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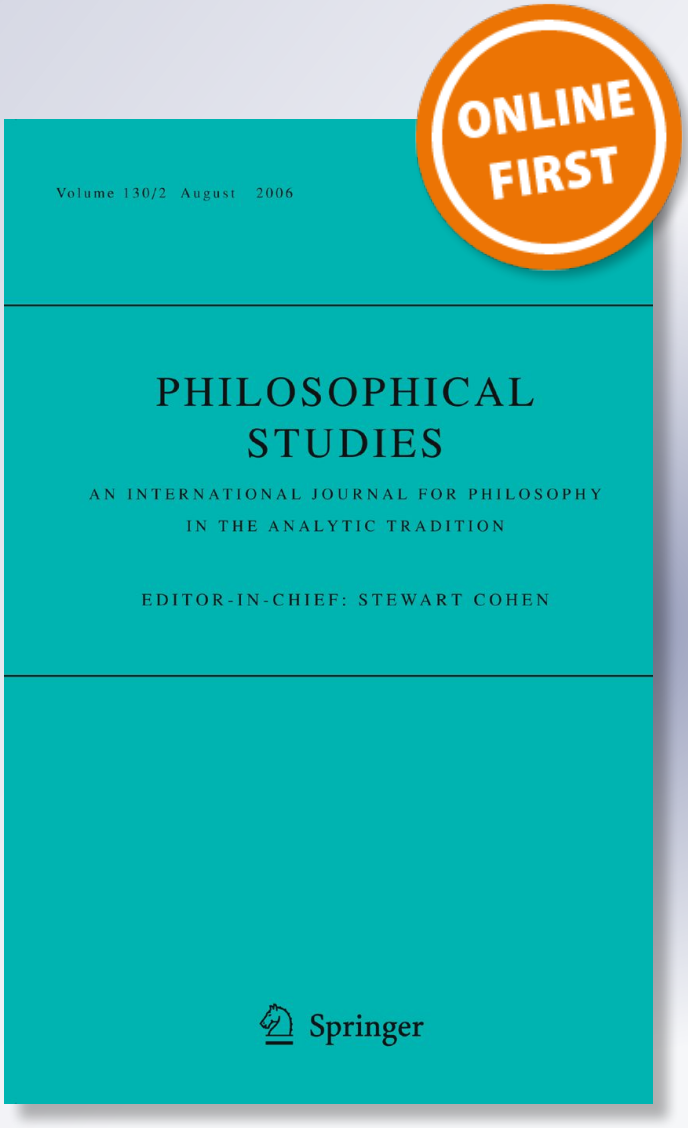

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Induction and inference to the best explanation

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Abstract In this paper I adduce a new argument in support of the claim that IBE is an autonomous (indispensable) form of inference, based on a familiar, yet surprisingly, under-discussed, problem for Hume's theory of induction. I then use some insights thereby gleaned to argue for the (reductionist) claim that induction is really IBE, and draw some normative conclusions.

Keywords Induction · Inference to the best explanation · Scepticism

1 Introduction

I wish to focus on two claims that have been made about the relationship between inference to the best explanation (IBE) and induction. The first is that IBE is an *autonomous* (indispensable) form of inference (Harman 1965). The second claim is that induction is a special case of IBE (Foster 1983; Harman 1965, 1968; Lycan 1988, ch. 9).

My aim in this paper is to adduce a new argument in support of the autonomy claim (Sect. 3), use some insights thereby gleaned to argue for the reductionist claim (Sect. 4), and draw some normative conclusions (Sect. 5). I begin with some clarificatory remarks.

What is an explanation? Do we increase our understanding of a phenomenon by showing it is likely to happen (Hempel 1965, p. 337),[1] by making it more familiar (Bridgeman 1927, p. 37; Stebbing 1933, p. 389; Dray 1957, pp. 79–80), by relating it to something that is already understood (Scriven 1970, p. 202), by unifying what

[1] The D-N model (Hempel 1965, pp. 335–376) is an extreme special case. By deducing the *explanandum* from initial conditions and a set of laws, we show that it was *certain* to happen.

