Meta-Empirical Support for Eliminative Reasoning

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Abstract

Eliminative reasoning is a method that has been employed in many significant episodes in the history of science. It has also been advocated by some philosophers as an important means for justifying well-established scientific theories. Arguments for how eliminative reasoning is able to do so, however, have generally relied on a too narrow conception of evidence, and have therefore tended to lapse into merely heuristic or pragmatic justifications for their conclusions. This paper shows how a broader conception of evidence, one that specifically acknowledges the relevance of meta-empirical evidence, not only can supply the needed justification but also illuminates the methodological significance of eliminative reasoning in a variety of contexts.

1 Introduction

Deductivist folk heroes, such as Isaac Newton, who (according to legend) deduced theoretical propositions directly from the phenomena, and Sherlock Holmes, with his guiding precept that "when you have eliminated the impossible, whatever remains, *however improbable*, must be the truth" (Doyle, 1981, 111),[1] have from time to time inspired philosophers to promote a method referred to variously as "eliminative induction" (Earman, 1992; Hawthorne, 1993; Kitcher, 1993; Norton, 1994; Weinert, 2000; Forber, 2011), "demonstrative induction" (Dorling, 1973; Norton, 1994, 2000; Laymon, 1994; Massimi, 2004; Magnus, 2008), "Holmesian inference" (Bird, 2005, 2007), or, adopting Newton's *façon de parler*, "deduction from the phenomena" (Dorling, 1971; Harper, 1990, 1997, 2011). The essence of the method is the elimination of explanatory possibilities by empirical evidence that disfavors them. In the most propitious cases, this process leaves a single possibility remaining, which, if the initial set of possibilities includes it, must evidently be, as Holmes instructs us, the truth.

Undoubtedly, such a method is, as Dorling (1973, 369) says, "of considerable significance and importance in actual scientific reasoning," and it has unquestionably led to success in many significant and important scientific episodes (not to mention being of considerable significance and importance in the reasoning of fictional detectives, in many significant and important detective novels). The multitude of historical cases investigated by philosophers in the last half century capably demonstrate this much. Most extensive is the literature on Newton's method of deduction applied to optical or gravitational phenomena (Dorling, 1990; Harper, 1990, 1997, 2011; Harper and Smith, 1995; Worrall, 2000). Other investigations have focused on electromagnetism (Dorling, 1970, 1973, 1974; Laymon, 1994; Norton, 2000), atomic or sub-atomic physics (Dorling, 1970, 1971, 1973, 1995; Norton, 1993, 1994; Bonk, 1997; Hudson, 1997; Bain, 1998; Massimi, 2004), and relativistic theories of gravitation (Dorling, 1973, 1995; Earman, 1992; Norton, 1995; Stachel, 1995).[2]

Despite the demonstrable historical importance of (what I will generally be calling) eliminative reasoning, the epistemological conclusions that commentators have drawn from these many cases are largely unsatisfactory. Although these conclusions have already met with incisive criticism in some individual cases (Laymon, 1994; Hudson, 1997; Bonk, 1997; Worrall, 2000), the general underlying difficulties with the method are subtle, involving as they do a variety of outstanding issues in the methodology and epistemology of science. My basic aims with this paper are to bring these difficulties to light, offer a satisfactory evidential means of resolving them, and

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[1] Emphasis is in original throughout unless otherwise noted.

[2] Applications in other sciences are rare. Examples from biology appear in (Forber, 2011) and (Ratti, 2015); (Bird, 2010) discusses cases in medical science. A related "Sherlock Holmes" strategy has also been discussed in the contexts of experiment (Franklin, 1989) and simulation (Parker, 2008).

show how this approach also informs the methodological import of eliminative reasoning. Although this particular resolution may not always be available in practice (whether in the retrospective analysis of historical cases or in contemporary assessments), due to a lack of the requisite evidence, the recognition that such a resolution is in principle possible nevertheless should put eliminative reasoning in a new methodological and epistemological light.

As it is usually conceived, eliminative reasoning can be divided into two basic steps: first, a positing of (explanatory) possibilities (which, at the risk of violating Newton's injunction to not frame hypotheses, we may call "hypotheses"), and second, a process of elimination of those possibilities by empirical evidence. Accordingly, there are two places where trust in the method may falter, at the first step or the second. Although the security of an eliminative inference depends on insuring that the second step, the eliminative process proper, is sound, the salient epistemological difficulties with this step will not be rehearsed here, for these difficulties are familiar, are straightforwardly soluble, and have no special bearing on eliminative reasoning per se. Rather it is the first step, the positing of possibilities, that involves the more significant obstacle to securing the epistemological character of an eliminative inference.[3]

I contend that no advocate of eliminative reasoning has offered a compelling epistemic justification for this first step of eliminative reasoning. Yet without a justification for this step of eliminative reasoning, no conclusion of any instance of eliminative reasoning can possess more than a merely "pragmatic," "heuristic," or otherwise epistemically equivocal status—that is, at least by the lights of what may reasonably be regarded as scientific epistemology (detective fiction, of course, may be another matter). I will begin (§2) by introducing some general considerations about eliminative reasoning and developing the basic justificatory problem just mentioned. The following section (§3) shows how proponents have typically equivocated in the face of this problem by offering "merely pragmatic" justifications or "merely heuristic" justifications in place of genuinely epistemic ones. In §4 I show that the epistemological problem can be overcome without lapsing into such anodyne pragmatic gesturing, by acknowledging broader forms of empirical evidence, specifically the kind identified by Dawid (2013, 2016, 2018) as "non-empirical" or (better) "meta-empirical" evidence. Such meta-empirical evidence is not only epistemologically significant for the method of eliminative reasoning but sheds significant light on eliminative reasoning's general methodological significance as well. I explore both its epistemological and methodological significance by way of an example from the context of contemporary cosmological research (§5), first by criticizing a contrasting analysis of this example due to Smeenk and then in light of the considerations previously developed in the paper. Finally, the conclusion (§6) summarizes how meta-empirical evidence solves the issues raised in this introduction, but also suggests how the "meta-empirical perspective" latent in this paper links epistemology and methodology, theory and practice, in a philosophically novel and productive way.

2 Justifying Eliminative Reasoning

Eliminative reasoning has generally been described as an essentially empirical method which can be divided into two principal steps: (I) the identification of a space of possible (explanatory) hypotheses (which can be construed as a preliminary pruning of logically possible hypotheses); (II) the systematic favoring and disfavoring of these alternatives on the basis of empirical evidence. Norton and Forber, for example, formulate it in this way explicitly:[4]

> I shall construe eliminative inductions broadly as arguments with premises of two types: (a) premises that define a universe of theories or hypotheses, one of which is posited as true; and (b) premises that enable the elimination of members of this universe by either deductive or inductive inference. (Norton, 1995, 29)

> The standard picture treats [eliminative induction] as a two-step inferential pattern: (1) construct a space of possibilities and then (2) use observations to eliminate alternatives in that space. (Forber, 2011, 186)

[3] Dorling describes eliminative reasoning as "the deduction of an explanans from one of its own explananda," (Dorling, 1973, 360), which, if apt, classifies it along with inference to the best explanation as a kind of "explanatory reasoning." Indeed, there is an obvious parallel between eliminative inference and the inference pattern known as "inference to the best explanation," as the latter follows a very similar two-step eliminative process (Lipton, 2004). Bird in particular seizes explicitly on this parallel, invoking Lipton's account of inference to the best explanation as a foil in arguing for his own account of "Holmesian inference," i.e., eliminative reasoning. Surveying the literature on inference to the best explanation, one finds that criticism of the method has largely focused on the second step (selecting the "best" explanation), as in, e.g., (van Fraassen, 1989). Nevertheless, the first step of inference to the best explanation, as with eliminative reasoning, should invite epistemological concern as well. Since this paper focuses on the justification of this step, many points I make here should be applicable to inference to the best explanation too, although I will confine any explicit remarks to the footnotes.

[4] Other notable examples involving similar descriptions include (Earman, 1992, ch. 7), (Kitcher, 1993, 238), and (Bird, 2005). As for less recent literature, Dorling (1973) mentions discussions of demonstrative induction by Johnson, Broad, and Kyburg, and Bird (2005) cites von Wright's 1951 book *A Treatise on Induction and Probability*. See those papers for the relevant citations.

Einstein's development of the general theory of relativity, studied extensively by both Dorling and Norton, is a salient example of eliminative reasoning. They describe Einstein's "method of discovery" as a process relying primarily on eliminative reasoning, one which simultaneously furnishes both the discovery of and the justification for the final theory. The crucial derivation made by Einstein, according to Norton and Dorling, is the derivation of the gravitational field equations, the basic law that picks out the possible relativistic spacetimes according to the theory. Here is Dorling's description of the eliminative inference which yields the Einstein field equations:

> The departure of such a space-time geometry from the flat space-time geometry of Special Relativity is described by its curvature tensor, and to accommodate gravitation the curvature must be some function of the matter distribution. Einstein determined this function in the following way. He insisted on second-order partial differential field equations, analogous to Poisson's equation (and hence linear and homogeneous in their second differential coefficients) to maximize agreement with the theoretical structure of the previously successful Newtonian theory. He required an energy-momentum-tensor-density source term, rather than a rest-mass density source term, for consistency with his lift-experiment requirements on optical phenomena. He required energy-momentum conservation for the source term and from this required that the divergence of the left-hand side of the field equations must vanish identically. These requirements serve . . . to determine the field equations uniquely, modulo the gravitational constant whose value was then fixed by the requirement of agreement with Newtonian gravitational theory in the appropriate limit. These fundamental postulates of his new revolutionary theory were thus simply the result of a deductive argument, taking as premises an "experimental fact" inconsistent with the class of theories to be superseded (i.e. special relativistic theories; Newtonian ones had already been superseded), further non-controversial experimental facts, and theoretical requirements which consisted of those theoretical parts of the previously successful theories which seemed still sufficiently plausible. (Dorling, 1995, 101)

Norton (1995, 54) summarizes the inference in the form of an argument, which I partially reproduce in the following table:

Universe of Theories:	Field equations [according to which the gravitation tensor is proportional to stress energy].
Eliminative Principle:	Principle of general covariance. . .
Eliminative Principle:	Requirement of Newtonian limit. . .
Eliminative Principle:	Conservation of energy-momentum. . .
Conclusion:	[The gravitation tensor] is the Einstein tensor. . .

