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ARTICLE

Theoretical Stability and Scientific Realism

Johannes Nyström

Stockholm University, Department of Philosophy, Stockholm, Sweden
Email: johannes.nystrom@philosophy.su.se

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Abstract

The frequency of major theory change in natural science is rapidly decreasing. Sprenger and Hartmann (2019) claim that this observation can improve the justificatory basis of scientific realism, by way of what can be called a *stability argument*. By enriching the conceptual basis of Sprenger and Hartmann's argument, this article shows that stability arguments pose a strong and novel challenge to scientific anti-realists. However, an anti-realist response to this challenge is also proposed. The resulting dialectic establishes a level of meaningful disagreement about the significance of stability arguments for scientific realism and indicates how the disagreement can ultimately be resolved.

1. Introduction

Scientific realists have recently sought novel ways to complement the main justificatory basis of their position, the no miracles argument (NMA), which holds that the only plausible explanation for the predictive success demonstrated by science is that predictively successful theories are approximately true (Putnam (1975); Boyd (1984); Smart (1985)). In light of the conclusion that rationally subscribing to realism on the basis of the NMA is contingent on a fairly strong conviction about the position's prior plausibility (Howson (2000); Magnus and Callender (2004)), realists have, in particular, tried to identify strategies that supplement the NMA with some further independent support for their position. One interesting strategy of this kind asserts that the *stability* of scientific theories can be used in support of realism. The core claim of the strategy may be stated as follows. Periods of theoretical volatility indicate that the constraints on theory building are weak and that abundant alternative theoretical structures are available to scientists. On the other hand, a period of theoretical stability, like the current stage of theory assessment in science, indicates that viable scientific theories are a scarce good. In this context, the epistemic status of one's theory at hand can be considered strong.

Fahrbach (2011*a*) was the first to propose an explicit argument for scientific realism based on theoretical stability.[1] Building on Fahrbach's idea, Sprenger (2016)

[1] See Wray (2013) for criticism of Fahrbach's argument.

and Sprenger and Hartmann (2019) carry out the currently most elaborate attempt to construct a stability argument, and they also formalize the argument in Bayesian epistemology. Sprenger and Hartmann conclude that their argument significantly improves the dialectical situation for scientific realism: "Observations of theoretical stability over an extended period of time can greatly increase the range of prior probabilities for which the NMA leads to acceptance of [a theory]" (Sprenger and Hartmann (2019), 100). Despite this promise, their argument has not attracted much attention in the literature. One initial diagnosis of this situation is that disagreement about the significance of theoretical stability is already well understood as disagreement about the significance of the historical record. The anti-realist charges the realist with overemphasizing the weight of the evidence amassed in favor of the current scientific description of the world, and the realist charges the anti-realist with overemphasizing the weight of the historical evidence against it. Ultimately, the anti-realist can account for a period of stability by pointing at the constraints imposed on theory building by the current scientific paradigm (Kuhn (1962)) and unconceived alternatives (Stanford 2001, 2006).

The aim of this article is to advance discussions on theoretical stability and scientific realism in two significant steps. First, it is argued that although Sprenger and Hartmann's original argument could plausibly be rejected on the basis of the described dialectic, an *enriched* version of their argument can be constructed that significantly improves it with respect to this problem. The enriched stability argument suggests that periods of theoretical stability can have significant epistemic implications with respect to scientific realism if they are situated at the end point of a consistent pattern of theory change. In that context, the surprise value of a stable period calls for an explanation of stability that goes beyond standard accounts of scientific theory building. Scientific realism may be offered as an explanation of that kind.

The second step is the construction of a possible response to this argument by the anti-realist. The response involves another nonstandard explanation of theoretical stability that does not imply realism. The anti-realist may argue that modern science finds itself in a quite special situation as a result of the uniquely large degree of empirical success demonstrated by scientific theories in the 20th and 21st centuries. In this situation, a conservative research strategy that sticks with the established theoretical framework(s) may typically look like the best option for realizing the core aims of science. Hence, given the exceptional success of the current scientific picture of the world, the stability that is characteristic of modern science is no surprise at all.

The resulting polemic between these two rival explanations of theoretical stability shows that stability arguments actually offer a progressive take on the realism debate that cannot be sorted under classical categories of disagreement. An enriched stability argument engages the historical record of theory change in *favor* of scientific realism and is countered by an anti-realist line of reasoning that points to the exceptional degrees of predictive success of our current best scientific theories. This polemic also implies that advocating for a nonconservative research strategy can be a good option for boosting trust in scientific realism.

The plan of the article is as follows. In section 2, Sprenger and Hartmann's stability argument in favor of realism is presented. The argument assumes a rational expectation on scientists to find and develop alternative theories within a roughly specified time frame, if such alternatives exist at all. In section 3.1, two core concerns about this assumption are raised on the basis of classical anti-realist perspectives on scientific theory building. It is then argued that enriching their argument by invoking a consistent prior record of theory change can act as a cogent and strong justification for an assumption of this kind and that enriching the argument in this way provides an empirical basis for a significant stability argument.

In section 3.2, an anti-realist response to the enriched argument is proposed. The response is based on an alternative explanation of stability that, even given a prior record of theory change, does not imply scientific realism. The suggested explanation is based on the concept of theoretical conservatism (Stanford (2019)) but provides a plausible general reason for assuming high degrees of conservatism in the current period of theory assessment in science: the exceptional degrees of predictive success demonstrated by our current best scientific theories. In section 3.2.2, Newtonian mechanics is employed as a case study in support of this alternative explanation. It is suggested that theory assessment in physics in the 18th and 19th centuries, in one important, respect resembles the situation today in many scientific disciplines. The dominant theoretical framework is exceptionally successful compared with earlier theories in the field, and this success may in part explain the long-term stability of that framework through the adoption of a fairly conservative research strategy. Section 3.2.3 refers to Fahrbach's scientometric investigation of scientific activity (Fahrbach (2011*a*)) in order to motivate the claim that current scientific theories are, by and large, indeed exceptionally successful.

In section 3.3, the suggested anti-realist response is given a formal interpretation as a constraint on a probability distribution in Sprenger and Hartmann's model of a stability argument. It is shown that this constraint strongly weakens the argument, even on otherwise generous concessions to the realist. Hence, significant disagreement about the significance of stability arguments can be established on the basis of this response. The two-step contribution of this article thereby establishes a meaningful basis for further discussions on theoretical stability. In section 3.4, some implications of the results for the recent debate on theoretical conservatism in science are discussed. Section 4 sums up the main results of the article and clarifies the current status of stability arguments in light of its contributions.

2. Sprenger and Hartmann's stability-based no-miracles argument

2.1. The conceptual framework

Sprenger and Hartmann's (henceforth, *SH*) stability argument is based on a meta-level observation about a scientific discipline. They consider a scientific theory T stable if and only if (iff) there is an *absence of alternative theories* to T in the discipline that persists over a long period of time (SH propose that 30 to 50 years is an adequate measure, but this specific choice plays no decisive role in their argument). SH claim that the failure of scientists in the discipline to come up with and develop rivaling theories in this time indicates that there is a significant scarcity of possible alternative theories or even that there are no possible alternatives at all. On this basis, they then conclude that the epistemic status of a stable theory can be considered significantly improved.

