What Can Armstrongian Universals Do for Induction?

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Abstract

David Armstrong (1983) argues that necessitation relations among universals are the best explanation of some of our observations. If we consequently accept them into our ontologies, then we can justify induction, because these necessitation relations also have implications for the unobserved. By embracing Armstrongian universals, we can vindicate some of our strongest epistemological intuitions and answer the Problem of Induction. However, Armstrong's reasoning has recently been challenged on a variety of grounds. Critics argue against both Armstrong's usage of inference to the best explanation and even whether, by Armstrong's own standards, necessitation relations offer a potential explanation of this explanandum, let alone the *best* explanation. I defend Armstrong against these particular criticisms. Firstly, even though there are reasons to think that Armstrong's justification fails as a self-contained defence of induction, it can usefully complement several other answers to Hume. Secondly, I argue that Armstrong's reasoning is consistent with his own standards for explanation.

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David Armstrong (1983) offers a reason to believe in the existence of universals: if we believe in necessitation connections among universals, then we can answer the Humean Problem of Induction. He argues that these necessitation connections provide the best explanations for *some* observed regularities, and they also entail that these regularities obtain in the unobserved parts of the universe. His theory of universals would thus receive some support, by entailing some of our most crucial epistemological intuitions. Since Armstrong's theory is inconsistent with Regularity accounts of laws and presents an alternative to many popular answers to Hume's Problem, it is perhaps unsurprising

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that Armstrong's reasoning has recently been challenged by a range of critics such as Gerhard Schurz (2008), Beebee (2011), Ruth Weintraub (2013), and Benjamin Smart (2013).

I shall argue that these criticisms are either mistaken or do not *entirely* undermine the usefulness of Armstrongian universals for answering the Problem of Induction. I conclude that accepting necessitation relations among universals could have significant benefits for the epistemology of induction. I shall not attempt to answer the much broader question of whether we should believe in Armstrongian universals. I begin by describing Armstrong's views in Section 1, then defend the robustness of their significance for induction in Section 2, and finish in Section 3 by considering whether Armstrongian universals truly provide potential explanations for observed regularities.

1 Armstrong's Argument

The Regularity "theory" of laws[1] is really a family of theories, which all affirm that there is no more to the laws of nature than membership of a suitably restricted subset of universal associations between actual things. The different Regularity theories vary in the restrictions they place on membership of the subset.[2] In *What is a Law of Nature?* (1983), Armstrong criticises this approach and advocates his own version of necessitarianism (drawing on his theory of universals) as an alternative. Among his many arguments, I shall only discuss those related to the Humean Problem of Induction. While he uses this problem to criticise the Regularity theory (Armstrong 1983: 52–59) I shall not explore this argument, except to note that nothing I say shall presuppose that Armstrong is correct on this point.

Instead, my focus is another strand of his reasoning: he claims that one of the advantages of his analysis of laws is that it helps the justification of induction (1983: 104–106). He believes that our inductions are justified by an implicit premise in our reasoning. Such "missing premise" justifications of induction can only succeed if we have a justified belief in the putative missing premise(s). Hume famously argued that such a missing premise does not seem to be available a priori (a logically necessary proposition would be too weak, while empiricism rules out a priori knowledge of non-logically necessary propositions) whereas an inductive justification for a missing premise would be circular.

However, many contemporary philosophers deny that all good reasoning consists of either inductive inferences or deductive reasoning from a priori principles. Inference to the best explanation (IBE) has become a popular supplement to our inferential toolkit in epistemology. Armstrong's strategy is to combine his account of laws with a factive view of IBE[3] to infer necessary connections among universals as the best explanation of at least *some* observed regularities. In turn, these necessary connections imply that

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[1] Smart uses the label 'Humean' for the Regularity theory. To simplify, I shall stick to one use of Hume's name for a philosophical view.

[2] For instance, the requirement that descriptions of the universal associations would be included in the best possible system of universal generalisations, in some sense of 'best' (Lewis 1973).

[3] A factive view sees IBE as a method for inferring hypotheses, rather than merely judgements like "Pursuing this hypothesis will be valuable for science." There are alternative interpretations of IBE in the literature. (Van Fraassen 1980: Chapter 6); (Nyrup, 2015).

the observed regularities also obtain in any unobserved parts of the universe. Consequently, if we observe some regularities and the necessary connections really are the best explanations of these regularities, we can justifiably infer the missing premise of the inductive reasoning, and therefore our induction is justified.

By "necessary connection", Armstrong means a contingent modal relation between the universals. In particular, F-ness cannot occur without G-ness. This modal relation is stronger than a mere regularity, because it entails counterfactuals: all gold conducts electricity, but a golden moon orbiting the Earth *would* also conduct electricity. In other words, the fact of something *being* an F necessitates its *being* a G. The modal relations among universals are logically and metaphysically contingent, in that they are among the non-necessary facts of the universe. This is a contrast with the Law of Non-Contradiction or the Principle of Bivalence, assuming that these are true. Among its other implications, the necessary connection between the universals of F-ness and G-ness entails the regular association of instances of F with instances of G.

Armstrong insists that the necessitation relation between F and G is *not* identical to the modally qualified universal generalisation $\Box\forall x(Fx \rightarrow Gx)$ (1983: 96–97). That would be a form of regularity analysis: one that is quantified over all possible worlds, for some suitable sense of 'possible'.[4] The necessary connection is between the universals, not the things possessing the universals. Armstrong symbolises the necessary connections as $N(u_1, u_2)$ for two universals u_1 and u_2.[5] The relation denoted by 'N' is itself a universal that relates universals to each other (1983: 88). As one would expect, such a proposition entails the universal generalisation $\forall x(Ax \rightarrow Sx)$ for predicates A and S corresponding to the universals u_1 and u_2, but the reverse entailment does not hold. The relation N is irreflexive, intransitive, non-contrapositable, and non-symmetrical (1983: 155–157).

