Resolving the Raven Paradox: Simple Random Sampling, Stratified Random Sampling, and Inference to Best Explanation

(9,007 words)

Abstract

We object to standard, simple random sampling resolutions of the raven paradox on the grounds that they relevantly diverge from scientific practice. In response, we develop a stratified random sampling model. It provides a better fit and apparently rehabilitates simple random sampling resolutions as legitimate idealizations of that practice. However, neither simple nor stratified models fare well with a second concern, the objection from potential bias. In response, we develop a third model on which we systematically check kinds of ways in which disconfirming cases—non-black ravens—might be caused. This provides a novel resolution of the paradox that handles both objections. Suggestively, this third approach resembles Inference to the Best Explanation (IBE) and relates confirmation of the generalization to confirmation of an associated law. We give it an objective Bayesian formalization and discuss the compatibility of Bayesianism and IBE.

Keywords: Raven Paradox, Simple Random Sampling, Stratified Random Sampling, Inference to the Best Explanation, Objective Bayesianism, Screening -Off

1. The Paradox

C.G. Hempel's (1945) raven paradox derives from a couple of *prima facie* plausible assumptions[1]:

Nicod Criterion (NC): A claim of form $(x)(Fx \supset Gx)$ is confirmed by any sentence of the form "i is F and i is G" where "i" is a name of some particular object.

Equivalence Condition (EC): Whatever Confirms (disconfirms) one of two equivalent sentences also confirms (disconfirms) the other.

Notoriously, since $(x)(Rx \supset Bx)$ is equivalent to $(x)(-Bx \supset -Rx)$, the data that an object is a non-black non-raven confirms "all ravens are black".

The paradox has generated a variety of resolutions, but the dominant contemporary paradigm is a Bayesian one that models the generalization's confirmation in terms of simple random sampling. We offer two objections to this resolution. Our first, the objection from scientific practice, concerns striking disparities between simple random sampling and confirmational practice. This motivates the development of a stratified random sampling model. It appears to vindicate simple random sampling as a legitimate idealization: the stratified approach provides a better fit with scientific practice but delivers essentially the same resolution. However, neither fares well against our second objection, the objection from potential bias.

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[1] Hosiassen-Lindenbaum's (1940) discussion of the paradox pre-dates Hempel's but attributes the paradox to him.

This motivates a different approach, one that retains stratification, but on which we confirm causal claims as opposed to mere generalizations. Moreover, the appropriate methodology is not random sampling, but one that strongly resembles Mill's method of agreement. Thus, it provides a very different resolution of the paradox. In addition to handling both objections, it provides a better fit with scientific practice, and hence, an all-round better resolution than random sampling approaches. It also compares favorably with Peter Lipton's resolution which invokes causal considerations but identifies something much like Mill's method of difference as providing the key to the paradox.

Finally, we note some suggestive connections between the approach and Inference to the Best Explanation (IBE) and between confirmation of the raven generalization and confirmation of a closely related law. We give the approach an objective Bayesian formalization and briefly discuss the compatibility of IBE and Bayesianism, addressing both van Fraassen's and Roche and Sober's objections to such a marriage.

2. Simple Random Sampling Resolutions

Bayesians model confirmation of a hypothesis, h, by evidence, e, relative to background beliefs, K, using Bayes's theorem:

$$P(h|e \ \& \ K) = P(e|h \ \& \ K) . P(h|K) / P(e|K)$$

On learning e, an agent updates her personal probability in h from her prior probability, $P(h|K)$, to her posterior probability, $P(h|e \ \& \ K)$. Thus, e *incrementally confirms* h, or simply *confirms* h,

relative to K if, and only if, $P(h|e \& K) > P(h|K)$. We may also speak of the *absolute* degree of confirmation of h, which is just h's probability. We shall say that h is *absolutely well confirmed*, or just *well confirmed*, for an agent if she gives h a high probability. What counts as a high probability is inevitably vague, and none the worse for that. Much of our initial discussion concerns incremental confirmation, but well confirming shall become salient later.

Standardly, Bayesians deflate the paradox by arguing that Hempel's counterintuitive confirmers provide negligible incremental confirmation of the raven generalization. To obtain this result they assume our evidence is obtained by *simple* random sampling from the universe i.e., it is assumed that each object in the population has an equal probability of being sampled. K is then specified to include reasonably plausible assumptions about the relative frequencies of non-black things and ravens. Together, K and the simple random sampling assumption mandate probabilities that resolve the paradox.

Comparative resolutions argue that a non-paradoxical instance, Ra & Ba, will confirm significantly more than a paradoxical one, -Ra & -Ba. Non-comparative, or quantitative, resolutions argue that the confirmation afforded by –Ra & -Ba is positive, but small. Both claims presuppose some measure of confirmation. A reasonable and popular choice here is the difference between the posterior and prior probabilities of h, $P(h|e \& K) - P(h|K)$. However, the relevant results hold for a variety of such measures.[2]

Comparative resolutions commonly take K to justify a distribution for which (a) $P(\text{-Ba}|K)$ > $P(\text{Ra}|K)$, (b) $P(\text{Ra}|(x)(Rx \supset Bx) \& K) = P(\text{Ra}|K)$, and (c) $P(\text{-Ba}|(x)(Rx \supset Bx) \& K) = P(\text{-Ba}|K)$. These

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[2] For details, see Fitelson and Hawthorne (2010a). Fitelson (1999) provides a discussion of such measures more generally.

accounts typically entail that non-black non-ravens are indeed confirming, although Fitelson and Hawthorne's (2010a) recent account renders confirmation by black ravens greater than that afforded by non-black non-ravens without that seemingly gratuitous entailment. Quantitative resolutions depend on (c) and a strengthening of (a), $P(\text{-}Ba|K) >> P(Ra|K)$.[3]

However, we're not concerned with the details of such proposals, but with two objections that apply generally.