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we have to accept (Kneale 1949, p. 91; Feigl 1970, p. 12; Friedman 1974; Kitcher 1989; Bartelborth 1999; Schurz, 1999) or by providing causal information—either about causal history (if the *explanandum* is a particular event) or (if the *explanandum* is a causal regularity) about the mechanism linking cause and effect (Salmon 1984; Lewis 1986; Miller 1987; Humphreys 1989; Lipton 1991)—or all of the above (Harman 1965; Lycan 1988)?

Fortunately, I needn't take a stand on this issue. My case for the indispensability of explanatory considerations and for the reducibility of induction will not rely on any particular account of explanation. I will be adducing clear-cut cases of explanation (understood intuitively) which *any* plausible theory of explanation must countenance.

The next clarificatory point pertains to IBE. Several considerations count towards the acceptance of theories: explanatoriness, simplicity, elegance, etc. When we accept a theory (at least partly) because it explains, this is a case of IBE.

The term 'induction', too, requires clarification. Following Peirce (1966), Foster (1983) and Niiniluoto (2004), I use the term 'induction' narrowly, and not as it is sometimes (Lehrer 1970; Swinburne 1974, p. 1; Lipton 1991; Howson 2000; Kuipers 2004; Okasha 2001) used, to apply to all non-deductive inferences. I shall use it to denote only *enumerative induction*—inference from a sample to the entire population or to the next case. This includes statistical inductive inference—from 'x out of y observed A's are B's' to 'The proportion of A's that are B's in the entire population is x/y' or to 'The probability of the next A being a B is x/y' (where $x < y$), although I will focus on the extreme case of the so-called "straight rule". So on my usage, we reason inductively when we infer that the sun always rises on the basis of daily sunrises in the past, but an inference from data about one type of thing (facial expression, e.g.) to another (some mental state) is not inductive.

My usage is, I think, more customary, but more importantly, makes for a straightforward formulation of the issues in dispute. If we use the term 'induction' widely, to apply also to inferences which invoke explanatory considerations, then, to be sure, IBE will be dispensable (because subsumed under "induction"). But the interesting question is whether it is dispensable relative to *enumerative induction*, and we focus on it by using the term 'induction' narrowly.

The indispensability claim should be clarified as well. We should, to begin with, distinguish between the descriptive and the normative issues. The first concerns the role that explanatory considerations have—as a matter of fact—in our inferential practice. I don't mean the inferences that we actually make: some of these are fallacious *by our own lights*. I have in mind, rather, the inferences sanctioned by standards that we (implicitly) accept and by which we are (imperfectly) governed. The second issue is the role explanatory considerations *ought* to have. I will consider the descriptive question first (Sects. 2, 3, 4), and then (Sect. 5) say something about the normative one.

Second, God can get along without invoking explanatory considerations, because he knows (everything) *directly*. So the idea is that explanatory considerations are indispensable *insofar as we are inferring*: their invocation allows us to derive conclusions we couldn't otherwise get (from the same premises).

Third, indispensability isn't an intrinsic property of a rule: it depends on what other inference rules are available. For instance, the rule of conjunction elimination (infer p from p & q) is indispensable if there are no other inference rules, but redundant in a system that includes Modus Ponens, which constitutes a complete system of deductive rules of inference (Mendelson 1979, 1.4). And the rule that allows us to infer from a sample to the next case is dispensable in a system which includes the rules of deduction and the rule that allows us to infer from a sample to the entire population: we can first generalise from the sample, and then use deduction to derive the statement about the next case from the generalisation. The indispensability of IBE is to be understood here as *relative to (enumerative) induction*.

Finally, I will adopt Harman's (1968, p. 531) construal of the claim that induction is IBE: "whenever enumerative induction warrants a conclusion, the same conclusion can be reached by inference to the best explanation".

2 Possible strategies for upholding the indispensability claim

To show the indispensability of IBE we can adduce beliefs in whose formation induction doesn't figure at all or for which it is insufficient. The former sort of case is provided (Harman 1965) by beliefs pertaining to the unobservable. Thus, the belief in Newton's second law of motion ($F = M \cdot a$) cannot be inductively-based. We haven't got the requisite evidence from which to generalise, because neither the force acting on a body nor its mass are observable. Our belief is warranted—if at all—because it best explains the phenomena: planetary motion, the tides, falling bodies, motion on inclined planes, etc.

