The Problem of Induction and "Inference to the Best Explanation"

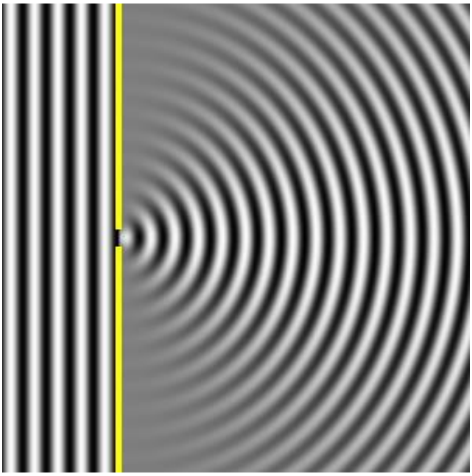
Richard Johns, updated March 2022

1. Huygens on scientific reasoning

Christian Huygens, in his *Treatise on Light* (1678), described a new "kind of demonstration" that is useful in science. This form of reasoning is now known as the "hypothetico-deductive method".

> One finds in this subject a kind of demonstration which does not carry with it so high a degree of certainty as that employed in geometry; and which differs distinctly from the method employed by geometers in that they prove their propositions by well-established and incontrovertible principles, while here *principles are tested by the inferences which are derivable from them.* The nature of the subject permits no other treatment. It is possible, however, in this way to establish a probability which is little short of certainty. This is the case when the consequences of the assumed principles are in perfect accord with the observed phenomena, and especially when these verifications are very numerous; but above all when one employs the hypothesis to predict new phenomena and finds his expectations realized.

One might wonder how the subject of optics is so special that it requires a new form of reasoning? Huygens' *Treatise* was actually the first book in the history of science that attempted a precise mathematical description of structures that are entirely invisible! Huygens proposed that light consists of vibrations within an invisible elastic material (which he called the 'ether') that fills all of space. Thus, while of course our eyes detect light, the true nature of light (the movements of the particles of ether, the wavelength of the vibrations, the shape of the wavefronts, etc.) are invisible. This is quite different from ordinary mechanics, which concerns visible particles (billiard balls, planets, etc.) Consider for example the standard diagram below, showing plane waves approaching a solid yellow barrier with a small hole in it, so that the hole becomes a source of spherical waves. These wavefronts are of course not actually visible, so that (as Huygens says) we have to judge this model by what it predicts concerning things that we *can* observe.



To see how this works, let H be some proposed scientific principle or law. To test H, we derive a prediction, or *observational consequence* from H, i.e. we show logically that if H is true then some observable event E should occur, when we do a certain experiment. Then of course we do an experiment to see if E *actually* occurs. Huygens notes that if E is observed to occur, then this does not prove with certainty that H is true. "I do not think that we know anything very certainly but all probably." But if H predicts many separate events that agree with observation, and especially if some of those observations were previously unknown, then Huygens says that H can be very probable, indeed almost certain. Huygens' argument therefore has the following structure:

1. H predicts phenomena E_1, E_2 and E_3
2. E_1, E_2 and E_3 are observed to occur

$\therefore$ H is probably true

Huygens doesn't say here that the 'principles' like H that can be used to predict phenomena also *explain* those phenomena, but in many cases this will be so. For example, in optics one can predict the shape, size and location of a shadow, in a given experiment, using the principle that light travels in straight lines. This principle also seems to *explain why* shadows form as they do. In general, scientific laws are taken to explain particular facts. For example, Newton's law of gravity explains (or helps to explain) why the earth orbits the sun along an ellipse.

2. Huygens' method is incomplete

Let us see how Huygens' form of argument handles a particular example. Let E_1, E_2 and E_3 specify the outcomes of three tosses of a coin, say *heads* in each case. Further, let H be the putative "law" that this particular coin *always* lands heads (i.e. the chance of heads is one). We see that premise 1 is true here – in fact the prediction is certain, having probability one, since according to H the coin is *bound* to lands each time. Also premise 2 is true. So is the conclusion true as well? Is H (the claim that the coin always lands heads) probably true? Can we perhaps even calculate its probability?

