Calling for Explanation: An Extraordinary Account

Abstract: Are there any facts that call for explanation? According to one possible view, all facts call for explanation; according to another, none do. This paper is concerned with an intermediate view according to which some facts call for explanation and others do not. Such a view requires explaining what makes some facts call for explanation and not others. In this paper, I explore a neglected proposal, inspired by the work of George Schlesinger, according to which facts call for explanation when they belong to *extraordinary type*s. I compare the merits of this view to those of several alternatives and respond to a salient objection. I end with a discussion of how the theory fares when applied to cosmological fine-tuning arguments.

Keywords: calling for explanation; strikingness; fine-tuning; Schlesinger; extraordinary types

1 Introduction

The claim that certain facts call for explanation appears all over the place. It appears as a premise in influential arguments such as reliability arguments against mathematical Platonism (Field 1989, 26), evolutionary arguments against moral realism (Street 2008, 207), fine-tuning arguments for the existence of God (White 2018) and fine-tuning arguments for the existence of multiple universes (Parfit 1998), to name a few. However, what precisely distinguishes facts that call for explanation from those that don't is rarely examined.

In this paper I explore a proposal that has been suggested almost three decades ago by George Schlesinger, and has been pretty much neglected ever since, except for some occasional

mentions in footnotes. Perhaps one reason for the neglect is that Schlesinger's own writing is full of ambiguities and confusions. For instance, it is difficult to understand precisely what his proposal was supposed to be a proposal for. Thus, the first task of this paper is to clearly describe a problem. I then draw from Schlesinger a proposed answer to this problem. The second reason for neglect is that Schlesinger's proposal faces an obvious challenge. Therefore, the second task of this paper is to address the obvious challenge. A third task is to compare Schlesinger's proposal to alternatives that have appeared in more recent literature. I argue that the comparison yields further reason to be attracted to Schlesinger's proposal. Finally, I put Schlesinger's proposal to work, examining its implications for fine-tuning arguments.

The view that some facts call for explanation and some do not call for explanation is an intermediate view between two extremes.[1] At one extreme lies the view that all facts[2] must be

[1] The precise relationship between the extreme views and the target view may be more complicated than presented here due mainly to two issues. First, it's not clear that when PSR theorists claim that every fact has an explanation, they mean the same type of explanation meant by those who claim that only some facts call for explanation. Second, the theorists that believe that nothing calls for explanation clearly believe that sometimes we have reason to believe that certain facts have explanations. If a scientist told me that she just discovered the explanation for fact x, that's a non-controversial reason to believe that fact x has an explanation. If there is a dispute between the view that some things call for explanation and the view that nothing calls for explanation, it must be about what sorts of reasons are good reasons to believe that a fact has an explanation, not whether there are any such reasons. Below I gesture at one thought, namely, that the dispute is about whether we can have a priori reasons to believe that a certain fact has an explanation (Nozick 1981, 121). I intend on addressing these two questions in future work.

[2] Throughout, I suppose that facts are the targets of explanation. If you prefer phenomena, states of affairs, events or some other term, just replace "facts" with your favored term. Schlesinger himself chooses events. However, events seem to imply being situated in time, whereas I believe that also facts that are not situated in time can call for explanation. Still, the arguments of this paper do not deeply depend on this choice.

explainable. According to this view, a theory should be rejected if it implies of any fact that it is brute. This view is a version of the controversial principle of sufficient reason, according to which every fact must have an explanation.[3] Proponents of this view think, for example, that the initial conditions of the universe[4] must be explainable not because there is anything unique about those conditions but because *every fact* must have an explanation. This view also typically underlies cosmological arguments for the existence of God. It argues that the mere fact that anything exists requires explanation, and it proposes theism as that explanation. However, because it also implies that God's existence requires explanation, an infinite regress may be inevitable, which is one reason to resist the view.[5]

At the other extreme lies the view that there is no such principle.[6] This view claims that we must discover empirically which facts are explainable and which are brute, and that simply considering a fact a priori gives us no way to figure out how likely it is to be explainable.

Note that sometimes when I use the term fact, it is shorthand for a hypothetical claim that if p was a fact then it would or would not call for explanation. The same goes for explanation. Explanation is often thought of as a success term, meaning that false propositions can't count as explanations. However, sometimes when I speak of explanations it will be shorthand for a hypothetical claim of the form: If the hypothesized explanans were true then it would explain the given explanadum.

[3] The principle of sufficient reason is most famously associated with Leibnitz, although it has had earlier adherents (Melamed and Lin 2016). Recent proponents of this view include Della Rocca (2010) and Pruss (2006).

[4] If there are no *initial* conditions, then just replace this with conditions in the distant past of our universe or just *conditions*.