For induction, Armstrong argues that there are *some* predicates F and G such that we can reason:

(P1)   A necessary connection between F-ness and G-ness is the best explanation of the regular association of observed instantiations of F with instantiations of G.

Therefore, by IBE,[6] (C_1) There is a necessary connection between F-ness and G-ness.

(P2)   If there is a necessary connection between F-ness and G-ness, then all instances of F are instances of G.

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[4] If we analyse laws of nature using such forms, for some axiomatization of '$\Box$', then we can still construct arguments similar to Armstrong's, and most of what I say will still be relevant.

[5] Unlike Armstrong, for my notation I shall use upper-case letters for predicates and lower-case letters for universals, to emphasise the distinction between the two. Note that Armstrong's justification of induction is not limited to cases of universals that can be formulated as simple binary relations; he also discusses (aleatory) probabilistic and functional relationships among universals (1983). Like Armstrong and most of his critics, I shall stick to simple examples of laws.

[6] By "IBE", I mean an ampliative inference of the form 'H is the best explanation of E, therefore H is true.' Such inferences can have many defeaters, e.g. we might know, on other grounds, that H is improbable; E might be a small subset of the relevant evidence, and so on. The features of a "best explanation" are controversial, but popular criteria include simplicity, elegance, fruitfulness, and coherence with antecedently plausible scientific theories.

Therefore, by *modus ponens* from C_1 and P_2, (C_2) All instances of F are instances of G.

Thus, according to Armstrong, our inductive inferences of general regularities are justified by necessary connections between universals. Our observations justify the metaphysical beliefs by IBE, and the metaphysical beliefs entail the inductively inferred hypotheses. We are thereby justified in inferring from experience to the unobserved via induction.

2 The Robust Significance of Armstrongian Universals

Armstrong's attempted justification of induction leans heavily on several controversial background assumptions. In particular, Armstrong's answer requires that we can reasonably use IBE in a factive way *prior* to inductively inferring any empirical hypotheses. Otherwise, the putative justification would be question-begging. However, this assumption is controversial in the literature on IBE. For example, that simpler explanations are more likely than relatively complex explanations, ceteris paribus, requires that simplicity is associated with truth, at least in the particular IBE's domain of inquiry. Many philosophers would say that we can only know this via induction. Beebee makes an analogous point regarding predictive power as an explanatory virtue: why should we think that hypotheses with greater predictive power are more likely to be true ceteris paribus (not just more valuable) than their competitors? (2011: 518). Similarly, Schurz argues that Armstrong's attempted justification of induction presupposes nature's uniformity (2008: 202). The general worry is that IBE cannot provide a self-contained justification of induction.

Part of the problem is that there are many theories of (factive) IBE. On some accounts, IBE is an independent form of inference, in the sense that the reasonableness of any given IBE (or at least many IBEs) is not derivative of the reasonableness of some other inference (Lipton 2004); (Douven 2013). Others try to assimilate IBE reasoning into Bayesian reasoning (Henderson 2014). A third approach, compatible with the Bayesian assimilation, is to regard IBE reasoning as derivative on antecedent inductions, e.g. that as far as we know simpler explanations are generally better approximations of the truth in some local domain, so that simplicity is a pro tanto positive indication that an explanation in that domain is approximately true (Salmon 1990). The problem for Armstrong is that, insofar as IBE is defensible at all, many philosophers would claim that it is in the third way, but this is exactly the sort of theory of IBE that is unhelpful for justifying induction.

A further problem is that Armstrong's justification of induction does not justify our belief in the existence of unobserved things. Given observations of black ravens and no non-black ravens, Armstrong's justification would justify our belief in the conditional 'If there are unobserved ravens, then they are black.' However, it would not justify our belief in the unconditional assertion *that* there are unobserved ravens. While 'Ravenness necessitates blackness' entails that any unobserved ravens would be black, if they exist, it does not entail that there are unobserved ravens.[7] Since many of our

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[7] It does entail that there are ravens, because Armstrongian universals must be instantiated to exist (Armstrong, 1983: 82), but they do not require any unobserved instantiations. This part of the content of N(r, b) is satisfied by our observations of black ravens, and therefore it is consistent with the possibility that these are the only ravens.

inductively obtained beliefs are unconditional (I believe *that* the sun will rise tomorrow, not just that if tomorrow occurs then the sun will rise) Armstrong's arguments leave many of our inferences unjustified. Concomitantly, Armstrong would provide relatively weak reasons for accepting his theory of laws instead of its rivals.[8]

However, even if these criticisms are correct, the explanatory relations that Armstrong has identified could still play a part in a larger response to the Problem of Induction. I shall argue that even if IBE cannot provide a self-contained justification of induction, Armstrong's arguments can be a useful part of a wider answer to Hume. Thus, Armstrongian universals have a robust significance for induction.

In particular, Armstrong's arguments can complement some other responses to Hume, like the Williams-Stove justification of induction and the Reichenbachian "vindication" of induction. I shall use 'statistical justifications of induction' (SJIs) to label such responses to Hume. They are heterogenous, but a common feature is that, even if they are successful, SJIs only directly warrant belief in approximate descriptions of regularities: they would justify induction for hypotheses like 'All or almost all X are Y' and '$100\% \pm \varepsilon$ of X are Y', where X and Y are molecular or atomic expressions and ε is a small positive non-zero fraction of X's, but not for universal generalisations like 'All X are Y' (Williams 1947); (Reichenbach 1971: 446); (Stove 1986: Chapter VI); (Salmon 1991).

