), this is not possible in science. While the aim of chess is stipulated as part of the game, the rules of scientific inquiry do not prescribe any particular aim.

I find this argument insufficient, although I agree that there is something odd with van Fraassen's analogy. In the case of chess, fulfilling its aim entails nothing more than its successful practice, whereas in the case of science, fulfilling its aim entails its successful practice *insofar* as it contributes to progress. This nevertheless does not entail the rejection of the relation between aims and progress, since, one merely needs to identify the rules of scientific inquiry that can account for this aim for the relation to hold. The problem is then that of identifying which rules make up the aim of science.

This way of thinking about aims does not presuppose any teleological discourse (as Rowbottom feared), since it is also compatible, for instance, with what Thomas Kuhn (1970) and Wolfgang Stegmüller (1976) regarded as backward-looking aims. As such, it does not presuppose the existence of any aim *towards* which science progresses; but rather, it merely establishes a criteria for assessing whether there has been progress at any given time, just in case that this aim is fulfilled. The relation between the aim of science and scientific progress is then such that the former determines the normative constraints on the assessment of the latter.

Desiderata for a good account of progress

In order to determine whether an account of progress is good, it is necessary to establish a set of desiderata derived from the problems above:

P1: Pessimistic meta-induction
P2: Problem of uncertainty
P3: Problem of non-miraculous success
P4: Problem of incommensurability
P5: Problem of theory-ladenness
P6: Problem of underdetermination of theory by evidence
P7: Problem of justification
P8: Normative problem

Now, each desiderata must respond to one or more problems, and every problem must result in at least one desideratum. This list is not exhaustive, but it allows us to frame the debate within reasonable constraints. The first desideratum is that it **should explain scientific success without appealing to miracles in the midst of uncertainty**, in order to respond to **P1**, **P2** and **P3**. The second desideratum is that it **should retain some degree of commensurability** to allow for the assessment of progress with respect to incompatible theories, in order to respond to **P4**. The third desideratum is that it **should account for theory-ladenness and underdetermination of theory by evidence**, in order to respond to **P5** and **P6**. The fourth desideratum is that it **should identify the criteria for determining the validity of theoretical claims**, in order to respond to **P7**. Finally, it **should identify the normative constraints (rules) making up the aim of science (relative to which progress is assessed)**, in order to respond to **P8**. Let us list these desiderata:

D1: Non-miraculous explanation of success
D2: Retention of partial commensurability
D3: Account of theory-ladenness and underdetermination
D4: Identification of criteria of justification

D5: Identification of normative constraints for assessing progress

In the next chapter, I evaluate the state-of-the-art with respect to these desiderata, in order to identify ways in which my account can improve upon it.

CHAPTER 2: STATE-OF-THE-ART WITH RESPECT TO SCIENTIFIC PROGRESS

Semantic approach (SA)

In 1980, Niiniluoto presented his 'semantic approach' to progress, which was notably influenced by Popper's idea of a gradual approximation to *the truth*. For Niiniluoto, the truth served as an "ideal limit," which he identified with a class of true statements. He defined 'progress' as the gradual approximation to this class of true statements, such that a change from theory φ to theory ψ is progressive iff ψ is closer to the class of true statements than φ. In order to account for this *closeness*, Niiniluoto borrowed the Popperian (1963) notion of 'truthlikeness', which is expressed in terms of similarity conditions between two states of affairs: a theory and the class of true statements (Niiniluoto 1984). Degrees of similarity are then represented in a scale of truthlikeness with its upper limit identified with *the truth*, and its lower limit identified with *falsehood*.

This approach is called 'semantic' because it assesses progress in terms of many-valued truth-functions, defined as degrees of similarity, which are determined by a "metric structure of the space of real numbers or real-valued functions" (Niiniluoto 2022, p. 33), corresponding to the distance between a theoretical proposition H and a cognitive goal C* (i.e. the class of true statements). 'Truthlikeness' is defined as 'how similar H is to C*'.[3] For example, assume that the class of true statements includes the proposition 'the speed of light is invariant across all inertial reference frames' (call it 'speed of light'), which is postulated by the special theory of relativity. Since special relativity contains H ('speed of light'), it is – as a whole theory– closer to C* (as assumed), than, say, classical mechanics, which does not contain H. Special relativity is therefore more truthlike than classical mechanics with respect to H, and the change from the latter to the former is 'progressive'.

[3] For a more precise definition of 'truthlikeness', see Niiniluoto (1984).

Niiniluoto represented this comparative similarity with a formula that quantifies the degrees of truthlikeness relative to C*:

A change from φ to ψ *is* progressive iff $Tr(\psi,C^*) > Tr(\varphi,C^*)$, and regressive iff $Tr(\psi,C^*) < Tr(\varphi,C^*)$,
where *Tr* stands for a variable whose value is the degree of truthlikeness,
$\psi = \{H_1, \ldots, H_k\}$ & $\varphi = \{Q_1, \ldots, Q_k\}$, and
$\{H_1, \ldots, H_k\}$ & $\{Q_1, \ldots, Q_k\}$ are sets of sentences expressing theoretical propositions in a universal language *U*.

U is restricted to a common conceptual framework, so that theories are compared with respect to the same framework. It is, for example, restricted to a conceptual framework that includes all and only electrodynamic entities and properties, as opposed to a conceptual framework that includes both electrodynamic, as well as thermodynamic entites and properties, since it makes little sense to compare degrees of truthlikeness between electrodynamic and thermodynamic theories. There might be some overlap between conceptual frameworks, such that some entities or properties might feature in both frameworks, but they are still different frameworks with incommensurable degrees of truthlikeness.

Epistemic approach (EA)

Bird's (2007, 2022) account is known as the 'epistemic approach', since it defines 'progress' as an accumulation of scientific knowledge. This approach requires that theoretical propositions yield well-founded true beliefs that accumulate into bodies of scientific knowledge. 'Well-foundedness' is defined in accordance with 'norms of correctness' (i.e. standards of justification), which rule out the production of *accidental* truths. A progressive sequence of theory changes involves the accumulation of well-founded true beliefs, such that the change from φ to ψ is progressive iff ψ produces more well-founded true beliefs than φ, by containing more true propositions (in accordance with the relevant norms of correctness).

Over time, these beliefs accumulate in progressive sequences, forming a more robust body of scientific knowledge. Scientists can nevertheless fail in this accumulation if their theories produce false beliefs. Hence, in order for knowledge to accumulate in a set of false (or less than true) propositions, these propositions must still be able to produce true beliefs. To do so, they cannot be merely truthlike, but must be *completely* true; where 'completely true' simply means 'true' (in a two-valued interpretation), as opposed to 'approximately true', which allows for degrees of truthlikeness (in a many-valued interpretation). Bird's strategy was to redefine *approximately true* propositions as *true approximate* propositions, with 'proximity' being a property of propositions, according to which they refer to that which they stand for in an *incomplete way*; while 'incompleteness' is a property of reference that fails to be exact or precise (this is my interpretation). He argued that '*approximately true* propositions' (i.e. propositions that merely approximate the truth) imply 'true *approximate propositions*' (i.e. true propositions that fail to refer exactly or precisely to that which they stand for). For example, take proposition *p*, standing for 'the speed of light is ~299,000 km/s' (call this 'speed of light#'), which is only approximately true insofar as it approximates H ('speed of light'). According to Bird, there is another proposition *q*, standing for '*approximately*, speed of light#', which is logically equivalent to *p*, but unlike *p*, *q* is (not merely approximately, but *completely*) *true*, relative to H. 'Approximate propositions' are then capable of producing well-founded true beliefs just in case that they are true.

Functional-internalist/problem-solving approach (PSA)

The term 'functional' is an umbrella term used to identify accounts of scientific progress whose constitutive aim is the fulfillment of some functional role. The most prominent functional accounts are those of Kuhn (1970) and Larry Laudan (1977). These are brought under the label 'problem-solving approach' (notably by Shan 2022b), since they are identified with the claim that "science progresses if more problems are solved or [they] are solved in a more effective and efficient way" (p. 46). On the one hand, Kuhn thought that problems are solved whenever a solution is sufficiently similar to the relevant paradigmatic solutions at the stage of normal science. If the available paradigmatic solutions are insufficient to solve the problems at hand, this signals a crisis and the need for a scientific revolution to enable new solutions. On the other hand, Laudan thought that a problem is solved by a theory if one can deduce the 'statement of a problem' from the theory, by means of its explanatory and predictive power, which is virtually equivalent to what Carl G. Hempel (1965) called the 'systematic power of a theory'. However, while Hempel required that an explanation be true, Laudan rejected this truth requirement, treating it as a 'utopian aim'. Although both accounts are usually evaluated as a pair, I focus primarily on Kuhn's account when evaluating the problem-solving approach.