Harman (1965) adduces another kind of case, in which the conclusion of the inference *is* about the observable, yet it is not clear how the inference is to be represented as a (possibly complex) case of induction. The onus of proof, he (plausibly) suggests, is on those who claim that it can. For instance, what generalisations is a detective invoking when he concludes—on the basis of the finger-prints on the murder-weapon, lack of alibi, motive—that the butler committed the murder? Here, the claim is not that induction doesn't figure in the belief's formation (the detective may well be relying on *some* inductively-based beliefs), but, rather, that it doesn't suffice.[2] This strategy is somewhat unsatisfactory, because it shifts the burden of proof to those who deny the indispensability of IBE. I will adopt a more ambitious tack, and adduce an argument to *show* that induction is insufficient.

3 A new sort of case to show the indispensability of IBE

My examples are derived from a familiar, yet surprisingly under-discussed, problem for Hume's theory of induction. According to Hume, the paradigm type of inductive

[2] Fumerton (1980) shows how *some* causal inferences can be viewed as inductive ones (by making explicit tacit assumptions). But he doesn't take up Harman's detective case challenge, and here, it is not clear how the reduction is to be effected.

reasoning involves a *constant* conjunction. "We remember to have had frequent instances of the existence of one species of objects; and also remember, that the individuals of another species of objects have *always* attended them…Thus we remember to have seen that species of object we call flame, and to have felt that species of sensation we call heat…Without any farther ceremony, we call the one cause and the other effect, and infer the existence of the one from that of the other" (p. 87, my italics; original italics removed).[3]

But as Price (1940, p. 7) points out, Hume misrepresents ordinary induction. We hardly ever encounter such invariable uniformities. I have often seen a flame without touching it so as to experience the heat. The same is true of Hume's famous billiard balls. "It is not the case…that the complex impression of one billiard ball striking another is always followed by an impression of the second one's motion. If I blink or faint or die just as the first impression is ending, the second one will never come into being" (Price 1940, p. 7).

The problem Price is highlighting can be clearly appreciated if we represent induction as follows, and note that these (not atypical) cases do not satisfy the premise.

All observed A's were observed to be B's

All A's are B's[4]

Several suggestions for contending with the difficulty suggest themselves. I will consider them, and then invoke the most promising one in support of the indispensability claim.

3.1 Imperfect induction?

There are also, Hume recognises, less auspicious cases: "we frequently meet with instances to the contrary" (p. 131). "[R]hubarb has [not] always proved a purge, or opium a soporific to every one, who has taken these medicines" (1777, pp. 57–58). What happens when the regularity is imperfect? "A contrariety of events in the past may give us a kind of *hesitating belief* for the future…the mind is determin'd to pass from one object to the other; but not with so entire a habit, as when the union is uninterrupted, and all the instances we have ever met with are uniform and of a piece" (pp. 132–133, my italics).

To see that Hume's discussion of these cases cannot help us with the difficulty, I will consider the inference to the next case and to the generalisation in turn. An imperfect conjunction, according to the suggestion, gives rise to an *uncertain expectation* regarding the next case. And the degree of certainty is the proportion of favourable cases: "That probability of causes…depends on a contrariety of experiments…when we transfer the past to the future, the known to the unknown,

[3] Unless otherwise suggested, references to, and quotes from, Hume pertain to the *Treatise* (1739).

[4] I am here ignoring complications engendered by Goodman's (1955) new riddle of induction. Let us assume that this *is* an instance of valid induction.

every past experiment has the same weight, and…'tis only a superior number of them, which can throw the balance on any side" (pp. 135–136). "If you weaken…the union…you weaken the principle of transition, and of consequence that belief, which arises from it" (p. 142). By way of contrast, "where the past has been entirely regular and uniform, we expect the event with *the greatest assurance*, and leave no room for any contrary supposition" (1777, p. 58, my italics).