[5] In defense of their view, proponents of PSR may argue that there's nothing wrong with an infinite regress or with self-explanatory facts in this context (Melamed and Lin 2016). I do not discuss this issue in this article.

[6] The following authors seem to express views along such lines: Callender (2004), Gould (1985, 395), Scriven (1966, 129), and Shanks (2004, 216). However, none of them elaborate their view sufficiently to determine for sure.

According to this view, the initial conditions of the universe—no matter how unique or fine-tuned they may be—do not call for explanation. Indeed, nothing calls for explanation. We may have reason to believe that there is some particular explanation for those initial conditions, via enumerative induction, considerations of simplicity, or some other theoretical consideration. However, according to this view, independent "calling for explanation" considerations do not exist.

Between these two extremes lies an intermediate view according to which there is a distinction between facts that call out for explanation and facts that do not. According to the intermediate view, the initial conditions of the universe must have some special feature in order to call for explanation. For example, some argue that their fine-tuning for life is such a special feature.[7] However, if there was nothing special about the initial conditions, then they would not call for explanation.

The claim that some fact "calls for explanation" is metaphorical, facts can't really call for anything, and it is used by different people to express different kinds of claims. Here are a few of them: "*x* calls for explanation" can mean (1) *x* psychologically motivates us to seek an explanation or (2) *x* gives us a practical reason to seek an explanation. In this paper, however, I will focus instead on (3) we have a reason to believe that *x* has an explanation. More specifically, it expresses the idea that, regarding a particular fact *x*, we have reason to believe that it is explainable even without discovering any particular explanation, and we therefore have reason to reject any theory that implies that *x* is unexplainable. In the way that I use the term, the type of reason intended is defeasible. That is to say that if *x* calls for explanation, that means that we

[7] See for example Leslie (1989), van Inwagen (1993), Parfit (1998) and White (2018).

have reason to believe that *x* has an explanation; however, this reason can be outweighed by other considerations, and we may end up concluding that *x* is, in fact, brute.[8]

The intermediate view must answer a pertinent question: What makes some facts special? Why do some facts call for explanation while others do not? Is there some property in virtue of which the facts that call for explanation do so? Michael Della Rocca (2010) rightly argues that a lack of an answer to this question constitutes a powerful argument against the intermediate view.[9] To address this challenge, I will examine a proposed (and mostly neglected) solution on behalf of the intermediate view.

The working hypothesis of this paper is that the intermediate view is correct, and that there is a property in virtue of which some facts call for explanation. I call this property *strikingness*. Precisely what does it mean for a fact to be striking? One prominent proposal, due to Paul Horwich (1982, 101), says that a fact is striking if it puts into doubt certain background beliefs regarding the circumstances. However, Horwich's account has been criticized by George Schlesinger (1991, 99–101), David Harker (2012) and most recently by Dan Baras (2018). In this paper, I defend and develop an alternative account drawn from the work of Schlesinger. Schlesinger's proposal is that a fact is striking if it belongs to an extraordinary type of fact. Schlesinger himself is not fully clear about some details of his proposal,[10] and he does not

[8] In this respect, I differ from Martin Smith's (2016) usage of the term. When Smith says that *x* calls for explanation, he means that *x* must have an explanation as a matter of necessity. There are further ways in which our usages of the terms disagree, but I do not dwell on this here.

[9] Della Rocca's conclusion is that we should therefore reject the intermediate view and accept the principle of sufficient reason.

[10] One of the important things he is not clear about is precisely what his account is an account of. In the chapter in which he presents the account (Schlesinger 1991, chap. 5) he often presents it as an account for

address a pertinent challenge to his view. For these reasons, his proposal has thus far been mostly neglected. The main purpose of this paper is to fill in these gaps. In addition, I compare Schlesinger's view with two proposals that have been put forward since the publication of Schlesinger's book, namely those of Thomas Nagel (2012) and David Harker (2012).

The remainder of this paper is structured as follows: In the next section, I explain what motivates the intermediate view. In section 3, I explain why we need the extraordinary type account and should not be satisfied solely with bare intuitions. In section 4, I lay out the account. Sections 5–7 discuss several concerns about this account. Two of those sections (5 and 7) compare the account to salient alternatives, and section 6 responds to what I take to be the most pressing worry. Finally, in section 8, I apply the account to the opening example: the fine-tuning argument.