3. The Objection from Scientific Practice

The standard resolutions share two assumptions that starkly diverge from scientific practice: (i) evidence is gathered by simple random sampling, and (ii) the only salient evidence concerns whether or not something is a raven and whether or not it is black. For instance, given the background belief that arctic climes have caused white variants of many typically non-white species, it's quite plausible that *arctic* ravens, if such there be, are also white. Thus, a sensible researcher will specifically seek them out to check that they are indeed black. More generally, she will be particularly interested in raven populations associated with distinctive conditions that might not implausibly be relevant to color and have little interest in mundane ravens of a kind that have already been frequently and consistently observed to be black. Thus, reasonable practice is incompatible with (i). It's also incompatible with (ii). For a reasonable researcher a black arctic raven's confirmational significance should differ from that of a non-arctic black raven

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[3] There are variations, but these constraints or close relatives, are commonly used. Fitelson and Hawthorne (2010a) reviews both types of accounts. Vranas (2004) provides detailed discussion and criticism of quantitative accounts.

of a kind with which she is already familiar—the magnitude of incremental confirmation should crucially depend upon such considerations. However, since the random sampler only conditionalizes on data regarding whether or not something is a raven or black, she does not even recognize being an *arctic* raven as salient. Moreover, resolutions where one randomly samples from the set of ravens are equally culpable.[4] Let's call this the Objection from Scientific Practice.

What this objection plausibly shows is that our practice more closely resembles stratified random sampling. In stratified sampling we divide our population into mutually exclusive and jointly exhaustive, relevantly homogeneous subpopulations, and then randomly sample each as a means to evaluate our hypothesis regarding the entire population.[5] There's good reason for a researcher to favor such a methodology. A stratified sampler who has already observed a substantial number of consistently black British ravens will get ever diminishing incremental confirmation of the generalization from further observations of (sigh!) *yet another* black British raven and will sensibly direct her research to other strata for which she currently has less data. By contrast, the simple sampler makes no such discriminations and will obliviously continue to sample from sub-populations, that were she pursuing the stratified approach, she could for all practical purposes, ignore. So, the stratified sampler makes more efficient use of her primary epistemic resources, time and effort.

Notwithstanding the better fit with scientific practice, this doesn't really undermine the standard resolutions. Random sampling resolutions are generally understood as idealizations of

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[4] See, for instance, Suppes (1966), Gaifman (1979), and Horwich (1982).

[5] Stratified sampling is a commonly used statistical methodology often employed in assessing voter preference for instance. See Stuart (1962) for one exposition.

scientific practice, and the scientist's division of cases into kinds to be individually researched might be legitimately neglected in resolving the paradox. After all, if a researcher specifically randomly samples arctic environments, the kinds of priors invoked by the simple random sampling model should equally apply to that subpopulation, and hence, the confirmation afforded by black arctic ravens will be significantly greater than that afforded by non-black non-ravens drawn from the Arctic *for the same reasons*. The same will hold *mutatis mutandis* for random sampling of other strata. So, we have essentially the same comparative resolution of the paradox. The quantitative resolution should similarly apply. For a more precise treatment, see the appendix.[6]

4. The Potential Bias Objection

Simple random sampling, it seems, survives our first objection. There is, however, a further problem. For a sample to be random each member of the population must have an equal probability of being sampled. However, since we're interested in whether *all* ravens are black, long-deceased and yet-unborn ones are part of the relevant population and have zero probability of appearing in our sample. So, we can't *literally* randomly sample the relevant population.

If we've good reason to think the contemporary population is representative of, or suitably resembles, the total population past, present, and future, we can justify sampling the former as an unbiased sampling procedure for the latter. However, that's far from obvious. For

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[6] In discussing the explanatory virtue of consilience, Thagard (1978, 84) observes that a stratified approach should be favored for "all ravens are black" but does not develop such a model. To my knowledge, a stratified approach has not been pursued in the literature.

all we know, past and future raven populations may be subject to selectional factors that cause non-black ravens but are not represented in the contemporary population. Numerous causal mechanisms might yield such ravens. For instance, the plumage color in a bird population may change as a result of Batesian mimicry—the bird's coloration adapts to mimic the warning coloration of some local species that is, for instance, toxic to predators.[7] Indeed, more mundane cases are potentially problematic. As a matter of contingent fact, there might only be non-contemporary arctic ravens or desert ravens, and so on. So, at least in the initial stages of our research, we're not justified in holding sampling the contemporary population as an unbiased procedure for the total population. Moreover, it's not obvious we can ultimately justify this claim. Let's call this the Potential Bias Objection.

Could simple random sampling, in itself, address this? Might we justify holding our sample as unbiased merely by sampling the contemporary population? Such sampling will indeed cover arctic regions, for instance, along with the rest of the planet. However, since it only involves conditionalization on a sampled object's being a raven / non-raven and being black/non-black, it does not register whether our sample includes arctic ravens. So, it cannot tell us that the contemporary population is representative, and hence, that our sampling is indeed unbiased.

We could attempt to address this by simply taking note of the additional data, recognizing observed ravens as *arctic* ravens, *desert* ravens, and so forth. Here's the rub. The data that would justify the judgement of representativeness, on its own, suffices to well confirm "*being a raven or something necessarily associated with that*, causes blackness" by an application of something much like Mill's method of Agreement (1868, 428), which states:

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[7] For a case of Batesian mimicry in birds, see Londono, Garcia, and Martinez 2015.

"If two or more instances of the phenomenon under investigation have one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon."

To the extent that we can gather data that ravens that are the product of all of the factors that might reasonably be causally relevant to raven color are all black, we have data that justifies high confidence in the above causal claim. And since it entails "all ravens are black", the latter's probability must be at least as high. Thus, well confirming the causal claim by observation of a suitable variety of black ravens alone well confirms the generalization. So, meeting the rational precondition for random sampling by observing a suitable variety of ravens eliminates the need for such sampling, and the need to attend to non-ravens at all. We just go looking for arctic ravens, desert ravens, and so forth.

Moreover, if we can't meet that precondition, random sampling is not merely redundant, it's non-viable. If, as a matter of contingent fact, there are no contemporary arctic ravens to be observed, it's "game over" for the random sampler. Not for the causal confirmer, however. Unlike the random sampler, but like real scientists, she's not restricted to passive observation of a pre-existing population. She can simply introduce raven populations to the Arctic and see how they fare. The experiment might be short and brutal, if ravens are indeed ill-adapted for arctic survival. In that case, she confirms the causal claim by confirming that arctic ravens are nomologically impossible: arctic environments cannot cause non-black ravens, because they cannot sustain multi-generational raven populations. On the other hand, it might be quite long-

term, if raven populations can indeed survive long enough for color selection to potentially manifest. Either way, she can well confirm the causal claim, and hence, the associated generalization.