According to Bird (2007), on this approach, progress is *assessed* from a standpoint that is *internal* to the scientific community, which is why he called it (not merely 'functionalist', but rather) 'functional-internalist', since it is the community that assesses the "amount and significance of the problems solved" (p. 48).[4] In Kuhn's own words:

> the nature of the community of [scientific specialists] provides a virtual guarantee that both the list of problems solved by science and the precision of individual problem-solutions will grow and grow […] What better criterion than the decision of the scientific group could there be? (p. 170).

This does not mean that progressive changes are the result of a decision by the community, but rather that the community is the one to *assess* whether a change is progressive or not. The community can reliably evaluate the empirical adequacy of competing theories and their ability to supply the necessary paradigmatic solutions, while progress is ultimately *assessed* from a standpoint *internal* to the community.

[4] This is how Kuhn's approach to progress is generally discussed in the literature, following Bird's (2007) framing of the debate.

Noetic approach (NA)

The term 'noetic' was introduced in the context of progress by Finnur Dellsén (2018, 2022) to identify his approach, according to which progress consists of an increase in scientific understanding. He treated 'understanding' as the capacity of the relevant members of a society to correctly *grasp* sufficiently accurate and comprehensive aspects of a cognitive target, which is understood in terms of dependency models encoding information about how the class of studied phenomena is dependent (or not) on something else (in terms of causation, constitution, grounding, etc.). Degrees of understanding (*qua* dependency models) are determined by a combination of factual accuracy and comprehensiveness. This means that science progresses whenever the community is able to correctly grasp these dependency models with a better combination of accuracy and comprehensiveness. For example, imagine a group of physicists disagreeing about the fact that the speed of light is invariable across reference frames. Those who agree with Einstein argue that light waves do not arrive at different times (in Michelson-Morley) because of the contraction of moving objects and the dilation or slowness of time (in accordance with Lorentz transformations) which are derivable from Einstein's two postulates (i.e. 'speed of light' and the invariancy of the laws of physics in all inertial frames).[5] This is more factually accurate than someone who tried to explain this phenomenon by means of an *aether drag* (as briefly explained above). This entails a higher understanding of the studied phenomena by those who agree with Einstein, which supports the claim that the change from classical to relativistic mechanics is progressive.

2.1. Evaluation with respect to the desiderata

Success without miracles

<u>(SA)</u> Niiniluoto's account is vulnerable to both the pessimistic meta-induction and the problem of uncertainty. The fact that theories are assumed to have some degree of

[5] See Einstein (1920/2015, pp. 65-67).

truthlikeness, does not make them immune to falsification, which means that they can in fact be less truthlike than originally thought. As Niiniluoto (2022) pointed out, “When the target C* is unknown, degrees of truthlikeness are likewise unknown” (p. 34). In response, he proposed the introduction of another Popperian notion, ‘verisimilitude’, which unlike ‘truthlikeness’, is also affected by a theory’s informativeness. Popper used these notions interchangeably, and Niiniluoto (2022) claimed (in a footnote) to do the same. However, they should be distinguished given the non-negligible challenge posed by these problems. Whereas truthlikeness is an objective ahistorical standard that *tracks* the distance between a theory and the class of true statements, measuring what Niiniluoto called ‘real progress’; verisimilitude tracks degrees of belief on a theory, represented in a Bayesian formula, quantifying what he called ‘expected progress’ by means of two variables: the value of truthlikeness and a credence distribution relative to the empirical import.

According to Bayesian epistemology, an agent has a doxastic attitude toward a proposition, call it a ‘propositional attitude’, which is represented in a formula by assigning it a numerical value. The propositional attitudes that Bayesians are interested in are ‘credences’, corresponding to degrees of belief. These are represented as distributions over a language in terms of real numbers assigned to a proposition based on how confident one is in believing it. For instance, if a scientist is 75% confident in ‘speed of light’, they should assign an unconditional credence ‘$cr(\mathrm{H}) = 0.75$’ (where ‘H’ refers to ‘speed of light’). It is called ‘unconditional’ because it does not depend on anything else than their degree of confidence or belief.

Bayesianism prescribes that one *ought* to update the value of one’s credences if one has a reason to be more or less confident in the proposition at stake. In the present context, the reason for this is the availability of (confirming or falsifying) empirical evidence, which constitutes the empirical import providing the basis for updating these credences, resulting in what is known as ‘conditional credences’. These are credences that are assigned to an ordered pair of propositions, indicating the degree of belief that an agent has on one proposition on the supposition that another proposition is true. These are represented as ‘$cr(H|E)$’. Then, if, at t_1, a scientist assigns ‘$cr(H) = 0.75$’, and, at t_2, (in observance of confirming evidence), they assign ‘$cr(H|E) \geq 0.75$’, then, at t_3, they should update their credence to ‘$cr(H) \geq 0.75$’. This is called ‘updating by conditionalization’, which means that an agent’s degrees of belief should *line up* with their conditional credences across time.

Niiniluoto’s (2017) solution to the pessimistic meta-induction and the problem of uncertainty was to measure degrees of verisimilitude relative to a conditional credence distribution and the expected degree of truthlikeness. The unknown value of truthlikeness is anticipated to correspond to the value of verisimilitude (*ver*), which is the expected value relative to a complete description ‘Ci’ in a formal language *F*, and a credence distribution for the degree of belief on ‘Ci’, given the empirical import (expressed by) ‘E’. This is represented in the following formula:

$$ver(H|E) = \sum P(Ci|E)\ Tr(H,Ci)$$

where H stands for a ‘theoretical proposition’, E stands for an ‘empirical proposition’ (standing for the empirical import, which contains all relevant[6] pieces of evidence), and Ci stands for a ‘complete description of the *unobservable*’ in *F*, such that

A change from φ to ψ *seems* progressive if and only if $ver(\psi|E) > ver(\varphi|E)$, and regressive if and only if $ver(\psi|E) < ver(\varphi|E)$
where $\psi = \{H_1, \ldots, H_k\}$ & $\varphi = \{Q_1, \ldots, Q_k\}$, and
$\{H_1, \ldots, H_k\}$ & $\{Q_1, \ldots, Q_k\}$ are sets of sentences expressing theoretical propositions in *F*.

He called this ‘optimistic realism’, since it anticipates that the degree of (estimated) verisimilitude will correspond to the degree of (real) truthlikeness. However, this faced some criticism, notably from Andrea Roselli (2020) and others. The main source of this criticism is that for there to exist a correspondence between these two variables, there should be a correspondence between Ci (i.e. a complete description in *F*) and C* (i.e. the class of true

[6] This ‘relevance’ condition tells us that the latter proposition should *count* for the conditionalization, such that if the piece of evidence is positively relevant ‘$cr(H|E) \geq cr(H)$’ should hold, and if it is negatively relevant ‘$cr(H|E) \leq cr(H)$’ should hold.

statements in *U*). However, there is *prima facie* no reason to claim that this will be the case. There is a sense in which Ci might even contradict C*, since a complete description in a formal language need not belong to the class of true statements in the universal language. This difference leaves an open gap between Ci and C*, and it becomes increasingly difficult to assess progress (*qua* increase in truthlikeness) in a non-question-begging way (given this gap).

This is where the no-miracles argument comes handy for Niinuloto, in the form of what he called the 'optimistic meta-induction', according to which the success of most successful theories is explained by an approximation to the truth. The argument can be formulated as follows:

> P1) Our best theories are extraordinarily successful,
> P2) Something must explain this success,
> P3) This success is explained by, either
> a) An approximation to the truth (Niiniluoto 2017), or
> b) A miracle,
> P4) It is unscientific to rely on miracles to explain scientific success,
> C1) Therefore, scientific success must be explained by an approximation to the truth.

Even though there is *prima facie* no reason to think that there is a correspondence between Ci in *F* and C* in *U*, there might be a positive reason to maintain this relation, given that we recognize the extraordinary success of science and the most plausible explanation of this success is (arguably) an approximation to the truth. Since the first desideratum for a good account of progress is that it should explain this success without appealing to miracles, Niiniluoto's account is able to satisfy it. However, this move only works if an approximation to the truth is the only (or most plausible) non-miraculous explanation of scientific success.