If you're like me, then you might be reluctant to say that H is probably true. For one thing, there are *many alternatives* to H that also predict E_1, E_2 and E_3, although not with certainty. For example the coin might actually be fair, with an equal chance of heads and tails (0.5), and just happened to have landed heads on these three tosses. The chance of this occurring, 1/8, or 0.125, is not particularly small. Also, if the coin *is* biased towards heads, it need not be as strong a bias as H claims. For example, if the chance of heads on each toss is 0.9 rather than 0.5, then the chance of 3 heads in 3 tosses is a very respectable 0.729. The hypothesis H is therefore just one possibility among many.

Another argument against H being probably true is the fact that it's hard to see how a coin could be made to land heads *every* time. The coin looks normal, let's suppose, with the Queen's head on one side and "tails" (no head) on the other. It seems rather unlikely that such a normal-looking coin *could* be sure to land heads on all tosses, doesn't it?

Notice how, in the previous paragraph, I said that H is "rather unlikely". This seems to be an assignment of probability (low probability in this case) but what kind of probability is this? It is certainly a kind of epistemic probability rather than physical probability or chance, as H is a proposition that is already either true or false, not a future event that may or may not occur. Notice also that this (low) probability has nothing to do with the data E_1, E_2 and E_3 (three heads), for these data actually *support* H, and thus cannot reduce its probability. E_1, E_2 and E_3 are *exactly* what we should expect to get, if H were true. So the improbability of H has nothing to do with the data, and is therefore said to be "prior to" the data.

Let's recap. We have some data, and a hypothesis that predicts the data perfectly. Yet we're reluctant to say that the hypothesis is probably true on this basis, for two reasons:

(1) The scheme takes no account of the *alternatives* to H that might exist, and

(2) The hypothesis H in question seems to have a low *prior* probability; it seems unlikely given our background information, or general knowledge of things.

It appears therefore that Huygens' argument scheme is incomplete, and we should investigate how to fill in these gaps.

3. Applying probabilities to induction

From what we have seen so far, scientific (or inductive) reasoning seems to involve at least two kinds of probability. The first kind measures the strength of an empirical prediction. A deductive (i.e.

certain) prediction has a probability of one, whereas we saw that the fair-coin hypothesis predicts three heads in three tosses somewhat weakly, with a probability of just 0.125. This degree-of-prediction kind of probability is technically known as the *likelihood of the evidence*, under some hypothesis, and is written P(E | H). The second kind of probability, the "prior" probability of the hypothesis itself, is written P(H).

Since inductive reasoning seems heavily dependent on probabilities, we should see what the probability calculus can tell us about it. The probability calculus is based on four axioms that are rational constraints on partial belief. A person whose degrees of belief fail to conform to the axioms of probability is thereby irrational or logically inconsistent. This can be argued for in a variety of ways. For example, one can derive some of the axioms from the principle that if two sets of gambles are "financially equivalent", in the sense of yielding exactly the same loss or gain in every possible outcome, then they are of equal value. So what does the probability calculus have to say about inductive reasoning?

In Huygens' argument form, the basic inference can be expressed as follows, using probability notation. For simplicity, I am including just one empirical statement E now, rather than three.

1. P(E | H) is high
2. E is observed to occur

$\therefore$ P(H | E) is high

Writing the argument in this fashion highlights an important detail, namely that the argument is essentially one of "turning the probabilities backwards", i.e. of going from P(E | H) to P(H | E). Is there any theorem of the probability calculus that enables us to turn probabilities around like this? Indeed there is – it is the famous theorem of Rev. Thomas Bayes. Bayes' theorem can be expressed in its simplest form as follows. (Note that the '*K*' in the subscripts refers to the background knowledge. See the probability handout on the iweb for more details about that.)

$$P_K(H \mid E) = \frac{P_K(E \mid H) P_K(H)}{P_K(E)}.$$

This form shows the basic fact that P(H | E) can be calculated from P(E | H), as Huygens requires, but it also shows that *other probabilities are logically needed*. The P(H) term, in the numerator, is the prior probability that we have already identified and seen the need for. The P(E) term in the denominator is harder to make sense of. It literally means the epistemic probability, in the state of knowledge *K*, of the evidence E itself. But how can E have a probability, apart from any hypothesis? Some hypotheses will predict E, and others won't, or to be more precise E will be predicted strongly by some hypotheses and weakly by others. But absent *any* hypothesis, surely there is no well-defined probability for E?