So, simple random sampling is either redundant or non-viable. Indeed, non-viability seems the likelier of the two. A broad range of factors might be causally relevant to raven color—arctic environments, desert environments, and a host of idiosyncratic environmental pressures—and we certainly should not *expect* data restricted to the contemporary population to evade bias in general.

Stratified random sampling fares no better. If there happen to be contemporary exemplars of ravens for each stratum, then since the strata are, by hypothesis, relevantly homogeneous, random sampling of individual strata is redundant; observation of a modest number of black arctic ravens should convince us that all arctic ravens are black. And if any strata lack contemporary exemplars, it's non-viable. So, confirmation of the causal generalization, and hence the entailed generalization, by the method of agreement should be the preferred research methodology.

5. The Causal Resolution

We need to make the causal resolution explicit. In general, what are the salient confirming evidence statements for "being a raven or something necessarily associated with that causes blackness"? We shall need evidence that ravens that are the product of the potentially relevant causal factor are black i.e., evidence of the form "Ra & Ba & Xa" where Xa states that a is a product

of the potentially relevant factor. If ravens of kind X are to be found, then we may acquire it by passive observation. If not, we acquire it by experimentation. If we try, and fail, to produce multigenerational populations of arctic ravens, say, the evidence statements will still concern *black ravens*, just dead ones.

This opens the door to a rather fast resolution of the paradox. We confirm the causal claim by *seeking* the salient data, by which we mean: we pursue that kind of data, adjust our probabilities by conditionalizing on it when we find it, and don't adjust our probabilities when we don't. A researcher who randomly samples from the raven population, as in the models noted in footnote 4, provides an example: she only conditionalizes on data furnished by ravens and ignores non-ravens.   The causal confirmer will seek data regarding arctic ravens by looking in places where such ravens might be caused i.e., the arctic. Whether she finds naturally occurring arctic ravens or is compelled to introduce them herself, and whether the introduced ravens survive or not, the salient confirming evidence concerns black ravens.[8] Since evidence of the form -Ra & -Ba, is not salient to the causal claim, she does not conditionalize on it, and hence, Hempel's paradoxical conclusion is false. [9]

The reason his argument is unsound is that the Nicod criterion is false: positive instances of (x)(-Bx $\supset$ -Rx) do not generally confirm it and its equivalents, because we ignore them. Moreover, the considerations that dictate the data that is confirming are not syntactic. They ultimately depend upon the character of the causal claim whose confirmation confirms the

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[8] If she finds non-black ravens, such evidence, is of course also salient, but disconfirming.

[9] We might express this by distinguishing the *logical* concept of confirmation from the *epistemic* concept: while "-Ra & -Ba" might confirm "all ravens are black" in the logical sense, it does not do so in the epistemic sense, because we ignore such evidence. See Fitelson and Hawthorne (2010b) for some discussion of this distinction in relation to the raven paradox. Throughout this paper, we use "confirms" in the epistemic sense.

generalization, the associated salient evidence, and our epistemic practice of seeking only salient evidence.

Can it be rational for an agent to *unapologetically* ignore the paradoxical evidence? The evidence that something is a non-black non-raven has no obvious bearing on the causal hypothesis, and that seems reason enough. The scientist or ornithologist already knows how to do elementary causal inference, and they're not required to justify discounting *prima facie* irrelevant data they might happen across. If that were a general requirement, we'd all be irrational.

Certainly, every unexamined object is, in some sense, a potential disconfirmer. So, other things being equal, conditionalizing on the data that the rock you just tripped over is a (non-black) non-raven, would be marginally confirming, since that data entails that it's not a disconfirmer.[10] But even an agent who recognizes this recondite point—one who, presumably, does not see Hempel's conclusion as paradoxical—does not have good reason to conditionalize. A policy of evaluating the import of such data would be a ludicrous expenditure of limited time, attention, and cognitive effort. Generally pursued, it would prevent us from gathering the data that could well confirm the generalization.

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[10] If an agent were to conditionalize, the confirmation afforded by e = -Ra & -Ba would be determined by the factor $P(e|h)/P(e) = P(e|h) / [(P(e|h)P(h) + P(e|-h)p(-h)] = 1 / [p(h) + \{p(e|-h)/p(e|h)\}p(-h)]$, with e confirming h if the denominator < 1. Since $p(h) + p(-h) = 1$, the denominator < 1 iff $p(e|-h)/p(e|h) < 1$. Plausibly, $P(-Ra \& -Ba|-h) < P(-Ra \& -Ba|h)$, since if h is true, there are no non-black ravens and so we should assign a probability of 1 that anything we stumble across is (-Ra & -Ba) v –(Ra & Ba) v (Ra & Ba), whereas if -h is true, we will presumably assign that a probability that is *marginally* less than 1. So, absent good reason to discriminate against stumbling across something that is -Ra & -Ba, some of that marginal "extra" probability should go to that kind of case. Thus, -Ra & -Ba would plausibly be weakly confirming if an agent were, implausibly, both competent and willing to make these rather involved estimates on the hoof.

Our favored resolution is one on which the agent ignores data of the form -Ra & -Ba, if she even tokens thoughts of such statements. However, we don't have to dig our heels in here. What is distinctive about our resolution is that we confirm the generalization by confirming an associated causal claim by the method of agreement, and that our data gathering proceeds by seeking the salient data, not by random sampling. Whether the paradoxical data we stumble across is universally ignored or occasionally conditionalized on is an incidental detail.

6. Formalizing the Resolution

We now give our resolution a Bayesian formalization. We've already covered the paradoxical evidence statements. However, we must also formally characterize the confirmation afforded by data furnished by black ravens of various kinds.