In response, one must ask whether 'an approximation to the truth is *really* the only (or most plausible) non-miraculous explanation?' In order to answer this, it is helpful to consider what is meant by 'miracles' here. As reviewed by Timothy McGrew (2019), the orthodox view of miracles states that they are interruptions of the course of nature by exceeding its productive power due to a violation of the closure of the natural world. Philosophers disagree about what exactly constitutes this closure, with many arguing that it is a collection of natural laws or regularities. Regardless of this, I suggest that one should not read Putnam's statement in this light, since by substituting the term 'miracle' with the afforementioned definition, it becomes clear how implausible this is: 'scientific success is explained by either approximation to the truth or else it would involve a violation of the closure of the natural world'. This claim creates a false dichotomy, but more worryingly still, it is unclear why miracles should even be considered as a plausible explanation of scientific success. As Michael Ruse (1982) has pointed out, "Science is about unbroken, natural regularity. It does not admit miracles" (p. 41). So, why even consider them as a plausible explanation in the first place? One can read Putnam's claim as a mere rhetorical device, but this would fail to make justice to the significance of the argument. In opposition to van Fraassen (1980) (and others), I do think that scientific success requires an explanation, and we should seriously consider that our options are limited. I return to this point later, where I argue that there is in fact a better non-miraculous explanation of this success that does not rely on a hypothetical approximation to the truth.

<u>(EA)</u> Bird's account is also vulnerable to the pessimistic meta-induction and the problem of uncertainty, since in order to assess a theory change as progressive, it must claim that at least one theory produces well-founded true beliefs. In order to claim this, it must claim to know that the given propositions (belonging to the theory) are true and capable of producing well-founded true beliefs. However, this is *prima facie* implausible, given the many skeptical and anti-realist arguments. One cannot *merely assume* that a theory is true. This would require that one already knows that it is true, but we have *prima facie* no reason to claim this. As Kuhn (1970) has argued, the case for a cumulative approach to progress is unjustified in the absence

of “external standards to permit [such] a judgment” (p. 108), which would require an independent argument for our epistemic access to the *truth*.

Instead of searching for these external standards, Kitcher (1993) has argued that the accumulation of knowledge can be understood in terms of processes of refinement of *reference potentials* and *explanatory schemata*. Reference potentials do not require the use of the same terms for the same referents, but rather only need to “enable us to trace familiar connections” in accordance to Richard Grandy’s (1973) *principle of humanity*. This principle states that we can attribute “a pattern of relations among beliefs, desires and the world [which is] as similar to ours as possible” (p. 443; cited in Kitcher 1993, p. 101). These reference potentials are theory-laden of explanatory schemata, which enable scientists to understand what other have said, even if the terms used by others are intranslatable to the terms used by them. These schemata can be improved upon by making them more inclusive of entities and properties, insofar as they either “[formulate] part of the objective independent order of nature” or “[present] part of an ideal system for organizing the phenomena” (p. 111).

On the one hand, it is clear how improvements leading to the formulation of the objective independent order of nature would warrant the accumulation of knowledge. But, since arguments from progress are supposed to support realism (and not the other way around), this would require that we already know that being more inclusive of *these particular* entities and properties entails a progressive change. Hence, it would beg the question against the anti-realist, with respect to our epistemic access to the *truth*. On the other hand, Hasok Chang (2017) has argued that this improvement can be done by means of ‘epistemic iteration’, which is the process of building on preceding stages of scientific development by means of refinement and self-correction toward a precise cognitive goal. This cognitive goal is identified with *reality*, which serves as a measure of success. Scientists accumulate knowledge in a series of iterative processes that enable the improvement upon the relevant explanatory schemata that become more inclusive of entities and properties that are part of an *ideal system* for organizing the phenomena, insofar as they approach this reality.

However, Chang does not treat reality as an explanation of success (as the classical realist does), but "rather success is the license for regarding something as real" (p. 236). However, this pragmatist move either a) provides a solution to the problems above, but is unable to satisfy the desideratum for an explanation of non-miraculous success, since success is not explained by the iterative process, but the other way around, or b) it does so at the expense of Bird's (classical realist) notion of knowledge, since, for Chang, truth is relative to reality, which is *licenced* by its success, and does not necessarily correspond to the objective mind-independent order of nature.

<u>(PSA)</u> The problem-solving approach takes a similar approach to van Fraassen's (1980) explanation of scientific success. Van Fraassen argued that successful theories are those that are selected precisely for their empirical adequacy warranting their success. This is in line with Kuhn's (1962/1970) evolutionary view of progress, according to which, science progresses in response to new problems and by virtue of its capacity to solve these problems. In order to solve them, scientists must thereby successfully find the necessary solutions. Therefore, the success of science needs no further explanation.

<u>(NA)</u> Dellsén's account is able to explain success in terms of accuracy and comprehensiveness, which means that the more accurately and comprehensively scientists are able to grasp dependency relations, the more successful their theories will be. One criticism of this view, raised by Niiniluoto (2022), is that it is reducible to the semantic approach, since it relies on an increase in factual accuracy that is reducible to degrees of truthlikeness. Dellsén responded to this by arguing that understanding cannot be reduced to degrees of truthlikeness, since one can achieve understanding by means of deliberate falsehoods in the form of

idealizations. However, as Insa Lawler (2022) has shown, in order for Dellsén's account to use these deliberate falsehoods, it must allow for a significant degree of inaccuracy. She argued that Dellsén cannot straightforwardly explain how science progresses with these deliberate falsehoods, even if they enable understanding by increasing the degree of comprehensiveness. Dellsén (2022) argued (following Michael Strevens's (2017) theory of idealization) that even idealizations (*qua* deliberate falsehoods) must involve some true propositions, which are largely unexpressed as part of the explanatory content, which is distinct from the literal content of the idealization. Yet, as Niiniluoto (2022) has argued, this solution again collapses into explanations by means of truthlikeness, since it is the truth that underlies this factual accuracy. The best explanation for why an idealization leads to an increase in understanding, whenever it involves a decrease in accuracy regarding the literal content of the idealization, is that it involves an increase in truthlikeness in its explanatory content: "[both] accuracy and comprehensiveness correspond to the demands of truth," he concluded (p. 41). This means that, although an *increase in understanding might be an indicator of progress*, the constitutive element of progress (enabling an increase in understanding) is the approximation to the truth (even if only at the level of the explanatory content of the relevant dependency models).

Partial commensurability

<u>(SA)</u> The Kuhnian critique states that once we are faced with incompatible theories, these become incommensurable because standards of evaluation are irreconcilable across paradigms whose reference is intransitive.[7] While Niiniluoto did not directly address this problem, his account does provide a solution, since standards of evaluation are relative to a formal language *F*, and the corresponding degrees of verisimilitude. Degrees of verisimilitude (of

[7] See Kuhn 1970, p. 103.

theories whose referents belong to *F*) become commensurable once the language is restricted to a common class of phenomena. Nevertheless, Niiniluoto still needs to explain how different theories are commensurable even if their terms seem to refer to different things when explaining the same class of phenomena. For example, 'speed of light' seems to refer to different things in classical and relativistic mechanics. The degrees of verisimilitude must nevertheless be comparable in order for progress to be assessable in terms of verisimilitude. However, this should not be possible if their conceptual frameworks contain different referents.

(EA) Bird (2022) argued that although theories are incommensurable across paradigms, there are certain standards of *explanatory goodness* that scientists acquire from training and exemplary practices. Although there is some degree of incommensurability across paradigms, one learns from the success of past theories 'what a *good theory* should be like', and then applies the same (or similar) standards of *goodness* to theories from competing paradigms. For example, since Newton's theory was very successful in explaining the motion of objects in the Earth, it can serve as an example of what a *good theory of motion* is, even if another incompatible theory (e.g. special relativity) ends up replacing it, by being (or providing) an overall *better theory of motion*.

(PSA) The problem-solving approach does not treat the problem of incommensurability as a problem, but rather as a feature of the structure of scientific revolutions. Therefore, incommensurability of theories across paradigms does not present a problem for this account.

(NA) One can conjecture that this is also not a problem for his account, since scientific progress is assessed by an increase in understanding, regardless of whether this increase involves a degree of conceptual incommensurability.

Theory-ladenness and underdetermination

(SA) Since Niiniluoto's account anticipates that degrees of verisimilitude will correspond to degrees of truthlikeness, progress is assessed independently of problems raised by the theory-ladenness of particular observations. Experiments may be theory-laden, and it may not be possible to interpret predictive failure independently from theoretical considerations, but a theory change only needs to be assessable with respect to the class of true statements, which is theory-independent, and thus avoids these problems. However, this solution is dependent on whether the anticipated correspondence between truthlikeness and verisimilitude is justified, which is itself dependent on whether an approximation to the truth is the only (or most plausible) non-miraculous explanation of scientific success.

(EA) Bird's (2022) response to these problems is that there are rational choices to be made in terms of which propositions to retain as reliable background assumptions or sufficiently justified claims. He introduced what he called the 'principle of rationality', according to which, "A rational individual should retain the [propositions] they do know and reject [those] they don't," on the basis of their *compatibility* with the empirical import (p. 187). His strategy was to introduce an asymmetry of theoretical propositions within a given theory, according to which some propositions are more rationally supported (given this compatibility). Then, one can test the less reliably supported propositions against the empirical import in a series of new experiments (as exemplified by the attempt to prove the existence of the luminiferous aether in Michelson-Morley).