In this case, step (I) is the identification of the set of possible field equations as the relevant space of possibilities; step (II) is the elimination of all such field equations save one, the Einstein field equation, by a series of eliminative principles. Assessing this particular argument logically and epistemologically, one observes that, logically, it is plainly intended to be deductive in character, and, epistemically, empirically grounded eliminative principles are conspicuous. Indeed, Norton maintains that, "with the possible exception of the principle of general covariance, these eliminative principles were empirically based" (Norton, 1995, 31), hence "the discovery process and the justification it spawned have substantial empirical foundations" (Norton, 1995, 31).

In general, however, not all eliminative arguments need have the specific logical and epistemological characteristics of this example. Indeed, it should be recognized that the complete process of eliminative reasoning is, in full generality, complex, both logically (deductive, inductive, and even possibly "abductive") and epistemologically (evidential, explanatory; theoretical, empirical).[5] A few comments should serve to illustrate this.

First, with respect to the logic of eliminative reasoning: While Einstein's eliminative inference has an evidently deductive (and even infallibilist) character (essentially following the familiar deductive pattern of disjunctive syllogism)—indeed it is one where *all* possibilities save one are eliminated by the gathered evidence, an "inference to the only explanation," as Bird (2007) describes it—inductive inferences may also have a conspicuously eliminative character as well. Hawthorne (1993), for example, argues that probabilistic-inductive reasoning should quite generally be seen as following an eliminative pattern. Recall that in Bayesian confirmation's conditionalization step ("Bayesian updating") there is a reshuffling of probabilities of hypotheses according to which are and which are not favored by the evidence according to Bayes' rule. However, besides this reshuffling of

[5] In this respect too eliminative reasoning resembles inference to the best explanation, which is also fairly described as a complex form of reasoning (Fumerton, 1980).

probabilities of hypotheses, the probability of the evidence that triggers the conditionalization step is also updated to probability one, which eliminates any hypothesis that is inconsistent with it.[6]

Second, with respect to the epistemology of eliminative reasoning: Whereas Norton draws attention to the fact that most, if not all, of Einstein's eliminative principles are empirically based, Dorling points out that sometimes principles have been grounded by scientists in other ways:

> Sometimes the high-level theoretical constraints invoked are claimed partly or wholly to follow from a priori justifiable principles, but more usually they are either merely claimed to be plausible inductive generalizations from all experience (as Newton claimed for his three laws of motion which functioned as theoretical constraints in the deduction of his gravitational force law), or, as in most later examples, they are merely claimed to be derived by inductive extrapolation from the successful parts of previous theories. (Dorling, 1990, 197)

It is fair to say, though, that eliminative principles invoked in practice generally do involve essential (and in propitious cases substantial) use of empirical evidence (or, at any rate, empirical generalizations) to eliminate hypothetical possibilities. Yet the application of such eliminative principles occurs only in step (II). One clearly must not neglect the character of step (I), the identification of a set of explanatory hypotheses, in characterizing eliminative reasoning epistemologically. If this first step is not substantially based in empirical evidence as well, then it would seem to be somewhat misleading to describe eliminative reasoning as a "substantially empirical" method.[7] Whether intended or not, doing so has the effect of pushing epistemological concerns about the justification of step (I) into the background.

To bring the issue of justification with step (I) front and center, let us first look at it by considering the simple, specific case of a general deductive eliminative inference. Suppose that we have determined a set of possible hypotheses $\mathcal{H}$, each of which adequately explains some evidence E. Gathering further evidence E', we find that the conjunction of corresponding propositions $\mathcal{H}$, E, and E' entails a generalization H, which can be represented by the subset $H \subset \mathcal{H}$ (or perhaps even by an individual hypothesis $h \in \mathcal{H}$). In this case, E' has been used to eliminate the complement of H in $\mathcal{H}$ ($E' \wedge H^c$ is a contradiction). This deductive use of elimination is the classic form of what was (in much earlier literature) called "demonstrative induction," whereby from "premises of greater generality" (i.e., $\mathcal{H}$) and "premises of lesser generality" (i.e., E and E') one infers a conclusion of "intermediate generality" (i.e., H or h) (Johnson, 1964, 210).[8] (In the following, I will frequently make use of these expressions, "premises of ... generality" to describe the parts of an eliminative inference.)[9]

The logic of a deductive eliminative inference is clearly impeccable, so let us examine its epistemology. Although one may always challenge the justification of the premises of lesser generality (i.e., E and E'), they are seldom regarded as epistemologically problematic, at least insofar as they are empirical (at least, *that* they are justifiable—precisely *how* they are justified as such and in general is a deeper philosophical issue). Of course, it is precisely the epistemic security of such empirical premises of lesser generality that is taken as a significant virtue in the favor of eliminative reasoning, for, as said, much of the inferential work in a deductive eliminative reasoning *is* based on them (hence there is less need to rely on what some philosophers would regard as dubious "inductive rules").

Epistemological scrutiny should therefore fall principally on the justification of the premises of greater generality, or, equivalently, the set of possible explanatory hypotheses (i.e., $\mathcal{H}$). Whether as inductive generalizations from experience or as individual empirical facts, the available empirical evidence (i.e., E and E') does not readily supply a justification for any set of possible explanatory hypotheses that subsumes it—at least not in any substantial sense, since the empirical facts by themselves can only determine, logically speaking, the set of logically possible hypotheses consistent with them (or, restricting to explanatory hypotheses, consistent with those that explain them). Therefore, if the justification of explanatory hypotheses is restricted to such empirical facts, then eliminative reasoning not only cannot have a "substantially empirical" character (except in the weakest possible sense) but also lacks an adequate and complete epistemic justification (i.e., for both steps (I) and (II) taken together).

Worries in this quarter have generally been papered over by proponents of the method however. Norton, for example, only demands that the "universe of theories" be "sufficiently large." In general, he remarks that the

[6] Even if the probability of the evidence is not set to unity, as in Jeffrey conditionalization, there is still a strong affinity with more obvious cases of eliminative reasoning, as the evidence systematically disfavors hypotheses that are not supported by the evidence (albeit without eliminating them definitively).

[7] To be sure, eliminative reasoning has more of an empirical character than inference to the best explanation, since the latter's second eliminative step invokes solely explanatory considerations in inferring an explanans, whereas the former invokes evidential considerations in its second step.

[8] See (Norton, 1994, 13) for a sensible way to distinguish between demonstrative and eliminative inductions, and why they are nevertheless essentially the same forms of reasoning.

[9] These terms are used merely for convenience, and no particular analysis of generality is intended.

success of an eliminative inference depends on "(a) our confidence in its premises and most especially our confidence that the universe of theories is sufficiently large; and (b) the strength of the inference used for elimination" (Norton, 1995, 59). Obviously the justification of the premises and the form of inference are important for any inference; thus, the only part of Norton's recipe that pertains to eliminative reasoning specifically is that pertaining to the justification of the universe of theories, which he states must be "sufficiently large" so that one may be "very confident that the correct answer to the problem at hand lies within the relevant universe" (Norton, 1995, 59). Additionally, he remarks that "these further hypotheses [i.e., $\mathcal{H}$] can be of such a general and uncontroversial nature that the acceptance of the theory [i.e., h] picked out is placed beyond reasonable doubt" (Norton, 1993, 2).

Generality and uncontroversiality, of course, are not usually regarded as reliable indicators of rational acceptability—at least by the lights of what may reasonably be regarded as epistemology (detective fiction, of course, may be another matter). What Norton has in mind, it seems, is that generality gives a means of controlling for the risk of choosing a particular universe of hypotheses. He claims that "the most satisfactory way of controlling [inductive] risk is to seek arguments in which the size of the universe of possibilities is very large and, correspondingly, the 'premises of greater generality' of the demonstrative induction are weak" (Norton, 1994, 15). This way is actually a very unsatisfactory way of dealing with inductive risk though. If the "size" of the universe of possibilities were all that mattered, then one could always simply choose, scot-free, all logically possible hypotheses (at least, those that are capable of explaining the premises of lesser generality) as the universe of theories. This obviously would get one nowhere inferentially, since, with the premises of greater generality thereby lacking any content (or, at least, any excess content over the premises of lesser generality), one would essentially be left with the premises of lesser generality (the evidence) and an *inductive* inference from these to the premises of intermediate generality—hence no longer an eliminative inference. Thus, although one can perhaps avoid some degree of "inductive risk" by choosing a universe of theories that is "sufficiently large," one must evidently also take some "inductive risk" by choosing a universe of theories that is "not too large."

I expect that Norton would concede as much. So perhaps he just means to indicate by his comments on the matter that he regards the premises of greater generality as being, at least *in principle*, on epistemically good grounds.[10] How so though? How are the premises of greater generality justified? Are they regarded as "unproblematic background knowledge"? Or does eliminative reasoning itself readily give us just such a principle? After all, one might say, as Dorling (1973, 365) does say, that the premises of greater generality are just the outcomes of a previous round of eliminative reasoning. (Unfortunately, it is no good saying this, for the premises of greater generality could never be justified in the ensuing infinite regress, as already noted by Worrall (2000).) Bird chooses to adopt a broad perspective on evidence in order to answer the question. Not only does he allow that *non-observational* evidence can ground the premises of greater generality but even that in many cases it is essential to do so. Remarking that "in general we can make knowledge-generating inferences from non-observational knowledge," he insists that "a restriction of evidence to the observed is implausible" (Bird, 2007, 432). However, by defending only the general idea that some generalizations of the kind are justifiable, he too, like Norton and Dorling, skips lightly over the justificatory problem, that is, by not explicating precisely how the premises of greater generality are specifically justified in the case of eliminative reasoning. It is not enough to gesture at the "in principle possibility" of some generalizations being justified, as Norton, Dorling, and Bird do, in order to defend the justifiability of the premises of greater generality in an eliminative inference specifically.