The term P(E) can be clarified by replacing with an equivalent sum, using the theorem of total probability, 2nd form (again see the "Probability Basics" handout). It shows that P(E) can be regarded as the *average* probability of E, under all possible hypotheses, where the average is weighted by the prior probabilities of these hypotheses.

$$P_K(E) = P_K(E \mid H_1)P_K(H_1) + P_K(E \mid H_2)P_K(H_2) + ... + P_K(E \mid H_n)P_K(H_n).$$

In this equation, H_1, H_2, … , H_n are all the possible hypotheses that might be used to explain E. They're assumed to be mutually inconsistent, so no more than one of them can be true. Obviously H, the hypothesis we're calculating P(H | E) for, is one of these hypotheses, so let's say it's H_1. In that case, you'll notice that the first term in the sum, i.e. $P_K(E \mid H_1)P_K(H_1)$, is identical to the top of the fraction in Bayes' theorem. The remaining terms in the expansion of P(E) are exactly similar terms, for all of the other possible hypotheses. Each term is the *prior* of some hypothesis, multiplied by the *likelihood* of E under that hypothesis.

Overall, therefore, the conclusion of an inductive argument, that H is probable given all our evidence, depends on just the priors and likelihoods of all possible hypotheses. (That's what the probability calculus says, anyway.) Now, Bayes' theorem might seem rather complicated, but we can simplify what it really means by using the fact that it consists of a lot of product terms like $P(E \mid H_i)P(H_i)$. If hypothesis H_i is to be a good or strong explanation of E, then $P(E|H_i)$ and $P(H_i)$ should both be as high as possible. The best hypotheses are those that

(i) seem plausible, given our background information, and

(ii) predict the evidence strongly.

Hence the product $P(E \mid H_i)P(H_i)$ seems like a good measure of the overall strength of H_i as an explanation of E, and so can be written $Strength(H_i)$. Bayes' theorem then simplifies to:

$$P_K(H_1 \mid E) = \frac{Strength(H_1)}{Strength(H_1) + Strength(H_2) + \ldots + Strength(H_n)}.$$

In other words, you can conclude that H is probably true (probability greater than 0.5) when its strength is greater than the strengths of all the alternatives added together. Thus, we see that Bayes's theorem accomplishes what we set out to do. It shows how to make Huygens' reasoning rigorous, by including some suitable consideration of alternative hypotheses and prior probabilities. Philosophers who use Bayes' theorem in this way are called *Bayesians*.

4. Bayesian inference and induction

Now let us see how this logical analysis applies to the problem of induction, and in particular to Hume's question of how inductive reasoning can be rationally justified. We are off to a good start, since Bayes' theorem itself is on a sound logical footing, being a theorem of the probability calculus. This means that it is a logical consequence of the probability axioms, and these in turn have been shown to be required for rational consistency. So, to show that an inductive conclusion is rationally justified, we only have to show that the input terms (the *Strength* terms) are rationally justified.

Each *Strength* term is a product of a likelihood with a prior, so let us consider these in turn. The likelihood terms are relatively unproblematic, since they are (more or less) logically derived from the hypothesis in question. Whether (or to what degree) H predicts E is basically just a logical relation between H and E, and such logical truths can be known *a priori*. It's actually not quite so straightforward as this, since in general, the value of P(E | H) will depend on the background knowledge *K*. That's why it's better to write the likelihood as $P_K(E \mid H)$, to make the dependence on *K* explicit. This role of the background knowledge in making predictions was noted especially by Pierre Duhem, and it forms the basis of what is known as the *Duhem Problem* (or sometimes the *Duhem-Quine Problem*).

[*Aside*: A good example of how predictions depend on background knowledge, in addition to the specific hypothesis being tested, is the problem of the stellar parallax for Copernicus (and Kepler and Galileo). The Copernican model of the universe made the earth a planet, orbiting the sun in a huge circle through the heavens. According to this model, there ought to have been an "annual stellar parallax", i.e. a shift in the apparent positions of stars, over the course of a year, due to the earth's own motion relative to the outer sphere of the fixed stars. Yet no such parallax was observed. Aristotle argued on this basis that the earth was stationary, and this seemed a very solid argument for almost the next 2,000 years. Copernicus on the other hand noted that this "prediction" of a stellar parallax, using his model, involved a background assumption, that the stellar sphere was comparable in size to the sphere of the earth's orbit. Copernicus calculated that if the stellar sphere was at least 7,000 times greater than the earth's orbit, then the annual stellar parallax would be "invisible to the eyes" – too small to detect.]