2 Why accept the intermediate view?

Typically, the intermediate view is motivated by generalizing intuitions that we have about specific examples. Imagine that a coin is tossed one hundred times and lands HTHHTHTTTH… and so on in, some inchoate sequence. The probability of an ordinary coin landing in any particular sequence is exceedingly low ($\approx 2^{-100}$), yet we feel no pressure to believe that this particular sequence must be explainable. Imagine that a different coin is also tossed a hundred

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when it is fitting to *feel* surprised or puzzled. At other times it seems he's accounting for when scientist have *practical* reason to investigate a phenomenon. Yet other times he equates extraordinariness with "demands an explanation" (for instance on p. 98), however he does not clarify what he means by that, and as we've seen, there is more than one possible interpretation. Only in chapter 6, in a different context, do we find the following incorporation in an *epistemic* thesis about confirmation along the lines of the intermediate view discussed in this paper: "[T]he status of a hypothesis is determined by the degree to which it alleviates our legitimate puzzlement generated by a unique sort of phenomenon" (p.119).

times and consistently lands HTHTHTHTHT... In this case, it seems implausible that the sequence is accidental. At least initially, it seems that this sequence must be explainable. Such examples are easily multiplied. A monkey is seated by a keyboard and types "jher90 b,mnERW8I QRLK". That a monkey would type this particular sequence is incredibly improbable, but it does not call for explanation. When another monkey types "My name is Curious George", however, that would call for explanation. Such examples suggest that there is a distinction between facts that call for explanation and those that do not.

Of course, what does or does not call for explanation must be relative to a credal state or state of background knowledge. If a ten-sided die is tossed twelve times and lands 314159265359, that sequence probably would not seem special to many people. However, once they learn that these are the first twelve digits of π, then plausibly, they should think that it calls for explanation. This should not be very surprising, nor does it turn the epistemic principle into a subjective principle in any problematic sense. Evidence, quite generally, is evaluated relative to a subject's credal state. For example, that a certain handkerchief was found at the scene of crime is of no epistemic significance, unless we also know that it belongs to one of the suspects. The theories that follow account for such agent relativity. They are all formulated using Bayesian probabilities, representing (with some idealization and modification) an agent's credal state at a given time.[11]

3 Why not bare intuitions?

Considering that the view is motivated by such intuitions, you may think that an answer to this paper's question is straightforward: essentially, we are looking for a reason to believe that facts are explainable. Now consider the examples from the previous section. In each case, our

[11] I thank [omitted for blind review] for suggesting that I clarify this issue.

discussion relied on our intuitions about those examples. So why not just settle for the idea that facts that call for explanation are all and only those that *intuitively* call for explanation?

My response is that we would like to better understand why we have these intuitions and whether and when they are justified. Consider testimony, for example. We may have intuitions that person *x* is trustworthy whereas person *y* is not. However, it is better not to rely on bare intuitions, which are prone to known biases; it is better when they conform to a well-structured theory. In addition, we can often provide more than just bare intuitions about why testimonies are reasons for belief. For example, we have inductive evidence that certain people are reliable, whereas we have evidence to the contrary regarding others. We should hope for the same with calling for explanation. It may turn out, for instance, that in some cases, we learn from experience through enumerative induction to expect certain kinds of explanations (such as that an intentional agent was involved), and in other types of cases, we expect other kinds of explanations (say, via mechanical laws of nature or laws of probability). If this turns out to be the whole story, then arriving at conclusions regarding the initial state of the universe, of which we have no experience, will turn out to be an illegitimate leap, as Hume famously argued.

True, there could be an irreducible strikingness property that we directly perceive through our strikingness intuitions, in which case there would not be much more to say than that we have these bare intuitions. This view admittedly has a mystical air to it, but so does the whole strikingness idea, and so do many philosophical views. That in itself is not an argument against the view. Nevertheless, for considerations of ontological parsimony, I believe that this possibility should remain a very last resort. It adds a new property and a new faculty to our ontology, which we should avoid if we can.

4 Schlesinger's account

Schlesinger's idea can be demonstrated using a toy example. A die is tossed twenty times and lands in the following sequence: 12345612345612345612. Surely, this sequence is striking. However, the fact that the prior probability of this particular sequence is extremely low is insufficient to explain why it is striking. The prior probability of 21454523332651115654 is at least as low, and yet this sequence does not call for explanation. Schlesinger suggests the following: A fact is striking iff it is both highly improbable *and* it belongs to a type of fact that is highly improbable.[12] 12345612345612345612 is not only improbable but belongs to a type of fact—namely, the orderly type—that is also highly improbable. Consider all the possible ways that a die can land if it is tossed twenty times. The vast majority of the possibilities are very messy. These include 15242366622253362555, 51455556253434436662, 21454523332651115654, and many, many others. Many fewer of the sequences are of the orderly type, which include sequences such as 12345612345612345612, 11111111111111111111, and 35353535353535353535. There are many more unorderly sequences than orderly sequences and therefore the probability of encountering the latter *type* of sequence is significantly lower than that of encountering the former.[13] Schlesinger's account can be formulated in the following way:

[12] Note that Schlesinger applies his account to events. Because events are not the only things that can call for explanation, I talk instead of facts.

[13] Interestingly, the idea of extraordinary types comes up in classic probability theory, for instance in Laplace (1902, 16–17) and in Von Mises (1957, 19–20). However, the context is different from the one in this paper.