We shall call these kinds *causally individuated kinds of Potential Disconfirmers* (hereafter, "kinds of PDs"). They meet two conditions. First, they are kinds of cases individuated by factors that an agent holds might reasonably be causally relevant to the production of disconfirming cases. Thus, corresponding to each kind of PD there is a background *belief* that an associated factor could, reasonably, be a cause of disconfirming cases. Second, to count as a kind of PD in the intended sense—i.e., one that can facilitate well confirming by merely observing a modest number of exclusively black ravens of that kind—she must have *a commitment* that there are no additional factors that might cause non-black ravens of that kind: *arctic raven* doesn't count as a kind of PD unless she has a commitment that there are no selectional factors other than being an arctic raven that might cause non-black arctic ravens. Identifying that commitment as a

background belief, and hence assigning it probability 1, is too demanding. More reasonably, we demand only *a sufficiently high degree of confidence* that there are no such factors. If the agent lacks an appropriate confidence, then either her set of kinds should be more fine-grained—if she can specify the further factors—or she's too ignorant by her own lights to rationally well confirm "being an arctic raven or something necessarily associated with that causes black plumage". However, granted the required sufficiently high degree of confidence, observing a modest number of black arctic ravens should well confirm the blackness of arctic ravens.

Formally, let background beliefs, K, determine that K_i is a kind of case for which the agent has the requisite sufficiently high degree of confidence that there are no additional factors causally relevant to property B, and let C_i be the corresponding causal generalization "being an entity of kind K_i or something necessarily associated with that causes the entity to be B". We impose a constraint on our agent's priors as follows. Define $P^* = P(.|K)$. Then, if e_1, e_2,..., e_n consists of confirming data of the corresponding kind i.e., each is a statement of the form $K_i a$ & Ba, then $P^*(C_i| e_1$ & e_2 &...& $e_n)$ must be high / well confirmed, for even modest values of n.[11]

We can view this constraint as embodying a particularly simple application of the method of agreement, what mathematicians might refer to as a degenerate case. Application of the method demands a prior list of potential causes we are willing to countenance: since there is an arbitrarily large set of circumstances in which a finite list of objects agree, there can be no inference to "the circumstance in which alone all the instances agree" without such a list. Our agent's high degree of confidence that there are no additional factors causally relevant to property B, in effect, reduces her list of candidates to one entry—being a case of kind K_i or

[11] But not equal to 1. We don't wish to preclude future disconfirmation of the generalization by conditionalization.

something necessarily associated with that. Hence, observation of a sequence of cases of kind K_i that are all Bs, demands she well confirms that "being an entity of kind K_i or something necessarily associated with that causes the entity to be B", and the constraint on priors enforces that.

So, let K_i be *arctic ravens*, and C_i the corresponding causal generalization, "being an arctic raven or something necessarily associated with that causes black plumage". Conditionalising on data of form K_ia & Ba furnished by a modest number of arctic ravens, will render $P^*(C_i)$ well confirmed. Further, let R_i be the corresponding generalization, "all arctic ravens are black". Since C_i entails R_i, $P^*(R_i| e_1$ & e_2 &...& $e_n) \geq P^*(C_i| e_1$ & e_2 &...& $e_n)$. So, such priors dictate that R_i is well confirmed by the corresponding observations of black arctic ravens.

Of course, merely well confirming the individual generalizations associated with each kind need not well confirm, or even substantially confirm, "all ravens are black", which is equivalent to their conjunction. It's not merely coherent but often very reasonable to have a high probability for each of a set of conjuncts and a low probability for their conjunction. On the other hand, if she *sufficiently well confirms* that each kind could not cause a non-black raven, the rational agent effectively eliminates all of the explicit reasons she has for thinking that each particular environment might cause non-black ravens.[12] Given that she has eliminated all of these potential, more parochial, explanations of the color of raven plumage, she should certainly countenance the hypothesis "being a raven or something necessarily associated with that causes blackness". And at that point, with all due respect to inductive humility, if she has done due diligence in identifying factors that might have been causally relevant to disconfirmation, she

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[12] Let's note that we now have two distinct notions of "sufficiently well confirmed" in play. The first is "sufficiently well confirmed *that there are no further factors relevant to a given kind of case that we can reasonably impose our Millian constraint on priors*. The second is the one just introduced.

has reasonably met the conditions for the over-arching application of Mill's method of agreement, one that takes the evidence provided by all the observations of black ravens from her set of kinds as well confirming "being a raven or something necessarily associated with that causes black plumage". Thus, it will be inductively rational for her to hold "being a raven or something necessarily associated with that causes black plumage", and hence the entailed generalization, well confirmed.

We should acknowledge that the great variety of factors that might play a role in natural selection could render due diligence very demanding. However, that merely reflects the reality of scientific research: well confirming a general hypothesis typically demands a wealth of causal knowledge. Individuals may propose hypotheses, but their confirmation often involves the accumulation of such knowledge by large numbers of scientists over decades or even centuries. On the other hand, it need not be a practical impossibility. In any case, let's assume she can do due diligence and rationally well confirm the general causal claim. How should we model her epistemic evolution?

It might seem tempting to employ some further constraint on priors. However, that would be misguided. Our agent individuated arctic ravens as a kind of PD, precisely because she *believed* that some factor specifically associated with that kind might cause non-black plumage. In sufficiently well confirming that arctic ravens are black, she reasonably comes to reject that belief, and the same goes, *mutatis mutandis* for the beliefs associated with other kinds of PDs.[13]

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[13] We're not claiming there's some threshold of degree of belief that in all epistemic contexts suffices for belief. In this case, however, while allowing that such a "threshold" may be a vague interval and may permissibly vary amongst individuals, we do hold that a reasonable agent will have one. Given suitable evidence, retaining the belief that being an arctic raven provides a reason to suspect disconfirmation amounts to an irrational degree of inductive humility. From a practical point of view, she moves below her threshold when arctic ravens cease to be a reasonable locus of research for this hypothesis.

Thus, her transformation involves a revision of background beliefs, in which she eliminates these kinds of PDs. At that point *raven* is the narrowest kind of potentially disconfirming case that she individuates.