Whenever we encounter an imperfect conjunction, we form, according to the "straight rule" that Hume is here endorsing, a "hesitant" expectation. How, then, can we account for our bestowing certainty on some of our expectations even when the regularities on which they are based are imperfect?[5]

"One wou'd appear ridiculous, who wou'd say, that'tis only probable the sun will rise to-morrow, or that all men must dye; tho''tis plain we have no further assurance of these facts, than what experience affords us" (p. 124). True, Hume says this is in another context. There is a distinction, he recognises, even among inductive inferences: "many arguments from causation exceed probability, although they do not arise from the comparison of ideas" (p. 124). "Proofs" are arguments which "are free from doubt and uncertainty". "Probability", by way of contrast, is "evidence which is still attended with uncertainty" (p. 124). But surely he would say that our prediction of tomorrow's sunrise is a case of a "proof", our "gappy" experience notwithstanding.

The proposed suggestion is even less auspicious when it comes to the inference to the generalisation. Here, it would not even have us forming a "hesitating belief", but, rather, taking the generalisation to have been falsified by a counter-example.

3.2 Fine-grained induction?

We have often seen snow without touching it so as to experience the cold. But we haven't had touching-snow experiences without experiencing cold. So can't we formulate our inductions so that the recalcitrant experiences no longer constitute counter-examples? Well, of course we *can* restrict ourselves to projecting only experiences identified in a way which is sufficiently fine-grained so as to render them uniform. But we *don't*. We often accept a generalisation on the basis of experience which isn't regular. For instance, I think snow is cold *tout court*.

3.3 The "community response"

Perhaps I eliminate the "broken connexions" by relying on the experience of others. Testimony, it might be thought (Baier 1991, p. 120), enables an individual to fill the

[5] Hume (1777, p. 113) recognises yet another kind of less auspicious case, in which the observed regularity *is* perfect, but inadequate for complete confidence in the generalisation. Thus, an Indian prince, who has always lived in a warm climate, is justified in "refus[ing] to believe the first relations concerning the effects of frost [i.e., water freezing]", but complete *certainty* that water never freezes would be rash for him. The freezing of water is not "contrary to his experience" or "miraculous"; it is merely "marvelous", "amazing", "not conformable to his experience". He is reasoning—by analogy—about an unfamiliar situation, which imperfectly resembles the one he has experienced. "If you weaken the… resemblance, you weaken the principle of transition, and of consequence that belief, which arises from it" (p. 142).

gaps in his experience, thereby rendering his inductions compatible with Hume's account. The division of epistemological labour, which Descartes' individualistic project has beguiled us into ignoring, is part and parcel of our lives as cognizers.

The "community response" doesn't eliminate the gaps in our experience. To begin with, we don't have testimony pertaining to all the gaps in our experience. I've had very few testimonies (even indirect ones) about snow and flames I watched from a distance. Their temperature was very seldom reported to me.

The second reason for thinking that the "community response" doesn't eliminate the "broken connexions" is that invoking the testimony of a witness adds another positive instance of the generalisation under question, but doesn't *eliminate* the counter-example (my own experience). A reason is required for thinking that my (recalcitrant) experience may be ignored. But such an explanation will render the witness otiose. So this is no longer the "community" strategy for coping with Hume's difficulty, and will be considered in Sect. 3.5.

3.4 Dismissing the putative counter-examples as irrelevant

Rather than *observing a failure of the regularity*, it may be argued, in the recalcitrant cases we are *failing to observe a regularity* (Price 1940, p. 56; Baier 1991, p. 113; Falkenstein and Welton 2001, p. 289).

The suggestion may be true, but is inadequate as it stands. A principled way of distinguishing between genuine counter-examples and irrelevant observations must be given. When I see the fire but do not experience heat, why do I dismiss the case as a failure to observe a regularity rather than taking it to show that the regularity fails?

3.5 The real solution

The most promising strategy for contending with the difficulty is suggested by Hume himself, in his discussion of our belief in the continuing existence of objects ("bodies"). He alludes to "the turning about of our head, or the shutting of our eyes" as responsible for the breaking of the "connexion" (p. 198), rendering intermittent our experience of objects.

In the cases Hume cites, we explain why our experience is irregular, and we infer to the truth of the explanation. I don't experience heat when watching a distant flame because heat dissipates. The flame is hot (now), although I don't feel the heat. I *would* feel the heat if I were nearer. My (gappy) experience is explained by the conjunction of the two generalisations: flames are hot and heat dissipates.