There is a remaining question about what makes propositions more or less reliably supported. Bird argued for an 'inference to the only explanation', according to which scientists rule out rival explanations in order to gain knowledge, through a process analogous to epistemic iteration (discussed above). There are at least two alternatives for what grounds this reliability. On the one hand, Bird proposes an objective standard that "may be correlated with the truth" (p. 223). Since propositions that are "assessed as more plausible [or reliably supported] are more likely to be true" (p. 221), and can thus be evaluated with respect to it. On the other hand, pragmatists (like Kitcher and Chang) would propose something akin to empirical adequacy by means of refinement of reference potentials and explanatory schemata on the basis of successful iterations by which *reality is licensed*.

<u>(PSA)</u> Similarly to the problem of incommensurability, these problems do not affect the problem-solving approach, since the fact that observations are theory-laden is just a feature of normal scientific practice. Scientists working within different paradigms are *living in different worlds*, as Kuhn famously expressed. Consequently, observations are *de facto* theory-laden.

<u>(NA)</u> One can similarly conjecture that these do not pose a significant problem for Dellsén's account, since an increase in understanding can be theory-laden as long as it contributes to an increase in accuracy or comprehensiveness, or both.

Justificatory criteria

<u>(SA)</u> Niiniluoto's account is vulnerable to the problem of justification because there is *prima facie* nothing preventing accidental discoveries from featuring in his assessment of progress. All that matters for a progressive change is that there is an increase in approximation to the truth, regardless of whether this involves well-founded or accidental discoveries. Additionally, Gustavo Cevolani and Luca Tambolo (2013) have argued that once one distinguishes between real and expected progress, as Niiniluoto does, one "fully acknowledges that evidence-dependent estimates of the verisimilitude of theories play a key role in the theory-choices made by scientific communities" (p. 929). Since verisimilitude is

updated relative to the empirical import, this satisfies the criteria of justification, by making any increase in verisimilitude well-founded (ruling out *fully* accidental discoveries).

Nonetheless, as any account using Bayesian formalism, his account still faces what is known as the 'problem of priors'. This states that for any credence distribution, there must be some scientifically respectable reason for setting one's prior distributions the way one does. These are the numerical values assigned prior to conditionalization. However, since these values are set according to unconditional credences that seem too subjective to be considered scientifically respectable, one cannot *prima facie* provide a scientifically respectable reason for setting them the way one does. Niiniluoto is presumably an objective Bayesian who believes that objective chances constrain the values assigned to these unconditional credences. This means that only one prior credence distribution is rationally permissible (i.e. that which tracks C*). However, this is vulnerable to the problems discussed above, and its plausibility is dependent on whether the anticipated correspondence between truthlikeness and verisimilitude can be justified.

<u>(EA)</u> While Bird's account can satisfy this desideratum with his criterion for well-foundedness, it still needs to explain what this well-foundedness implies. In 2022, he proposed that "what makes science worthy of respect is that it is properly founded on evidence" by the continuous improvement upon past practices (p. 168). This should be enough to satisfy this desideratum.

<u>(PSA)</u> The problem-solving approach provides the necessary justificatory criteria by means of empirical adequacy (or something of this sort). However, one problem that it still faces is that this might allow for the contribution of *false solutions* to *false problems*, which at first glance are difficult to justify as legitimate scientific practices. Bird (2022) used the example of Pliny and Solinus, who believed that 'goat's blood could soften diamonds', (call this

‘goat’s blood’) as an example of a *false problem* (i.e. ‘what explains ‘goat’s blood’?’), to illustrate this issue. The *false solution* was that ‘since “Jesus was a scape-goat,” and he could soften “the hardest of hearts,” it followed that “goat’s blood would soften the hardest of substances, diamond”’ (p. 46). Since this is a solution to a problem, it would represent a progressive episode, insofar as it could be justified by its empirical adequacy, according to the problem-solving approach. However, this is counterintuitive.

I do not find this as worrisome as Bird does, since as Philip Kitcher’s ‘water temples of Bali’ example has shown, some scientific breakthroughs have involved finding (what are thought to be) ‘*false solutions* to *false problems*’. In this example, Kitcher analyzed the findings from a report on the effects of the *Green Revolution* in Bali (B. I. P. 1988; found in Barker & Kitcher 2013). The report concluded that Western scientists (who tried to improve upon the successful irrigation system used by traditional Balinese farmers) were unable to develop a successful system that could substitute traditional practices. Traditional farmers had developed a system on the basis of social interactions around the practice of religious rituals. These farmers were trying to solve the problem of coordinating their irrigation system with their rituals and existing social practices. This was considered a ‘false problem’ from the standpoint of Western scientists who neglected the importance of these rituals and social practices in the development of a successful irrigation system. In spite of this, the Balinese were more successful in developing their irrigation system precisely because of their observance of these rituals and social practices. This should by any reasonable standard be considered a progressive scientific episode due to its success. Therefore, although some might reject this, I think that some problems are *just thought* to be false problems, but in reality are very real problems with very real solutions that in fact contribute to scientific progress.

<u>(NA)</u> The justificatory criterion in the noetic approach is the capacity to correctly *grasp* sufficiently accurate and comprehensive aspects of the world. Dellsén (2016) followed

Stephen R. Grimm (2011) in defining ‘grasping’ as the “ability not just to register how things are, but also an ability to anticipate how certain elements of the system would behave, were other elements different” (p. 89; cited in Dellsén 2016, p. 82). These ability to register how things are (and how to make subjunctive judgments in relation to this registry) enables scientific understanding and justifies the theories’ validity claims, whenever they in fact register how things are (or how they would otherwise be).

The aim of science

<u>(SA)</u> According to Niiniluoto’s account, the aim of science is the cognitive goal C*. The rules of scientific inquiry are whatever helps scientists reach or approximate this aim. However, since C* is unknown, this is treated as a utopian aim (following Laudan 1977), and is subject to the problems discussed above. The normative criterion of progress is in this case uncertain. Hence, it becomes difficult to determine how one *ought* to pursue and assess progressive changes relative to it.

<u>(EA)</u> There are two ways in which the aim of science can be constructed out of the rules postulated by Bird’s approach. On the one hand, Bird (2022) argued that the aim of science, like any other cognitive system, should be related to the aim of belief, which he identified with a teleological function whose ‘correctness’ is defined as the production of *true* beliefs. By identifying the accumulation of knowledge as the aim of science, it follows that the more true beliefs can be produced, the more knowledge can accumulate, and thus the more science can progress. On the other hand, one could argue for a backward-looking aim, according to which, a progressive change would merely involve the accumulation of knowledge *from* one time (prior to the change) to another (after the change).

However, in both cases, this faces the problem of distinguishing between progressive and regressive sequences of theories, as argued by Niiniluoto (2017). He presented a thought experiment, in which he asks us to imagine a sequence of propositions (H_1, …, H_k), that approximate the truth in varying degrees. Then, following Bird's proposal, we should redefine this sequence as ($A(H_1)$, …, $A(H_k)$), standing for a sequence of *approximate* propositions, all of which are true (in order to produce well-founded true beliefs and yield knowledge). If the first sequence involves an increase in approximation to the truth, Bird's proposal can mimic its progressive nature. However, if it involves a decrease in approximation to the truth, his proposal cannot mimic its regressive nature. In spite of it being regressive (by stipulation), his interpretation would still involve an accumulation of knowledge, since it involves an increase in the amount of true propositions producing well-founded true beliefs. Hence, it is progressive according to Bird's account, but regressive (by stipulation). Therefore, one cannot always redefine *approximately true* propositions as true *approximate propositions*. This only works when the accumulation of knowledge results in a progressive episode. However, one should not just assume this, since there are plenty of examples of epistemic failure in the history of science that has nevertheless contributed to scientific progress, as surveyed by Tamara Horowitz and Allen Ira Janis (1994) in their essay collection on *Scientific Failure*.

This is a problem because the connection between the aim of science and progress gets lost in the process, since the aim of science (i.e. the accumulation of knowledge) can be fulfilled even within a sequence of regressive changes (as shown by Niiniluoto's thought experiment).

<u>(PSA)</u> According to the problem-solving approach, the aim of science is to supply solutions to problems, which can then be used to assess progressive episodes. An important detail to keep in mind, and most significantly for the purposes of this thesis, is that this eliminates the possibility of a scientific community overlooking progressive developments. Insofar as this is the case, this approach cannot account for certain episodes in the history of science in which a community overlooks these developments. Some would contest the claim that the problem-

solving approach entails this kind of internalism, but insofar as progress can only be assessed from a standpoint internal to the community and "does not depend on features that may be unknown to them" (Bird 2007, p. 69), I would consider such a view as *internalist* (as is done in the literature).