> **Extraordinary**: A fact E calls for explanation iff and because there is a relevant type of fact T such that E is a member of T and the probability that T will be instantiated is very low.[14]

What is a relevant type? We will return shortly to this problem. For now, let us clarify the relevant probability function. Suppose that a die is tossed hundreds of times and keeps landing 3434343434… By now, the sequence 3434… has become ordinary in a sense, and we should expect the die to continue landing in this sequence. However, this is a clear example of a striking sequence. A plausible interpretation of Schlesinger's account must imply that this sequence is still of an extraordinary type, even if it becomes highly expected. The relevant probability function cannot therefore be that which represents the current credal state of the agent. A more plausible interpretation in this context is something like: what the agent should have expected given all of our information regarding the circumstances of the tosses (such as the physical symmetry of the die), but excluding the specific information about the actual outcomes of the tosses. Luckily, I am not the first to raise this idea. I'm following the footsteps of Lewis (Lewis 1981) in his famous analysis of objective probability from the perspective of a subjectivist. Lewis names the excluded evidence "inadmissible evidence."[15] In the die example, the idea is that if I set aside my knowledge of the actual toss outcomes, the probability of the type of outcome of which 34343434… is an instance, is extremely low. It seems natural to think that the relevant type here is very orderly sequences, or sequences that follow a recognizable pattern.

Now that we have our account on the table, we must ask whether this account is correct.

[14] We need not add the additional condition that E is improbable, because if the instantiation of T is improbable, that implies that E, which is a member of T, is improbable as well.

[15] Unfortunately, I do not know of any precise theory that explains how admissible evidence is distinguished from inadmissible evidence.

5 Orderly facts

At this point, you may think the examples given so far suggest a different account—that the facts that call for explanation are all orderly or follow a recognizable pattern. Consider sequences of dice tosses that call for explanation. Examples such as 353535353, 12345654321, and 44444444 come to mind. What they have in common is that are organized in a pattern that we recognize. Nagel adheres to such a view when he writes:

> [S]ystematic features of the natural world are not coincidences, and I do not believe that we can regard them as brute facts not requiring explanation. Regularities, patterns, and functional organization call out for explanation—the more so the more frequent they are. (Nagel 2012, 47)

The account can be formulated as follows:

> **Orderly**: A fact E calls for explanation iff and because E is highly orderly.

Of course, this account will need a supplementary account of orderliness, but for now, I set this task aside.[16] Surely, we can more often than not identify orderliness when we see it. We should distinguish two questions: (1) Is order, perhaps with low probability, a *sufficient* condition for calling for explanation? (2) Is order a *necessary* condition for calling for explanation? Nagel

[16] One salient candidate account is to measure order using Kolmogorov's complexity measure. Kolmogorov's basic intuition is that we can measure the complexity of a sequence by considering the length of the algorithm needed to reproduce the sequence. The more compressible the algorithm, the less complex the string. For the sequence 353535353535353… we can just tell the computer to repeat 35 x number of times, so it is very simple and, in our terms, very orderly. For fully random sequences, on the other hand, we can do no better than to dictate to the computer the full string. Schechter utilizes this idea when he suggests that a fact calls for explanation if it can be described using a simple rule (Schechter 2010, n. 33). For a textbook on Kolmogorov Complexity, see Li & Vitányi (2008).

clearly claims that order is *sufficient* for calling for explanation, but he says nothing about whether it is *necessary*.

We can construct thought experiments that suggest that order is not a necessary condition of facts that call for explanation:

> **Crazy Stone**: Suppose I toss a stone, and instead of moving in a typical parabolic trajectory, it starts moving around randomly, up, down, right, left, and all around.[17]

Intuitively, the crazy stone would call for explanation despite being quite the opposite of orderly. Moreover, it seems to call for explanation in the same sense that the aforementioned orderly sequences call for explanation. Order, then, is not a necessary condition for calling for explanation. Sometimes, perhaps against a backdrop of order, *disorder* calls for explanation.

Nagel may still be correct that order is a sufficient condition for calling for explanation. However, it may also be something weaker: instead of being a sufficient condition, it may be a contributing factor, or a defeasible reason to believe that something is explainable. In that case, we should wonder whether order is a basic reason to believe that a fact is explainable or whether it is a reason to believe that a fact is explainable only because order is extraordinary (i.e., there are fewer orderly possibilities than there are unorderly possibilities). Perhaps it is worth trying to develop Nagel's suggestion along these lines. However, at present, my interest is in the fact that Schlesinger's account accommodates the crazy stone more easily than Nagel's view does. Given our background knowledge of the laws of nature and how stones typically move, the probability that a stone will move randomly is extremely low. Thus, random movement of a stone is not only

[17] I thank [omitted for blind review] for suggesting this example to me.

an improbable *token* fact; it is also a *type* of fact that is improbable. Therefore, Schlesinger's account predicts correctly that it calls for explanation.