SH understand a possible alternative to an established theory T to be a theoretical structure (conceived or unconceived) that (i) satisfies a set of (context-dependent) theoretical constraints, (ii) is consistent with available evidence, and (iii) makes distinguishable predictions for some future observation(s). Crucially, they do not consider theoretical structures that are empirically equivalent to T as alternatives because they do not satisfy (iii). For this reason, SH do not take their stability argument to bear *directly* on the question of whether or not T is approximately true.

The reason that they nevertheless take the argument to bear on the question of scientific realism is connected to their understanding of the NMA, which they construe as a two-step argument. First, T's predictive success is taken to indicate that T is empirically adequate (i.e., consistent with all possibly collectible data). Second, T's empirical adequacy is taken to indicate that T is true. Although both steps are necessary for accepting the full realist conjecture, justifying the first step of the NMA is already an important result for the realist and is in line with a realist interpretation of T. However, this step is far from trivial. According to SH, the pessimistic meta-induction (PMI) (Laudan (1981)) is already a threat at this level: "Laudan's PMI … teaches us that there have often been false theories that explained the data well (and were superseded later). In other words, empirically non-adequate theories can be highly successful" (Sprenger and Hartmann (2019), 86).

In light of the PMI, amassing additional evidence of T's empirical adequacy is of crucial importance for justifying the first step of the NMA. According to SH, T's stability over time may act as support of this kind. Call this argument the *stability-based NMA*.[2]

The stability-based NMA can be considered a synthesis of the NMA and the so-called no-alternatives argument (NAA) formalized and discussed by Dawid et al. (2015). However, there is a crucial difference between what one may call the "classical" NAA presented there and the version of NAA that SH deploy in the stability-based NMA. The core role of a classical NAA is to generate trust in a theory in the absence of relevant empirical data. It is based on the observation that despite significant efforts, scientists have failed to come up with alternatives to the theory at hand. The argument infers from that observation that the spectrum of theories that makes distinguishable predictions within the current empirical horizon is probably fairly limited. Hence, the classical NAA addresses a *local* spectrum of alternatives and is therefore an argument in favor of the *viability* of a scientific theory, that is, its empirical adequacy within the current empirical horizon.

In SH's stability-based NMA, the NAA is deployed to strengthen a realist interpretation of an already empirically well-confirmed theory. To this end, it is deployed as an argument in favor of the unrestricted empirical adequacy of T and therefore addresses the *global* spectrum of alternatives. This version of the NAA is consistent with the core epistemic mechanism of the classical NAA and can therefore be considered an extension of its scope toward the question of scientific realism. The extension is interesting and is in line with interpreting scientific realism as a meta-level hypothesis about the scientific research process that makes a wide range of

[2] "The NMA" and "the stability-based NMA" will henceforth be taken to refer to the first inferential step of the full two-step NMA structure, as construed by SH.

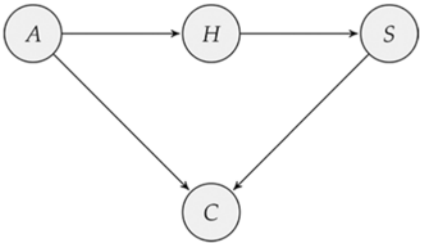

Figure 1. SH's Bayesian network representation of the stability-based NMA.

observational predictions about that process rather than only as a post hoc explanation about an instance of predictive success.

2.2. The Bayesian model

SH model the stability-based NMA with the help of propositional variables *H, S, C, A*:

H := T is empirically adequate.
¬ H := T is not empirically adequate.

S := T is predictively successful.
¬ S := T is not predictively successful.

C := T is not stable (scientists have developed an alternative to T).
¬ C := T is stable (scientists have not developed an alternative to T).

A_j:= There are exactly *j* possible alternatives to T that (i) satisfy the relevant theoretical constraints, (ii) are consistent with currently available data, and (iii) give distinguishable predictions for some set of future measurements or observations.

Next, SH make the following assumptions, which they consider "equally plausible for the realist and the anti-realist" (Sprenger and Hartmann 2019, 91):

B1: The variables satisfy the (conditional) independencies in the Bayesian network structure of figure 1.
B2: If T is empirically adequate, it will succeed: $p(\mathrm{S} \mid \mathrm{H}) = 1$.
B3: T is not more or less probable to be empirically adequate than any other possible alternative theory that is consistent with available evidence: $p(\mathrm{H} \mid A_j) = 1/(j+1)$.
B4: The probability that T is stable over time is a decreasing function of the number of possible alternatives to T:[3,4]

[3] SH first propose the set of conditional probabilities $p(\neg \mathrm{C} \mid \mathrm{A}_j, \mathrm{S}) := 1/j+1$, but they concede that this choice can be considered realist biased and instead propose the set given by equation (1). Because equation (1) is more neutral and more flexible, the argument discussed here is based on equation (1).
[4] α is supposed to measure the degree of scientific conservatism in the discipline and is set $\alpha = 4$ in SH's model, but it may be varied according to circumstance. More on this later in the article.

$$p(\neg C|A_j, S) = e^{-\frac{1}{2}\left(\frac{j}{\alpha}\right)^2}. \tag{1}$$

B5: The probability that there exist (additional) alternatives to T is the same no matter how many alternatives to T are already known to exist:[5]

$$p(A > j| \bigvee_{k=j}^{\infty} A_k) = p(A > j+1| \bigvee_{k=j+1}^{\infty} A_k) \text{ for all } j \geq 0. \tag{2}$$

Two probabilities are left unspecified by SH's assumptions and are considered free parameters of the argument: $p(A_j)$ and $p(S \mid \neg H)$. The distribution across $p(A_j)$ reflects one's prior opinion on the number of alternatives to T. Given **B3**, this prior belief determines the core prior $p(H)$ and is therefore considered a free parameter. $p(S \mid \neg H)$, that is, the probability that T is successful given that T is not empirically adequate, is considered controversial on the basis of the PMI and is therefore difficult to numerically constrain.