Although the deductive case by itself fully indicates the problem of justification for eliminative reasoning and serves to adequately indicate the lack of any genuine solution to it on the part of proponents, it is worth briefly examining the case of inductive eliminative reasoning too (for completeness if for nothing else). Although one could simply rephrase the previous deductive argument form in an inductive form, it is worth probabilifying it as well in order to capture the additional considerations that come along with it. Little changes still, except that (i) we require that all the formal elements from before be rendered as elements of a probability space, where we regard the total probability of the set of hypotheses $\mathcal{H}$, $\mathrm{pr}(\mathcal{H})$, as one, and (ii) we require that upon obtaining new evidence E' one updates the probability of $\mathcal{H}$ according to some appropriate conditionalization formula (like Bayesian conditionalization). For some subset of hypotheses H, with prior probability $\mathrm{pr}(H)$, the evidence will lead to confirmation of the hypothesis, that is, $\mathrm{pr}(H|E') > \mathrm{pr}(H)$, and for the rest, no change or disconfirmation: $\mathrm{pr}(H^c|E') \leq \mathrm{pr}(H^c)$ If the evidence E' is strong enough, it may be the case that the probability is "overwhelmingly high" for one of the hypotheses $h \in \mathcal{H}$, in which case we may then draw the well-grounded inductive inference that "h is probably true."

Despite the differences introduced by probabilifying inference, the skeleton of the eliminative process, it seems, is much the same in the inductive case as the deductive one. As already noted, in Hawthorne's view, "Bayesian inductive inference is essentially a probabilistic form of induction by elimination," for, as he sees it,

[10]Norton's preferred approach now to defending inductive inferences (including the first step of an eliminative inference) is his "material theory of induction," according to which all inductive inference is local. See, e.g., (Norton, 2003, 2005). This approach would require a separate discussion, so I set it aside in this paper.

"the very essence of Bayesian induction is the refutation of false competitors of a true hypothesis" (Hawthorne, 1993, 99).[11] While moving from deductive eliminative reasoning to inductive eliminative reasoning involves a change from binary belief (and infallible inference) to graded credences (and fallible inference), the basic eliminative reasoning strategy is manifestly recognizable in both cases.

An epistemological appraisal of the probabilistic approach to inductive eliminative reasoning naturally encompasses more elements than deductive eliminative reasoning. In general, one should consider the justification of the set of hypotheses $\mathcal{H}$ forming the probability space, the evidence (E and E'), the probabilities that appear (the likelihoods and priors), and even the updating rule itself. The epistemological issues arising with these elements have proved to be much more challenging and controversial than analogous issues in the deductive case (Howson and Urbach, 2006; Sprenger and Hartmann, 2019; Sprenger, 2020). As with the deductive case, however, most of these issues do not have a special bearing on the process of eliminative reasoning so much as on (probabilistic) inductive reasoning in general. Moreover, also like the deductive case, regardless of the ultimate source of justification for such elements of probabilistic inductive reasoning as prior probabilities, likelihoods, and conditionalization, there is no doubt that in many scientific cases (especially applications in statistical reasoning) the relevant premises and inferences *are* sufficiently justified, even if it may not be completely clear *how* they are justified as such and in general.

What remains outstanding in the context of inductive eliminative reasoning, then, is the problem of justification with step (I). The analogous issue to the justification of the general premises in deductive eliminative reasoning, step (I) of the eliminative process in the deductive case, is, in the case of inductive eliminative reasoning, the justification of the underlying set of hypotheses (and, when probabilified, a probability function associated with them). The general issue of justifiying an underlying set of hypotheses in probabilistic induction is seldom acknowledged by philosophers—perhaps because it is thought that there are straightforward ways of addressing it. For example, it might be supposed that one may simply allow for all logical possibilities in the space of hypotheses.[12] However, for probabilistic induction to be informative about hypotheses, there must be an initial restriction on the possible hypotheses which are to be confronted by empirical evidence; otherwise, one simply "gets out what one puts in": the empirical evidence E (as analogously argued above for deductive eliminative reasoning).

Another factor which may be at work in obscuring the need for a justification of the underlying set of hypotheses in eliminative reasoning (and probabilistic inductive reasoning in general) is a widespread "subjectivism" in connection to probabilistic induction, according to which choices of hypotheses and their probabilities may be chosen with considerable freedom (or, in the extreme case, arbitrarily). While radical freedom in such choices is (widely regarded as) an inadequate supposition, even relative freedom allows for the introduction of heuristic, pragmatic, and other not-properly-epistemic considerations to enter into the calculus. Indeed, many regard it as a virtue of the Bayesian approach that it is able to neatly divide the objective (evidential) and subjective factors in an appropriate way. In some sense this is correct, but, be that as it may, there remains a threat of mixing up the objective and subjective factors in an epistemological analysis, which, as I will argue in the next section, is precisely what one finds in prominent defenses of eliminative reasoning.

3 Heuristic and Epistemic Aspects of Eliminative Reasoning

In the previous section, I argued that the outstanding epistemological issue for eliminative reasoning is the justification of the premises of greater generality, the premises that identify the set of explanatory hypotheses. There is an easy way out of this particular difficulty though: just accept that the choice of $\mathcal{H}$ is ultimately a heuristic or pragmatic matter. The price to pay, of course, is that the conclusions obtained by eliminative reasoning are then at best conditional on those conclusions' (epistemically unjustified) assumptions.

Although some philosophers may be willing to make this concession (Earman, 1992; Forber, 2011), many advocates of eliminative reasoning would find it unpalatable. To them, the whole point of advocating eliminative reasoning is to demonstrate the security of (at least some) scientific knowledge against various skeptical theses, especially the underdetermination of theory by empirical evidence. Norton, for example, takes the consequences of conceding in the face of epistemic challenges like this to be dire, for in this case he holds (somewhat over-dramatically) that "our understanding of the world—scientific and nonscientific alike—is little more than myth and delusion, and our attempts at rationality are no better than childish games" (Norton, 1994, 7).

The issue that a prospective advocate of eliminative reasoning faces can thus be set as a dilemma: On the one hand, if one looks to eliminative reasoning as a means to secure the epistemic status of scientific knowledge, then, as I have argued, one must resolve the problem of justification of the premises of greater generality. On the

[11] Vineberg (1996), in her critique of Kitcher's defense of eliminative induction against a Bayesian alternative, and (to a certain extent) (Earman, 1992) echo Hawthorne's view about Bayesian reasoning.

[12] Indeed, it might be supposed that one must if one assumes that no evidential process can result in a logically possible hypothesis with some non-zero probability becoming zero probability (or vice versa).

other hand, if one instead concedes that these premises are *merely* heuristically or pragmatically justified, then one thereby gives up on the corresponding conclusions being fully epistemically justified, for they depend on (epistemically) unjustified assumptions.

Although discussions of eliminative reasoning have not been carried out explicitly in terms of this dilemma (i.e., in terms of a contrast between heuristic and epistemic justification), the debate over whether eliminative reasoning is different than hypothetico-deductive reasoning—which has been an important part of the discussion—is implicitly based on this distinction. Dorling and Norton draw attention to eliminative reasoning precisely because they regard it as better justified, epistemically speaking, than hypothetico-deductive reasoning. However, if the premises of greater generality cannot be justified, then eliminative reasoning fails to be, epistemically speaking, a scientific method distinct from hypothetico-deductivism (Laymon, 1994; Worrall, 2000)—namely, because the premises of greater generality must be taken as assumed, just as they are in the hypothetico-deductive approach. As hypotheses in the hypothetico-deductive approach are suppositional, they can only have a merely heuristic or pragmatic function; hence, so too would hypotheses about the universe of explanatory hypotheses in the context of eliminative reasoning.[13]

Proponents of eliminative reasoning, at least those who wish to secure a genuine epistemic standing for (certain) cherished scientific theories, have often claimed genuine epistemic justification for the premises of greater generality while only providing a heuristic justification for them. That is, they offer arguments that the premises of greater generality are justified (although this justification is in fact only heuristic) and then conclude that the conclusion of the eliminative inference is (epistemically) justified. The equivocation is not always apparent (perhaps even to those employing it), for often the questionable assumptions are papered over, as said above. This is especially so when they are relegated to the "unproblematic background knowledge," which Laymon frames as a dialectic challenge: "the problem for supporters of demonstrative induction is then that of finding ways to keep hypotheses and theories [by which he means the conclusions of intermediate generality] in the confirmational limelight and to keep the general principles [by which he means the premises of greater generality] in the unproblematic background" (Laymon, 1994, 27).[14]

To see how epistemic and heuristic justifications are problematically mixed in some commentators' discussions of eliminative reasoning, the language employed by Norton and Dorling when defending key premises is particularly revealing. Both frequently emphasize the "confidence" one can have in the premises of an eliminative inference. Norton, as we have seen, claims that the premises of greater generality can be placed "beyond reasonable doubt" by their generality, this generality making one "very confident" that the "correct" theory is within the universe of theories selected. Dorling also frequently mentions the "plausibility" of these premises as grounds for a corresponding confidence.[15] Whereas "reasonable doubt" and "correct" readily suggest genuinely epistemic readings, "confidence" and "plausible" tend to suggest heuristic or pragmatic readings. Of course, if one reads "confidence" as *rational confidence in the truth of* and "plausible" as *probable*, then everything is epistemologically aright. But if all such expressions are intended in this way uniformly, then one expects some valid, objective reason for attributing the corresponding rational confidence or objective (inductive) probability. However, you will not find anything of the kind in what Norton and Dorling have written about eliminative reasoning. Instead, what grounds they do supply are reasons for mere acceptance rather than belief (e.g., that the premises of greater generality are "uncontroversial"). Hence, it is more reasonable to read Norton's "confidence" and Dorling's "plausible" to signify mere acceptance rather than justification. For the sake of consistency (and to avoid equivocation), that means that one should read "beyond reasonable doubt" and "correct" in appropriate pragmatic or heuristic terms. Hence, a consistent interpretation of Norton and Dorling's accounts of eliminative reasoning are (at best) pragmatic or heuristic in character.[16]

Is it possible to attribute an objectively justified probability to the premises of greater generality? While some skeptics may deny it (insisting, perhaps, that only a mere plausibility can be assigned), it is evident from scientific practice that there are at least some justified attributions of probabilities to theoretical hypotheses, namely

[13] The only difference between the two forms of reasoning would then be a simple "logical" distinction, namely, that the conclusion of a deduction in the context of hypothetico-deductive reasoning is an empirical proposition (E in the examples above), a proposition of "lesser generality," whereas the conclusion of a deduction in the context of eliminative reasoning is a theoretical proposition (H or h in the examples above), a proposition of "intermediate generality."