Should we account *raven* as a kind of PD, in our technical sense of that term? No, it's not that she has identified some specific factor associated with ravens that might be causally relevant to the production of non-black ravens. Quite the opposite: granted that she has done due diligence, she's highly confident there are none. At this point, should she *believe* that "being a raven or something necessarily associated with that causes black plumage" and the entailed generalization? Inductive humility likely counsels against that. However, the large number of black ravens that she has observed—the ones that furnished the data she used to eliminate her initial set of kinds of PDs—should well confirm "being a raven or something necessarily associated with that causes black plumage" and hence the generalization.

Let's flesh that out formally. Her old distribution had the background belief "being an arctic raven provides a reason to suspect disconfirmation", and so, assigned that proposition probability 1. Her new distribution drops that belief and indeed should assign the proposition a low probability. Hence, her new distribution cannot be obtained by conditionalization on the old.[14] Second, in addition to dropping the beliefs that individuated her initial set of kinds of PDs, her new set of background beliefs, K, includes the belief that she has done due diligence for the kind *raven* i.e., it assigns a high probability to there being no factors that might cause non-black ravens. Let's call this new distribution P**(.) = P(.|K). Since it assigns a high probability to there being no factors that might cause non-black ravens, it should also conform to our Millian

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[14] If p(A) =1, then p(A|B) = 1 for arbitrary B.

constraint on priors i.e., given that C is "being a raven or something necessarily associated with that causes black plumage", the constraint dictates that $P^{**}(C | e_1 \& e_2 \& ... \& e_n)$ has a high value, for even modest values of n, where the e_j statements are now simply of the form Ra & Ba. Since her numerous prior observations of black arctic ravens, black desert ravens and so forth, provided her with a large number of such data reports, P^{**} should assign each of those probability 1. So, there is a more than modest conjunction of such statements for which $P^{**}(e_1 \& e_2 \& ... \& e_n) = 1$. Hence, $P^{**}(C)$ must be high / well confirmed, and hence, also $P^{**}(R)$, where R = "all ravens are black". Her new distribution assigns both claims high probability. Thus, our demand that a rational agent's distribution respects our Millian constraint on priors disposes of the problem of old evidence, at least for this kind of case.

Let's be the first to acknowledge that we haven't specified a rule that uniquely determines the new distribution. There may not be such a rule. What we're happy to defend is that her rational evolution is constrained by the above considerations. That evolution, which we informally characterized as an application of something much like Mill's method of agreement, has been formally decomposed into a set of applications of a degenerate version of Mill's method of agreement, one associated with each kind of PD, a change in background beliefs corresponding to the elimination of the associated set of potentially relevant causal factors, and a final application of the degenerate method of agreement across the set of ravens.

7. Is the Causal Resolution Idiosyncratic to the Raven Generalization?

We argued that the sampled raven population may not be representative because it does not include past and future ravens. This might suggest that potential bias is not a concern for more typical scientific generalizations. However, this is not so.

Consider another of Hempel's examples, "all sodium salts burn yellow". The absence of historically contingent selection effects on the chemical character of sodium salts makes it reasonable to suppose that contemporary sodium salts are indeed representative of all. However, unlike ravens, they're not restricted to the planet Earth, or indeed our local group of galaxies. So, again, we cannot literally randomly sample the population of contemporary sodium salts or any population that includes them. And as before, to justify the judgment that our sample is representative we would have to ensure that it includes cases of each chemical kind of sodium salt whose specific character might give us reason to suspect disconfirmation, and indeed, any variations in crystal structure or other allotropic factors that might provide such reason. So, as before, meeting the rational precondition for random sampling demands we observe a suitable variety of sodium salts, which will suffice to well confirm the causal generalization and hence the entailed generalization, rendering random sampling redundant and giving us no reason to pay attention to non-sodium salts. And if, as will often be the case, finding samples of a given kind of sodium salt is inefficient or a practical impossibility, we do what scientists do on a daily basis, proceed by experimentation i.e., attempt to synthesize the relevant chemical. So, here again, the causal approach is preferable.

The broader applicability of our causal approach might prompt the suspicion that we've shown too much: that we can only randomly sample in situations where we can more readily confirm an associated causal claim. However, that doesn't follow. In cases where the total population or suitable strata of that population can literally be randomly sampled, as in an election poll, quality control at a widget factory, or a survey of household income, there need be no concerns about representativeness, and no attendant confirmation of an associated causal claim. Moreover, there are countless cases where literal random sampling is impossible, but potential bias is not a concern because background knowledge allows us to reasonably expect the total population to be relevantly similar to the sampled one. If we think that the distribution of factors causally relevant to the incidence of pancreatic cancer in the contemporary population will remain constant for future populations, we will happily use random sampling of the contemporary population as a guide to incidence in future populations and total populations across time.

As with the raven generalization, we should acknowledge the daunting prospect of well confirming that an almost limitless variety of kinds of sodium salts all burn yellow. If each allotrope of each chemical kind of sodium salt is a kind of PD, such a research project seems unmanageable. However, the consolidation of kinds manifested in our formal treatment of the raven applies here at multiple levels. Observation of a sufficient variety of yellow burning allotropes of sodium halides may cause us to consolidate allotropes of sodium chloride, sodium fluoride, and so forth, into one kind of PD, *sodium halide*, eliminating the need to observe each allotropic and chemical kind in that family; uniform yellow burning among a variety of other kinds of PDs that we have some initial reason to suspect may behave similarly can prompt other

consolidations, until ultimately we are left with one kind of case, *sodium salt*. An initially intractable research program evolves, through unification of kinds, into a manageable one.[15]

Whether we find or synthesise a given kind of sodium salt, the confirming evidence statement will be of the form Sa & Ya & Xa, where X specifies the particular factor that might be causally relevant to color of burning. If we fail to find a sodium salt by seeking, we will, as before, reasonably ignore the data. And if we ultimately determine the nomological impossibility of some putative kind of sodium salt, the evidence statements that confirm impossibility, and hence the causal generalization, will not be of form "-Sa & -Ya". Chemists may repeat an experiment many times and try a variety of different ways of synthesizing a compound or crystal structure, before succeeding. Correspondingly, judging that a kind is nomologically impossible will often depend upon multiple strands of evidence each with its idiosyncratic specification of the relevant conditions. If we fail to synthesise a theoretically postulated novel crystalline form of a sodium salt, the relevant evidence statement might be "The sample, a, obtained by crystallization with gradual cooling of the supersaturated solution was a pure sodium salt, but did not have the hypothesized crystal structure." Or if the product is not a sodium salt, the color with which it burns will be irrelevant. Hempel's paradoxical conclusion is false here too, because we are confirming the generalization by confirming causal claims.