This situation leads to a well-known problem for the classical (non-stability-based) NMA. The inequality $p(H \mid S) > f$, where f is some relevant threshold like 0.5, can only be inferred given that values for these parameters are chosen carefully. In other words, the classical NMA only convinces those who take H to be sufficiently probable already to begin with (Howson (2000); Magnus and Callender (2004)). However, SH's stability-based NMA significantly improves the realist's situation with respect to this problem, as may be demonstrated by an evaluation of the argument:

$$\begin{aligned} p(H|\neg C, S) &= \frac{p(\neg C, S, H)}{p(\neg C, S)} \\ &= \frac{\sum_{j=0}^{\infty} p(A_j) p(\neg C|A_j, S) p(S|H) p(H|A_j)}{\sum_{j=0}^{\infty} p(A_j) p(\neg C|A_j, S)\left(p(S|H) p(H|A_j) + p(S|\neg H)(1 - p(H|A_j))\right)}, \end{aligned} \tag{3}$$

where $p(A_j)$ can be expressed (via **B5**) as a function of $p(A_0)$ (Sprenger and Hartmann 2019, 102–103):

$$p(A_j) = p(A_0)(1 - p(A_0))^j. \tag{4}$$

B1–**B5** then generate the following expression for the conditional probability of H, given ¬C and S, evaluated in figure 2:

$$\frac{\sum_{j=0}^{\infty} e^{-\frac{1}{2}\left(\frac{j}{\alpha}\right)^2} \frac{1}{j+1} \left(p(A_0)\left(1 - p(A_0)\right)^j\right)}{\sum_{j=0}^{\infty} e^{-\frac{1}{2}\left(\frac{j}{\alpha}\right)^2} \left(p(A_0)\left(1 - p(A_0)\right)^j\right)\left(\frac{1}{j+1} + p(S|\neg H)\left(1 - \frac{1}{j+1}\right)\right)}. \tag{5}$$

[5] SH interpret this assumption as follows: "finding an alternative does not, in itself, raise or lower the probability of finding another alternative" (Sprenger and Hartmann (2019), 91). However, this assumption is not fundamentally about the probability of *finding* alternatives, as no reference is made to *C*, but rather about the *existence* of alternatives. Hence, a more formally accurate formulation is chosen here.

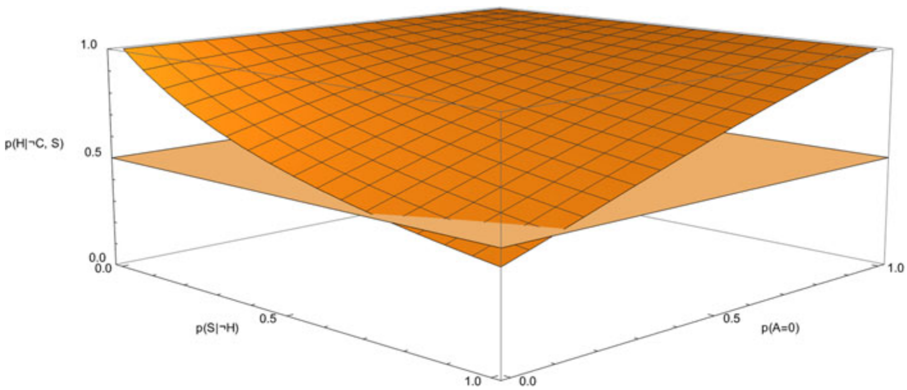

Figure 2. The results of SH's stability-based NMA, with $\alpha = 4$, contrasted with hyperplane $z = 0.5$.

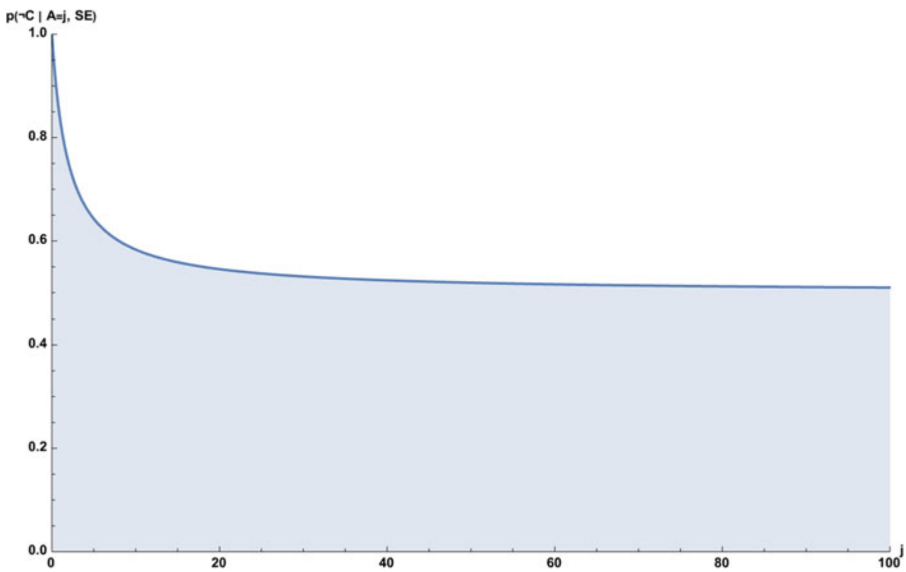

Figure 3. A graphical visualization of equation (7) with a visual cutoff at $j = 100$.

The results of the stability-based NMA may be understood to generate a reversal of the situation with respect to the described problem of a prior commitment to realism. This argument only supports anti-realism if one very carefully chooses parameter values that retain an anti-realist position. In other words, the stability-based NMA only fails to convince the strong skeptic who considers the hypothesis that T is empirically adequate to be highly improbable to begin with.[6]

[6] It is pertinent to note that although the stability-based NMA has a wider scope than the classical NMA, it does not solve the fundamental problem identified by Howson (2000)—namely, that the validity

3. On the significance of stability arguments

3.1. Stability and the historical record of theory change

The driver of the improved results of the stability-based NMA is SH's assumption B4. According to this assumption, a period of theoretical stability in a scientific discipline should only be expected given that the number of possible alternative theories in the discipline is very small. However, this is precisely the kind of assumption that anti-realists may take issue with on the basis of their understanding of the process of scientific theory building. When faced with a period of theoretical stability, they may rather conclude that scientists are not sufficiently cognitively and technically competent (with respect to mathematical machinery, for example) to come up with an alternative at all and that the relevant alternatives therefore remain unconceived (Stanford (2001), (2006)). Call this the problem of *cognitive capacity*. Another common explanation of stability associated with an anti-realist perspective on theory building is that scientists are working under the strong constraints of the current scientific paradigm (Kuhn (1962)) and therefore do not spend much resources on (genuinely) exploratory theorizing or that they simply reject rival theoretical frameworks before they have the chance to develop into fully fledged alternatives. Call this the problem of *conservatism*. In this light, SH's claim that their assumptions are neutral with respect to the realist and anti-realist alike is questionable.[7]

Now, it is true that SH only offer what they call a "possibility result" (Sprenger and Hartmann (2019), 89), and they state that one condition on the improved results of the stability-based NMA is that the scientific discipline in question is not too conservative and that the α parameter (see equation [1]) therefore takes a small value. In conservative disciplines, which correspond to a larger value of α, stability will be expected even in light of a large number of possible alternatives, and therefore, "stability is the default state of [the] discipline and will not strongly support the NMA" (Sprenger (2016), 185). However, SH do not propose any criteria that may be used to identify conservative (or cognitively limited) disciplines. In the absence of criteria of this kind, it is not clear whether and how their possibility result can be realized. Hence, the argument is at risk of being sorted to already existing disagreements between realists and anti-realists about the process of theory building in science, and a relevant question is therefore whether SH's argument can be understood to improve the realist's dialectical situation at all.