Before examining the complementary relations between SJIs and Armstrong's justification of induction, I shall outline some of the details of the former to explain why this limitation of SJIs is intrinsic and how SJIs are still live options in the debates about induction. The Reichenbachian justification (developed further by Salmon) concerns a version of the Straight Rule of Induction. Given the acceptance of a sample report asserting the relative frequency f of a property[9] φ in an n-fold set that all also have the property ψ, this version of the Straight Rule requires you to infer that the relative frequency of φ in the population of ψ's is equal to $f \pm \varepsilon$. Reichenbach appeals to the statistical fact that if there is a limiting relative frequency l of φ in the set of things that have ψ, then as n tends towards infinity, f will eventually approximate l within the particular margin of error ε. In other words, there is a sense in which this rule is guaranteed to converge towards the true value for the limiting frequency in the population, provided that the limiting frequency exists. For example, suppose that our only evidence with respect to ravens is a statement that 100% of the ravens that we have observed are black. If we infer that all ravens, within a margin of error of 1%, are also black, then we are following a rule that will converge upon the true frequency of blackness among ravens in the limit, provided that this limit exists.[10] Therefore, provided

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[8] Much of what I say below will also be adaptable to other justifications of induction that employ IBE and necessary relations; for example, see Tyler Hildebrand's recent version of this approach (Hildebrand, 2016). If the rationality of IBE presupposes that inductive reasoning is rational, then we cannot use IBE to justify induction. There is an analogy here with a familiar Humean point regarding the uniformity of nature: if our belief in the uniformity of nature presupposes that inductive reasoning is rational, then we cannot appeal to this belief to justify induction. Note that this latter point is not the same as inductive scepticism. It would only constitute a conclusive argument for Humean inductive scepticism if we also believed that an appeal to the uniformity of nature was the only way of justifying induction.

[9] Reichenbach talks about "events", in the statistician's sense of this term. As this term is likely to be misleading for many readers, I have phrased his position in terms of properties instead. The difference is not very important for the essence of his justification of induction.

[10] If the limiting frequency does not exist, which is a mathematical possibility for infinite or potentially infinite sets, then obviously induction won't help us find it, but that is a small slight against induction, because no other method will either.

that we do not have the misfortune to live in an extremely chaotic universe, this version of the Straight Rule will eventually "work" in a sense that Reichenbach argued was epistemically significant.

There are a number of fundamental challenges to Reichenbach's approach to the Problem of Induction. Perhaps the most important is that there are many other rules of inference that have the same asymptotic features as Reichenbach's recommended version of the Straight Rule, yet which are very different from our inductive practices. However, attempts to justify induction along Reichenbachian lines persist, especially in the rich body of work on formal learning theory (Steel, 2010). For my purposes, the key point is that the asymptotic feature that Reichenbach identifies only holds for a version of the Straight Rule that has a margin of error. Therefore, even if a Reichenbachian justification were to succeed, the inference rule that would be justified would always involve a margin of error, so that what we could infer *directly* as a result of the Straight Rule would be approximate hypotheses of the form 'l is equal to $f \pm \varepsilon$' rather than precise hypotheses of the form 'l is equal to f' and it is the only latter that entails hypotheses of the form 'All X are Y'.

Another sort of SJI is the Williams-Stove justification.[11] If successful, the outcome would be similar to the Reichenbachian justification in many respects. Informally, an uncontroversial mathematical principle of combinatorics says that, in a finite and well-defined set, the frequency of φ among ψ's in a considerable majority of large subsets will be no further than ε away from the set's frequency of φ among ψ's. Even less formally, large subsets are representative of their finite supersets. While I have left "considerable majority" and "large" imprecise here, their meaning can be made exactly precise. Furthermore, their exact proportionate relations can be calculated: one can determine, for a given subset size n, the *minimum* proportion of n-fold subsets in an indefinitely large finite set that are no further than ε away from the set's frequency of φ among ψ's. It is important to appreciate that it is not the *relative* size of the subset in proportion to the superset that guarantees these facts; it is the *absolute* cardinality of members in the subset.[12] Donald Williams tried to use this combinatoric fact to justify our intuitively rational inductive practices in general; David Stove attempted the more modest task of proving *some* of our inductions to be justified. In both cases, the idea was that if we do not know any defeaters for an inference from a sample report of a large subset of a population P to a statistical generalisation of the form 'The frequency of φ among ψ's in P is $f \pm \varepsilon$', then the epistemic probability of the statistical generalisation given the sample report is high. Stove's main example was that relative to the evidence that a 3020-fold sample of ravens, exactly 95% of which are black, the hypothesis that the frequency of blackness in "the population of ravens, each at least 100 cc in volume and no two overlapping, on earth between 10,000 BC and AD 10,000" is near 95%, would be highly probable (Stove, 1986: 71).

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[11] This is a common name in the literature for this justification of induction, but the basic idea goes back at least as far as a discussion by Josiah Royce (1913: 82–88).

[12] Of course, we can sometimes calculate even higher minimum proportions if we know that the subset is a relatively large proportion of the superset. At the trivial extreme, if the subset has the same cardinality as the superset, then they are extensionally identical and the subset must be perfectly representative. These facts are fine for induction, but the main point of interest in the Williams-Stove justification is that even if the population has an indefinitely large (but finite) cardinality, then we can use the absolute cardinality of the subset to calculate minimum proportions of representative subsets, and these can minimum proportions can be high if our samples are large.

Like the Reichenbachian justification, the Williams-Stove justification persists in the more recent literature on induction: some recent proponents include Timothy McGrew (2001) as well as Campbell and Franklin (2004). Like the Reichenbachian justification, there are many possible objections to both the bold Williams and modest Stove versions of this justification of induction (Hempel 1960); (Indurkhya 1990); (Maher 1996); (Lange 2011). Again, for my purposes here, the important point is that if the Williams-Stove justification (or something very much like it) is successful, then it would establish high conditional probabilities for some approximate statistical generalisations given reports of large sample data and our total evidence.[13] Once again, the margin of error ε would be an ineliminable feature of what was *directly* warranted by the justifications. Anything more would require further epistemological labour.