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[15] This kind of consolidation might be related to a notion of dynamical consilience, although not one subsumed by the model in Thagard (1978).

8. Causal Approaches, Law Confirmation, and Inference to the Best Explanation

We should contrast our resolution with Peter Lipton's (2004, chapter 6) which, like ours, depends upon causal considerations. Unlike us, Lipton does not focus on confirmation of the generalization. Rather, he treats the raven hypothesis as a causal claim (2004, 97):

> …there is something in ravens, a gene perhaps, that makes them black. Moreover, the hypothesis implies that this cause, whatever it is, is part of the essence of ravens (or at least nomologically linked to their essence).

He then explicates its confirmation in terms of contrastive inference (chapter 5), a close relative of Mill's method of difference. Some contrapositive instances of a causal claim, those that have a similar causal history to its positive instances, will provide evidence for the claim. Thus, for Lipton, a non-yellow burning flame in which no sodium salt is present (relevant contrapositive) plus the yellow flame obtained upon introducing a sodium salt (contrasting positive) together support "all sodium salts burn with a yellow flame" construed as a causal generalization. However, typical contrapositives—a white shoe, say—are not relevantly similar to any positive instance and provide no support. Hence, Hempel's paradoxical conclusion is false.

Since our methodology closely resembles Mill's method of agreement whereas Lipton's is a method of difference, they fundamentally differ. Of course, our account primarily concerns confirming the generalization. Confirmation of the causal generalization is a means to that end, but that doesn't explain the difference. There's a mistake in Lipton's analysis. Given that we

antecedently know, as we do, that each sodium salt's constitution causes the color with which it burns, even contrapositive instances that are suitably similar to the positive ones are not directly relevant to whether *some aspect of its essence /something necessarily associated with being a sodium salt* causes yellow burning. They merely provide evidence against the claim that something necessarily associated with *some more inclusive kind than sodium salts* causes yellow burning. The same point applies *mutatis mutandis* to Lipton's construal of the raven hypothesis. The method of difference is just not relevant to the confirmation of Lipton's causal claims, given reasonable background beliefs.[16] Similarly, when we seek to confirm the generalization, whether or not some broader generalization subsumes it, is beside the point.

Even though our primary concern is the generalization, our account does support a suggestive connection with the confirmation of laws or broad causal generalizations. In *some* cases where, as with the raven generalization, (i) we cannot confidently exclude any nomologically possible kind of PD from actuality, and (ii) we confirm using our methodology, we will confirm a generalization by well confirming a closely related general law / causal generalization. This is a striking result. Moreover, given the immense causal variety of our universe, condition (i) will plausibly be realized for numerous scientifically interesting generalizations. And, as per our discussion of the sodium salt generalization, potential bias concerns will often favor the causal methodology over random sampling. Thus, while we're not joining Goodman (1955) in ascribing a sweepingly general role for law confirmation, it is

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[16] Notwithstanding his official resolution, Lipton gives his method of difference no role in confirming the (causal) raven hypothesis itself, because ornithological observations cannot determine whether ravens and some suitably similar but non-black non-ravens differ only in regard of a gene responsible for blackness. He concludes (2004, 99): "Contrastive inference avoids the raven paradox, but it does not account for the way we support the raven hypothesis itself." So, even he recognizes his method of difference as, at least, inessential to the confirmation of such claims.

reasonable to surmise that confirmation of numerous scientific generalizations goes hand in hand with law confirmation.

There's also a suggestive connection with Inference to the Best Explanation (IBE). We well confirm "all ravens are black" by well confirming the explanatory generalization "being a raven or something necessarily associated with that causes black plumage" and decisively disconfirming the causal relevance of other factors and associated, more parochial, explanations of ravens' colors: in confirming that arctic ravens are black, we confirm that being an *arctic* raven is irrelevant to color, and disconfirm associated explanatory hypotheses such as "Being an *arctic* raven or something necessarily associated with that causes it to be white", and similarly for other parochial explanations. Thus, we well confirm one explanatory hypothesis by eliminating multiple competitors from serious consideration.

Notoriously, IBE and Bayesianism are commonly held to make poor bedfellows. Given the *prima facie* reasonableness of the causal research strategy, and its apparent fit with quotidian scientific methodology, we might be tempted to say, "so much the worse for Bayesianism". However, we have pursued a Bayesian account. Let's assess the marriage.

As per section 6, beliefs and confidences about possible causal dependencies are taken to impose rationality constraints on our priors. These constraints, which effectively enforce particular applications of the degenerate version of Mill's method of agreement, are not suggestions. If you don't think that observations of a modest number of black arctic ravens should well confirm that all arctic ravens are black, *even though you are highly confident that no arctic ravens differ in respects that are causally relevant to color*, then you're as close to inductive skepticism as be damned. You're certainly not a competent scientist. So, we reject the

reconciliation of subjective Bayesianism and IBE advocated by Lipton and others, on which IBE is a mere heuristic that may be rationally rejected by an agent with conflicting priors.[17] Our view combines IBE and a species of objective Bayesianism. *Objective* here need not require that, given background beliefs, K, rationality demands conformity to some unique probability distribution P(.|K). However, distributions that violate the above synchronic constraints—and, as we can readily envision, others required to enforce other inductively rational species of causal / explanatory inferences—are not rationally acceptable.