A core claim of this article is that a general criterion of the described kind can be identified. This criterion is based on the brief but interesting remark of SH that in conservative disciplines, stability is the *default state*. The function of this statement is to stress that observations of stability are not in need of any special explanation in disciplines that are conservative. As such, it reinforces SH's claim that they provide a possibility result. A stronger interpretation of this remark, however, is to take it to

of the argument is contingent on a specific set of prior probabilities that are not constrained by the core premise of the NMA. As modeled by SH, this is still true for the stability-based NMA.

[7] A similar problem befalls the classical NAA (Dawid (2013); Menon (2019)). Classical NAA can be strengthened in light of this problem by a meta-inductive argument from the (local) viability of earlier theories in the research field (Dawid (*Forthcoming*)). However, this strategy does not work in the context of the stability-based NMA because the latter addresses the unrestricted (global) empirical adequacy of T.

identify conservative disciplines with those that do not have a *demonstrated history of theory change*. Exactly in disciplines without a history of theory change, stability is default. Indeed, adopting this criterion is a natural strategy for the proponent of a stability argument because it also addresses the problem of cognitive capacity. In disciplines with a demonstrated history of theory change, scientists were, at previous points, sufficiently cognitively competent to develop alternative theory.

The general importance of this criterion may be emphasized by considering two different ways to understand stability arguments. In a naive understanding, a stability argument points to an observation of stability and asserts that realism can explain it. The issue with this understanding is that the assertion that an observation of stability is consistent with scientific realism is of marginal importance on its own. What conclusions are drawn on the basis of that observation will be determined by one's background beliefs. Are scientists in the given field cognitively and technically competent? Are they actively trying to develop alternatives? If the answer to those questions is no, or if there are no relevant data in support of a specific answer, observations of stability may be considered trivial and hence cannot underwrite a strong argument in favor of realism. In a more sophisticated understanding, the argument identifies an observation of stability as *unexpected* relative to background beliefs and asserts that those beliefs therefore fail to explain it. Only at this stage is realism proposed as an explanation. This understanding may in principle lead to a strong argument.

Pointing at a historical record of theory change, as suggested here, offers a cogent basis for putting forward the latter kind of argument. Because the current period of theory assessment in many scientific disciplines is situated at the end point of a fairly consistent pattern of theory change, this pattern can be inductively projected into the future. In other words, a historical record of theory change underwrites the belief that scientists are typically cognitively capable *enough*, and typically liberal *enough* with respect to allocating resources to exploratory theorizing, to find and develop alternative theory within some (roughly) specified amount of time. Seeing this pattern broken in the current period of theory assessment is therefore genuinely surprising. The surprise value of that observation may act as the basis of a significant stability argument. Call this an *enriched* stability argument.

The enriched argument provides an empirical basis for assuming a fairly high degree of theoretical creativity in a scientific discipline and therefore offers a plausible justification for **B4**. According to SH's formalism, this amounts to assuming a small value for the parameter α such that the conditional probabilities $p(\neg \mathrm{C}|A_j, \mathrm{S})$ decrease quickly in j (see equation [1]). Conceptually, this means to assume that observations of stability would be surprising if a large number of possible alternatives were in principle available to scientists at the current stage of theory assessment. Hence, the surprise value of an observation of stability can be naturally interpreted as being based on a probability distribution of that kind. Of course, the exact specification of this distribution will vary depending on context and prior beliefs. Whether or not this distribution is sufficient, given some specified set of priors, for generating posteriors that support a realist position will depend on the specifics of each situation and therefore lies beyond the scope of this article. The core function of relying on an enriched stability argument is to adopt an argument structure where the expectation on scientists to find and develop alternative theories within a roughly specified time frame can in principle be provided an empirical basis.

The enriched argument also involves a better-motivated choice of time frame for assessments of theoretical stability. SH's choice of 30 to 50 years is based on concerns about unfit theories being artificially sustained: "to rule out preservation of a theory by a series of degenerative accommodating moves, [stability] should be evaluated over a longer period, e.g. thirty to fifty years" (Sprenger and Hartmann (2019), 90). However, this choice is fairly arbitrary and, moreover, does not take into account the specific context within which the stability claim is put forward. The enriched argument instead relativizes the time frame across which stability is assessed to the historical record of theory change in the discipline. If earlier periods of theory assessment all lasted well over 50 years before a rival theory was developed, concluding that a current theory is stable just because it survived for 50 years fails to underwrite the claim that the lack of alternatives is surprising. Only when a clear discontinuity with respect to the historical pattern of theory change is observed can the enriched argument get off the ground.

The plausibility of the enriched stability argument may be motivated by general considerations in the history of science that support the claim that theory change has historically been a fairly frequent occurrence. Some versions of the PMI may lend themselves well to this task; Stanford's record of theory change in physics, chemistry, medicine, and biology (Stanford 2001) and Laudan's list of theories discarded in instances of theory change (Laudan 1981) are promising candidates.[8] Both Stanford and Laudan take their lists to be representative of "a seemingly endless array of theories" (Stanford (2001), 9) that "could be extended *ad nauseum*" (Laudan (1981), 33).

Although these considerations motivate the initial plausibility of enriched stability arguments, realists who want to rely on an argument of this kind are still tasked with providing an assessment of their chosen discipline or research field that supports the claim that this field exhibits a consistent historical pattern of novel theory building. This is a nontrivial task for several reasons. Theories are seldom developed as a complete and individuated package and presented to the scientific community at a given time; they may be better understood as constructed over a long series of steps from a core concept to a fully developed theory. Hence, a clear pattern of novel theory building may be difficult to describe in terms of definite temporal units. Furthermore, theory change may occur in one subdomain of the discipline while stability is maintained in another. However, the enriched stability argument does not imply that the realist must be in the position to provide a complete account of the history of the relevant discipline but rather that an overall appraisal of the history of that discipline should justify the conclusion that there has been a fairly consistent pattern of novel theory building. Of course, whether or not that appraisal in fact supports that conclusion or not is still an important and complex empirical question.

Furthermore, even if realists are able to provide an assessment of the described kind, a critic may still point at the complex web of social and institutional factors in play in scientific theory building and hold that those factors must be taken into consideration when projecting past theoretical creativity toward the future.[9] For

[8] Lyons (2002) and Vickers (2013) make substantial additions to Laudan's list.

[9] Thanks to an anonymous reviewer for this journal for pressing this point.

example, realists may argue that their meta-level assessment of the research strategy in a given discipline motivates ascribing a small probability of a theoretical shift in the near future, even if the discipline in the past has instantiated a fairly consistent pattern of theory change.[10] On the one hand, an enriched stability argument is based on a finite series of prior instances of theoretical creativity and is therefore structurally incapable of responding to this argument. Hence, this strategy is always a live option for the critic. On the other hand, it is based on a description of a research field that must be empirically motivated on a case-by-case basis. In this light, proponents of an enriched stability argument may just suggest that in the absence of a strong motivation of this kind, the statistical basis of their argument is sufficient to justify a future projection of theory change.