[14] The main critiques of individual cases of eliminative reasoning (Laymon, 1994; Bonk, 1997; Hudson, 1997) tend to proceed by drawing the background assumptions into the limelight, in order to expose them as deserving far less acceptability than they seem to when relegated to the "unproblematic" background.

[15] Dorling, adopting a generally subjectivist Bayesian point of view, regards the pattern of reasoning to be applicable whenever "we could have more initial confidence" in the general hypotheses than the deduced generalization" (Dorling, 1973, 360), but he also, in agreement with Norton, suggests that "a hypothesis is placed at a considerable advantage if it can be shown to be required by the facts provided we *assume* certain plausible general principles (Dorling, 1973, 371) (emphasis added). See also his comments in (Dorling, 1995, 101).

[16] If the constraints are not even heuristically acceptable, then eliminative reasoning might still have a certain psychological value, which, as Worrall suggests is not nothing: "here I think it should be acknowledged that a Newtonian deduction, whatever its accreditational value from a logical point of view, may have great accreditational value *psychologically speaking*" (Worrall, 2000, 69).

those which feature in paradigmatically successful cases of probabilistic inductive reasoning (after all, if there were not, then statistics, lacking the requisite objectivity, would have limited application in practice). In what circumstances does one have such an adequate justification? To my mind, the most significant factor is whether one has adequate knowledge of the relevant probability space (in particular, its limits). Accordingly, for the conclusion of an instance of eliminative reasoning to be secure (or even just probable), it depends on the premises of greater generality being accorded an objective probability (rather than a mere plausibility), and this plainly requires adequate knowledge about the limits and nature of the relevant probability space. One might object, however, that there is a significant disanalogy between the paradigmatically successful cases of inductive reasoning (in statistics, say) and the case of hypotheses concerning "universes of hypotheses": for, whereas the space of possibilities normally considered in statistical reasoning is a space of *concrete* possibilities ("it was either Miss Scarlett, Rev. Green, Colonel Mustard, Professor Plum, Mrs. Peacock, or Mrs. White"), the space of possibilities in theoretical reasoning concerns collections of *abstract* hypotheses. Since an adequate assessment of the latter probabilities demands that one have a handle on the space of such alternative theories (including unanticipated ones), one might question whether that is even possible.

Norton himself holds that "it is not too difficult to make some assessment of the magnitude of the risk buried in [these premises of greater generality]" (Norton, 1994, 17), but, given the points made above, he must mean here a mere "plausibility assessment." For sure, it is not to difficult to make some "assessment" of the magnitude of risk by simply guessing, thinking about one's "unproblematic background knowledge," etc. A genuine epistemic assessment of this "magnitude of risk," by contrast, is hardly a "not too difficult" matter. What is required is a means of assessing the scope of relevant alternative theories in the given explanatory context, and this kind of assessment involves considerable scientific work, for at least part of making such an assessment is exploring the space of relevant alternatives by trying to *actually develop theories*.

Among the proponents of eliminative reasoning, Earman appears to best appreciate the importance to the method of actually developing alternative theories, for it is a feature which he incorporates into his own (pragmatic) version of eliminative reasoning. His case study, like Norton's and Dorling's, focuses on gravitational theory, and begins with the observation of the dominance of Einstein's theory in the early 20th century. Reflecting on the fact that some theories become well-established, like Einstein's, he observes that "a theory may become dominant by default or by remaining standing when the Sherlock Holmeses of science have 'eliminated the impossible"'(Earman, 1992, 173). In other words, a theory may become dominant because it is proposed without there being any alternative or because it is the outcome of a process of eliminative reasoning. He argues that Einstein's general theory of relativity "falls somewhere between these extremes." Although Einstein himself did eliminate some number of spacetime theories, his method was not exhaustive:

> Although Einstein did not engage in a systematic exploration of alternative theories of gravity, he did offer a *heuristic* elimination in the form of arguments that were supposed to show that one is forced almost uniquely to the [general theory of relativity] if one walks the most natural path, starting from Newtonian theory and following the guideposts of relativity theory. (Earman, 1992, 173–4, emphasis added).

Although he begins with Einstein's theory, Earman's historical narrative mainly picks up after Norton's and Dorling's (who focus only on Einstein's development of general relativity), detailing in particular how general relativity became the default theory of gravity due to there being little work dedicated immediately after its development to finding alternatives (that is, it became the default due to the lack of any serious alternative). Was there in fact no possible alternative (as the eliminative inference detailed by Dorling and Norton would lead us believe) or had scientists simply not invested the time to search for any? Earman remarks that "the exploration of the space of possibilities constantly brings into consciousness heretofore unrecognized possibilities" (Earman, 1992, 183), which appears to indicate that he regards the proliferation of alternatives as always cognitively possible. Whether or not such theoretical underdetermination always exists in principle, at least in the case of gravitation there was indeed an array of alternatives, a "veritable 'zoo'," waiting to be discovered once the search began. Clearly, such a proliferation of alternatives definitively undermines the objectivity of any "plausibility" assessment of the universe of theories in an eliminative inference which suggests that Einstein's theory is highly probable. Earman rightly concludes from this evidence that "physicists of earlier decades were not rationally justified in according Einstein's [general theory of relativity] a high probability" (Earman, 1992, 182).

Earman proposes that the "zoo" of possible alternatives to gravity can only be tamed by building a "theory of theories of gravitation," namely, one which hypothesizes that any theory of gravitation must have certain specified theoretical properties (e.g., is geometrical, covariant, tensorial, derivable from an action principle, etc.). In this way, the eliminative process can be continued at a "higher" level, which involves finding the empirical means to acquire further evidence that can be used to eliminate the newly conceivable alternatives at that level.

Assimilating theory exploration and classification to eliminative reasoning, Earman thus offers a distinctive reformulation of the eliminative program:

> The main business of the program, eliminative induction, is propelled by a process typically ignored in Bayesian accounts: the exploration of the possibility space, the design of classification schemes for the possible theories, the design and execution of experiments, and the theoretical analysis of what kinds of theories are and are not consistent with what experimental results. (Earman, 1992, 177)

As welcome as Earman's improvements to the simpler version of the eliminative method are—where the program meets with success, it will certainly lead to *incrementally* improved justification for the conclusion of the eliminative inference—there is no avenue for making a novel argument which justifies the premises of greater generality. Each of the higher levels of classification faces the same basic justification problem as in the simpler version of the eliminative method. And while shedding the burden of justifying theoretical assumptions by simply *defining* or *stipulating* a classification scheme of hypotheses may certainly be heuristically sensible (at least in some contexts)—for example, because of the way it structures an empirical context for investigation—it represents no genuine advance in securing, epistemically, the conclusions of the eliminative process, for the incremental improvement in epistemic justification is equivocal (and potentially negligible).

I emphasize that nothing of what I have said is meant to suggest that a method which is "merely" pragmatic or heuristic is deficient, or otherwise an unimportant part of scientific methodology. Heuristic appraisal is principally focused on assessing the "fruitfulness" of a theory rather than its potential for epistemic justification, which is in keeping with the idea that heuristics are aimed at problem-solving rather than truth or validity specifically (Nickles, 1981, 1987, 1988). Whether justification is merely pragmatic or merely heuristic, it differs importantly from the canonical notion of justification, epistemic justification, that buttresses the concept of knowledge. It is precisely this difference that leads one to say of a heuristically justified theory that it is "pursuit-worthy" (Laudan, 1977) and not yet fully "belief-worthy" (as it would be if it were epistemically justified).[17] While "mere" plausibility arguments can certainly guide us towards accepting hypotheses that are "pursuit worthy," they cannot transform merely plausible premises into epistemically justified conclusions.

4 Meta-Empirical Support for Eliminative Reasoning

As we have seen, previous proponents of eliminative reasoning lapsed into merely pragmatic justifications for the premises of greater generality—seemingly almost inevitably. I suggest that the reason why they were unable to epistemically justify these crucial premises is because they confined themselves to a too narrow conception of evidence. These premises' justification depends, I claim, on a broader kind of evidence, specifically what has been called meta-empirical evidence. I argue in this section that this kind of evidence provides a means to genuinely justify the method of eliminative reasoning.

To see how a broader kind of evidence is required here, it is helpful to see how the general justification problem is related to the problem of underdetermination of theory by empirical evidence: To fully justify eliminative reasoning, the premises of greater generality (those which select a particular set of hypotheses) must be justified—that is the problem of justification. But how can such premises be justified? Evidently, their full justification depends on positive reasons to think that (relevant) alternative hypotheses are disfavored (or do not exist), such that the particular set of hypotheses identified by the premises are left without alternative—that is, it depends on underdetermination being overcome in that context.

The underdetermination problem has been persistent in the philosophy of science primarily because it is not clear what legitimate evidence can overcome it however. In the face of the underdetermination problem, one well-worn strategy, of course, is to simply take a pragmatic or heuristic attitude to theories and theoretical justification (as does, e.g., van Fraassen (1980)). As we saw above, even when they do not necessarily mean to take up such attitudes, proponents of eliminative reasoning tend to lapse into them.