Thus we have two justifications of induction, both still live options in the controversies on Hume's problem, both of which have an intrinsic limitation. Suppose, for the sake of argument, that at least one SJI is viable. How could it and Armstrong's justification mutually benefit each other? Firstly, a successful SJI would help with the dependency of Armstrong's own justification of induction on IBE. If it is true that factive IBE depends on contingent empirical presuppositions (such as correlations between particular theoretical virtues and truth in particular domains) and if these hypotheses can be rendered as approximate generalisations like 'All or almost all F are G', then SJIs could justify the use of IBE in the particular cases where Armstrong needs it. To give a very simplified example, if $N(r, b)$ is better than a rival explanation[14] of the blackness of observed ravens on the grounds of unifying phenomena, and we have prior inductive reasons to expect truth to be correlated with unification in the local domain of ravens' colours, then we are warranted in believing that the explanatory superiority of $N(r, b)$ gives us at least some reason to be comparatively confident in $N(r, b)$ over its rivals. Moreover, Williams has offered justifications (to date unchallenged *given* his SJI claims) of the inference of hypotheses with existential implications regarding the unobserved, which would fill a hole in Armstrong's justification of induction (Williams 1947: 105–112).

Secondly, Armstrong's justification of induction could be helpful even if we had an adequate SJI. We seem to have rational confidence in logically contingent universal generalisations of the form (i) 'All F are G', but these are not logically equivalent to hypotheses of the form (ii) 'All or almost all F are G'. Nor do instances of (ii) imply the corresponding instances of (i). Finally, their epistemic probability connections are also weaker than one might expect: while 'All or almost all F are G' cannot be less probable than 'All F are G', it is possible that 'All or almost all F are G' has a very high probability and 'All F are G' has a probability of zero. This problem can be partly mitigated by saying that 'All F are G' is at least approximately true given 'All or almost

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[13] Since the minimum proportion rises in accordance with n, it would also establish some weaker epistemic probability claims for inductions using smaller samples. More generally, it would vindicate the intuition that, ceteris paribus, a weak induction from a sample frequency to a population frequency gradually becomes a stronger induction as the sample size increases, and this strength increases as the sample size tends towards infinity.

[14] Such as the hypothesis that "All observed ravens are observed and black but some unobserved ravens are white".

all F are G'.[15] However, supposing also that there are enough instances of F, this approximate truth might also obtain for contraries like 'Just 99.9999% of F are G'. Thus, there is still a problem of justifying our *comparative* confidence, in some cases, for hypotheses of the form 'All F are G' given suitable confirming observational evidence.

If Armstrong's justification of induction is adequate when supplemented by an SJI to provide grounds for the use of IBE as a (perhaps contextually) reliable method, then we can have IBE grounds in favour of $\forall x(Fx \rightarrow Gx)$ over its contraries, because Armstrongian necessitation claims such as $N(f, g)$ imply such universal generalisations and also thereby exclude their rivals. Indeed, we could augment Armstrong's IBE arguments for the necessary relations among universals by our confidence in '$100\% \pm \varepsilon$ are F are G' even as ε tends to zero, if our confidence is itself justified in virtue of an SJI. Consider some examples: we have large samples of humans less than 2.8 m (the tallest known human, Robert Wadlow, was about 2.72 m) and of mortal humans. However, while we seem to be justified in favouring 'All men are mortal' over approximately similar statistical generalisation, the same does not seem to be true for 'All human beings are less than 2.8 metres'. This can be explained in an Armstrongian way: a necessitation relation between the universals (a) humanity and (b) height of less than 2.8 m is not the best explanation (or even a good explanation) of the correlation between the two. Indeed, 'a height of less than 2.8 metres' does not even seem to pick out a universal. In contrast, one can at least imagine a plausible argument that a necessitation relation between being human and being mortal is the best explanation of the association between the two.[16] In short, in at least some cases, Armstrong's reasoning could help supply the important missing link in SJIs between the justification of approximate statistical generalisations and our comparative confidence in universal generalisations.

Thirdly, inferences of the necessary connections postulated by Armstrong are arguably a part of science that any justification of induction should be able, in principle, to establish as rational. Advocates of SJIs have sometimes claimed that induction presupposes nothing (Williams 1947: Chapter 6); (Stove 1986: 4–6). By this claim, they mean that we do not have to presuppose anything beyond our observational evidence when we make inductive inferences: even to prove that induction is justified, we only have to appeal to non-contingent principles of mathematics. Thus, according to them, it is not true that induction presupposes the uniformity of nature, nor a principle of causality, nor the existence of universals or essences, nor the existence of a benevolent creator-god, nor the presence (or absence) of free will, nor the vast multitude of other presuppositions that philosophers have attributed to induction since Hume started the trend. Yet, if SJIs restrict scientists to inferences of accidental regularities, then they limit the types of universe where induction can justify belief in some interesting facts. If the universe is governed by Armstrongian laws of nature, then SJIs would be unable to justify any belief in this significant fact. By contrast, in a

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[15] I mean 'approximately true' in an informal sense of approximating a parameter/proportion, rather than the Popperian sense.