6 The obvious challenge

Schlesinger's account faces an obvious challenge. If any set of facts can count as a type, then it follows that any improbable fact belongs to an improbable set—or type—of fact. For instance, any improbable fact E belongs to the type that includes E alone, {E}, which is just as improbable as E. However, one of this paper's background assumptions is that many improbable facts do not call for explanation. Therefore, this version of the account surely fails. Lacking a restriction on how types are individuated, it would also be true that every fact belongs to a probable type of fact and, according to Schlesinger's line of thought (although it does not follow deductively from the above formulation), this should imply that no fact calls for explanation. The account must therefore assume a distinction between relevant or natural types and irrelevant or non-natural types. And the big question is whether there is, in fact, any such distinction. This is the challenge that I address in the current section.

Schlesinger himself anticipates the worry and suggests the following:

> Working scientists have no difficulty recognizing at once whether a given event belongs to one or the other type (without ever having attempted to articulate the vital distinction between them), and acting accordingly. (Schlesinger 1991, 104)

His statement is not an account of types of facts but, if true, is a significant epistemic claim. According to Schlesinger, even if we lack at present an account of types of facts, we have clear intuitions as to how facts should be typified. If that is correct, then Schlesinger's account has

some hope. It might still be true that all and only the facts of extraordinary types call for explanation.

A number of authors have rejected Schlesinger's account because they doubt that any account of natural types is plausible.[18] In defense of Schlesinger's account, I will develop a partners-in-guilt argument. Calls for explanation are not the only context in which we encounter a need to identify relevant types. I will describe three other contexts that push us to assume that there are natural ways of individuating tokens into types, even without a detailed account of types.

The first partner-in-guilt is a reference class problem that arises in the theory of probability. Although there are multiple reference class problems, here I refer to a particular epistemic variant of the problem, described by Alan Hájek (2007, 583). To estimate the probability of a particular event, we take into account statistical information from a class of similar cases or else we apply a principle of indifference to possibilities among which we have no privileging evidence. The problem is that, typically, there is more than one relevant statistic or more than one set of possibilities that we may consider.

Hajek gives the follow example. Suppose that John Smith is contemplating whether to purchase life insurance. To do so, he must determine the probability that he will live another 11 years. John Smith is a healthy, middle-class, 51-year-old male who does not exercise regularly. We may have some data on how many people between the ages of 51 and 62 have died over the past 20 years, but that might not be the relevant data, because not all of them were healthy. We may

[18] This is pretty much where the debate regarding Schlesinger's account has ended prior to this paper (Manson 1998; Bostrom 2002; Urbach 1992; Griffiths and Tenenbaum 2007, 184–85). Note that Griffiths and Tanenbaum's concern is with characterizing *coincidences*, and it is not clear whether they adhere to the intermediate view at issue here.

have data regarding the deaths of healthy people between the ages of 51 and 62 over the past 10 years, but relying on this data may not work because it does not account for the technological advances of the past five years. And so on and so forth. In fact, it is not possible to find any statistical evidence about people who have all and only the exact properties that John Smith possesses because only John Smith possesses those properties, and we do not yet know how much longer he will live.

This example illustrates the epistemic reference class problem. It is an epistemic problem of forming probabilistic beliefs because we have no clear theory that tells us how to incorporate the various bits of statistical evidence that we possess. Yet somehow, we have not given up the practice of calculating probabilities and using them as guides in making decisions. Somehow, we are able to determine a relevant reference class at least well enough to make it worthwhile—for us, for insurance companies, for weather forecasters, and for many others—to continue calculating probabilities and using them as guides to decision-making. In doing so, we must assume that there is some relevant type to which the token being assessed belongs. Therefore, our ability to calculate probabilities suggests the following argument:

(1) We often make good probability assessments.

(2) Good probability assessments rely on locating a token event within a type of event.

(3) Therefore, we can often locate a relevant type of event.

My second partner-in-guilt comes from theories of epistemic justification and theories of knowledge. Many such theories rely on an attribution of token beliefs to relevant types of beliefs. The most obvious example is process reliabilism (Goldman 1979), according to which the degree of reliability of belief-forming processes determines the degree of justification of the beliefs they

produce. Some have argued that reliabilism should be rejected because there is no viable distinction between genuine and arbitrary belief-forming processes. This has come to be known as the "generality problem" for reliabilism (Conee and Feldman 1998). You may therefore think that process reliabilism ought to be rejected and some other theory that does not face a generality problem accepted instead. However—and this is often overlooked—the problem does not only plague reliabilism.[19] There are reasons to believe that even the best sensitivity- and safety-based theories of knowledge or of epistemic justification require a similar attribution of a token belief to a relevant belief-forming process or method.