Is there an alternative explanation which would render my experience a counter-example? Well, perhaps the heat of the fire is correlated with its distance from *me*: fire is cooler the more distant from it I am. But this won't explain all the (relevant) phenomena. Why does someone much closer than me to the flame feel greater heat than I do? And why do we get different readings from thermometers situated at different distances from the flame? There isn't here a rival explanation to the one suggested by Hume.

The same analysis can be given in the case of Hume's billiard balls. When I look away, I don't see the billiard ball beginning to move upon being struck, because the ball isn't in my visual field. If I were looking at the ball, I *would* see it begin to move.

Is there an alternative explanation? Again, we could suppose that the ball moves upon being struck iff *I* observe it. But, again, this would not explain why, when the ball isn't moving (when I am not observing it), it seems to other observes (facing it) that it *is*.

There are cases in which a putative counter-example leads us to reject a generalisation. If I didn't experience heat when in close proximity to the fire, that would count against the generalisation that fire is hot. And looking at the billiard ball and not seeing it move would count against the generalisation that billiard balls move when struck.

Here is another example. I sometimes, but not always, experience a migraine after eating cheese, and consider this to be a *bona fide* counter-example to the generalisation 'Eating cheese always causes me to have a migraine'. Why is this case akin to being close to the fire and feeling no heat or looking at the billiard ball and not seeing it move?

If the generalisation 'Eating cheese always causes me to have a migraine' is to explain my (non-uniform) experience, then, in analogy with our thinking that although we do not feel the fire's heat, the fire *is* always hot, we must suppose that cheese always causes me to have a migraine, but sometimes the pain is unfelt. But this is very implausible (if not incoherent). Pains are *self-intimating*: if I don't feel pain, I don't have it.[6] As Reid (1785, I.i, pp. 18–19) says, "When I am pained I cannot say that the pain I feel is one thing, and that my feeling it is another thing. They are one and the same thing, and cannot be disjoined even in imagination. Pain, when it is not felt, has no existence".[7] So we are left with just one explanation: that I sometimes don't have pain after eating cheese. And these cases constitute genuine counter-examples to the generalisation.

The analysis of these examples shows them to be instances of IBE. And because they are not cases of induction (the requisite premise—constant conjunction—isn't true), they establish the claim that IBE is an autonomous (indispensable) form of inference.

Gaps in our experience are, in fact, much more ubiquitous than these examples suggest. As Gomberg (1976) points out, it is in the nature of causal inference that when it is made, even on the basis of experience which is perfectly regular, it is triggered by the experience of the cause *without that of the effect*. If the effect *is* experienced, the belief in its occurrence is the result of *perception* (observation), and not inference. This is not "gappy induction" as we have defined it, but it invites the same sort of (explanationist) account. We ignore the "gap" if it is best explained by the generalisation, and we construe it as a counter-example if it is best explained

[6] Can I have an unfelt pain when I am unconscious or asleep? Maybe, but I am assuming that sometimes I am conscious—having eaten the cheese—and do not feel pain in my head.

[7] Moore (1922, pp. 91–2) demurs, implausibly to my mind. "The question whether anything exists, when it is not perceived…seems to me to be one which can only be settled by observation…observation might justify us in concluding that…pains…do not exist, when they are not perceived".

by its negation. For instance, if a prediction pertains to the future ('The window pane *will* break upon being hit by the stone that has just been thrown at it'), it is obvious why we do not experience the effect (the window pane breaking) upon encountering the cause (seeing a stone being thrown): the prediction itself implies that the experience of the effect *is yet to come*, and we do not experience future events.

Things are more complicated when we infer from an effect to a cause. If we are inferring from a present effect to a cause in the distant past, we can explain why the conclusion isn't experienced: we weren't there. But if I watch the window pane for a while and then see it shattering without seeing a stone hitting it, the best explanation of the gap is that the window pane was *not* struck by a stone. If the putative cause had occurred, I would have seen it.

The kind of gap that Gomberg has discerned does not occur when the inference is to concurrent properties. If I predict that the next raven to be observed will be black, I do not (typically) observe it to be a raven without observing its colour. So there is no gap here to be explained away.