For example, it is debated whether Einstein was aware of Michelson-Morley at the time of his formulation of special relativity. Vladimir Ugarov (1979) argued that Einstein was unaware of it, since there is "no mention" (p. 346) of the experiment in the 1905 paper *On the Electrodynamics of Moving Objects*. Ronald S. Shankland (1963) also wrote about a conversation he had with Einstein, where he presumably told him that "he had become aware of it through the writings of H. A. Lorentz, but *only after 1905* had it come to his attention!" However, this is disputed by some readings of Einstein's paper:

> [The asymmetry of Maxwell's equations] together with the *unsuccessful attempts to detect a motion of the earth relative to the "light medium,"* lead to the conjecture that not only the phenomena of mechanics but also those of electrodynamics have no properties that correspond to the concept of absolute rest (my emphasis) (Einstein 1905, p. 124).

It is very plausible that Einstein was referring to Michelson-Morley by these "unsuccessful attempts to detect motion relative to *the* "*light medium*," [i.e. the luminiferous aether]." Moreover, as Jeroen van Dongen (2009) more recently noted, there exists some evidence (in letters prior to 1905) that suggest that he was indeed aware of it. However, it is still subject to debate how much influence it actually played in his formulation of the theory, since Einstein's main motivation was not the experimental results of Michelson-Morley (or any other experiment for that matter). He was rather troubled by the inherent assymetry, in Maxwell's equations, between electric and magnetic fields (which appeared differently for systems in stationary and motion states), as well as by the conflict between the principle of relativity in classical mechanics and the invariancy of the speed of light, which led him to reform the former principle.[8]

Yet, even if the experiment played no major role in the formulation and subsequent acceptance of special relativity, or even if this role can be neglected, it still makes sense to say that the results of Michelson-Morley contributed to the fact that the change to relativistic mechanics was progressive. If the problem-solving approach is indeed internalist (in the sense explained above), then it faces the problem of explaining how these overlooked developments can contribute to progress (if they in fact do). This is relevant because if the aim of science is internal to the community, but there are progressive episodes that are overlooked by the community, then the connection between the aim of science and progress is equally lost, since in such cases there would be instances of progressive changes that do not fulfill the aim of science. Below, I argue that my account can explain better these episodes of overlooked developments that nevertheless contribute to progress and fulfill the (postulated) aim of science.

<u>(NA)</u> Whereas Dellsén's account can postulate an aim that is dependent of certain rules of scientific inquiry (i.e. whatever enables an increase in understanding), there is a risk that this will again collapse into the semantic approach, given that (as explained above) understanding must respond to a requirement of factual accuracy that is possibly reducible to degrees of truthlikeness.

2.2. General assessment

[8] Einstein (1920/2015) explained that the motivation to reform this principle (rather than rethinking the invariancy of the speed of light) was that "The epoch-making theoretical investigations of H. A. Lorentz […] show that experience in [the domain of electrodynamics and optics of moving bodies] leads conclusively to a theory of electromagnetic phenomena, of which the law of the constancy of the velocity of light *in vacuo* is a necessary consequence" (p. 30). Hence, the principle of relativity had to be reformed.

In summary, the following table presents an overview of the contemporary accounts with respect to the proposed desiderata:

Table 1
Overview of contemporary accounts

	Semantic account	Epistemic account	Functional-internalist account	Noetic account
D1: success without miracles	Unclear	Unclear	✓	Unclear
D2: partial commensurability	✗	✓	✓	✓
D3: theory-ladenness and underdetermination	Unclear	Unclear	✓	✓
D4: justificatory criteria	Unclear	✓	✓	✓
D5: the aim of science	Unclear	✗	✗	Unclear

(SA) it is unclear whether it satisfies **D1**, **D3**, **D4**, and **D5**, since, as I will argue below, there is a better explanation of scientific success that does not rely on miracles nor a hypothetical approximation to the truth. It fails to satisfy **D2** because it still needs to explain how different theories, whose terms seem to refer to different things, can belong to the same conceptual framework, and thus be comparable in the assessment of progress.

(EA) it is unclear whether it satisfies **D1** and **D3**, unless one introduces a pragmatist turn, in order to avoid the problems discussed. More problematic, however, is the fact that it fails to satisfy **D5**, since it is unable to distinguish between progressive and regressive sequences of theory changes, and the connection between aim and progress gets lost in the process.

(PSA) fails to satisfy **D5**, since it is unclear whether it can explain episodes in which a scientific community overlooks a progressive development, which seems to allow for progressive changes that do not fulfill the aim of science (and vice versa).

(NA) it is unclear whether it satisfies **D1** and **D5** since it collapses into an explanation by means of truthlikeness, and thus is subject to the same problems faced by it.

In conclusion, the main motivations for my account are thus: a) to provide a better non-miraculous explanation of scientific success (than a hypothetical approximation to the truth), and b) to explain episodes in which a community can overlook a progressive development, while still accounting for it being a progressive development that fulfills the aim of science (in order to account for the normative constraints on the assessment of progress). At the same time, it should be able to satisfy the remaining desiderata.

CHAPTER 3: FUNCTIONAL-EXTERNALIST AND PERSPECTIVAL ACCOUNT

After surveying the state-of-the-art and identifying how I can improve upon it, I now introduce my proposal for an account of progress. According to this account, science progresses by minimizing the amount of *success miracles*, which are unlikely, unreliably supported claims and *ad hoc* postulates necessary for a theory to be predictively successful. Determining whether a claim or a postulate is a success miracle is always relative to a scientific perspective. Finally, the amount of *success miracles* is quantifiable by means of a formula (introduced below) that measures and compares the *miraculousness* of theories within and across perspectives.

3.1. Non-miraculous explanation of success (D1): minimization of *miraculousness*

I argued above that one should not interpret the no-miracles argument as a dichotomy between an explanation due to miracles and a hypothetical approximation to the truth. Instead, I suggest the following interpretation:

Since *success miracles* are unlikely, unreliably supported theoretical claims or *ad hoc* postulates, they are the things that *would make* the success of a theory *appear miraculous*. For a theory to be *miraculously successful*, it means that it lacks the relevant degree of empirical support, and must make use of these claims and postulates to justify itself. One can read Putnam's claim accordingly: 'scientific success is explained by either an approximation to the truth or else it would require the addition of unlikely, unreliably supported theoretical claims or *ad hoc* postulates'. This argument is then no longer about the absurdity of an explanation due to violations of the natural order, but about the relative irrationality of theoretical claims made without sufficient empirical support.

Instead of postulating a hypothetical approximation to the truth as the only alternative to a miraculous explanation, scientific success can be explained by what I call the 'minimization of *miraculousness* thesis'. This thesis states that the less *miraculous* these claims and postulates are, the more empirically supported a theory –as a whole– will be, and the more successful it is likely to be. My alternative explanation of success is that science is more successful whenever it minimizes the amount of unlikely, unreliably supported claims and *ad hoc* postulates that are necessary for a theory to be sufficiently empirically supported, and thus predictively successful. Thereby, my account provides a better non-miraculous explanation of scientific success and satisfies **D1**.

3.2. Partial commensurability (D2), theory-ladenness and underdetermination (D3): theories as *sets of perspectival modules*

Based on Darrigol (2022), Ronald Giere (2006), and Michela Massimi (2022), I treat theories as sets of theoretical propositions with a perspectival modular structure. This means that theories are composed of modules (which are theoretical entities with different domains of application), which correspond to sets of theoretical propositions and background assumptions. For example, 'special relativity' is composed of the module corresponding to a set of propositions containing Einstein's two postulates, as well as the module corresponding to a set of background assumptions containing Maxwell's equations and Lorentz transformations. This modular structure gives rise to scientific perspectives, which are (historically situated) points of view from which one observes and analyzes the studied phenomena. These involve employing standard methods that are adaptive to circumstances of inquiry internal to the perspectival framework, which, according to Ronald Giere (2006), is defined according to "grand principles" and their experimental application (p. 14).

As Michela Massimi (2022) pointed out, there is no single accepted definition of 'scientific perspectives'. However, a commonly attributed feature is that they are related to the practice of scientific communities (something akin to what Kitcher (1993) called 'consensus practices'). There are different ways of illustrating what this means. One is to add perspectival qualifications to theoretical claims as a *mark* of their respective perspective. For example, '*from the point of view of relativistic mechanics,* mass, energy and momentum are unified under "the body's mass, which in different frames appears as different combinations of its energy and momentum" (Lange 2001, p. 238)'; as opposed to, '*from the point of view of classical mechanics*, "physics recognized […] two fundamental laws [i.e. conservation of energy and conservation of mass, that] appeared to be quite independent of each other" (Einstein 1920/2015, p. 58)'. Another is to describe them by means of analogies. Giere (2006) offered an analogy with *color vision perspectives*. Massimi (2022) described them as 'windows on reality' that act as "inferential blueprints" (p. 14) for representations of reality from a particular "vantage point" (p. 31).