[16] The details of these examples are illustrative: it is sufficient for my point that Armstrong's explanatory claims could be conceivably the best explanation in *some* cases and that there are *some* universal generalisations that seem better evinced than statistical generalisations that assert a slightly lower and precise relative frequency. If not, then Armstrong has much bigger problems than anything that Smart raises!

universe with no necessary connections, induction will not fail in this respect. Put another way, in an Armstrongian universe there would be a branch of the scientific method that could yield epistemic fruits, but SJIs would not prove that we are justified in picking from that branch, whereas in a non-Armstrongian universe there would be no loss. Induction's success in identifying key features of the universe would therefore be partly contingent on the nature of the universe.[17] More generally, even supposing that the Regularity theory is correct, it does not seem to be a presupposition of scientific reasoning, but SJIs are only sufficient for inferences of laws if we presuppose that regularity laws are all that we need to know. Regardless of whether it is true that induction has no presuppositions, it certainly helps the case of philosophers (such as Williams and Stove) who have believed in this lack of presuppositions if inductive inferences of Armstrongian laws of nature, as well as regularity statements, could be justified. Advocates of SIJs have generally adhered to Regularity theories, but induction's success would have greater robustness if it could provide epistemic grounds for non-Regularity theories such as Armstrong's.

Finally, Weintraub (2013: 211) criticises Armstrong's justification of induction on the grounds that it does not account for the inductive inference of regularities prior to the development of the relevant theoretical law statements, such as the hypotheses of universal regularities in pre-Newtonianism (like Archimedes' Principle or regular general patterns for the tides) that were subsequently explained by Newtonianism. Combining Armstrong's reasoning with an SJI might justify both these protean scientific inferences and inferences of Armstrongian law statements. The idea would be that the extrapolations of approximate regularities could provide the phenomenal generalisations that can be explained by theoretical laws. Of course, I am not arguing that this is the normal procedure in science, nor even that it ever happens. The point is that SJIs, if successful, would justify exactly the sort of inductive inferences of phenomenal generalisations that lack an underpinning by putative natural laws; as Weintraub notes, such generalisations remain unjustified given Armstrong's arguments.

To sum up this section thus far: Armstrong's justification of induction could be useful for the justification of induction even if we suppose it fails as a self-sufficient justification of induction due to (1) its reliance on IBE as a pre-inductive mode of inference, (2) its lack of justifications for propositions with existential implications about the unobserved, or (3) Weintraub's argument that scientists infer pure regularities prior to hypothesising theoretical law statements. All of these criticisms essentially allege the existence of fatal gaps in Armstrong's arguments. However, a successful SJI would fill in these gaps. Armstrong's justification of induction would still not be self-sufficiently satisfactory, e.g. because it would depend on an antecedent inductive justification of IBE. Yet the Armstrongian ideas would be non-redundant, because SJIs have their own limitations that would be addressed by Armstrong's suggested use of IBE or at least its possibility.

There might be other reasons why an Armstrongian justification of induction might fail and it is possible that an SJI would not be able to address them. My arguments in this section are not intended to be exhaustive. They simply address what I think are the

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[17] In a narrow sense of induction still being rational, this would not mean that induction presupposes anything, but in the broader sense of its success rate for identifying types of fact, induction would presuppose a particular sort of universe.

greatest challenges for Armstrong's justification, except the following issue: it is largely an empirical question whether there are actually any regularities that suffice as explananda for Armstrong's reasoning. If philosophers of physics like Nancy Cartwright (1983) are correct, then it seems that there are very few universal regularities to be explained, and therefore perhaps no cases where Armstrongian laws of nature constitute the best explanation of those regularities. More generally, Cartwright argues that we live in a "dappled world" where lawful behaviour (in accordance with anything simple and non-ad hoc enough to be meaningfully called a 'law') by parts of nature are rare exceptions rather than the norm (Cartwright 1999). That would be fine for the rationality of induction: while we would have discovered (by induction!) that many of our bolder extrapolations from the observed to the unobserved and from the known to the unknown[18] were unreliable due to nature's disuniformity, it could not prove that any extrapolations (bold or modest) from experience will be irrational, both because that would be self-refuting and because it is only inductions to a non-dappled cosmos whose unreliability would be discovered. Yet such a dappled world would *not* be fine for the IBE arguments for Armstrongian universals, insofar as these presuppose that there is an orderly and simple system of exceptionless regularities that scientists have discovered which requires metaphysical explanation.

However, even supposing a lack of suitable explananda for IBE arguments along the lines that Armstrong suggests, his universals would not be idle for the philosophy of induction. For inductivists who believe that an SJI is successful, Armstrong's explanatory relations offer a route by which scientists *could* identify that we live in a universe governed by Armstrongian laws. We *could* use induction (in a sense broad enough to include IBE) to discover those sorts of empirical claims if we live in an Armstrongian universe and if our evidence fits the pattern that Armstrong's reasoning requires. We have one more sense in which induction would not presuppose something, in line with the aspirations of Williams and Stove.

An anonymous reviewer asks whether we can specify a chaotic world in which induction does not work. This is interesting for at least two reasons: firstly, it raises the question of whether induction is in the same boat as Armstrongian reasoning in its dependence on the contingent character of the universe; secondly, it raises the question of whether we can justify using induction if we know that we are (or might plausibly be) in such a universe. The answer depends on what "work" means. As the reviewer points out, we can always make some successful extrapolations: even if the universe is as chaotic as it can be such that you are still able to make rational inductions, there will still be a successful inference we can make: "The observed universe is chaotic, therefore the universe in general – including in the future – is chaotic" will be successful. Even if we suppose a sudden outbreak of order after a long period of chaos, akin to Hesiod's synthesis of Ancient Greek cosmological myths (Evelyn-White, 1915), a sophisticated inductive rule would identify the emergence of order and inductively extrapolate (among other things) that the universe is characterised by a mixture of chaos and order. If the universe returned

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[18] The most magnificent example is the famous case of Newtonian physics. The confidence of many scientists and philosophers in this theory prior to the twentieth century is very hard to overstate: see Lord Brougham and E. J. Routh (1855: ix–xxviii and 1–2), for an appraisal of Newton's accomplishments that is fascinatingly jarring to modern philosophers of science. By experience, most scientists and philosophers of science have learned to be more reticent about such grand inductive extrapolations, but it is still tempting to believe that we *shall* discover something akin to the Newtonian system, at least in physics, if we look long and hard enough.

to chaos, then this induction would nonetheless be vindicated. It is this sort of metaphysical robustness of induction that advocates of SJIs emphasise when they say that induction presupposes nothing (Williams 1947, Chapter 6). There is thus a sense in which induction and Armstrong's reasoning are not in the same boat: the success of the latter, but not the former, presupposes a degree of order that can accommodate lawlike regularities among universals. Furthermore, it is conceivable (though very controversial) that SJIs could justify at least some inductions in such a universe, and unlike Armstrong, their advocates aspire to do so.