3.2. Predictive success and theoretical conservatism

In the previous section, an enriched stability argument in favor of scientific realism was described. The argument claims that observations of stability become epistemically significant with respect to realism when theory change is expected based on a historical record of theory change. As proposed earlier, the anti-realist may reject this expectation on the basis of a discipline-specific assessment of the factors at play in theory building in the discipline. However, the question of whether the anti-realist can also give a more general explanation of the current period of stability in science that is consistent with the anti-realist position arises. In other words, is there some *structural* explanation of theoretical stability that (i) cannot be rejected by pointing at the historical record of theory change but that (ii) does not speak strongly in favor of realism? The aim of this section is to show that an explanation of exactly this kind can be provided.

The suggested explanation is based on the concept of theoretical conservatism but provides a strong and highly general basis for assuming high degrees of conservatism in the current scientific landscape. This explanation can be constructed as a three-step line of reasoning. First, a correlation between predictive success and theoretical conservatism is asserted. Hence, large degrees of predictive success in a scientific discipline are accompanied by an expectation that the discipline will tend to constrain the distribution of resources for exploratory theorizing. Second, it is claimed that this correlation is substantial. In effect, theories in that discipline will be expected to be stable. Finally, the point is made that because the currently endorsed theories in science enjoy significantly higher degrees of predictive success compared with theories of the past, today's scientific disciplines are probably much more conservative, and the stability of the current stage of theory assessment is therefore not very surprising. On this basis, the anti-realist then concludes that this stability cannot underwrite a strong argument in favor of scientific realism.

Now, granting strongly increasing degrees of predictive success may look to the anti-realist like conceding too much to the realist. However, if realists are forced to retreat back to classical NMA-type reasoning, they are no longer able to point at an additional independent line of justification for their position, which is the core role of

[10] For example, realists could point at peer-review bias (Lee et al (2013)), an aversion toward risky research (Braben (2004)), and a structural inability to identify innovative research proposals (Luukkonen (2012)).

a stability argument. Asserting a strong correlation between predictive success and stability is therefore an effective counterargument to the enriched stability-based NMA, as will be demonstrated formally in section 3.3. Call the problem identified by this line of reasoning the *new problem of conservatism*. The following three sections flesh out and further motivate the problem.

3.2.1 Professionalization, peer review, and hierarchy

Recently, Stanford (2019) has argued for the following conclusion: "Today's scientific communities are almost certainly more effective vehicles for testing, evaluating, and applying theoretical conceptions of various parts of the natural world than were their historical predecessors, but ... we have compelling reasons to believe that they are actually less effective than those same predecessors in conceiving, exploring, or developing fundamentally novel theoretical conceptions of nature in the first place" (Stanford (2019), 3931).

Stanford identifies three core reasons for taking this claim seriously. First, the professionalization of natural science has led to stronger constraints on what kind of research questions are deemed genuinely scientific because those constraints delimit science as a professional enterprise. Today, departure from those constraints comes with the risk of losing one's livelihood. Second, securing funding for research projects today typically requires approval from a committee of peers who assess the proposal from the perspective of theoretical orthodoxy. The scarce resources available in a scientific discipline encourage those committees to prioritize research that already, from the start, is understood to probably generate progress, even if minor, over risk-taking exploratory theorizing. Finally, the hierarchical structure of modern science ensures that research is conducted primarily under the leadership of senior researchers. Therefore, creativity and intellectual independence are constrained by existing frameworks for conducting research, both by content and method. Skills passed on to younger generations of scientists are specifically designed to prepare them for careers in the established institutional and theoretical context.

These points suggest that today's scientific communities are quite conservative with respect to allocating resources for exploratory theorizing. However, it is not clear to what extent they identify a clear discontinuity with respect to earlier periods of theory building in science. It seems equally plausible to assume that the professionalization of science has been a gradual process over a long period of time; that peer review has, in some form, always been a core part of the scientific process; and that the process of preparing younger generations of scientists for independent research is almost by definition constrained by the current understanding of the theoretical and methodological situation. Moreover, it may be equally plausible to claim that the current stage of theory assessment in science allocates *more* resources to exploratory theorizing, simply because of the sheer growth in the size of the scientific enterprise (Forber (2008); Godfrey-Smith (2008)). On its own, Stanford's claim therefore does not seem to be a sufficient reply to the realist who takes the stability of the current stage of theory assessment in science to be genuinely surprising.

One way to strengthen Stanford's argument in light of this complexity is to identify some feature of the current period of theory assessment that can (i) be connected to conservatism but (ii) was not prominent during past periods of instability. In other words, is there some reason to believe that conservatism has strongly accelerated during the current period of theory assessment in science?

Pointing at the uniquely large degrees of predictive success exhibited by currently endorsed theories in science offers a cogent way of identifying a feature of this kind. This claim is based on the idea that the level of conservatism of a scientific discipline varies with the degree to which that discipline builds theory that can satisfactorily account for the continuous assembly of empirical data. Hence, the accelerated development of the scientific institutions that foster conservatism—professionalization, peer review, and hierarchy—are at least partly the result of the high success rate of scientific theory in the last century rather than simply the natural progression of a maturing scientific enterprise or sociological degeneration.

In fact, this claim may be understood to follow directly from the mission statement of natural science. One of its main goals is to construct empirically successful theories. Strategies that propel science toward that goal are understood to be fruitful and productive. Hence, if currently endorsed theories are persistently successful with respect to that goal, the risk/reward calculation of exploratory theorizing in an environment with limited resources will naturally favor theories that are already successful. The same understanding implies that if the theories currently endorsed in science had been less successful in accounting for empirical data, the same disciplines would have been less conservative. In that context, the risk/reward calculation looks quite different.

3.2.2. Theoretical stability in Newtonian mechanics

The new problem of conservatism may also be supported by an example from the history of natural science. Newtonian mechanics (NM) is widely considered a cornerstone of anti-realist rejections of the NMA (e.g., Laudan (1981); Vickers (2013)). Despite being tremendously successful, NM eventually proved incapable of accounting for the continuous assembly of empirical data and was eventually replaced by general relativity. Hence, it nicely captures the intuitive and formal motivation behind rejecting the classical NMA. SH note that NM also constitutes a (probabilistic) counterexample to the stability-based NMA because it was a highly stable (according to their definition) and predictively successful theory that was nevertheless eventually proven empirically nonadequate (Sprenger and Hartmann (2019), 89). Proponents of an enriched argument fare better with respect to this problem because they must not assume that realism provides a good explanation of the apparent stability of NM. Such proponents may claim that in the absence of a consistent prior pattern of theory change in physics, cognitive limitations and conservatism are better explanations of this stability. However, in conjunction with the new problem of conservatism, NM also presents a problem for the enriched argument.