So, what are the problems? Bas van Fraassen (1989, 166-9) has argued that an agent who implements IBE by following a rule that gives a probability boost to a hypothesis that best explain the data in addition to that provided by conditionalizing on the data, is irrational by virtue of being vulnerable to a diachronic Dutch book. One component of our Bayesian story is a constraint on priors, not a distinct updating rule. So, we have no cause for concern there. The other component, where we introduce a new distribution that demotes some background beliefs, might expose us to a diachronic Dutch book. But, what of it? Once we acknowledge—as surely we must—the need to sometimes discard beliefs, we must reject conditionalization as a general policy for updating. And as Douven (2013, 430) among others, observes, even if we accept Dutch book as specifying synchronic constraints on epistemic rationality, there's no inconsistency in holding conflicting views on the fairness of bets at different times. Indeed, such diachronic conflicts result from Bayesian conditionalization itself.

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[17] Indeed, as Jonathan Weisberg (2009) argues, IBE can't survive a marriage with subjective Bayesianism, since freedom to pick priors is a license to ignore explanatory considerations.

Roche and Sober's (2013) problem invokes the subjunctive, E, "If H were true and O were true, then H would explain O". They argue that, in general, $P(H|O \& E) = P(H|O)$ i.e., O screens-off E from H, and conclude that H's explanatoriness is evidentially irrelevant. From there, they argue against the reconciliation of IBE and Bayesianism. Now, our constraint on priors is not in the requisite form for their argument. It demands that $P^*(C_i | e_1 \& e_2 \& ... \& e_n)$ is high where each e_j is a statement of the form $K_j a \& Ba$. However, what C_i explains is that, *given that a_j is a raven, a_j is black*, for each e_j. Or we could say that it explains the agreement between the cases i.e., that all the observed ravens agree in being black. We're happy to retain our constraint in the form that directly matches the method of agreement, rather than pressing it into a form directly specifying the probability for C_i conditional on the evidence it explains, but of course that doesn't constitute a defense.

There have been several critical responses to R & S's argument. Let's just register our own, quite general, concern with screening-off as a criterion of evidential relevance. R & S motivate it using a *prima facie* reasonable Bayesian criterion for evidential relevance:

O is evidentially relevant to H if and only if $P(H|O) > P(H)$.

However, if P(.) is our current personal probability, and we have antecedently learned O—O is a background belief already codified in P(.)—then $P(H|O) = P(H)$, even if learning O rendered P(H) higher than *it would otherwise have been*. So, O can be evidentially relevant to H, even if the inequality does not hold. Certainly, once O has been codified in P(.) it has no *additional* relevance, but that's beside the point.

In R & S's case where we are considering the evidential relevance of one proposition to another conditional on taking a further proposition's relevance into account, the problem is particularly salient. For instance, suppose O, "the barometer reading is falling", is my evidence, for F, "the air pressure is falling", which in turn is my evidence for H, "there will be a storm". Further, suppose that O is my sole evidence for F i.e., I will believe F only if I believe O. In that case, $P(O|F) = 1$, and so, $P(H|F \& O) = P(H|F)$ i.e., F screens-off O from H. However, O is manifestly evidentially relevant to F, and hence, to H. Thus, screening-off is a poor criterion of evidential relevance.

So, even if C_i were screened off from the associated E by the data it explains, that would not show the evidential irrelevance of explanatoriness. In the barometer case, a counterfactual criterion provides a better rule of thumb for evidential relevance: if I hadn't learned O, then I wouldn't have learned, or at least raised my probability for, F or H. Similarly, if the causal relations specified by C_i *hadn't* been the best explanation of aspects of the data specified by e_1 & e_2 &...& e_n, inductive rationality, as embodied by Mill's method, *wouldn't have* demanded that it be strongly confirmed by that data. Whether or not some associated screening-off claim is true, C_i's explanatoriness is manifestly evidentially relevant.

9. Conclusion

We've provided good reason to reject both simple and stratified random sampling resolutions of the paradox and developed our own causally driven resolution. There are also intimations of a broader confirmation theory that can cover generalizations for which—as is often true in the

sciences—random sampling is susceptible to potential bias. On this confirmation theory, confirming a generalization may be intimately linked to confirming a closely related explanatory law or causal generalization and looks like a species of IBE. So, our resolution should not only be of interest to students of the paradox, but to confirmation theorists more generally and epistemologists with an interest in IBE.

Appendix: Stratified Random Sampling for (x)(Rx $\supset$ Bx)

We divide the population into homogeneous strata: arctic environments, desert environments, etc. We then confirm each corresponding generalization "all arctic ravens are black", "all desert ravens are black", and so forth, by randomly sampling each stratum. The data considered has either the form "Ra & Ba & S_ia" or "-Ra & -Ba & S_ia" where S_i = "is from stratum i". We shall show that both the comparative and quantitative resolutions follow given two weak and reasonable additional assumptions.

Comparative Resolution:

For each stratum, we make assumptions corresponding to the standard assumptions for simple random sampling (as per section 2):

(a') $P(\text{-Ba \& } S_i a \mid K) > P(Ra \text{ \& } S_i a \mid K)$

(b') $P(Ra \text{ \& } S_i a \mid (x)((Rx \text{ \& } S_i x) \supset Bx) \text{ \& } K) = P(Ra \text{ \& } S_i a \mid K)$

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(c') $P(-Ba \& S_ia \mid (x)((Rx \& S_ix) \supset Bx) \& K) = P(-Ba \& S_ia \mid K)$

These guarantee that, using the difference measure of confirmation, black ravens from each stratum confirm the corresponding generalization more than non-black non-ravens from that stratum. Writing $P^* = P(. \mid K)$, we have:

$P^*((x)((Rx \& S_ix) \supset Bx) \mid Ra \& Ba \& S_ia) - P^*((x)((Rx \& S_ix) \supset Bx)) > P^*((x)((Rx \& S_ix) \supset Bx) \mid -Ra \& -Ba \& S_ia) - P^*((x)((Rx \& S_ix) \supset Bx))$

Hence:

    (i) $P^*((x)((Rx \& S_ix) \supset Bx) \mid Ra \& Ba \& S_ia) > P^*((x)((Rx \& S_ix) \supset Bx) \mid -Ra \& -Ba \& S_ia)$

We shall use (i) in deriving the comparative claim for the raven generalization itself to which we now turn.