On the other hand, it does not take much chaos for our inductive inferences to be unreliable in general. Firstly, if a population is not entirely uniform (e.g. it is neither the case that all F are G or that no F are G) then we can be unlucky and only manage to observe unrepresentative samples, so induction fails to work *for us* and *for that population*. It is logically possible that this might happen for most of the target populations of our inductions, and in this sense induction would fail to "work". However, even for a particular population, if it is finite, then most of the large samples that we might logically (but perhaps not in practice) observe will be representative within a margin of error, as advocates of the Williams-Stove SJI point out. Secondly, as Reichenbach noted, in an infinite universe, even inductions that we could make from indefinitely large sample sizes might fail, because infinite populations do not necessarily have limiting frequencies, and there might be no mean distribution of a property in an infinite population.[19] Thus, there is a strong sense in which induction can fail to work for a chaotic and infinite population, but perhaps this is a reason not to make unconditional inductive inferences in such cases. Little seems to be lost for science if we follow Reichenbach and make *conditional* inductions such as "If there is a limiting relative frequency for F in G, then it is approximately that which we have observed" or "If a population mean exists, then it is probably close to the mean of our samples." Therefore, while induction is more robust than Armstrongian reasoning and while there are some important senses in which induction must "work", there are other important senses in which it might not.[20]

An anonymous reviewer also raises the question of whether and how inductive arguments in mathematics (henceforth "mathematical extrapolation", to avoid confusion with mathematical induction) could be justified by a synthesis of an SJI and Armstrongian reasoning. There is one type of mathematical extrapolation that is no trouble for such a synthesis: inferences from relations among samples of numbers (or other mathematical objects) to statistical generalisations or predictions about finite populations, e.g. "The first million digits of π are random, so probably the second million digits are also random." These are not essentially any different from the non-mathematical inductions that are putatively justified by SJIs, in that the same

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[19] For example, if there are an infinite number of distinct particles that can be positively or negatively charged, and charge is distributed as a Cauchy frequency distribution or a Landau frequency distribution, then there is no universal population proportion to be discovered for population charge, and thus any extrapolation of proportion that we might make by induction (no matter how numerous our samples) will be mistaken.

[20] I do not regard the latter as problem for induction, because what is important for induction's rationality is whether particular inductions are *reasonable*, and (this is disputed) I do not think that necessary success is a precondition of inference's reasonableness. Substantiating this claim would take me far astray. I merely make it to indicate that I am not conceding anything valuable to inductive sceptics by saying that there are important senses in which induction might not "work".

underlying mathematical theorems obtain regardless of whether their domains are mathematical or non-mathematical. For example, we might extrapolate from the randomness of the first million digits of π to a statistical generalisation that all or almost all of an indefinitely large (but finite) population of digits of π are random. The precise nature of the laws in the Armstrongian element of the synthesis would presumably have to be modified since Armstrongian laws are metaphysically contingent and mathematical laws are presumably not, but this is not an epistemically important change, nor one that is inconsistent with Armstrong's goals for his laws.

The trickier cases are mathematical extrapolations to universal generalisations about infinite populations, such as "The first million digits of π are random, so probably *all* the million-fold sequences of digits of π are random." The interpretation of such arguments is controversial (Pólya 1954) (Franklin 1987) (Baker 2007). However, if one thinks that we have a priori knowledge of mathematics, then it is conceivable that we could have defeasible knowledge about uniformities that could justify our beliefs that particular samples are representative with respect to some target populations and properties in mathematics, such as the first two million digits of π and randomness. This knowledge about uniformities might be very local, so that rational mathematical extrapolation would proceed similarly to how John Norton has recently argued it proceeds that inductive reasoning in natural science (Norton 2003); (Norton unpublished). Hume objected to a priori knowledge of such representativeness postulates in non-mathematical science, but even Hume did not object to a priori knowledge in mathematics, and consequently it is not apparent that Humean sceptical doubts about mathematical extrapolations – even to infinite target populations – are a problem.

A general defence of Armstrong's arguments or any SJI is beyond my scope here. It is certainly debated whether he actually offers the *best* explanation of any regularities: Beebee, as a Regularity theorist, challenges him on this point (2011: 510). There are also necessitarian rivals (Ellis, 2001); (Bird, 2007), although it is possible that these necessitarian rivals could make analogous contributions to the philosophy of induction. My claim in this section is that Armstrongian universals could make the contributions that I have suggested, rather than that they are the only way of making these contributions. Despite these limitations, in this section I have given several reasons why the explanatory relations that Armstrong describes could justify some of our epistemic intuitions, *even supposing* that his reasoning fails as a self-contained justification of induction.

3 Do Armstrongian Universals Explain?

3.1 Smart's Criticism

Smart (2013) has recently provided an extensive critique of Armstrong's reasoning. Part of Smart's article deals with Armstrong's critique of Regularity theories. Smart argues that regularity theorists are no less able to justify induction than Armstrong. He first presents a type of justification of induction that is compatible with the Regularity theory, but ultimately rejects it, and instead relies on criticisms of Armstrong's own attempts at justifying induction. Smart apparently thinks that *both* the Armstrongian and the Regularity theorist are currently unable to justify induction, though he does not

embrace inductive scepticism (Smart, 2013: 320–331). I agree that Hume's Problem does not require us to reject the Regularity theory, albeit for different reasons. (I think that induction can be justified in a way that is neutral with respect to the Regularity theory or its rivals, but that claim is beyond this article's scope.) However, while I grant that the Regularity theory is not a barrier to answering the Problem of Induction, this point still leaves open the question of whether Armstrong's justification is successful.