Anyhow, ignoring the Duhem problem for the time being, the likelihoods are logical in character, and hence knowable *a priori*, just as arithmetic is knowable. Now what about the priors?

In this reading I shall argue for the follow claims about priors:

(i) The priors cannot be justified in purely logical terms.

(ii) If rationalism is true then the priors can be justified, though using methods that are not purely logical.

(iii) If empiricism is true, then the priors cannot be justified at all.

First let us think back to the discussion of rationalist arguments in physics, concerning colliding particles, bodies in free fall, levers and so on. The rationalists (Descartes, Huygens, Leibniz, Stevin, Euler, etc.) were attempting to derive laws governing these situations *a priori*, from pure reasoning. For example, Huygens used the principle that a symmetrical cause must have a symmetrical effect, as well the relativistic principle that laws of physics must hold in uniformly-moving frames as well as in rest frames. If such rationalist arguments are completely successful, then the law thus derived (*a priori*) will have a prior probability of 1. Bayesians, however, do not *require* the prior probability of a successful scientific theory to equal 1. For one thing, scientific theories are never certain, even at the best of times. ("I do not think that we know anything very certainly but all probably", as Huygens said.) More importantly, the probability of the theory will be raised if it is supported by empirical evidence, i.e. by empirical statements that the theory predicts, and which are also observed to be true. Huygens' collision laws might only be plausible when first argued for rationally, but when they're found to be in perfect agreement with the data, in hundreds of different types of collision, we become confident that they're the sober truth.

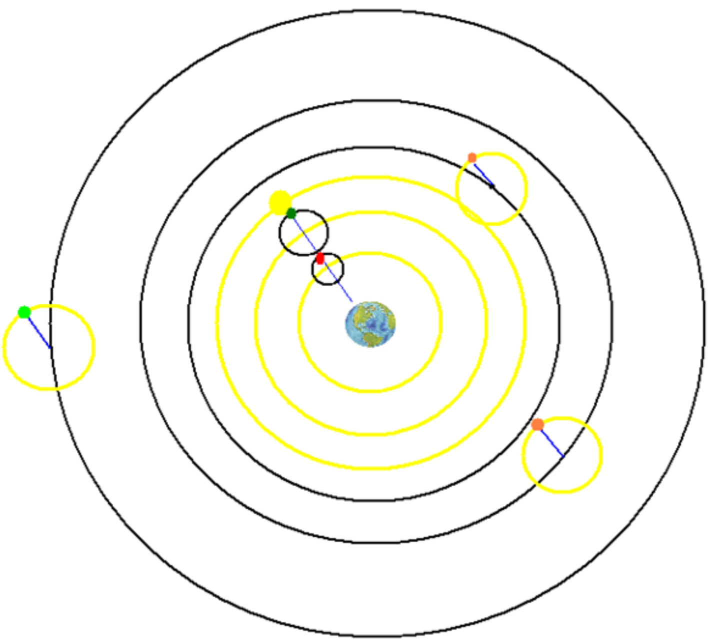
So a Bayesian rationalist looks to be in good shape with respect to scientific reasoning. There are many *a priori* arguments in the history of science that are strongly intuitive (not yours, Descartes) and which agreed with the data – that was only observed later in many cases. Moreover, the Bayesian method of reasoning requires only that *a priori* arguments render a hypothesis plausible, having a justified prior probability substantially different from zero, which seems a modest and realistic goal.

On the other hand, the rationalist arguments in question do not appear to be 'purely logical' in character, like those of arithmetic for example. Consider the case of equal bodies on a collision course at equal speed, so that the two bodies are mirror images of each other. It is a logical truth that *one cannot infer* an asymmetric state after the collision—symmetry absolutely *must* be conserved under logical consequence. Yet the actual occurrence of a symmetric outcome cannot be inferred with certainty. Such an inference requires all kinds of assumptions about the relation between conceptual things (ideas, propositions, probabilities, etc.) and concrete, physical things. For example, we might use the principle that causes logically entail their effects, but is that principle itself a logical truth? It doesn't seem to be. For one thing the notion of 'cause' is highly mysterious! This principle is nothing like acknowledged logical truths, such as 'a plane figure with 3 sides must have 3 angles', which is conceptually clear and undeniable.