Given exhaustiveness, "All ravens are black" is equivalent to the conjunction of the generalizations corresponding to the strata:

$P^*((x)(Rx \supset Bx)) = P^*((x)((Rx \& S_1x) \supset Bx) \& \ldots \& (x)((Rx \& S_nx) \supset Bx))$, for the appropriate n.

For each i, we can write:

$P^*((x)(Rx \supset Bx)) = P^*((x)((Rx \& S_1x) \supset Bx) \&...\&(x)((Rx \& S_{i-1}x) \supset Bx) \&(x)((Rx \& S_{i+1}x) \supset Bx)\&...\&$

$(x)((Rx \& S_nx) \supset Bx) \mid (x)((Rx \& S_ix) \supset Bx). \quad P^* (x)((Rx \& S_ix) \supset Bx)))$.

Since, in general, $P(A \& B|C) = P(A| B \& C) . P(B|C)$ [18] we obtain:

$P^*((x)(Rx \supset Bx)|Ra \& Ba \& S_ia) = P^*((x)((Rx \& S_1x) \supset Bx) \&...\&(x)((Rx \& S_{i-1}x) \supset Bx) \&(x)((Rx \& S_{i+1}x)$

$\supset Bx)\&...\& (x)((Rx \& S_nx) \supset Bx) \mid (x)((Rx \& S_ix) \supset Bx) \& Ra \& Ba \& S_ia). P^*((x)((Rx \& S_ix) \supset Bx) \mid Ra$

$\& Ba \& S_ia)$.

And

$P^*((x)(Rx \supset Bx)|-Ra \& -Ba \& S_ia) = P^*((x)((Rx \& S_1x) \supset Bx) \&...\&(x)((Rx \& S_{i-1}x) \supset Bx) \&(x)((Rx \&$

$S_{i+1}x) \supset Bx)\&...\& (x)((Rx \& S_nx) \supset Bx) \mid (x)((Rx \& S_ix) \supset Bx) \& -Ra \& -Ba \& S_ia). P^*((x)((Rx \& S_ix) \supset$

$Bx) \mid -Ra \& -Ba \& S_ia)$.

Since we have shown:

(i) $P^*((x)((Rx \& S_ix) \supset Bx)| Ra \& Ba \& S_ia) > P^*((x)((Rx \& S_ix) \supset Bx)| -Ra \& -Ba \& S_ia)$, for each i.

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[18] As we can see by expanding the conditional probabilities on each side of the equation. The right-hand side is [P(A & B & C) / P(B & C)].[P(B & C) /P(C)] which is just P(A & B & C)) /P(C) i.e., P(A & B| C).

Using the difference measure, we can infer that black ravens drawn from any stratum confirm

"all ravens are black" more than non-black non-ravens drawn from that stratum, granted the

following two weak and reasonable assumptions:

(A1) Finding a black raven in stratum i does not decrease our probability for all ravens from other

strata are black conditional on the claim that all ravens from stratum i are black.

(A2) Finding a non-black non-raven in stratum i, does not increase our probability for all ravens

from other strata are black conditional on the claim that all ravens from stratum i are black.

Respectively:

(A1) $P^*((x)((Rx \& S_1x) \supset Bx) \& \ldots \& (x)((Rx \& S_{i-1}x) \supset Bx) \& (x)((Rx \& S_{i+1}x) \supset Bx) \& \ldots \& (x)((Rx \& S_nx)$

$\supset Bx) \mid (x)((Rx \& S_ix) \supset Bx) \& Ra \& Ba \& S_ia) \geq P^*((x)((Rx \& S_1x) \supset Bx) \& \ldots \& (x)((Rx \& S_{i-1}x) \supset Bx)$

$\& (x)((Rx \& S_{i+1}x) \supset Bx) \& \ldots \& (x)((Rx \& S_nx) \supset Bx) \mid (x)((Rx \& S_ix) \supset Bx))$

(A2) $P^*((x)((Rx \& S_1x) \supset Bx) \& \ldots \& (x)((Rx \& S_{i-1}x) \supset Bx) \& (x)((Rx \& S_{i+1}x) \supset Bx) \& \ldots \& (x)((Rx \& S_nx) \supset$

$Bx) \mid (x)((Rx \& S_ix) \supset Bx) \& -Ra \& -Ba \& S_ia) \leq P^*((x)((Rx \& S_1x) \supset Bx) \& \ldots \& (x)((Rx \& S_{i-1}x) \supset Bx)$

$\& (x)((Rx \& S_{i+1}x) \supset Bx) \& \ldots \& (x)((Rx \& S_nx) \supset Bx) \mid (x)((Rx \& S_ix) \supset Bx))$

Thus, we obtain the same comparative result regarding incremental confirmation of "all ravens are black" as we did for each of the stratified generalizations, essentially reproducing the resolution afforded by standard simple random sampling resolutions.

Quantitative Resolution:

If we are pursuing the quantitative approach, we use (c') and (a''), $P(-Ba \ \& \ S_ia \mid K) \gg P(Ra \ \& \ S_ia \mid K)]$. For ease of reading, let $P^{**} = P(.\mid K)$ for these background assumptions, K.

This will reproduce the quantitative result for the generalization corresponding to each stratum:

$P^{**}((x)((Rx \ \& \ S_ix) \supset Bx)\mid -Ra \ \& \ -Ba \ \& \ S_ia)$ is negligible.

As per our derivation of the comparative result:

$P^{**}((x)(Rx \supset Bx)\mid -Ra \ \& \ -Ba \ \& \ S_ia) = P^{**}((x)((Rx \ \& \ S_1x) \supset Bx) \ \&...\&(x)((Rx \ \& \ S_{i-1}x) \supset Bx) \ \&(x)((Rx \ \& \ S_{i+1}x) \supset Bx)\&...\& (x)((Rx \ \& \ S_nx) \supset Bx) \mid(x)((Rx \ \& \ S_ix) \supset Bx) \ \& \ -Ra \ \& \ -Ba \ \& \ S_ia). \ P^{**}(\ (x)((Rx \ \& \ S_ix) \supset Bx) \mid -Ra \ \& \ -Ba \ \& \ S_ia).$

And since the first term in the product must be less than 1, the confirmation afforded "all ravens are black" is (at least as) negligible.

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