My suggestion, again, is that this happens because they restrict themselves to a too narrow conception of evidence. According to the more or less "canonical" conception of empirical evidence that gives rise to the

[17] Incidentally, these remarks provide a further opportunity to illustrate how heuristic and epistemic concepts are mixed up by proponents of eliminative reasoning. Norton and Dorling, for example, seek to link the method with both justification and "discovery," Dorling saying that "the method of demonstrative induction can also, in principle, play a significant role in the logic of discovery as well as in the logic of justification" (Dorling, 1973, 371), and Norton that "since [Einstein's] induction is a rational process and, at the same time, a justification of the theory, we have: the generation of the theory proceeded hand in hand with the development of its justification" (Norton, 1995, 31). Their common suggestion that eliminative reasoning is linked to both discovery and justification is unsurprising, however, once one recognizes that their accounts of eliminative reasoning are in fact merely heuristic or pragmatic in character, which are, of course, precisely the features that are principally involved in discovery methods. Lacking a genuine epistemic justification for the outcome of a process of elimination, the link between justification and discovery they suggest, then, is merely a link between *heuristic* justification and discovery.

standard underdetermination problem, legitimate evidence for a hypothesis includes only those empirical facts that fall within the scope of the hypothesis. In other words, empirical facts entailed by a hypothesis (or, more accurately, by it in concert with suitable auxiliary hypotheses) are the only pieces of evidence relevant to the justification of the hypothesis. Empirical facts such as these, however, are (by themselves) inadequate for overcoming underdetermination—not only because they are unable to differentially support hypotheses whose scopes they are within but *also* because they cannot discriminate what the possible alternative theories *are* (except crudely, by consistency).

Among the proponents of eliminative reasoning, Norton is the one most explicitly motivated by the problem of underdetermination, for he sees a concerningly wide gulf between the deliverances of "practical science" and the "underdetermination thesis":

> There is a serious contradiction between a thesis increasingly popular amongst philosophers of science and the proclamations of scientists themselves. The underdetermination thesis asserts that a scientific theory cannot be fully determined by all possible observational data. Scientists, however, are not so pessimistic about the power of observational data to guide theory selection. The history of science is full of cases in which they urge that the weight of observational evidence forces acceptance of a definite theory and no other. Thus our science text books teach us to accept the approximate sphericity of the earth, the heliocentric layout of planetary orbits, the oxygen theory of combustion, and a host of other theoretical claims simply because the evidence admits no alternatives. (Norton, 1993, 1)

> Typically, a scientist is pleased to find even one theory that is acceptable for a given body of evidence. In the case of a mature science, there is most commonly a single favored theory to which near certain belief is accorded and which is felt to be picked out uniquely by the evidence. Challenges to the theory from aberrant hypotheses or experiments are rarely considered seriously. (Norton, 1994, 4)

In these passages, we clearly see that Norton correctly identifies the basic condition for (fully) overcoming underdetermination, namely, that the evidence eliminates all alternatives, picking out the correct theory uniquely. Thus, it would seem that Norton recognizes the key principle for justifying the premises of greater generality. Indeed, Norton appears to also recognize the need to go beyond a narrow conception of evidence in order to overcome the underdetermination problem, for he goes on to observe (after the first quoted passage) that "the case for the underdetermination thesis depends in large measure on an impoverished picture of the ways in which evidence can bear on theory" (Norton, 1993, 1). However, as argued above, he is mistaken when he supposes that eliminative reasoning addresses the problem by introducing new, "richer" ways in which evidence can bear on theory. Insofar as a more or less canonical conception of empirical evidence is maintained, eliminative reasoning is not epistemologically distinct from hypothetico-deductive reasoning (which, of course, is the very method that occasions worries about underdetermination). If hypothetico-deductivism faces the underdermination problem because empirical evidence is confirmationally equivocal between alternative hypotheses, then eliminative reasoning faces the same problem, since empirical evidence is confirmationally equivocal between alternative sets of hypotheses.

Thus, if underdetermination is to be overcome in a way that secures the epistemic standing of theoretical hypotheses, what is needed is a broader conception of evidence: not only the "narrow" empirical evidence "from below" which features in the canonical perspective on empirical evidence but also a kind of evidence "from above" or "from the side." The canonical conception holds that the only facts that can serve as evidence are these empirical facts "from below"—only these constitute empirical evidence proper. It follows that the only place to look for the kind of evidence which could overcome the underdetermination problem is what would be called "non-empirical" facts according to the canonical conception.[18]

What is "non-empirical evidence" though? Many, I expect, will immediately think of the family of "theoretical virtues," such as simplicity, explanatory power, fruitfulness, and the like. Of course, these virtues could only properly be *evidence* if they have epistemic rather than merely heuristic import. From a generally evidentialist point of view in epistemology, evidence is simply that which justifies, and a standard recipe for assessing justification in epistemology is, "does the putative evidence make the conclusion more likely (to be true)?" Does the possession, then, of some one of these virtues by a theory in and of itself make that theory more likely (to be true)? The preponderance of counter-examples and the ease with which positive examples are readily explained away (as merely pragmatic or heuristic) leads me to set this proposal aside as an unpromising line of epistemic justification for eliminative reasoning (although I recognize that this claim is controversial, with many arguments offered on both sides in what has become a long-running debate—see, e.g., (McMullin, 1982; Douglas, 2009)).

If theoretical virtues and similar "non-empirical" factors exhausted the possibilities for "non-empirical evidence," then perhaps we would have to resign ourselves to the nature of theoretical reasoning (and eliminative

[18]As noted previously, Bird is one of the few proponents of eliminative reasoning who is sensitive to this need to broaden the prevailing perspective on evidence, and he does endorse the inferential (and justificatory) role of non-empirical ("non-observational") evidence.

reasoning specifically) being merely pragmatic or heuristic. However, "non-empirical" here merely signifies that the purported evidence is not empirical in the *narrow* sense mentioned above. Thus, there remains the (easily overlooked) possibility that there are kinds of empirical evidence (i.e., in a broader sense) which are nevertheless "non-empirical" in the narrow sense. Empirical evidence in the broad sense would comprehend any empirical fact, obtained by observation or experiment, that could potentially function as evidence for a hypothesis, whether within the scope of that hypothesis or not. All that would be required is that such a fact is evidentially relevant to the hypothesis. By the fact of being empirical, facts of this kind are already evidence *in principle* (unlike the theoretical virtues just considered, which need their epistemological credentials demonstrated), so all that must be shown is relevance in order to be properly regarded as evidence *in fact*.[19]

So, is there some kind of broad empirical evidence that is evidentially relevant to the premises of greater generality in eliminative reasoning? Indeed there is. Dawid has shown that a particular class of evidence, which he has called "non-empirical" in the past (because it is not empirical in the narrow sense) and meta-empirical more recently, is evidentially relevant to assessments of local limitations to underdetermination (i.e., limitations with respect to the relevant explanatory context) (Dawid, 2013, 2018). Hence meta-empirical evidence is confirmationally relevant to suppositions about the set of explanatory hypotheses itself (and therefore legitimately regarded as evidence *in fact*). The meta-empirical evidence identified by Dawid is clearly empirical in the broad sense (and not the narrow), since it involves observations about the scientific research process itself (which is why it is called "meta-empirical" rather than simply "empirical"). As he says,

> Non-empirical confirmation is based on observations about the research context of the theory to be confirmed. Those observations lie within the intended domain of a meta-level hypothesis about the research process and, in an informal way, can be understood to provide empirical confirmation of that meta-level hypothesis. (Dawid, 2016, 195).[20]

Meta-empirical evidence is precisely the kind of evidence that can serve to justify the selection of a set of hypotheses for eliminative treatment. In this way it furnishes us with one specific and important means of securing the justification of the premises of greater generality of eliminative inferences and thereby the conclusions of these inferences themselves. The remainder of this section will clarify the nature of meta-empirical evidence and show how it can justify the premises of greater generality of an eliminative inference.

Firstly, to illustrate the use of meta-empirical evidence, I begin with one kind of argument in which it is used, which Dawid calls the "no-alternatives argument."[21] The conclusion of this kind of argument is that underdetermination is limited by there being no alternatives to a given hypothesis. The relevant meta-empirical evidence which can be used to support this conclusion is the number of alternative theories that have turned up during the search for alternatives to the given hypothesis. Consider an analogous, commonplace example: surely the deductive powers of a Sherlock Holmes are not needed to infer from the fact that no (or few) Easter eggs turn up after a thorough hunt for Easter eggs in a delimited area that there are (probably) no (or few) Easter eggs remaining hidden. Plainly, the results of such a search are relevant to assessing hypotheses regarding the total number of Easter eggs that have been hidden. Similarly, if a concerted search for alternative theories turns up no alternatives, then that fact should likewise be regarded as genuine evidence relevant to the hypothesis that there are no alternatives. By the same token, if a concerted search for alternative theories turns up numerous alternatives, then that is compelling evidence against there being few alternatives (just as we saw in Earman's case of gravitational theories in the 20th century).

Not all will find meta-empirical arguments like this intuitive. Critics are inclined to dispute the evidential significance of meta-empirical evidence in mainly in two ways: some argue that there can never be evidence for limitations of underdetermination, and others that such evidence is always negligible.

In the context of a no-alternatives argument, the first counter-argument would be that the exploration of the space of alternatives inevitably leads to the discovery of alternatives (as we saw Earman suggesting above). If such proliferation were always possible, then a no-alternatives argument would be futile (also in support of the premises of greater generality in an eliminative argument). Of course, in the commonplace example, the number of hidden Easter eggs is obviously not inexhaustible in a given Easter egg hunt. So are theoretical possibilities somehow

[19] Of course, for those too accustomed to the narrow conception of empirical evidence, it may seem that any empirical facts falling outside of the scope of the relevant theoretical hypotheses cannot possibly be confirmationally relevant to those hypotheses, hence not evidence. However, the role of "indirect" evidence in hypothesis confirmation is in fact long- and well-established—in any case, at least since (Laudan and Leplin, 1991). Additionally, some may be drawn to the narrow conception of empirical evidence by the manner in which evidence typically appears in probabilistic inductive schemes, like Bayesian confirmation. Nevertheless, such schemes are not at all committed to a narrow reading of empirical evidence, for broader forms of evidence can be modeled within them (more or less) straightforwardly (Dawid et al., 2015; Dawid, 2016; Dardashti and Hartmann, 2019).

[20] The formal way of understanding such confirmation requires certain niceties, which can be rendered in the Bayesian framework (Dawid et al., 2015).

[21] Further kinds and applications of meta-empirical evidence are detailed in (Dawid, 2013).

inexhaustible (that is, in a given scientific context) in a way that Easter eggs are not? Scientifically speaking, all we have to go by is the evidence: the facts show that sometimes scientists find many relevant alternatives, sometimes very few. The question then is whether the facts reflect the actual limits of underdetermination in that context or that there are other factors that explain them. Obviously, meta-empirical evidence is defeasible in this way, for usually there are alternative possible explanations for the meta-empirical facts: the scientists did not look hard enough, their theorizing was too limited, etc. Nevertheless, it is important to recognize that the defeasibility of evidence does not undermine the *evidential status* of evidence—it may simply make it weaker or negligible.