The suggestion that we respond to gaps in our experience by invoking IBE might seem circular: it is justified as best explaining our inferential practice. But the circularity is benign, because we are not attempting to *justify* IBE. Our aim is descriptive, so it is, in fact, rather *fitting* that the essential role of IBE should itself be established by invoking IBE.

4 Induction as IBE

Since we are (initially, at least) considering the descriptive question, and attempting to *characterise* our inferential practice, the claim to be defended here is that when we reason inductively (when we infer a generalisation from a uniform sample), our inferential practice will assess the inference by IBE standards.[8] How is the claim to be established?

Harman (1968, p. 531) invokes his account of explanation in support of the reductionist claim. He argues that a law explains the observed correlation, and that is why we are entitled to infer from the sample to the generalisation. A hypothesis is more explanatory than another, according to Harman, insofar as it is "a better, simpler, more plausible (and so forth) hypothesis… [i.e., if] it is a better hypothesis in the light of all the evidence" (1965, p. 91). And if explanatoriness is the sum total of all the positive features a statement can have, a generalisation "explains" its instances.[9] But this is 'explanation' in a Pickwickian sense. Ennis (1968) is correct

[8] Fumerton (1980, p. 597) defends the opposite suggestion: that every IBE is, in fact, a case of induction. But his argument is a *non sequitur*. Even if (contentiously) IBE must be *defended* by invoking its success in cases where the conclusion is observable and then reasoning inductively to its success *tout court*, this doesn't show that IBE does not *invoke* explanatory considerations that are irreducible to induction.

[9] For Lycan (1988, p. 130), too, simplicity, coherence, fruitfulness and initial plausibility all contribute to the explanatoriness of a hypothesis. Confusingly, Lycan's list of the features which enhance explanatory goodness (1988, p. 130) includes explanatoriness (in addition to simplicity and fitting what else one already believes). Here, Lycan seems to be using the term 'explanatoriness' more narrowly.

in objecting that the generalisation 'All ravens are black' provides a poor explanation of the fact that the ravens that I happened to observe were black. An explanation increases our *understanding* of the *explanandum* (Lipton 1991), something that simplicity and fruitfulness do not do.

On Hempel's(1965) D-N account of explanation, a lawlike generalisation explains its instances: the statement that all ravens are black entails—in conjunction with the statement that object *a* is a raven—that *a* is black. Lange (2000, p. 17) claims, in a similar vein, that a nomological generalisation explains its instances. And Lipton concurs. The hypothesis 'All ravens are black', he suggests (1991, p. 101), "may provide some sort of explanation for the blackness of a particular raven".

Foster (1983) and Armstrong (1983) suggest—by way of offering a solution to the problem of induction—that a nomological generalisation explains its instances (and can, therefore, be rationally inferred from them). An accidental generalisation—by way of contrast—they claim, does not explain. The past consistency of gravitational behaviour, Foster illustrates, calls for some explanation. And the explanation, he suggests (1983, p. 89), is that "gravitational behaviour is the product of natural necessity: bodies have hitherto always behaved gravitationally because it is a law of nature that bodies behave in that way".[10]

I must confess that to me, these explanations seem pretty feeble. Gravitational behaviour (objects falling to the ground, the tides, etc.) is explained by Newton's laws. We do *not*, by way of contrast, understand why objects accelerate towards the earth upon being told that they do so because their doing so is a law of nature. Correlatively, we can infer Newton's laws, because they do explain the observations. We can then infer the observable generalisations ('Bodies accelerate towards the earth') from Newton's laws. But this inference is deductive, rather than inductive. If (as was the case before the advent of Newtonian mechanics) the generalisations were derived inductively, the explanationist is yet to show that the inference involved explanatory considerations.

Even if (contentiously) the (lawlike) generalisation is understood as implying that there is something that is "part of the essence of ravens" (Lipton 1991, p. 106), the bare claim that there is *something* in ravens that causes their blackness doesn't satisfactorily explain the blackness. A genuine explanation will invoke the ravens' genes, perhaps. But the hypothesis was confirmed before anybody knew about genes, and its confirmation, too, must be shown to fit the explanationist account.