The theoretical situation in physics in the 18th and 19th centuries may, in one important respect, be understood to resemble the situation of the current period of theory assessment in science. The acceptance of NM was motivated by its unparalleled empirical track record. The theory was far more successful than any other theoretical structure, past or present, known at that time, in particular with respect to its novel predictions. The existence of Neptune, the return of Halley's Comet, and Earth's oblate shape (Lyons (2002)), to name a few of those predictions, reassured physicists that the NM paradigm would continue to deliver successful predictions. Hence, empirical anomalies were not typically treated directly as reasons to reject NM and look for alternatives; instead, they mainly invoked solutions within the framework of NM. Some solutions of that kind were successful, whereas others, like Le Verrier's hypothesized bodies of mass between the sun and Mercury being responsible for the anomalous advance of the perihelion of the latter, were not. However, the continuous lack of progress on the latter issue was not taken as a reason for major revision of the research strategy; rather, it provoked more and more elaborate solutions within the existing theoretical framework, and much theoretical and observational work was being devoted to solutions of that kind (Baum and Sheehan (1997), 127–144). Physicists at the time had, in light of their experience with NM's record of predictive success, no strong reasons to believe that the limits of NM were surpassed at that point, and they were therefore not inclined to devote substantial resources to searching for solutions to the existing anomalies on the basis of a fundamentally novel theoretical framework.[11]

The process of theory assessment in NM is consistent with the perspective on theoretical stability offered by the new problem of conservatism. The historically exceptional degrees of predictive success enjoyed by this theoretical framework led the physics community to the belief that solutions to new and existing scientific problems could probably be continuously devised within that framework. This belief in turn informed a fairly conservative research strategy. Although the details of this case study can be further nuanced, the overall picture is consistent with the new problem of conservatism and may therefore be understood to motivate this problem as a serious explanatory strategy in the context of a stability argument in the current period of theory assessment in science.

3.2.3 Are today's theories exceptionally successful?

At this stage, a genuine concern about the problem described earlier may be raised: Are currently endorsed theories in science, at large, in fact exceptionally predictively successful compared with theories of the past, and can predictive success really be quantified and measured in a way that may serve as the basis of the new problem of conservatism?

One answer to this question is based on scientometric data. Fahrbach (2011*a, b*) estimates that as a result of the exponential growth of scientific activity in the last century, about 80% of scientific work ever carried out has been conducted after 1950, after which "refutations among [our current best scientific theories] have practically not occurred" (Fahrbach 2011*a*, 17). He then assumes that at a general level, "more scientific work results in the discovery of more phenomena and observations, which, in turn, can be used for more varied and better empirical tests of theories. More

[11] A brief mention of a more recent example is also motivated here. When quantum field theory became the most popular framework for developing theories about particle interactions in the early to mid-20th century, physicists already recognized foundational issues with the theory connected to its breakdown at high-energy scales, and much theoretical work was devoted to finding possible alternatives. However, after the standard model of particle physics, a product of quantum field theory, was exposed to empirical testing in the 1970s and turned out to be successful, most of the theoretical and experimental work in physics was carried out within the framework of quantum field theory. "As the 1980's began . . . all experimental results were henceforth to be compared to the standard model—the new 'paradigm' of the field. Where anomalies cropped up, they were usually resolved by moderate elaboration of the theory (or by uncovering errors made by the experimenters)" (Hoddeson et al. (1997), 29).

varied and better empirical tests of theories, if passed, amount to more empirical success of theories" (Fahrbach (2011*a*), 14). By these estimations, which seem fairly reasonable, one may conclude that the current period of theory assessment in science is in fact exceptionally successful.

For these reasons, the line of reasoning followed here leads to a general statement that the critic of an enriched stability argument may understand to be fully cogent and fairly plausible. The new problem of conservatism emphasizes the correlation between predictive success and conservatism. On this basis, it asserts that the fact that currently endorsed theories perform so well over repeated attempts to refute them in itself explains their stability. In other words, *stability is exactly what one should expect* to observe in scientific disciplines that work with theoretical structures that turn out to be exceptionally successful.

3.3. Formal analysis

How should proponents of the new problem of conservatism update their beliefs about scientific realism in light of observations of theoretical stability? In order to formally analyze this question, the concept of *degrees of predictive success* must first be offered a formal interpretation. Ideally, these degrees would be represented in their entirety. However, in the interest of simplicity and clarity (and keeping the structure of the formal model similar to SH's), let S^E be a binary propositional variable such that S^E:= T demonstrates *exceptional* degrees of predictive success, and let $\neg S^E$ be its negation.

The new problem of conservatism asserts that the theoretical stability demonstrated in the current period of theory assessment is not very surprising but in fact expected, given the exceptional degrees of predictive success exhibited by our currently endorsed scientific theories. Expectancy, at the very least, must be considering something more probable than not. "Alice expects rain today" plausibly expresses (at least) that Alice considers it more probable that it will rain today than that it will not rain today. Hence, a proponent of the new problem of conservatism will subscribe to the claim that, given exceptional degrees of predictive success, the probability of stability is larger than 0.5. Formally:

$$p(\neg C|S^E) = \sum_{j=0}^{\infty} p(\neg C|A_j, S^E)p(A_j) > 0.5. \tag{6}$$

At first glance, a natural way to modify the stability-based NMA in order to satisfy this inequality is by increasing the value of the α parameter as proposed by SH (see equation [1]). However, it turns out that α is not sufficiently flexible for this purpose. For all positive integer values of α, $e^{-\frac{1}{2}\left(\frac{j}{\alpha}\right)^2}$ converges to zero in $\lim_{j\to\infty}$. Therefore, the sum $\sum_{j=0}^{\infty} p(\neg C \mid A_j, S^E)p(A_j)$ also converges to zero in $\lim_{j\to\infty}$. Hence, in order to retain equation (1), a proponent of the new problem of conservatism is forced to assume that the number of alternatives to T is already limited from the start. However, by accepting equation (6), one makes the claim that because of theoretical conservatism at least in part caused by T's exceptional success, T's stability remains quite probable even if an infinite amount of possible alternatives could in principle be found. For this reason, a subscriber to equation (6) cannot be plausibly modeled as accepting equation (1).[12]