Thus, the first counter-argument leads to the second, which alleges that meta-empirical evidence can never be significant evidence—for example, because in every case it is far more likely that the alternative explanations obtain (for example, that the scientists have not looked hard enough when they cannot find alternatives).[22] Of course, if by "likely" the objector means that they just find alternative explanations more psychologically satisfying or more plausible, then there is nothing worth disputing here. If by "likely" they mean instead that they think the available evidence suggests it, then that should be considered on a case by case basis—for what evidence could indicate that meta-empirical evidence is insignificant in *all* cases?

In any individual case, the allegation of negligibility may certainly carry weight, for potentially there may well be strong evidence that could make a counter-explanation compelling. For example, a proliferation of alternative theories, as already said, obviously rebuts the hypothesis that underdetermination is strictly limited in that context (and does so directly). By the same token, however, each alternative explanation that undercuts the alleged meta-empirical evidence likewise has its own possible rebuttals. Moreover, it should be kept in mind that the conclusion of a meta-empirical argument can be made more robust by appeal to further, independent meta-empirical arguments that support that same conclusion—particularly if they also act as rebuttals to one another's defeaters.[23] Whether the defeaters of meta-empirical arguments are stronger than the arguments themselves thus depends in each case on a careful assessment of the relevant, available evidence.

Some contexts are more hospitable to meta-empirical arguments, some less. Dawid's characterization of meta-empirical arguments was first motivated by an interest in the epistemic status of string theory, a theory whose adherents strongly support the theory despite an acute lack of empirical evidence (Dawid, 2006, 2009; Camilleri and Ritson, 2015). While his general philosophical arguments coupled with this one striking example may sway some to acknowledge the validity of meta-empirical evidence and reasoning in science, establishing the general epistemic significance of meta-empirical arguments surely cannot rest on a single example, especially as any individual case is likely to be controversial (due to a wide variety of contextual factors). Thus, what is called for to decide the validity of meta-empirical evidence is a thorough investigation into historical and contemporary cases alike, focused on the role of meta-empirical considerations in scientific reasoning in these cases. No doubt this would shed light on their evolving presence, role, and significance.

Even in advance of these investigations, it is clear that string theory is a peculiar example due to its special nature as a theory of everything (physical). More typically we should expect that meta-empirical arguments will at best favor, perhaps even only weakly, a collection of hypotheses for further elaboration and investigation. The need for further investigation may in particular suggest the need for an empirically-driven process, one which may reveal overlooked regularities or other empirical facts that can then contribute to a reformulation or elaboration of the theoretical assumptions that led to them. Eliminative reasoning, of course, is precisely just such a process. Obviously, the need for further investigations of this kind holds just as well if the arguments that bolster the premises of greater generality are only plausibility arguments. In that case, however, the process of eliminative reasoning has an importantly different, essentially exploratory character. In this scenario, eliminative reasoning is unable to provide further guidance on the way forward theoretically: Does one expand the universe of theories in some plausible direction or another, in keeping with Earman's program? Or does one seek to use the results of the eliminative process to further articulate the assumptions of that process? If there is some degree of meta-empirical justification for the initial set of hypotheses, however, then there is a corresponding motivation to articulate the favored hypotheses further in light of the outcome of the process of eliminative reasoning (that is, by making use of novel empirical insights obtained through that process).

This contrast is methodologically significant and important for understanding the role of eliminative reasoning in scientific method, so a brief discussion of it will be taken up in the final section (§5). However, the primary aim of this paper has been accomplished: to demonstrate the possibility of adequately justifying eliminative

[22] In this vein, Magnus (2008, 311) suggests an argument from inductive risk on behalf of the empiricist against eliminative reasoning (or demonstrative induction): "a demonstrative induction typically requires both high-externality observation reports and premises that constrain the form of admissible theories. These latter constraints are non-empirical, and so (the argument goes) expose us to more risk than an ampliative argument from the same observations to the same conclusion." Without any meta-empirical evidence with which to objectively assess the degree of inductive risk, however, such a claim rests on, as Magnus says, mere psychological speculation.

[23] This kind of robustness is in fact a crucial feature of Dawid's overall argument for the legitimacy of "non-empirical confirmation" and in particular its application to string theory (Dawid, 2013). In particular, he cites three distinct meta-empirical arguments which have the effect of rebutting one another's defeaters and also robustly supporting a common conclusion.

inferences. I claim to have established this much in this section. If there are meta-empirical arguments strong enough to support the premises of greater generality, then eliminative reasoning can lead to a conclusion that is genuinely epistemically justified (to some corresponding degree) through the further process of elimination of alternatives. Meta-empirical arguments justifying step (I) in concert with eliminative arguments in step (II) may thus establish a degree of genuine trust in the conclusion of eliminative arguments; lacking some defensible "non-empirical" justification of the premises of greater generality, eliminative reasoning may still be employed but only as a pragmatic or heuristic method, one which accordingly fails to result in genuinely epistemically justified conclusions.

5 The Methodological Context of Eliminative Reasoning

By having strong meta-empirical support for the premises of greater generality, some well-established scientific theories may be "rationally reconstructable" in the form of a fully justified eliminative argument (even if they were not so justified at the time of their development). In typical cases, though, the application of the eliminative method will plausibly rest on meta-empirical arguments (or other indirect justifications) which are insufficiently strong to fully justify eliminative inferences. In these circumstances, there is an important methodological difference between eliminative inferences that are partially justified by meta-empirical arguments and those that are merely pragmatic or heuristic in character—that is, those that rely on premises of greater generality which are simply taken to be "merely plausible."

I will explore this difference in this section only by way of an example, as a complete account would require a separate treatment. I choose as my case the theory of cosmological inflation, as it is a salient example of a theory, like string theory, where scientists working in the field have a high degree of trust in the theory despite the continued lack of empirical confirmation of many core (in principle, empirical) predictions of the theory. Although inflationary theory has been a topic of intense discussion and controversy among physicists for decades (Penrose, 1989; Brandenberger, 2000, 2008, 2014; Hollands and Wald, 2002; Turok, 2002; Ijjas et al., 2013, 2014; Guth et al., 2014), philosophers have recently weighed in on the epistemic status of the theory as well: Dawid (forthcoming) and McCoy (2019), for example, support cosmologists' conclusions on meta-empirical and explanationist grounds, respectively, while Smeenk (2017, 2019) argues that, despite some important empirical successes, the theory so far fails to meet a "higher standard of evidence," a standard which he extracts from philosophical analyses of certain historical cases, like Newtonian gravitation, which appear to meet it.[24]

I will first sketch the "large-scale" contours of the history of inflationary cosmology that are needed to provide relevant context for my discussion.[25] Inflationary theory was initially motivated (in the early 1980s) by its unified solution to certain "fine-tuning" problems with the long-standing standard model of cosmology, the hot big bang model. The development of the inflationary scenario led quickly to models with distinctive predictions concerning the character of observable anisotropies in the cosmic microwave background radiation.[26] Eventually these were strikingly confirmed by successive satellite observational programs (e.g., WMAP, Planck). Stronger observational evidence for the theory is hoped for by confirming predictions of a distinctive ratio of scalar-to-tensor perturbations from observations of the polarization of the cosmic microwave background, but so far these hopes are unrealized. On the theoretical front, however, an extensive model-building program has led to a "zoo" of inflationary models over the course of the past decades. Although many of these models have been ruled out by the ongoing observational program, there remains a notably strong feeling in the field that model-building is relatively unconstrained within the inflationary paradigm.

Although everyone agrees that the confirmation of the theory's predictions of cosmic microwave background anisotropies is an important success of the theory, from the point of view offered in this paper, this kind of "empirical confirmation" is by itself equivocal with respect to the program's future viability, for the observational results do not distinguish between alternatives. Therefore, on the strength of these successful predictions alone, inflationary theory certainly does not deserve to be regarded as "settled theory." How, then, does one explain inflationary theory's pre-eminence in the field of cosmology and the relative dearth of genuine alternatives to the theory (alternative theories to inflationary theory, not alternative models of inflation, of which, as said, there are many)? Critics, like Earman and Mosterín (1999), tend to favor explanations based on social factors like "group think" and "popularity," and eschew arguments based on methodological or epistemological considerations, especially those that promote a novel perspective, for in their view inflationary theory is not sufficiently epistemically grounded.

[24] A few other notable critical discussions of inflationary theory by philosophers can be found in (Earman and Mosterín, 1999; Smeenk, 2014; McCoy, 2015).

[25] More complete historical details of inflationary theory's history may be found in (Smeenk, 2005, 2018).

[26] An anisotropy is a difference in a physical quantity from the mean in a certain direction (from the Earth, in this case).

5.1 A "Higher Standard" of Empirical Evidence

The latest critique of the status of inflationary theory comes in a series of recent papers by Smeenk (2017, 2018, 2019), who advocates an approach to theory assessment that involves holding theories to a "higher standard of empirical evidence." In applying this standard to the case of inflationary cosmology, he returns a largely negative verdict on inflationary theory's current epistemological status and its future prospects. Like with the analysis of Earman and Mosterín (1999), this verdict is in sharp contrast with the more positive assessment of the theory by most cosmologists and, as I will argue in the following subsection, the assessment made possible by recognizing the epistemic significance of meta-empirical evidence. Smeenk's analysis, and its shortcomings, will provide a useful counterpoint to the meta-empirical analysis which follows.