Let us start with sensitivity. The simplest formulation of sensitivity says that if p were false, then S would not have believed that p. According to Robert Nozick's tracking account of knowledge, sensitivity is a necessary condition for knowledge. Nozick himself already noticed that if sensitivity is not relativized to methods, then it falls prey to counterexamples such as the grandma case. In that case, grandson is brought to grandma; she sees that he looks healthy and judges that all is well with him. This is quite a reliable way of forming a belief about her grandson's health. Is it sensitive? If grandson were sick, his parents would not bring him to grandma, and they would tell her that all is well in order not to worry her. Therefore, if grandson were not healthy, grandma would still believe that he is. Therefore, according to the simple formulation of sensitivity, the belief is insensitive, and if sensitivity is a necessary condition for knowledge, then grandma does not know that grandson is healthy. This result is counterintuitive. Nozick suggests (and this is a very intuitive move to make) that sensitivity should therefore be relativized to a belief-forming method: if p were false and the belief-forming method remained

[19] For additional arguments for the same conclusion, that the generality problem is everybody's problem, see Adler & Levin (2002) and Bishop (2010).

stable, then S would not believe p (Pritchard 2008; Nozick 1981, 179). In the grandma case, if grandson were sick, then grandma would form her belief based on parents' testimony, not based on looking at her grandson. Therefore, it is not a relevant counterfactual scenario. There are, of course, other problems with sensitivity and further epicycles that a sensitivity account must incorporate to match our intuitive judgments. My intention here is not to provide a satisfying account of sensitivity but rather to claim that whatever the best sensitivity account turns out to be, we have good reason to believe that it will include a relativization to belief-forming methods or processes.

Safety based accounts face a similar challenge (Pritchard 2008). The simplest formulation of safety says that S's belief that p is safe iff S could not have easily had a false belief about p. Now, we can construct an imaginary scenario in which S could have easily formed a false belief about p using a different belief-forming method. Our intuitions will likely be that p's belief should still count as safe and can amount to knowledge. Consider grandma again. Suppose that it could have very easily been the case that grandson is sick and she wrongly trusts parents' testimony. Given that her current belief is formed using a very safe belief-forming method, doesn't she count as knowing that grandson is healthy? Intuitively, she does.

Thus, it seems very likely that knowledge and justified belief have something to do with reliability, sensitivity, or safety of belief-forming processes or methods. That means that if we have any hope of identifying instances of knowledge or epistemic justification, then we must be able to locate the relevant belief-forming process or method. Dividing individual beliefs according to general belief-forming processes or methods is a way of dividing beliefs into types, which is similar to what we have to do with types of facts. Therefore, let us just speak of dividing beliefs into types. If you are more convinced that we can identify knowledge and

epistemic justification than that there are no genuine individuations of belief-types, then you should believe that genuine individuations exist even if, at the moment, you lack a good theory of how belief-forming processes or methods are individuated. Our ability to identify knowledge and epistemic justification suggests the following argument:

(4) We often identify knowledge and epistemic justification.

(5) Identifying knowledge and epistemic justification depends on identifying a relevant belief type for a belief token.

(6) Therefore, we can often identify a relevant belief type for a belief token.

My third partner-in-guilt is enumerative induction. Goodman's (1983) new riddle of induction demonstrates that our most basic inductive practices rely on an ability to identify projectable properties and distinguish them from non-projectable properties. In such inductions, projectable properties serve as a way to divide particular observations into relevant types of observations. Our ability to apply enumerative induction suggests the following argument:

(7) We can often rationally apply enumerative induction.

(8) Rational application of enumerative induction depends on identification of the relevant types of token data.

(9) Therefore, we can often identify relevant types of token data.

We have reason to believe that a natural reference class exists, even if we have no elaborate theory of how natural reference classes are distinguished from non-natural reference classes. We likewise have reason to believe that natural belief-forming methods exist, even if we have no theory of how natural belief-forming methods should be distinguished from non-natural belief-forming methods. And we have reason to believe that there are projectable properties even if we

have no precise theory that explains what distinguishes non-projectable properties from projectable ones. In all of these cases, we must identify a relevant type for a given token. So why not think we could be in the same position with regard to calling for explanation? I conclude that if Schlesinger's account must assume that there are relevant or natural types[20] and that we are able to identify them, this is not a heavy price to pay and definitely not a conclusive reason to reject the account. Rather, we may have to accept this assumption anyhow, regardless of Schlesinger's account.