In order to evaluate the implications of the problem for the stability-based NMA, a new set of conditional probabilities $p(\neg\, C \mid S^E, A_j)$ must therefore be adopted. First, this choice should be consistent with SH's core assumption B4 that $p(\neg\, C \mid S^E, A_j)$ are decreasing in j with the extreme case $p(\neg\, C \mid A_0, S^E) = 1$. Second, it should satisfy equation (6) given any possible probability distribution across A. Finally, it should be maximally generous to the realist who adopts an enriched stability argument, in order to ensure that the problem is distinguished from other considerations. Hence, $p(\neg\, C \mid S^E, A_j)$ should decrease quickly in j. The following distribution may be understood as the simplest formally adequate way of satisfying the described conditions (figure 3):[13]

$$p(\neg C | A_j, S^E) = 1/\big(j+2\big) + 0.5. \tag{7}$$

Adopting this distribution leads to the following expression for the posterior probability of H, evaluated in figure 4:

$$p(\mathrm{H}|\neg\mathrm{C}, \mathrm{S}^E) = \frac{\sum_{j=0}^{\infty}\Big(\frac{1}{j+2}+0.5\Big)\frac{1}{j+1}\big(p(\mathrm{A}_0)\big(1-p(\mathrm{A}_0)\big)^j\big)}{\sum_{j=0}^{\infty}\Big(\frac{1}{j+2}+0.5\Big)\big(p(A_0)\big(1-p(A_0)\big)^j\big)\Big(\frac{1}{j+1}+p(S^E|\neg H)\Big(1-\frac{1}{j+1}\Big)\Big)}. \tag{8}$$

An apt comparison is with the classical NMA, evaluated in figure 5, which can be extracted from SH's model with the following expression:

$$p(H|S^E) = \frac{\sum_{j=0}^{\infty}\frac{1}{j+1}\big(p(\mathrm{A}_0)\big(1-p(\mathrm{A}_0)\big)^j\big)}{\sum_{j=0}^{\infty}\big(p(\mathrm{A}_0)\big(1-p(\mathrm{A}_0)\big)^j\big)\big(\frac{1}{j+1}+p(S^E|\neg\mathrm{H})\Big(1-\frac{1}{j+1}\Big)\big)}. \tag{9}$$

The results show that modifying the stability-based NMA according to the new problem of conservatism generates an argument that does not amount to a substantial improvement over the classical NMA. Looking at an example makes the comparison more concrete. A fairly skeptical anti-realist may assign a quite small value to $p(A_0)$, say, 0.01. In SH's model based on equation (1) with $\alpha = 4$, this assignment still allows significant flexibility with respect to the other free parameter, $p(S^E \mid \neg\, H)$, which is just required to be in the interval [0, 0.75] in order to conclude $p(H \mid \neg C, S^E) > 0.5$. In the modified argument, based on equation (7), an interval of [0, 0.13] is required, and for the classical NMA, the interval required is [0, 0.08].[14]

[12] As an aside, note that this result contradicts SH's claim that their assumptions are neutral with respect to the realist and anti-realist: here is a plausible anti-realist position that cannot be represented by their assumptions.

[13] Of course, these conditions can be satisfied with other choices, which may side more to the anti-realist or even more to the realist. However, the same is true for equation (1). It is therefore important to recognize a general limitation of both SH's original argument and the modified argument evaluated here. Both reveal the coarse implications for scientific realism of adopting a certain view on theoretical conservatism rather than numerical values for actual credences.

[14] At this point, the realist may retaliate: $p(S^E \mid \neg H)$, that is, the probability that T is exceptionally successful given that T is not empirically adequate, may be considered smaller than $p(S \mid \neg H)$, that is, the

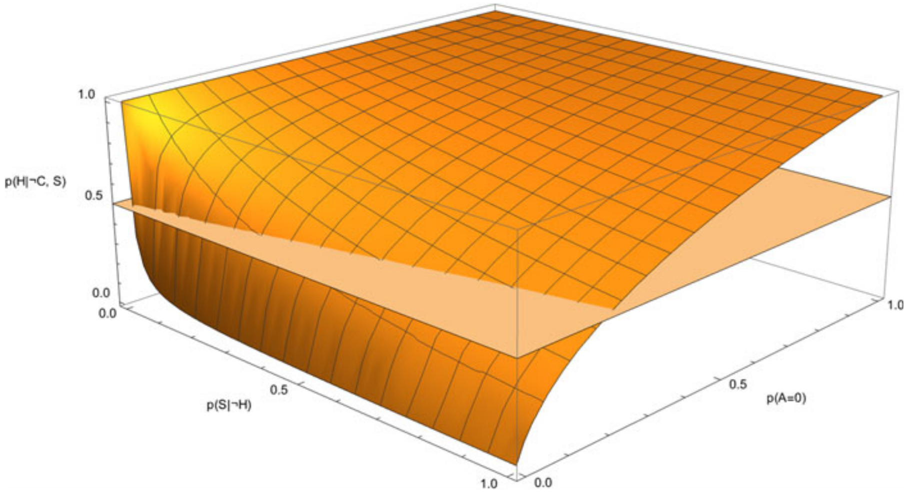

Figure 4. The results of the modified stability-based NMA.

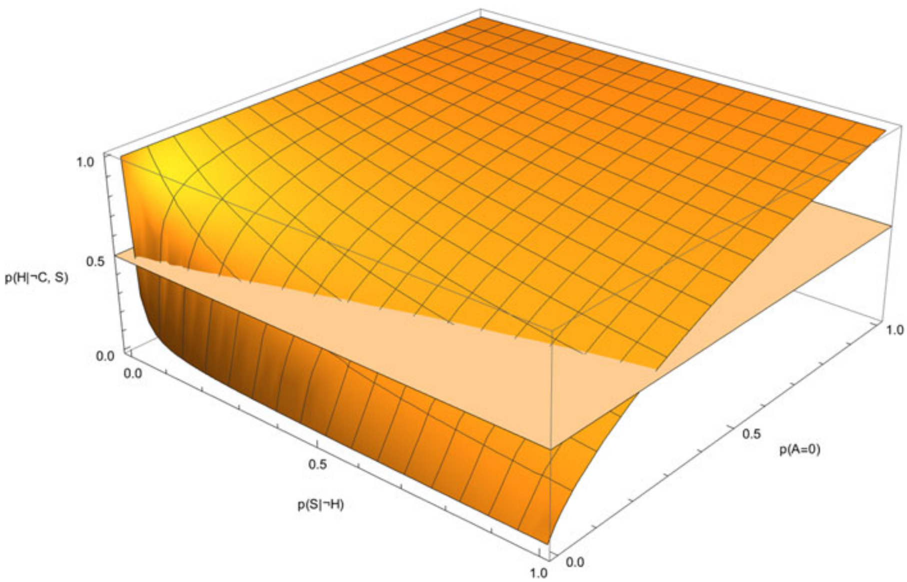

Figure 5. An evaluation of the classical NMA, as extracted from SH's model.

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probability that T is "normally" successful given that T is not empirically adequate. Hence, a smaller $p(S^E \mid \neg H)$ could perhaps be motivated. In fact, Fahrbach (2011*b*) may be understood to argue this exact point. However, (i) the acceptable interval ([0, 0.13]) is very small, and (ii) whether assigning a very small value $p(S^E \mid \neg H)$ can be motivated at all is ultimately decided by the dynamic between the PMI and the classical NMA. Hence, the stability argument, as such, does not significantly contribute to realists' justification in this case but merely shifts the goalposts slightly in their favor.