Perspectives are similar to Kuhnian paradigms, but there are some differences. Most notably, the use of perspectives does not imply conceptual inconmensurability. For example, Kuhn (1962/1970) argued that "Einstein's theory can be accepted only with the recognition that Newton's was wrong" (p. 98), by which he not only meant that 'Newton's theory is false, if Einstein's is true', but also that one is not even talking about the same things or *living in the same world*.[9] Perspectives are not *holistic* incommensurable referential networks, but rather, they are operative cognitive systems with practical application in observation and theorizing. They arise from modular structures and provide the necessary theoretical framework for the observational perspective.

A modular structure is comprised of a symbolic universe, a set of interpretative schemes, and methods for determining the schemes' behavior in accordance with certain laws

[9] Kuhn concluded his argument for the incommensurability between the Newtonian and Einsteinian paradigms by claiming that: "Our argument has, of course, explained why Newton's laws ever seemed to work. In doing so it has justified, say, an automobile driver in *acting as though he lived in a Newtonian universe*" (my emphasis) (p. 102).

of the symbolic universe. A perspective (understood within this modular framework) "severely limits the aspects of an experimental setup about which we may reasonably have doubts," such that certain "well-established" theoretical modules are used as the basis for comparing the predictive power of competing theories that are not part of the same symbolic universe (Darrigol 2022, pp. 97-98). Thereby, it is able to account for the theory-ladenness of empirical observations, and satisfy **D3**.

This symbolic universe allows us to compare the predictive power of all and only those theories that match their interpretative schemes. For example, there is a clear difference between classical and relativistic theories with respect to their symbolic universes. The former contain the proposition 'there exists a privileged frame of reference, relative to which Newton's laws of motion and gravitation hold' (call this 'Newtonian relativity'), while the latter contain the proposition 'there exists no privileged frame of reference, but rather the laws of physics are invariant across all inertial frames of reference' (call this 'Einsteinian relativity'). Nevertheless, both kinds of theories share an interpretative scheme comprised of well-established theoretical modules, such as the recognition (in the late 1800s) of Maxwell's equations and Lorentz transformations.[10] In the aftermath of Michelson-Morley, relativistic theories made progress by enabling predictions that made no use of the *aether* postulate (i.e. a *success miracle* in my terminology), in observance of Maxwell's equations and Lorentz transformations, while classical theories had to introduce this postulate, or modify it on the basis of these equations.[11] These interpretative schemes allow for the interaction of theoretical modules across perspectives, such that even if the shift from classical to relativistic mechanics represented a progressive change, classical theories are not entirely abandoned, rather, they remain as a *shadow* of the relativistic symbolic universe (this is my terminology). As Darrigol

[10] Although the first one to recognize Lorentz transformations as an axiomatic set was Henri Poincaré (1905), the hypothesis was already widely known since the publication of George Francis FitzGerald's (1889) and Henrik Antoon Lorentz's (1992) papers on the length contraction of moving objects.
[11] See footnote 5.

(2022) pointed out, the knowledge acquired from a theory "is not suddenly lost in this process [of progressive changes]," but is often "advantageous to keep using older theories in restricted domains," such as the application of classical mechanics in commonplace engineering practices (p. 97). This presents another difference with Kuhn's account, since, according to Kuhn (1962/1970), one cannot treat classical mechanics as a limiting case of special relativity (even within restricted domains) because the "fundamental structural elements of which the universe to which they apply is composed" was altered in the revolutionary passage from the classical to the relativistic paradigm (p. 102). However, as Giere (2006) argued, the translatability of terms comprising this fundamental structure is not necessary to deal with the theoretical problems involved in applying (e.g.) classical mechanics to commonplace engineering practices, even if, as I have proposed, there are significant differences in the symbolic universes of their respective theoretical modules. For example, it is possible for classical mechanics to have a domain of application (within the relativistic modular structure) to systems whose velocities are much slower than the speed of light (as expressed by the use of the term 'Newtonian limit'). It is not so much that engineers (or anyone else) live in a different world, but rather that all calculations made under these conditions (determined by their domain of application) treat the speed of light as *de facto* infinite, so that classical mechanics can be successfully applied in these cases. Thereby, "Radical incommensurability"[12] can be avoided, and **D2** satisfied.

3.3. Justificatory criteria (D4): prior distributions are *inherited from successful perspectives*

Since I attempt to quantify and compare degrees of *miraculousness,* my account, as any other account using Bayesian formalism, is still vulnerable to the problem of priors. This states that for any credence distribution, there must be some scientifically respectable reason

[12] Darrigol 2008, p. 208.

for setting one's prior distributions the way one does. Let us represent this formally to understand what the problem implies.

At t_1, S introduces a theoretical proposition H (to which they assign a *prior* unconditional credence). At t_2, they run an experiment for which they receive outcome E (i.e. an empirical proposition standing for the empirical import). At t_3 (in the aftermath of the experiment), S updates the credence distribution by conditionalization, which means that

cr_3(H) = cr_1(H|E), which according to Bayes' theorem, means that

$$cr_3(H) = ((\boldsymbol{cr_1(E|H)}\ \boldsymbol{cr_1(H)})/cr_2(E))$$

However, one still needs to justify the prior credence distributions (in **bold**) by some scientifically respectable reason. One can argue that the interpretation of previous observations provides such a reason. However, these observations cannot go back forever (as the problems of theory-ladenness and underdetermination have shown), since then one must ask: 'where does one get the credence distributions for interpreting past observations?', and so on *ad infinitum*. The subjective Bayesian solution to this problem is that we get these values from the accumulated observational data on the basis of some (background) *epistemic standards,* which result in a set of *hypothetical prior distributions* arising from our previous subjective, or rather *intersubjective* (as I propose), commitments.[13] The question then becomes: 'according to what epistemic standards should one interpret these observations, and which hypotetical prior distributions are reasonably justified by these epistemic standards?' I propose that they are *inherited* from *a successful scientific perspective*. But, this raises the question: 'what is a 'successful scientific perspective'?'.

I follow Giere (2006) in assuming the 'uniqueness of the world' as a methodological maxim for the construction of scientific perspectives. This means that one begins by *assuming*

[13] According to Kitcher (1995), this solution gets something wrong about the *kind of cognitive variation* involved in actual scientific practice. He wrote in a footnote: "[Subjective Bayesianism] generates the wrong kind of cognitive variation, assuming that people are uniform in some relentless propensity for conditionalization, but that there is enormous lattitude in their initial judgments of probability" (p. 69). Responding to this criticism would require some more historical work, which would deviate too much from the purposes of this thesis. Nonetheless, I recognize that this is a problem that I will eventually have to deal with.

that the world has a "unique causal structure," towards which several perspectives will eventually converge (p. 34). Massimi would probably disagree with this point, since for her, the way perspectives work "in no way resembles *convergence to something*," but rather resembles a journey "to explore uncharted territories" (p. 20). I agree with her on the latter point, but I do not think that the kind of convergence introduced by this methodological maxim is incompatible with it.[14] The idea of a unique structure should not be controversial, since a kind of ordering of nature seems to be intrinsic to scientific inquiry. This unique structure is defined in accordance with the *expected* perspectival convergence, such that the multiple perspectives *make up* this structure. This is not a metaphysical thesis, but a purely methodological one. Moreover, it does not imply that there is *one* perspective that will eventually *make up* this structure in its totality, since it does not introduce any proposal for a potential *Theory of Everything*.

The justification for each of these perspectives, and their *expected* convergence, comes from the *predictive success* of the individual constitutive modules belonging to each perspective. The more successful a scientific perspective is, with respect to the predictive power of each of its modules, the more justified it is in claiming that it contributes to *making up* this unique structure. The epistemic standards for setting up the hypothetical prior distribution the way one does arise from each perspective, and are justified by the perspective's contribution to *making up* this unique structure. These hypothetical prior distributions are a direct response to these contributory claims, since epistemic standards "govern" how one reacts to pieces of evidence (as functions from the empirical import to the respective credences) (Titelbaum 2022, p. 107). Hence, we can derive a scientifically respectable reason for setting one's prior distributions the way one does, which over time

[14] For instance, one way of reconciling these views is by incorporating a so-called 'open-minded Bayesian' approach to my quantification of *miraculousness*. For a discussion of open-minded Bayesianism, see Wenmackers & Romeijn (2016), and Sterkenburg & de Heide (2022).

leads to new credence distributions by conditionalization. Thereby, it provides the necessary justification for assessing theory changes, and satisfies **D4**.