While Smeenk explicitly acknowledges the importance of the eliminative method in science, he does not support the claims of earlier proponents of the method, like Norton and Dorling, who argued that it yields secure, trustworthy conclusions in science. Trustworthy conclusions must instead meet a higher standard of empirical evidence. As Smeenk puts it, they must acquire "multiple, independent lines of evidence, in order to mitigate the theory-dependence of evidential reasoning" (Smeenk, 2017, 222). This theory dependence is roughly the the fact pointed out previously, namely that hypothetico-deductive (including eliminative) reasoning only issues in conditionally valid conclusions (i.e., conditional on the epistemically unjustified theoretical, hypothetical assumptions made). He draws attention to two exemplary kinds of argument which can satisfy this standard and which have been made frequently in the history of science: (1) what are sometimes called "overdetermination" arguments and (2) what I will call "refinement," or, following Chang (2004), "iterability of measurement" arguments. These arguments are often intended, much like eliminative reasoning, to resolve the tension between the apparent security of some scientific theories according to practitioners and the apparent insecurity of those same theories according to the "underdetermination" argument.[27]

Applying this general perspective to inflationary theory, he grants that there is some degree of support for the theory by overdetermination (of theory by evidence), since there are various ways that inflationary parameters may show up in independent phenomena. However, he suggests that this overdetermination is offset by a large degree of underdetermination, taking as his evidence the (allegedly) many possible alternative theories which can reproduce the relevant predictions of inflationary theory. In the face of underdetermination, he suggests that the only way forward is to gather more observational evidence, which can be used to eliminate these alternatives (one by one, like suspects and scenarios in a game of Clue). He dismisses, however, the possibility of any further overdetermination arguments in favor of inflation, saying that the theory is so flexible (in its possibilities for model-building) that it can be easily tuned to fit any other observations that may be obtained. Regarding the second strategy, refinement, Smeenk claims that it is simply not viable in the context of early universe cosmology, due to the impossibility of independently checking (observationally) the discovery of a new feature. Distinctive, inflationary-scale phenomena are beyond our probative reach without the assumption of a cosmological theory of the early universe.

Thus, since inflationary theory has exceedingly limited prospects for meeting his higher standard of evidence, it would seem that Smeenk's final assessment of inflation can only be that it is a hopelessly speculative theory, with dim prospects for empirical success of a lasting kind. I claim, however, that these two kinds of arguments praised by Smeenk are, like prevailing accounts of eliminative reasoning, based on a too narrow conception of evidence, and are ultimately equivocal without supplementation by meta-empirical (or other indirect) evidence. Hence his assessment of inflationary theory is too limited to comprehend the theory's full epistemological and methodological merits. I will defend my claim by treating overdetermination and refinement arguments in turn.

The overdetermination-style arguments lauded by Smeenk are members of a family of arguments long discussed by philosophers under a variety of names: consilience, robustness, triangulation, common cause, etc. The most famous and widely discussed example is Perrin's case for atomism, which was centered on the many phenomena involving the overdetermined Avogadro's number, but other examples include the fine structure constant in quantum electrodynamics (Koberinski and Smeenk, forthcoming) and the charge and other properties of the electron (Norton, 2000) (to mention a couple from authors discussed in this paper). These arguments essentially involve the empirical determination of a physical property described by a particular theory through multiple, independent means. A salient intuition that supports the epistemic significance of overdetermination arguments is that a theory which unifies a diverse set of ostensibly independent phenomena is much more likely to be true, since its competitors will tend to fail to adequately account for the diverse phenomena without ad hoc and conspiratorial adjustments to accommodate them. Thus, when one reads about Perrin's appeal to multiple conceptually independent experiments for determining Avogadro's number N, involving different combinations of measurable

[27] Interestingly, the three arguments favored by Smeenk—eliminative, overdetermination, and iterative—bear a striking "family resemblance" to the three meta-empirical arguments identified by Dawid (2013) (as Smeenk (2019, 317) himself points out). These arguments are the no-alternatives argument (akin to eliminative reasoning), the argument from unexpected explanatory coherence (akin to overdetermination arguments), and the meta-inductive argument (akin to epistemic iteration arguments).

quantities, one's impression is that the atomic hypothesis is more strongly supported than the mere sum of the evidence acquired.

Smeenk himself justifies the overdetermination argument in the following way:

> If the atomic hypothesis were false, there is no reason to expect these combinations of measurable quantities from different domains to all yield the same numerical value, within experimental error. This claim reflects an assessment of competing theories: what is the probability of a numerical agreement of this sort, granting the truth of a competing theory regarding the constitution of matter? The overdetermination argument has little impact if there is a competing theory which predicts the same numerical agreements. In Perrin's case, by contrast, the probability assigned to the agreeing measurements of N, were the atomic hypothesis to be false, is arguably very low. (Smeenk, 2017, 209)

Such an argument is equivocal however (at least in the absence of substantial additional assumptions). All that Smeenk manages to muster in favor of Perrin's conclusion is that the "likelihood" of the measurements given the falsity of the hypothesis is "arguably very low." Obviously, this is the attribution of a mere plausibility ("plausibly very low"), not an objective probability. Without genuine evidence supporting the claim that the likelihood is low, an intuitive attribution of plausibility is of course the best to which one can aspire, but such an attribution of plausibility surely cannot be the basis of a genuinely justified conclusion.

Even were a low probability justifiably attributed to this likelihood, the argument would still be inadequate, for it supposes that the probability of the atomic hypothesis, given Perrin's empirical evidence, is decided merely by the ratio of likelihoods: the likelihood of that evidence given the truth of the atomic hypothesis and the likelihood of that evidence given its falsity. Likelihood reasoning like this is well-known to be unreliable—at least without strong additional assumptions (e.g., on what alternatives there are and what the prior probabilities of the set of all hypotheses are). To convert the conclusion of likelihood-style reasoning like this into a genuinely justified conclusion, one should expect that these additional assumptions be justified in the context of reasoning, not left off or left implicit.

Overdetermination arguments like those advocated by Smeenk thus evince several of the themes discussed previously in this paper: the slide into mere plausibility reasoning, the need for genuine justifications of probability attributions (which cannot come from empirical evidence in the narrow sense but only from empirical evidence in the broad sense), and the conflation of heuristic and epistemic justification. Overdetermination arguments, like eliminative reasoning, are unquestionably important in the history of science, but the current state of their philosophical analysis is inadequate. For this reason, I believe they demand a novel philosophical analysis, one which would show what their true methodological and epistemological significance actually is; I fully expect that such an analysis would show that they do not furnish a truly "higher standard of evidence" without supplementation with properly meta-empirical (or other appropriate indirect) evidence.

Turning to refinement arguments, these too are members of a family of similar styles of argument that have been long discussed by philosophers, and also under a variety of names: reflective equilibrium, epistemic iteration, the method of successive approximation, the methodology of scientific research programs, etc. With respect to the iterability of measurement in particular, the most detailed analyses have focused on the several centuries of successive solar system modeling and testing of Newton's theory of gravitation (Harper, 2011; Smith, 2014) and the evolution of the measurement of temperature (Chang, 2004). This style of argument has also been invoked in the refinement of measurements of the fine-structure constant (Koberinski and Smeenk, forthcoming) and in the historical development of the concept of the electron (Bain and Norton, 2001) (to mention a couple from authors discussed in this paper). The basic idea behind such arguments is that modeling and measurement are iteratively refined in an approximating process, especially by exploiting empirical anomalies to successively refine the empirical description of the phenomena using the resources of the background theory.

The general strategy is perhaps most familiar in contemporary philosophy of science as part of Lakatos's methodology of scientific research programs (Lakatos, 1970), specifically as realized in the contrasting methodological roles of positive and negative heuristics.[28] Recall that according to Lakatos a theory that is able to address empirical anomalies in a way that later leads to confirmed novel predictions undergoes a "positive problem shift"; a theory that is unable to make novel predictions when forced to accommodate empirical anomalies instead undergoes a "negative problem shift." Lakatos's picture, however, offers no means for assessing the *future* viability of a theory, for it is a backwards looking assessment (as Lakatos readily admits). There is thus a significant lack of connection in Lakatos's account (and in refinement arguments more generally) between its prospective use of positive heuristics and its retrospective standard of epistemic justification. From this point of view, why should

[28]Despite largely sharing the methodological picture of Lakatos, authors advocating the iterability of measurement differ significantly in their epistemological and metaphysical proclivities. Smith, for example, strongly de-emphasizes the epistemic significance of theories in favor of low-level empirical generalizations of measurements, while Chang (2004) advocates a pluralistic, pragmatic interpretation of iteratively improving measurement.

one trust the positive heuristic to lead to epistemic success or regard the standard of justification as practically useful or valuable?

Smeenk argues that the iterability of measurement *does* underwrite an argument for epistemic success. He claims that Smith's analysis of Newtonian gravitational theory indicates that it would be an "enormous coincidence for a fundamentally incorrect theory to be so useful in discovering new features of the solar system" (Smeenk, 2017, 210). But what grounds the intuition that this coincidence disfavors alternatives? Without an argument that indicates the existence of genuine limitations to underdetermination, all one can say is what Lakatos said: the theory *has* experienced "positive problem shifts," its "positive heuristics" have been useful, and the theory is so far "empirically corroborated." Such an assessment, of course, does not do justice to the universally regarded status of Newtonian gravitation theory (within its appropriate context of application), which the supporters of "Newton's way of inquiry" are at pains to secure, and it does not make sense of why that program was as successful as it was. Hence iterability arguments face the same dilemma that was outlined previously for eliminative reasoning: either they must be properly justified to account for their epistemic success, or they must be regarded as at best heuristic in character. Without belaboring the point, I will simply suggest again that the meta-empirical perspective would do much to illuminate the evident success of refinement arguments in the history of science.

To conclude, Smeenk, recognizing that canonical perspectives on theory confirmation fail to do justice to the trustworthiness of many of our successful scientific theories, is rightly motivated to look for stronger means of justification. While the eliminative, overdetermination, and iteration arguments which he draws attention to are all undoubtedly important argument schemes in this regard, I have argued that they are ultimately unsuccessful without a broader kind of evidence that is suitable for underwriting them. My suggestions is that this kind of evidence will (in many cases) be meta-empirical evidence, because it is by its nature precisely the kind of evidence that is relevant for assessing the generic premises that figure in these arguments.