Smart contends that Armstrong's reasoning falls at the first hurdle: the inference of a necessary connection by IBE. He claims that the relevant explananda are statements of observed regularities such as $\forall x(ORx \rightarrow OBx)$, where 'OR' stands for 'observed to be a raven' and 'OB' stands for 'observed to be black'.[21] In this context, 'to observe' is a success verb: I can only observe that Fa if a does, in truth, have F. In the same vein, observation statements are not purely phenomenal descriptions: it is not enough that I experience a as F-ing for 'Fa' to be a true observation statement. Smart grants that $N(r, b)$ entails $\forall x(Rx \rightarrow Bx)$, where r refers to the universal of ravenhood and b refers to the universal of blackness.[22] However, he points out that the explanandum of $\forall x(ORx \rightarrow OBx)$ does not follow from Armstrong's suggested explanans, which is the conjunction of (1) $N(r, b) \rightarrow \forall x(Rx \rightarrow Bx)$ and (2) $N(r, b)$ (Smart, 2013: 323)) To verify Smart's criticism, consider the following set of statements:

(1) $N(r, b) \rightarrow \forall x(Rx \rightarrow Bx)$
(2) $N(r, b)$
(3) ORa
(4) ¬OBa
(5) Ra
(6) Ba

This set is consistent. Note that (3) and (6), which state that a is black and observed to be a raven, do not imply that a is *observed* to be black. For example, we could observe that a is a raven by feeling it in a dark room, yet be unable to determine its colour. Or we could see it using sunglasses, so that we could visually determine that it has the shape of a raven (and any other required features for identifying its species given our background knowledge) but the tint of our sunglasses means that we are unable to determine its colour. Contrary to the explanandum $\forall x(ORx \rightarrow OBx)$, the statements (3) and (4) imply ¬(ORa → OBa). Since there is a consistent set of statements in which the explanans is true and the explanandum is false, the former (propositions (1) and (2)) does not imply the latter. Smart argues that Armstrong has failed to demonstrate that he offers an explanation of the phenomena, and *a fortiori* he does not demonstrate that he offers the best explanation.

A defender of Armstrong might be tempted to reply that the explanans of (1) and (2) entails $\forall x(Rx \rightarrow Bx)$ by *modus ponens*, and argue that $\forall x(Rx \rightarrow Bx)$ explains $\forall x(ORx \rightarrow OBx)$. Smart correctly identifies a problem here: Armstrong denies that

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[21] We might also characterise this data as a conjunction of the form ((ORa → Oba) ^ (ORb → OBb) ^ … ^ (ORn → OBn)). This would have the advantage of excluding the special case where $\forall x(ORx \rightarrow OBx)$ is true because we have not observed any ravens. I shall stick to Smart's approach for simplicity's sake, with the implicit qualifier that $\forall x(ORx \rightarrow OBx)$ is not vacuously true.
[22] Like Smart, I shall use 'ravens' and 'black' as stand-ins for predicates denoting more fundamental properties, and thus depend on your sense of charity.

general regularity claims can genuinely explain observed regularities.[23] The universal generalisation $\forall x(Rx \rightarrow Bx)$ is a mere regularity. Therefore, Armstrong's own standards of explanation entails that it cannot perform the suggested explanatory work, and so Armstrong could not utilise this explanatory chain to formulate his IBE argument (Smart 2013: 324).

An Armstrongian might argue that regularities cannot explain *except* when they are also explained by a necessary connection.[24] However, even if we grant that mere regularities can explain, there are still problems. For instance, explanation does not seem to be a transitive relation. One classic example of an apparent failure of transitivity comes from E. J. Lowe:

> "For want of a nail the shoe was lost,
> For want of a shoe the horse was lost,
> For want of a horse the rider was lost,
> For want of a rider the battle was lost,
> For want of a battle the kingdom was lost,
> And all for the want of a horseshoe nail." (Lowe 1980: 50)

Each step in this chain might be a genuine explanation, yet the nail's absence does not seem to explain that the kingdom was lost. Even if we grant that explanation is transitive in chains of deductively sound inferences, there is still an issue, because $\forall x(Rx \rightarrow Bx)$ does not imply $\forall x(ORx \rightarrow OBx)$.[25] At a minimum, a defender of Armstrong would need may additional arguments on contentious points in the philosophy of explanation to establish that the sequence from (1) and (2) to $\forall x(Rx \rightarrow Bx)$ to $\forall x(ORx \rightarrow OBx)$ is a genuine explanatory chain, while nonetheless making the significant concession to Regularity theorists that mere regularities can be explanatory.

3.2 An Alternative Explanandum

Instead of that line of argument, I shall point to an alternative explanandum for $N(r, b)$.[26] First, consider the role of observation in Smart's explanandum: it demarcates the subset of ravens that we have *observed to be ravens* from ravens in general. We can also pick out another interesting known regularity by using 'observed' in the sense of being observed *at all* as an object. Assume that, for all of the ravens that we have

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[23] "That all F's are G's is a complex state of affairs which is in part *constituted* by the fact that all observed F's are G's. 'All F's are G's' can even be rewritten as 'All observed F's are G's and all unobserved F's are G's'. As a result, trying to explain why all observed F's are G's by postulating that all F's are G's is a case of trying to explain something by appealing to a state of affairs part of which is the thing to be explained. But a fact cannot be used to explain itself. And that all *unobserved* Fs are Gs can hardly explain why all F's are G's." (Armstrong, 1983, p. 40).