One potential worry is that this might lead to an infinite regress: if the hypothetical prior distribution is justified by the predictive success of the modules belonging to the relevant perspective, which also determines the prior distributions that are the result of past updates by conditionalization, which are themselves justified by the predictive success of the modules belonging to the relevant perspective (and so on *ad infinitum*), then there is a point at which the argument breaks down to unjustified assumptions about what this unique structure is like. This might be true, but we must consider that, as Kitcher (2022) has already explained, "Science grows out of a long prior history of practical inquiry" and "Thanks to [a historical] division of epistemic labor, [we have] benefited from one another's explorations of the environment" (p. 202). It is this long history of practical inquiry and division of epistemic labor that has helped us make sense of the world as a unique structure, according to which certain epistemic standards arose for judging whether our theories are successful or not (by means of their perspectival contribution to *making up* the unique structure of the world).

3.5. The aim of science (D5): minimizing miraculousness *to the limit*

This account postulates that the aim of science is to minimize the degree of *miraculousness*, such that a theory change involving a decrease in *miraculousness* is progressive and *ought* to be pursued. In order to determine whether a theory is more-or-less miraculous (than another), I quantify degrees of *miraculousness* by means of what I call the '*mir*-variable' and the amount of *ad hoc* postulates that it contains. The value of this *mir*-variable corresponds to the degree of *miraculousness* needed for a theory to be predictively successful, such that the smaller its value, the more predictively successful it will be. I represent this in a formula, according to which the value of *mir*(H|E) *approximates* the degree of belief on the indicative conditional that 'H is false, if E is true' (i.e. E → ~H). In lay terms, this means that an agent has a degree of belief that 'a theoretical proposition is false, if an empirical proposition is true', which then means that the more this conditional holds (i.e. the more the empirical import supports the rejection of a theoretical proposition), the more *miraculous* the proposition will be.

The Bayesian rule of negation tells us that the credence on a proposition being false on the supposition that another proposition is true is equal to the distance of the credence on the first proposition being true on the supposition that the second one is also true, from the status of complete certainty (fixed at '1'). That is: $cr(\sim H|E) = 1 - cr(H|E)$. If we apply Bayes's theorem, this means that

$$cr(\sim H|E) = 1 - ((cr(E|H)\ cr(H))/cr(E))$$

As Michael Titelbaum (2022a) has explained, the credence on a proposition A, on the supposition that another proposition B is true, does not generally equal the indicative conditional 'if B, then A' (or 'if B, then ~A', in the case of *mir*). However, I follow Ernest *Adams's thesis*, which states that the *credence on an indicative conditional* ('if B, then A') *equals a conditional credence* of the form '*cr*(A|B)' (Adams 1975). This is justified by means of "'appropriateness' considerations" (p. 3), according to which it is *appropriate* to assert that '*cr*(B → A)' is equal to '*cr*(A|B)', to the degree that it "[assures] the soundness of [one's] reasoning [in accordance with the] desire to reach 'the right' conclusion by it"' (pp. 69-70).[15] For Adams, 'reaching the right conclusion' meant *arriving at the truth*, in the pragmatist

[15] This thesis (also known as 'Ramsey's test') was first proposed by Frank P. Ramsey (1931) and Richard Jeffrey (1965), and then revised by David Lewis (1976) and Frank Jackson (1987). More recently, it has been criticized by Alan Hájek (2012) and Igor Douven (2015). In response to this criticism, Michał Sikorski (2022) has proposed that an indicative conditional ('if B, then A') is assertable just in case that the corresponding conditional credence (*cr*(A|B)) is high and its antecedent is positively relevant for its consequent. This presumably means, for our purposes, that it should be possible to assert that 'E → ~H' equals *cr*(~H|E), only if *cr*(~H|E) is high and *cr*(~H|E) > *cr*(E). However, more work needs to be done in this respect.

sense of influencing our decisions and actions. In the context of my account of progress, this means that it is appropriate to assert that '$cr(E \rightarrow {\sim}H) = cr({\sim}H|E)$', to the degree that it enables future success. Then, the value of $mir(H|E)$ *approximates* the credence on this indicative conditional '$E \rightarrow {\sim}H$'. This is formally represented as:

$$mir(H|E) \approx cr(E \rightarrow {\sim}H) = 1 - ((cr(E|H)\, cr(H))/cr(E))$$

Finally, to compare theories, we look at the sum of their *mir*-values (i.e. those of their constitutive modules), as well as their amount of *ad hoc* postulates, in order to determine which has been more *miraculous* across the relevant span of time. This means that φ is more miraculous than ψ if the *mir*-value sum of its constitutive modules is higher, or if it contains more *ad hoc* postulates, or both, such that

The change from φ to ψ is progressive iff
a) $\text{mir}(\varphi|E) > mir(\psi|E)$,
where $mir(\varphi|E) \approx (cr(E \rightarrow {\sim}H_1) \text{ X } \ldots \text{ X } cr(E \rightarrow {\sim}H_k))$ &
$mir(\psi|E) \approx (cr(E \rightarrow {\sim}Q_1) \text{ X } \ldots \text{ X } cr(E \rightarrow {\sim}Q_k))$[16],
b) φ contains more *ad hoc* postulates than ψ, or
c) both (a) & (b).

Let us illustrate this by means of two examples:

Example 1: Let H stand for 'aether', Q stand for 'speed of light', and E stand for the empirical import at a given time. At t_1, E stands for the experimental observations prior to Michelson-Morley. We stipulate the numerical values for each credence distribution:

$cr_1(H) = 0.8$ (since 'aether' was broadly accepted prior to Michelson-Morley)
$cr_1(Q) = 0.2$ (since 'speed of light' was not broadly accepted)
$cr_1(E|H) = 0.8$ (by stipulation)
$cr_1(E|Q) = 0.8$ (by stipulation)
$cr_1(E) = 0.9$ (by stipulation)

Therefore, $cr_2(H|E) = 0.71$, which means that $cr_2(E \rightarrow {\sim}H) = 0.29$, such that $\boldsymbol{mir_2(H|E) \approx 0.29}$; and $cr_2(Q|E) = 0.1778$, which means that $cr_2(E \rightarrow {\sim}Q) = 0.8222$, such that $\boldsymbol{mir_2(Q|E) \approx 0.8222}$.

[16] This works only in case that the constitutive modules are independent (i.e. their truth-values are independent of each other), which is a (plausibly innacurate) simplification. Nevertheless, a more precise formalization is beyond the scope of this thesis, which is why I treat these constitutive modules as independent (for the sake of simplicity only).

This means that, prior to Michelson-Morley, ‘speed of light’ appeared more miraculous than ‘aether’. At t_3 (in the aftermath of Michelson-Morley), these values are updated by conditionalization, such that

cr_3(H) = 0.71 (following conditionalization)
cr_3(Q) = 0.1778 (following conditionalization)
cr_3(E|H) = 0.05 (since ‘aether’ makes Michelson-Morley highly improbable)
cr_3(E|Q) = 0.75 (since ‘speed of light’ makes it highly probable)
cr_3(E) = 0.5 (to allow for the possibility of experimental error)

Therefore, cr_4(H|E) = 0.071, which means that cr_4(E → ~H) = 0.929, such that ***mir*₄(H|E) ≈ 0.929**; and cr_4(Q|E) = 0.2667, which means that cr_4(E → ~Q) = 0.7333, such that ***mir*₄(Q|E) ≈ 0.7333**.

This means that, in the aftermath of Michelson-Morley, ‘aether’ appeared more miraculous than ‘speed of light’.[17] A change from φ (containing ‘aether’) to ψ (containing ‘speed of light’) is *pro tanto* prescribed at t_4, since it is progressive, given that mir(φ|E) > *mir*(ψ|E). This satisfies condition (a).

Example 2: In 1898, Wilhelm Wien introduced an alternative model of the aether (as a modified version of George G. Stoke’s (1848) hypothesis,[18] which postulated that the aether was *completely dragged* in and around moving matter). Wien’s model described the aether as being *completely dragged* by the movement of the Earth, since, according to him, this should be proportional to its gravitational mass. This would explain the null results of Michelson-Morley, since the aether would then be indetectable relative to the motion of the Earth.

[17] This of course would not work if Sikorski’s thesis (see footnote 15) is correct and adequate for quantifying success miracles, since both cr_4(H|E) and cr_4(Q|E) are lower than cr_3(E), and thus would not fit his criteria for the *appropriateness* of the assertability of either cr_4(E → ~H) or cr_4(E → ~Q). I do not think that the same criteria should apply equally to the quantification of success miracles. However, this discussion is beyond the scope of this thesis.

[18] This hypothesis was originally entertained by Augustin-Jean Fresnel (1818) as an attempt to explain why the stars do not look differently depending on the Earth’s rotation around the sun. Fresnel’s hypothesis, contrary to Stokes and Wien, was that the aether was only *partially dragged*, and only the *excess* aether (over and above the aether in a vacuum) could be *completely dragged* by a moving object. Fresnel was influenced by Thomas Young (1804) who proposed that “the luminiferous aether pervades the substance of all material bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees” (quoted in Schaffner 1972, p. 23).