Hence, although the modified stability-based NMA does amount to a slight upgrade to the classical NMA, the upgrade is not of substantial significance. The basic uncertainties associated with attaching numerical values to the free parameters of the NMA may be understood to offset a single minor improvement. The important conclusion is that, just like the classical NMA, the modified stability-based NMA requires commitment to quite specific parameter values in order to generate results that support scientific realism.

3.4 Who should advocate for nonconservatism?

Before concluding, a brief aside is motivated. The results of the analysis carried out previously offer a contribution to a recent exchange between Stanford (2015) and Dellsén (2018) on theoretical conservatism and scientific realism. Stanford argues that realists should embrace conservatism because they consider it probable that the currently endorsed theoretical picture of the natural world is, by and large, approximately the correct one. Conversely, an anti-realist perspective on science supports a liberal strategy of allocating resources to exploratory theory building. Against Stanford, Dellsén claims that scientific realists should embrace non-conservatism even if they do not consider it probable that exploratory theorizing will generate promising new theory. Failed exploration of that kind amounts to evidence in favor of the endorsed theory because this failure is consistent with the understanding that the endorsed theory is the (approximately) correct one. The conclusion of the analysis in the present article is consistent with the view put forward by Dellsén. A conservative scientific discipline cannot act as a good basis for a strong stability argument in the context of scientific realism. Hence, for the realist, advocating nonconservatism will, in the long run, generate a stronger basis for an argument of that kind.[15]

It is important to recognize, however, that this implication is connected to scientific realism. And one should not assume that the aims of science and the philosophy of realism are always fully aligned. If an emergence of conservatism in science is considered fully the result of a sociological development that is detrimental to the research process, it may look natural to advocate for nonconservatism. However, the line of reasoning followed in section 3.2 suggests that the situation is more complicated. A reason why one may propose a recent emergence of conservatism in science is the increasing degree of predictive success demonstrated by scientific theories. In this situation, conservatism may be considered a fruitful

[15] Oriti (2019) and Dawid (2022) discuss a similar issue in the context of the classical NAA. Oriti claims that the NAA supports conservatism because inferring that there probably are no alternatives implies that a search for such alternatives will be fruitless. Against Oriti, Dawid argues that the NAA actually supports a nonconservative strategy: "the more energy has been invested in alternative research programs without success, the more powerful a NAA can become. In this light, it is in the epistemic interest of the dominant research program that alternative research programs are pursued with vigor and in sufficient breadth" (Dawid (2022), 70). This discussion shows interesting parallels between the problem of realism and situations of scarce evidence in science. In both cases, the core problem is that observational data in favor of a theory at hand are insufficient for justifying a given level of trust in the theory. And in both cases, a nonconservative research strategy can be the best option for boosting that trust.

research strategy, given that a core aim of scientific research is to produce empirically successful theory.

This understanding is consistent with Tambolo and Cevolani's (2023) more general claim that whether or not theoretical conservatism should be preferred in a scientific discipline depends on what the "main cognitive aim" of the discipline is and the degree to which scientists in the discipline expect currently endorsed theory to continue realizing that aim. From this perspective, it is not at all clear that advocating nonconservatism leads to a more successful scientific enterprise. Whether or not it does depends on what one considers to be part of the core aims of science against which that success is evaluated, along with to what degree the current theoretical description of the world is successful in realizing those aims.

4 Conclusions

The line of reasoning followed in this article has served two core aims. First, an enriched form of a stability argument for scientific realism has been proposed. Enriching the argument addresses a core worry about SH's original argument: Why are observations of theoretical stability in science in need of any special explanation at all? In section 3.1, it was suggested that restricting stability arguments to disciplines that have a historical record of theory change offers a cogent answer to this question. A long period of stability is genuinely surprising because scientists are typically creative enough to develop novel theory in those disciplines. On this basis, enriched stability arguments can be meaningfully employed in defense of scientific realism and offer a novel challenge to classical anti-realist accounts of scientific theory building.

Although the enriched argument is a dialectical upgrade for the realist, a second aim of this article was to show that anti-realists are not without resources to defend themselves. Against the enriched argument, the *new problem of conservatism* was proposed in section 3.2. The problem is based on the claim that predictive success is strongly correlated with conservatism. Hence, periods of theory assessment that enjoy exceptional levels of predictive success compared with earlier periods can be expected to be more stable, and it was suggested that an anti-realist may explain periods of stability on this basis and therefore reject even an enriched stability argument in favor of realism. The plausibility of this explanatory strategy was supported by a case study from fundamental physics. It was then suggested, based on Fahrbach's scientometric work on scientific activity, that our current best scientific theories are, by and large, indeed exceptionally predictively successful. The formal analysis carried out in section 3.3 showed that subscribing to this problem strongly undermines the improved results of the stability-based NMA. This result also implies that realists who wish to rely on a stability argument to justify their position should advocate a nonconservative research strategy.

The analysis of the new problem of conservatism should not be taken to suggest that science proceeds in exactly the manner described by that problem. Rather, it concludes that understanding the research process on the basis of this problem amounts to a cogent and defensible position for a critic of stability arguments. Ultimately, the problem indicates that to the extent that stability arguments are put forward in order to offer a case for realism that is also convincing to the anti-realist, they are in need of further substantiation. One strategy to that end is to offer an empirically based description of the current stage of theory assessment in science that strongly supports the conclusion that this period is fairly liberal with assigning resources to exploratory theorizing. Whether or not such a description in facts supports that conclusion is, of course, an open question.

The general message of the article is that realists and anti-realists alike access viable strategies for advancing the debate on stability beyond classical disagreements of the realism debate. Notably, these strategies show that the historical record of theory change does not unequivocally support an anti-realist position and, on the other hand, that the empirical evidence in favor of the current scientific picture of the world does not unequivocally support a realist position. The resulting dialectic is promising because it implies that the disagreement can ultimately be dissolved by the continuous assembly of empirical data. If the current period of stability extends far into the future, the anti-realist can, at some point, no longer hold that the lack of alternative theory is not genuinely surprising without committing to an absurd description of science as a theoretical preserve fully devoid of traces of creativity. Hence, such realists would need to retreat to other philosophical disagreements in order to retain their position. On the other hand, the emergence of just a small set of alternatives to our currently endorsed theories should already be cause for alarm for the proponent of a stability argument in favor of realism. The anti-realist may, then, in a PMI-style move, suggest that realism is not the only plausible explanation of theoretical stability, even in disciplines that exhibit a high degree of theoretical creativity.

Given the current state of the scientific realism debate, any development that opens the opposing camps to the risk of being at variance with observational evidence must be considered a welcome addition. Testing scientific realism and anti-realism against new empirical data is of crucial importance for the status of the debate as a rational disagreement about a genuinely testable philosophical hypothesis. For this reason, stability arguments in the enriched form presented here are clearly progressive and should be paid their due attention by realists and anti-realists alike.

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