[24] I am grateful to an anonymous referee for suggesting this position as an interpretation of Armstrong.

[25] One might say that $\forall x(Rx \rightarrow Bx)$ plus some of our background knowledge about the reliability of our perception in the contexts of our observations implies $\forall x(ORx \rightarrow OBx)$. However, the inferential link here is likely to be probabilistic rather than deductive: I believe that if there are black ravens in the room, I shall probably observe them, but not certainly.

[26] Eduardo Castro (2016: 438–443) also attempts this sort of response to Smart, but does not prove that his alternative explanandum is entailed by Armstrong's putative explanans, nor does he argue for the claim that the latter explains the former.

observed, we know that all of them are also observed and black. I shall formalise this fact as $\forall x((Ox \wedge Rx) \rightarrow (Ox \wedge Bx))$, with 'O' for 'observed at all by our current epistemic community. I mean 'epistemic community' in roughly the sense of Bas van Fraassen (1980: 17-19). Our epistemic community consists of those entities with whom we can exchange beliefs, justifications, doubts, and so on. This 'can' is relative to the intrinsic properties of the possible members of these communities: there is a sense in which I cannot epistemically trade with David Hume, but that is only due to the extrinsic fact that we live at different times. In science today, our epistemic community consists of (an overwhelmingly large subset of) human beings. In the future, it could consist of aliens, androids, genetically engineered gorillas, and so on. Obviously, the boundaries of our epistemic community are vague (at what point of mental disability, if any, does someone cease to be a person with whom we can trade scientific beliefs?) but that is unproblematic here. In the primordial inductive case, philosophers seem to have assumed that the relevant epistemic community consists of the individual person doing the inductive reasoning.[27]

This seems like a more plausible explanandum for Armstrong's explanans than Smart's suggestion. Perhaps what Smart actually had in mind. Again, I stipulate that such statements must not be merely vacuously true. Armstrong's proposed explanans entails this alternative explanandum:

(1) $N(r, b) \rightarrow \forall x(Rx \rightarrow Bx)$
(2) $N(r, b)$
(3) $\forall x(Rx \rightarrow Bx)$ (*Modus ponens*, 1, 2)
(4) $Oa \wedge Ra$ (Assumption)
(5) Ra ($\wedge$ Elimination, 4)
(6) $Ra \rightarrow Ba$ (Universal Instantiation, 3)
(7) Ba (*Modus ponens*, 5, 6)
(8) Oa ($\wedge$ Elimination, 4)
(9) $Oa \wedge Ba$ ($\wedge$ Introduction, 8, 7)
(10) $(Oa \wedge Ra) \rightarrow (Oa \wedge Ba)$ ($\rightarrow$ Introduction, 4−9)

Since a is arbitrarily selected, we can infer:

11   $\forall x((Ox \wedge Rx) \rightarrow (Ox \wedge Bx))$ (Universal Generalisation, 10)

Consequently, Armstrong's explanans entails at least one explanandum that we possess in the context that he and Smart consider. We do not have to suppose that the predicate 'O' picks out a universal, because the only postulated necessary connection is between r and b. Furthermore, premise (1) is true in virtue of how Armstrong defines the relation N (1983: 85).[28] Therefore, the only contingent part of the explanans is (2). And if a proposition A implies a contingent proposition B when A is conjoined with logically

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[27] My arguments do not presuppose this claim.
[28] Provided that we use $N(r, b)$ to mean what Armstrong calls an "iron law", which are exceptionless. He also proposes ceteris paribus laws among universals, which he calls "oaken laws" (Armstrong 1983: 149). I am only discussing the former in this article.

necessary propositions, then A implies B *simpliciter*.[29] Hence, N(r, b) implies $\forall x((Ox \wedge Rx) \rightarrow (Ox \wedge Bx))$.

Famously, there is more to explanation than entailment. However, there is no obvious reason to expect further problems for Armstrong. Those who require that explanations must be true will say that the combination of (1) and (2) will only explain (10) if (1) and (2) are true. Nonetheless, the salient point for Armstrong's purpose is that these hypotheses offer *potential* explanations, which does not require the truth of the explanans. Additionally, while there are famous counterexamples to entailment as a sufficient condition for explaining event-statements (Bromberger 1966: 92–93) I do not know of any analogously persuasive counterexamples to entailment as a sufficient condition for the potential explanation by a *lawlike* statement of another *lawlike* statement. Furthermore, when one of the lawlike statements is not just a regularity statement, but rather a full-blooded necessitation claim like (1), it is *a fortiori* even harder to think of potential counterexamples. However, my argument needs no such general claim, because I am only claiming that (1) and (2) are potential explanations of (10), not that any similarly related statements explain each other.

Smart would be correct if $\forall x(ORx \rightarrow OBx)$ were the only explanandum that Armstrong could explain with his universals. However, I have argued that there is at least one alternative in the sort of context they imagine. You might doubt that this context is the right way to frame Hume's Problem. Perhaps rightly so. Still, *if* we grant their assumptions, then Armstrongian metaphysics does possess explanatory potential. My arguments are compatible with the existence of *other* potential explananda that could be explained using necessary connections among Armstrongian universals. My point is simply that there is at least one type of explanandum that the necessary connections can explain.

4 Conclusion

Beyond the points discussed, there are further challenges that critics of Armstrong might raise. For example, the factive interpretation of IBE has been questioned (van Fraassen 1980: Chapter 6). However, contrary to what the critics I have covered have said, Armstrong's reasoning is cogent, and the explanatory relations that Armstrong identifies have a robust potential methodological significance for induction. As for the conceptual analysis of our notion of natural laws or the empirical question of whether our universe is actually lawful in Armstrong's sense, I have said nothing.

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[29] All of my uses of 'implies' refer to classical implication.

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