Admittedly, as long as we lack a clear account of relevant types, it may be difficult to determine the relevant type of any given fact. This creates a problem for assessing the account's plausibility. Given any fact that intuitively calls for explanation, it will be difficult to rule out the possibility that it belongs to an extraordinary type. Moreover, given any fact that intuitively does *not* call for explanation, it will be difficult to rule out the possibility that there is no genuine extraordinary type to which it belongs. That's not to say that we cannot examine the account. If Schlesinger is correct in his epistemic claim, then we could have a clear intuition that some fact does not call for explanation despite belonging to an extraordinary type, and that would count against the account. Still, we could have hoped that more conclusive tests would be available.

[20] Though I sometimes speak of natural types, I caution the reader that I do not take a stand here on the precise relationship between the natural types that, if my argument is sound, epistemologists need to posit, and the natural types of metaphysicians. Metaphysicians (see for instance van Inwagen (2014) and Hawthorne and Dorr (2013)) are interested in figuring out which types genuinely map onto reality whereas epistemologists are interested in figuring out which types should be used in various inferences or which help explain why certain beliefs are justified or count as knowledge and others not. These may not be the same.

In general, unfalsifiability is never in itself a reason to discount a theory. However, it *is* a reason to be less impressed by the theory's success in explaining examples. If a theory is able to explain any possible outcome—that is, if it does not clearly rule out any outcomes—then examining those outcomes will provide little support for the theory.

If we did have some way of identifying the relevant types, we would clearly be in a better position to evaluate Schlesinger's theory. However, my claim is that the absence of such a method does not itself count significantly against Schlesinger's theory.

7 Contrary to expectations

Earlier I compared Schlesinger's account to Nagel's and demonstrated the advantage of Schlesinger's account in the case of the crazy stone. However, cases such as the crazy stone may suggest a third account, proposed by David Harker, that facts call for explanation iff they are contrary to an expectation. In this section, I compare Schlesinger's account to Harker's and argue that they may in fact be coextensive.

Consider again the crazy stone. Notice that even if the stone were to move in an orderly trajectory but not in the kind of parabolic trajectory that we expect based on past experience, then the trajectory would intuitively call for explanation. It seems that, in this case, it is not disorder per se that calls for explanation but rather the defiance of our well-established expectations.[21] We have tossed things into the air many times before and observed how they move. We expect a parabolic movement when a stone is tossed, and the crazy stone strikes us as

[21] Throughout, expectation should be read as a normative fact (that which one ought to expect), not as a descriptive fact (that which one actually expects).

calling for explanation because it defies this expectation. Perhaps, then, a fact calls for explanation if it is contrary to an expectation that we hold?

This suggestion has been put forward by David Harker (2012):

> E is [striking] for an agent with initial credences P iff and because:
>
> 1. Given the agent's background beliefs about the circumstances (C), E is highly improbable [$P(E|C) \approx 0$]
>
> 2. Given the agent's background beliefs about the circumstances (C), there is an expectation (A) that is contrary to E [$\exists A:(P(A|C)$ is high & $P(A\&E) = 0)$]

Unfortunately, the account, as it stands, faces the following difficulty: Harker places no restrictions on what can count as an expectation. Sometimes he implies that any proposition that we initially have reason to consider highly probable counts as an expectation. However, this implication cannot be correct. If it were, then his account would collapse into the first condition, which Harker agrees is insufficient. Here's why: For any E that fulfills the first condition, there will be an A that trivially fulfills the second condition—namely, the negation of E. Therefore, any E that fulfills the first condition will count as surprising.

The intuitive response on behalf of Harker is that not any highly probable proposition can count as an expectation. Surely, there would be something unnatural about the claim that we expect the dice not to land in the particular sequence 16423461244634562435. On the other hand, we *do* expect that the long sequence will have this random look to it (i.e., that it will not follow some recognizable pattern). But if expectations in the sense relevant here occur apart from high probability, then what are they, and how are they identified?

Harker's difficulty in identifying expectations is reminiscent of Schlesinger's difficulty in identifying types of facts. Interestingly, with a few additional assumptions, the two accounts end up being extensionally equivalent.[22] That is, Unexpected and Extraordinary are extensionally equivalent if the complement of every type of fact is a potential expectation (that is, if it were highly probable, then it would count as an expectation) and the complement of every potential expectation counts as a type. The most natural way to arrive at this result is to say that the complement of every relevant type is also a relevant type, and we only have expectations regarding *types* of facts, not token facts.[23]

Thus, it seems reasonable to think of Harker and Schlesinger's accounts as complementing each other rather than as competing.

8 Is cosmological fine-tuning extraordinary?

Now that we have our extraordinary account in hand and have responded to some challenges, we can return to our opening example and see how it fares.

Some of the old arguments for the existence of God are premised on the principle of sufficient reason: the idea that everything must have an explanation. According to those views, the existence of our universe, no matter what our universe is like, must have an explanation. However, the principle of sufficient reason has long fallen out of favor; thus, the old arguments are commonly replaced with newer ones, premised on the idea that there is something special

[22] Harker himself only discusses Schlesinger's critique of Horwich's account; he is silent about Schlesinger's positive account.