5.2 A Meta-Empirical Assessment of Inflationary Cosmology

Returning to the case of contemporary cosmology, it was mentioned above that there is currently an extensive inflationary model-building program on the theoretical side and an ongoing eliminative program on the observational side. One possible aim of such an eliminative program is to fix a subset of viable inflationary models by observational data. Proponents of eliminative reasoning should, it seems, laud the extensive model-building practice of inflationary cosmologists for its potential incorporation into the eliminative program currently being carried out, as one might suppose that one of these models will survive the successive eliminations. From the point of view offered in this paper, however, without any meta-empirical or other indirect evidence supporting the inflationary paradigm itself, one must regard the outcome of this eliminative program as equivocal. That is, while on the *assumption* of inflationary theory certain models may indeed be picked out by observations, there can be no strong warrant for these models (nor for the inflationary paradigm as a whole) on the basis of these observations alone.

According to Earman's version of the eliminative program, by contrast, all would seem to be well in this case. If the result of the eliminative program does yield some small set of observationally-favored models, then one can proceed further by generalizing theoretical assumptions beyond those of the inflationary paradigm and subsequently continue the eliminative program at a "higher level." Yet there is something critically lacking in "pragmatic" approaches to eliminative reasoning like Earman's. From the merely pragmatic perspective, there is no *methodological* reason to trust inflationary theory over any other account of the early universe in guiding an eliminative program—other than, of course, the "pragmatic choices" (i.e., guesses) of the involved scientists. Relying on a merely pragmatically-justified "choice" to drive research forward (or worse, one that is no better than random guessing) is a poor strategy indeed according to basic tenets of heuristic appraisal (specifically, what Peirce and his followers, such as Nickles (1989), refer to as the "economy of research"). Reducing risk in this scenario should instead involve a vigorous search for alternatives *to* inflation, not merely for alternative models *of* inflation.

This search for alternative theories is not at all what one sees in practice. Although some effort has gone into the development of alternatives, the pre-eminence of inflationary theory in the field has led to the focus primarily being on developing models of inflation rather than theoretical alternatives. To be sure, there are suggestive alternative approaches, but, pace Smeenk, for now none can legitimately claim much genuine epistemic credit of their own (except through accommodating some successes achieved already by the inflationary paradigm).[29]

Should one then conclude that cosmologists are poor assessors of justification and risk? Or do cosmologists in fact have the good sense to see that inflation is epistemically or heuristically favored somehow? Such questions

[29] As Brandenberger, one of the major proponents of various alternative approaches, says, "none of the alternative scenarios are without problems. In fact, one may argue that none of them address all of the classic problems of Standard Big Bang cosmology as well as inflation does" (Brandenberger, 2014, 119).

actually make little sense from the methodological perspective of Earman, as he accepts the possibility of unlimited proliferation of hypotheses and (accordingly) regards hypothesis selection as simply a matter of "making bets."[30] Informed by this perspective, one can see how Earman and Mosterín, in their critical appraisal of inflationary cosmology from 20 years ago, would draw such an equivocal conclusion about the status and future prospects of the theory:

> Despite the widespread influence of the inflationary paradigm, we do not think that there are, as yet, good grounds for admitting any of the models of inflation into the standard core of cosmology. Nevertheless, we neither expect nor wish inflationary cosmologists to be swayed by our reservations. It is creative physicists, not philosophers of science, who must *place the bets* that count on which avenues of research will prove to be fruitful. If the bets of the inflationary cosmologists prove to be correct, we will be the first to applaud. (Earman and Mosterín, 1999, 46, emphasis added)

Despite Earman and Mosterín's reservations (which in any case are based on criticisms of the theory that have proven widely off the mark, due either to misunderstandings or due to their dependence on merely contingent aspects of the theory as it developed), the subsequent confirmation of inflationary predictions should, it seems, have them vigorously applauding today. But what kind of philosophy is this? On offer is little more than a "guess-and-check" methodology and a "wait-and-see empiricism." What kind of normative guidance is this? If empirical success in science is always just a matter of "the lucky guess," then a severe methodological skepticism threatens (McCoy, 2019). We can certainly do better than skepticism; after all, science has done.

Thus, it is philosophically more sensible to suppose that cosmologists' initial "bet" to adopt the inflationary approach was actually—to some extent, at least—properly and well-motivated heuristically, that is, that it was part of an effective research strategy in the context of investigation—if not even substantially epistemically justified. Dawid (forthcoming) and Dawid and McCoy (2020) opt for this approach in presenting an account which aims to make sense of the development of inflationary theory, in particular by showing how it enjoys and has enjoyed a partial degree of genuine meta-empirical support throughout its history. This begins with the initial motivation for inflation, namely its alleged solutions of the standard model of cosmology's fine-tuning problems. Although precisely what the problems are and what solutions inflationary theory offers is a delicate issue (McCoy, 2015, 2017, 2018, 2020), these are clearly part of the justificatory story as understood by cosmologists themselves, as witnessed by their presentation in nearly every textbook and review on the theory.[31]

Supposing that inflationary theory's solutions of these problems give it some preliminary and partial justification, then the effort to obtain verifiable predictions is an essential next step in order to perform an empirical check on that preliminary justification. After its initial successes, however, the theory's further progress is hampered by a number of challenges: limited horizons for further empirical testing, various conceptual problems with the approach, and the recognition that the theory is relatively unconstrained in its possible model realizations. Thus, while there is some indication that the theory is on the right track, these problems lead to the recognition that further elaboration and development of the theory is necessary for further progress. Lacking firm theoretical guidance, cosmologists therefore turn to an eliminative program, *not* as a way of establishing some particular inflationary model as "correct" (there is insufficient justification of the theoretical assumptions for that) but rather as a means to elicit essential empirical guidance in further elaborating and developing the theory. Although this phase of research has all the appearance of mere "accommodation" of observation, as alleged, for example, by (Ijjas et al., 2013), this is to misunderstand the methodological point of the current approach, which is to gain new, empirically-driven insight into how to develop the theory in a productive direction.

The point of this section was to show that recognizing the methodological significance of meta-empirical evidence can helpfully illuminate the employment of eliminative strategies in science, as the example of inflationary cosmology is meant to illustrate. We can roughly discern three approaches based on the degree of meta-empirical evidence available: First, a theory that is very well-supported by meta-empirical arguments permits an eliminative approach which uses empirical evidence to determine the appropriate model of the theory for its description, one which is therefore strongly justified epistemically. Second, a theory that is only partially supported by meta-empirical arguments, like the theory of cosmological inflation, can make use of an eliminative approach to yield valuable empirical guidance on how to further articulate and develop the theory in a progressive way. Finally, a theory that is not well supported by meta-empirical arguments may still employ an eliminative approach, but there is a significant risk that it could be a fruitless and wasteful program from a heuristic point of view. More reasonable in such a situation is an active search for alternatives (by constructing them, by posing scientific problems

[30]Good bettors, of course, do rely on evidence to make their bets effective. Unlimited proliferation, however, largely undercuts the possibility of an evidence-informed bet.

[31]The analysis of (McCoy, 2019) claims that the justification of inflation at this stage is epistemic in character, but if we distinguish between genuine epistemic justification and merely heuristic justification, as done in this paper, then the argument there is equivocal between these two possibilities.

that yield them as solutions, etc.). By contrast, an evidential perspective that eschews "non-empirical" evidence as "unscientific" simply cannot see the valid methodological and epistemological reasoning behind these different research strategies employing eliminative reasoning: to the naive pragmatist-empiricist, beyond empirical evidence (in the narrow sense) everything simply looks like a matter of context and pragmatics...which is an impoverished image of real science indeed.

6 Conclusion

Eliminative reasoning is an important method which has been employed by scientists throughout the history of science—although it has only achieved significant recognition as such since the work of Dorling in the 1970s. While some philosophers have seen in it a distinctive means for securing the epistemic standing of theoretical hypotheses, their efforts to articulate the justification for the conclusions of eliminative inferences have unfortunately fallen short, in particular by not paying sufficient attention to the justification of the first step of the process: the identification of a space of possible (explanatory) hypotheses. What one finds instead are merely pragmatic, heuristic, or vague, overly general arguments rather than genuinely epistemic justifications.

My diagnosis of the trend of these arguments to lapse into equivocal justifications was that the evidential resources which they avail themselves of are too limited to justify the kind of premises that constitute this first step, namely, those premises which I have been referring to as the premises of greater generality. Relying solely on empirical evidence, narrowly construed, threatens these arguments with the problem of underdetermination of theories by empirical evidence. What is needed, I suggested, is evidence of a broader kind that shows that underdetermination is limited in the relevant explanatory context. Meta-empirical evidence is precisely this kind of evidence.

With appropriate meta-empirical evidence grounding step (I) of eliminative reasoning and empirical evidence grounding step (II), eliminative reasoning can in principle issue in genuinely epistemically justified conclusions. The significance of meta-empirical evidence is not only epistemological but also methodological however. While heuristics and epistemology have generally been cleaved apart and opposed dialectically in the traditional methodologies recognized in the philosophy of science (e.g., because heuristics concern the "fruitfulness" of a theory while epistemology concerns whether it is true or not), meta-empirical evidence brings these two aspects of methodology together. What a scientist needs is a reason to expect that his or her approach will be successful (epistemically speaking), whether with his or her current theory or a future successor. As Dawid says, "the strongest reasons for working on a theory in this light are those that have an epistemic foundation suggesting that the theory in question is likely to be viable" (Dawid, 2019, 103). I identified three distinct cases which differ in how strong the epistemic foundation is: A strong meta-empirical justification for a theory gives grounds for trust in that theory; it substantially confirms it. A partial meta-empirical foundation for a theory gives some grounds for trust in it but also suggests the need to improve and elaborate that theory, for example, by making use of empirical guidance obtained through an eliminative method, as well as the need to seek a stronger meta-empirical justification (e.g., by searching for alternatives). Finally, a poor meta-empirical foundation for a theory indicates, heuristically speaking, that "betting" on that theory is risky—the evidential situation recommends looking for better evidence, for example by problematizing the theoretical context, seeking alternative theories, etc.

The meta-empirical perspective on evidence thus demonstrates its principled significance not only to eliminative reasoning but scientific method more generally (detective fiction, of course, may be another matter). What remains to be investigated in more detail, however, is how fruitful the perspective is in applications, whether to historical case studies (like atomism) or contemporary practice (like string theory and inflationary cosmology), and how it can be applied to other methods examined by philosophers of science, such as the overdetermination and refinement arguments briefly discussed already here.

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