Some will not share my intuition that these aren't satisfactory explanations. But to show that induction is in fact IBE, I don't have to reject other arguments to the same conclusion. The argument I will now offer will either bolster the explanationist case or entirely constitute it.

Our discussion of "gappy" induction suggests that a generalisation explains a fact about *us*: our (irregular) experience with flames, for instance. So, analogously,

[10] In a somewhat similar vein, but in a different context (rebutting the regularity theory of causation), Strawson (1989, p. 30) claims it is absurd that "there is, quite definitely, no reason at all" for the fact that "these massy physical objects…[behave] in perfectly regular ways". The explanation (to whose truth we may reasonably infer) is that "there definitely is something about the nature of the world given which it is regular" (1989, p. 22).

even when there are no gaps in our experience, the generalisation explains a fact about us: our encountering only positive instances. The generalisation 'All ravens are black' is part of the causal story of how the uniform population of black ravens led to our observing only black ravens. This is true even if (implausibly to my mind) it explains the positive instances: the two explanations aren't in competition.

Some rival hypotheses, e.g., that someone is biasing the sample (Harman 1965, p. 91) do not explain as well: an explanation is required as to *how* the biasing is brought about. But there *are* incompatible hypotheses that explain equally well. Thus, the supposition that there are (even many) non-black ravens can be conjoined with a scenario about how the sampling mechanism engendered a uniform sample (of black ravens): for instance, only black ravens frequent the places we go to. This would perfectly explain, *if true*, why we ended up observing only black ravens. But we rule it out because it is less probable. And the fact that we invoke considerations of plausibility doesn't impugn the claim that this is a case of IBE: our construal of IBE does *not* involve the (implausible) claim that *only* explanatoriness counts; it is only committed to it *contributing* to the credibility of a hypothesis (Lipton 1991; Day and Kincaid 1994). And it is (*inter alia*) because the generalisation explains (in the everyday sense of the term) that we infer its (probable) truth.

Here is another example. Suppose I sample a large number of balls from an urn, all of which I note to be red. If I then conclude that all of the balls in the urn are (probably) red, this, too, is a case of IBE. The supposition that all the balls in the urn are red explains my (uniform) experience. Other hypotheses about its constitution explain (if at all) less well. To say that something happened by chance is not to explain its occurrence.

This example also serves to rebut the popular suggestion that accidental generalisations *aren't* confirmed by their instances (Armstrong 1983, p. 5; Dretske 1977, pp. 256–260; Goodman 1955, p. 73; Hempel 1942, p. 35; Kim 1992, p. 11; Moore 1962, p. 12; Scheffler 1964, pp. 222–225).[11] We can make it even more resounding if we suppose that the content of the urn is known to be the result of a random process (Sober 1988, pp. 95–96). So even if (implausibly to my mind) laws explain their instances, this is not enough to account for our inferring in the urn case and others of its ilk.

[11] How are we to construe the claim? According to Lange (2000, p. 111), it means we can only be justified in believing an accidental generalisation if we have checked all the instances. But the accidental generalisation may be very probable even before we check its instances ('All the people in this room weigh less than 300 lb', e.g.). Neither does it mean that the probability of the generalisation isn't increased when an instance is encountered (Goodman 1955), since the positive instance rules out one way in which the generalisation could be false (Lange 2000, p. 113). If $P((x)(Ax \rightarrow Bx)) > 0$, then $P((x)(Ax \rightarrow Bx)/(Aa \wedge Ba)) > P((x)(Ax \rightarrow Bx))$. The correct explication of the intuition is that a positive instance does not increase the probability of *an unexamined case* satisfying the generalisation, and doesn't increase the probability of the generalisation over and above that engendered by eliminating a falsifier (Dretske 1977, p. 256).

5 Scepticism

Familiarly, Hume's characterisation of our inductive practice is too lenient. We do not—as he has us doing—project *every* experienced regularity: a grue emerald does not confirm the generalisation "All emeralds are grue" (Goodman 1955). We now see (Sect. 3.5) that his characterisation is also too stringent. We project some *imperfect* experiential regularities.

We now have a more adequate characterisation of our ampliative practice.[12] What implications does it have for our epistemological situation?