[23] In the context of metaphysicians discussion of natural types, van Inwagen (2014, 182) argues against the view that the complement of a natural type is a natural type. However, this might be one place where the metaphysicians' natural types don't map onto the epistemological natural types. See note 20 above.

about our universe—in particular, its fine-tuning for life—that calls out for explanation (van Inwagen 1993; White 2018). That is, in order for a planet like Earth with living beings like humans to exist, the initial conditions and laws of the universe must be incredibly finely tuned. For example, if there had been any slight difference in the rate of expansion of the universe after the Big Bang, the strong nuclear force that binds atomic nuclei, or the force of gravity that keeps our planet at just the right distance from the sun, our world would turn into chaos. Suppose that the extraordinary account of strikingness is correct. What would it imply with regard to cosmological fine-tuning? Does the fine-tuning of our universe for life call for explanation?

This formulation of the question accords well with Schlesinger's theory. That is, the question is formulated in relation to a type of universe (one that is fine-tuned for life) rather than a token universe (our particular universe). Several candidate types may be relevant in this context: fine-tuned for life, fine-tuned for non-chaos, and fine-tuned for the evolution of conscious beings. However, we need not decide among these candidates because doing so would not make a difference to the arguments. Is a finely tuned universe an extraordinary type of universe? If we assume that the universe could have been any way we could imagine—the laws of nature could have been any set of laws or there could have been no laws at all; the initial conditions could have been anything, including an empty universe; the fundamental forces of the universe could have had any strength—then the types of universes noted above are extraordinary. If any extraordinary fact calls for explanation, then the fine-tuning of our universe calls for explanation as well.

However, there is a unique problem with the initial conditions of the universe that do not exist in other instances, a problem that seems to undermine the initial reason we have to think that those initial conditions are explainable: that is, any possible initial conditions of our orderly universe

would be equally extraordinary, and for the same reasons. For example, positing the existence of an intelligent designer of our universe won't help because the existence of the intelligent designer would be an initial condition that calls for explanation for the exact same reason: we can imagine so many universes in which no intelligent designer existed. In order to stop the regress with an intelligent designer, it needs to be the case that intelligently designed universes are not extraordinary. I see no reason to believe that intelligently designed universes are any more or less extraordinary than undesigned universes that are fine-tuned for life.[24]

Here is another way of presenting the problem: We suppose that the initial conditions of our universe must be explainable because they are so extraordinary. But what could possibly explain those initial conditions? The explanation would have to be some restriction on the possible initial conditions that would render them less extraordinary. What could provide such a restriction? Positing a creator, or some further law of nature? If it reduces extraordinary coincidences, then doing so would provide us with a better theory. However, it cannot do so because those laws of nature or the existence of a creator should also be considered extraordinary and for the same reasons. Thus, there cannot be any theory that will do better than just positing the initial conditions as an extraordinary coincidence.

One way to avoid this result is to deny the premise that the initial conditions could have been anything, without restriction. However, I have no idea what sort of restrictions on how the universe could have been could both be reasonable and render the initial conditions of our

[24] Perhaps I'm too quick to dismiss the (perhaps orthodox) view that if God exists, then God's existence is metaphysically necessary. I confess that I was never able to understand why this view should be accepted. I say a bit more, and provide helpful references, in [omitted for blind review].

universe ordinary. As things stand, it seems to me that any theory regarding initial conditions will be on par as far as extraordinariness is concerned.

You may think that there is another way of avoiding the regress: positing that our universe is one of many universes, each with different initial conditions (Parfit 1998; Leslie 1989). This is known as the multiverse hypothesis. In this hypothesis, given that there are so many different universes, it is not very surprising that one of them is fine-tuned for life. However, there remains a worry: The existence of such a multiverse can call for explanation as well and for the same reasons. After all, there are so many alternative possibilities. There could have been one universe, or two, or three. There could have been many universes, but all with the same initial conditions. To attain the desired result, it seems multiverse theorists might be imagining some mechanism that constantly produces new universes with random initial conditions. However, the existence of such a mechanism would call for explanation, and for the exact same reason.

Where does this leave us? I think that with regard to the fine-tuning of the initial conditions of our universe, all theories are equally committed to initial conditions that are striking but unexplainable. Therefore, this commitment cannot serve as a reason to reject one theory over another. We already said at the outset that reasons to believe that a fact calls out for explanation are defeasible, so we haven't discovered a possibility that our theory forbids.

9 Conclusion

To conclude, while I do not pretend to have solved all of the difficulties that Schlesinger's account faces, I do hope to have convinced you that it deserves more respect than it has

previously received and is a good candidate for an account of which facts call out for explanation.[25]

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[25] [Acknowledgements omitted for blind review]

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