However, decades later (in 1925), the Michelson-Gale experiment (named after A. A. Michelson and Henry G. Gale; first proposed by Michelson in 1904) was conducted. This was a modification of the original Michelson-Morley experiment to determine whether the motion of the Earth had an effect on the propagation of light around it. The experiment showed that an *aether effect* was perceptible through the rotation of the Earth, which contradicted Wien's model. Assuming an absolute reference frame (i.e. that of classical mechanics), this would be incompatible with the results of Michelson-Morley, which showed that the aether could not be moving relative to the Earth. Therefore, this proved that the aether postulate was *ad hoc*, and thus a *success miracle* (in my terminology). Since special relativity made no use of the aether in order to explain the propagation of light, it was less miraculous than those theories that did. This means that a change from φ (containing 'aether') to ψ (not containing 'aether') was *pro tanto* prescribed, since it is progressive, given that φ contains more *ad hoc* postulates that ψ. This satisfies condition (b).

What these examples show is that classical theories were more miraculous (than special relativity) because they a) were less empirically supported (i.e. had a higher *mir*-value), and b) required the introduction of more *ad hoc* postulates.

Now, there are two reasons for calling this account 'functional-externalist'.[19] First, it is *functional* because progress is defined as the fulfillment of a functional role (i.e. the minimization of *miraculousness*), which is also postulated as the aim of science, so that it can satisfy **D5**. Second, it is *externalist* because it assesses progress by means of something external to the community's assessment. That is, the degree of *miraculousness*. In this sense, it can be contrasted with the problem-solving approach, which is thought to be *internalist*

[19] This is not the only functional-externalist account. Shan (2019) proposed an account that defines 'progress' in terms of usefulness and the repeatable reliability of exemplary practices. Comparing and contrasting our accounts is nevertheless beyond the scope of this thesis.

(since, according to the literature, it assesses progress from a standpoint *internal* to the community).

Let us illustrate this difference in the context of overlooked progressive developments. According to my account, it still makes sense to claim that Michelson-Morley contributed to the fact that the change to relativistic mechanics was progressive, even if Einstein (or the broader scientific community) had been unaware of it at the time of his formulation of special relativity. All that matters is that the empirical import *provides a reason* to update the degree of *miraculousness*, in order to assess the change accordingly. It is not necessary for a scientific community to *actually* know this degree. The aim of minimizing *miraculousness* is the same regardless of the community's epistemic status. The reason encountered is a normative reason with counterfactual power, such that 'even if the community is ignorant of the degree of miraculousness, *they still ought to minimize it to the lowest degree possible (approaching an ideal zero-limit)*', because *were they to know it*, *they would aim to minimize it (rather than not)*. Therefore, since the aim of science is in accordance with this normative provision, my account can satisfy **D5**.

Lastly, I would like to react to one possible objection. There is a sense in which degrees of *miraculousness* seem reducible to degrees of *verisimilitude*. One reason for this is that, just as the verisimilitudinarian account postulates that there is a cognitive goal Ci (which stands for a complete description of the *unobservable*), my account postulates that there is an ideal limit towards which the *mir*-variable aims, which is the 'zero-limit'. One could argue that this ideal limit is reducible to the cognitive goal Ci. Consequently, the aim of science would be reducible to the aim of science in the semantic approach. Nevertheless, this objection misses the point that even if the aim of the *mir*-variable is an ideal limit that is reducible to a complete description of the *unobservable* (which I am not sure it is), the aim of science is not the same as the aim of the *mir*-variable. This is only part of the story. The aim of science is the minimization of *miraculousness*, whether this implies aiming at this ideal limit or restricting the amount of *ad hoc* postulates, or both.

However, the objector might still resist this answer, by arguing that my account comes too close to Popper's (1934/1959), and thus collapses into a kind of *fallibilism* that could be understood in the framework of the semantic approach. The reason for this objection is that Popper also introduced a normative restriction to the inclusion of *ad hoc* postulates, since he was concerned with the risk that these postulates could be included to prevent a theory's falsification. In response, he proposed that scientists *ought* to *not* include *ad hoc* postulates to prevent their theories from being falsified. Popper's notion of falsification was introduced as a demarcation criteria between science and pseudo-science. For him, the exclusion of *ad hoc* postulates was not merely a maxim for good scientific practice, but rather it excluded their use as a legitimate mechanism to prevent falsification. He faced some backlash for this, since it also ruled out some examples of (what is considered) legitimate scientific practice. My account is not vulnerable to this criticism, since I merely identify the aim of science with the minimization of *miraculousness* to the largest extent possible (i.e. approaching the ideal 'zero-limit'). This does not rule out the inclusion of *ad hoc* postulates into scientific theories. My account allows for the possibility that successful theories might include *success miracles*, insofar as they do not hinder the theories' success. Falsification does not play such a key role, as it does for Popper.

However, this means that my account still comes too close to Lakatos's (1978/1989) (rather than Popper's), in the sense that propositions in the so-called 'protective belt' (which corresponds to the modules that exceed the core of perspectival assumptions) can be modified, and *ad hoc* postulates can be added, so as to make a theory more compatible with the empirical import. However, the aim of science should still be to minimize the amount of these claims and postulates to the extent possible, just in case that their *mir*-value is too high or they are *ad hoc*. This implies an important difference between my account and Lakatos's, since I propose that the assessment of progress is done at the level of theory changes (focusing on the relevant constitutive modules and their *mir*-values or *ad hoc*-status), rather than at the level of whole research programmes or perspectives.

CONCLUSION

In this thesis, I presented a new account of scientific progress, according to which, science progresses by minimizing the amount of *success miracles*. These are unlikely, unreliable claims and *ad hoc* postulates that are necessary for a theory to be predictively successful in light of the empirical evidence available to a scientific community at a given time. I used a perspectival modular framework, according to which theories are sets of propositions within a perspectival modular structure. Then, I developed a formula for quantifying these claims and postulates, enabling the assessment of progress by comparing theories within sequences of theory changes. Finally, I evaluated this account with respect to the same desiderata that I used to evaluate the most prominent contemporary accounts of progress. I argued that my account is able to improve upon them and successfully satisfy these desiderata.

In an attempt to position myself within the broader historico-philosophical debate on the problem of progress, I argued that although there are some similarities with Popper's, Kuhn's and Lakatos's accounts (as well as those of their followers), there are some important differences that make my account a novel alternative to consider. As such, I hope it can contribute to a deeper understanding of scientific advancement and its evaluation.

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ANALYTIC VOCABULARY

Bayesianism

- Adam's thesis
    - Appropriateness considerations
- Credence distribution
    - Conditional credences
    - Problem of priors
        - Hypothetical prior distributions
    - Propositional attitude
    - Unconditional credences

Epistemic approach

- Approximate propositions
    - Incomplete reference
- Explanatory schemata
- Explanatory goodness
- Inference to the only explanation
- Principle of humanity
- Principle of rationality
- Reference potentials
- Reality
    - Ideal system
- Well-founded true beliefs

Functional-internalist/problem-solving approach

- Internalism
- False problem
- False solution
- Scientific paradigms
    - Crises
    - Normal science
    - Scientific revolutions

Functional-externalist and perspectival account

- Minimization of *miraculousness* thesis
    - *Success miracles*
        - *Ad hoc* postulates

            - *mir*-variable
    - Scientific perspectives
        - Epistemic standards
            - Intersubjective commitments
        - Modular structures
            - Interpretative schemes
            - Symbolic universe
            - Well-established theoretical modules
        - Uniqueness of the world methodological maxim
            - Making up the unique causal structure

Classical mechanics

- Aether postulate
    - Aether drag
    - Aether effect
- Lorentz transformation
- Luminiferous aether
    - Aether postulate
    - Aether drag
    - Aether effect
- Maxwell's equations

Noetic approach

- Idealization
- Understanding
    - Accuracy
    - Comprehensiveness
    - Grasping

Problem of the assessment of progress

- Empirical import
- Observable phenomena
- Problems of progress
    - Normative problem
    - Pessimistic meta-induction
    - Problem of incommensurability
    - Problem of non-miraculous success

        - Scientific success
    - Problem of theory-ladenness
    - Problem of underdetermination of theory by evidence (Duhem-Quine thesis)
- Progress with respect to theory change
- Unobservable entities and properties

Semantic approach

- Conceptual framework
- Fallibilism
    - Falsifiability
    - Informativeness
    - Research programmes
        - Protective belt
- Inference to the best explanation
- Optimistic meta-induction
- Truthlikeness
- Verisimilitude

Special theory of relativity

- Einstein's two postulates
- Michelson-Morley experiment
- Newtonian limit