In one way, our situation has improved. Hume's sceptical argument against induction loses its bite, because the practice it purports to brand as irrational isn't ours. So even if its conclusion is true, it doesn't constitute a threat to our rationality. So has our normative situation improved? This would be the case if the newly characterised inferential practice were less vulnerable to scepticism. For instance, if Popper (1972) is right in claiming that science is purely deductive, we have a reason for rejoicing, since we think that scepticism about deduction is less plausible than inductive scepticism.[13]

Alas, IBE is at least as vulnerable to scepticism as induction. We can here ignore arguments against IBE with conclusions about the unobservable (Berkeley 1710, §18; Hume 1739, p. 212; Cartright 1983; Ladyman et al. 1997).[14] Our concern is with inferences from (observed) instances to a generalisation. And if the instances are observed, the next instance is observable. But we do need to worry about arguments that purport to show that IBE even to the observable is unjustified. And at least one of these is as formidable as Hume's argument against induction.

Hume's argument against induction has the following premises: (1) The conclusion of an inductive argument isn't logically entailed by its premises. (2) Every inductive argument assumes that nature is *uniform*; "that the future will be conformable to the past". (3) The principle of uniformity, which must be warranted if induction is to be justified, cannot be justified a priori. (4) The principle of uniformity cannot be justified a posteriori, since such a justification would be circular. It would itself be inductive, inferring nature's uniformity *tout court* from its uniformity in the past, thus presupposing—like *any* inductive argument—that nature is uniform.

The argument can easily be transformed into an argument against IBE (to the observable): (1) The conclusion of an IBE isn't logically entailed by its premises. (2) Every IBE argument assumes that nature is explicable. (3) The principle of explicability, which must be warranted if IBE is to be justified, cannot be justified a priori. (4) The principle of explicability cannot be justified a posteriori, since such

[12] We ignore gaps if they can be explained away in a sufficiently simple and plausible manner. IBE also yields an adequate response to Goodman's riddle. The green and grue hypotheses both explain our experience. But the former is simpler (Hesse 1969).

[13] I take it that Popper's attempt isn't successful (Newton-Smith 1981, ch. III).

[14] Ladyman, Douven, Horsten and van Fraassen (1997, p. 316) argue that IBE to the unobservable involves added ontological commitments. Hume (1777, p. 153) argues that "here experience is, and must be entirely silent. The mind has never anything present to it but the perceptions, and cannot possibly reach any experience of their connexion with objects".

a justification would be circular. It would either be based on IBE's explanatory power or on its success in the past. In the first case, it would be straightforwardly circular (invoking IBE to justify IBE). In the second case, it would be an inductive argument, and—by the reductionist claim—IBE, and so again, circular.

Perhaps the argument can be rebutted. But the same rebuttal will be available for Hume's argument against induction. So the re-characterisation of our inductive practice does not improve our epistemological position.

van Fraassen (1989) adduces additional arguments against IBE (even to the observable). First, IBE takes us to the best explanation among those we have considered. And even if the best explanation is likely to be true, we have no reason to suppose that the best explanation is to be found among the theories we have considered (van Fraassen 1989, p. 143). Indeed, van Fraassen argues (1989, p. 146), it is very likely that among the explanation we haven't considered, there are many that are as good as the explanation that is the best among those we have considered. Finally, IBE requires us to violate the Bayesian rule of conditionalisation (van Fraassen 1989, pp. 160–170).

Perhaps the arguments can be rebutted.[15] But clearly the re-characterisation of our practice has saddled us with *additional* burdens to discharge, and at best leaves our epistemological situation as it was.

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[15] I am persuaded by Weisberg's (2009) response to van Fraassen's claim that IBE violates the Bayesian rule of conditionalisation. If, as subjective Bayesians think, any prior probability distribution is permissible, then, to be sure, $P(T_1/E)$ may be smaller than $P(T_2/E)$ even if T_1 better explains E than does T_2, and assigning it higher credence in response to E because it better explains it is incompatible with conditionalising. But, Weisberg (2009, p. 137) replies, for an *objectivist* Bayesian, explanatory considerations *constrain* permissible conditional probabilities. So if T_1 better explains E than does T_2, $P(T_1/E)$ will be *greater* than $P(T_2/E)$.

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