The non-logical character of *a priori* arguments in physics is also illustrated by considering the question of whether space itself is 'isotropic', which means that it is the same in all directions. (Woven cloth, for example, is not isotropic, as the warp threads are often different from the weft, and the cloth's properties in diagonal directions will be different again.) Now that the matter has been raised, we see that the symmetry arguments in the collision problem all assume the isotropy of space. But surely the isotropy of space isn't a logical truth either? I assume that Leibniz would say that God could make a world in which space isn't isotropic, but instead has a grain as cloth does. And wouldn't he be correct? At best, we can infer isotropy only from the *wisdom* of God, his desire to make the best possible world, and so on. It is doubtful whether life could exist in a non-isotropic world, for example, especially if it is to live on a rotating planet!

5. Copernicus and the avoidance of 'ad hocery'

Another example that illustrates the non-logical nature of *a priori* judgements is Copernicus's main argument for a sun-centred universe. The starting point of this argument is a very curious feature of Ptolemy's earth-centred model. (Note that this model had been the scientific orthodoxy for about 1,300 years). Ptolemy's model, shown below, has all seven planets (the moon, Mercury, Venus, the sun, Mars, Jupiter and Saturn) orbiting the earth. (The moon isn't shown in the diagram.) Apart from the sun and moon, all the planets move along 'epicycles', i.e. they orbit in a small circle around a point that itself orbits the earth. That's perhaps curious enough, but the *really* odd feature is that many of these circular motions are perfectly synchronised with each other, as shown by the yellow circles in the diagram.



The sun orbits right around the celestial sphere once per year, of course – that's what defines a year. Now, since Mercury and Venus never stray far from the sun, Ptolemy's model fixes the centres of their epicycles so that they always lie on the earth-sun line, as shown. With the superior planets, those above the sun, you'll see that their epicycles are shown in yellow as well, since these orbits also take exactly one year, and the position of the planet on its epicycle exactly matches the sun's position on its orbit. Ptolemy was forced to construct the model this way by the observed fact that the superior planets undergo retrograde (backwards) motion when they're in opposition to the sun.

From a Ptolemaic perspective, there's no apparent reason why so many orbits should be synchronised to the solar orbit. This feature of the model is therefore said to be *ad hoc*, which means that it is there for the sole purpose (*ad hoc* = 'for the purpose') of making the model fit the empirical data. A feature of a theory is *ad hoc*, in other words, to the extent that it is empirically driven rather than determined rationally.

The Copernican model, as we all know, placed the sun at the centre of the universe, a made the earth a planet – the third rock from the sun, orbiting between Venus and Mars. How did this explain the observed motions of the other planets? Well, the fact that Venus and Mercury always appear close to the sun is explained rather simply by saying that they *are* actually close to the sun! They dart around the sun, not the earth, and on very short leashes.

Now, what about the odd phenomenon of 'retrograde' motion, where the superior planets sometimes stop their usual easterly motion in the heavens, go backwards (i.e. west) for a short while, and then continue east again? And why does this always happen when they're on the opposite side of the celestial sphere from the sun? According to Copernicus all the planets, including earth, orbit the sun in the same direction (eastward). But the earth is moving faster than the three higher planets, being closer to the sun, and will overtake each of them sometimes. Of course, during such overtaking events, the earth and (say) Mars are on the same side of the sun, so that from the earth's perspective Mars is opposite the sun. And as earth overtakes Mars, Mars appears to be going backwards, like a slow truck that you're passing on the highway.

In other words, the data that, for Ptolemy, require arbitrary, contingent features to be added to the model, are rationally necessary within that of Copernicus. Build any universe you like, consisting of planets orbiting a star. As long as you're on one of the planets in the middle, some of the other planets will always appear to be close to the sun, and the others will move retrograde during opposition.

It should be stressed that, *empirically* speaking, Copernicus's model was no more accurate than Ptolemy's. The big advantage was that it was much less *ad hoc*. Of course even Copernicus's model was somewhat *ad hoc*, as all theories are. No theory can be determined by reasoning alone! For example, the speeds of the planets, and their orbital diameters, were *ad hoc* for Copernicus, among many other aspects. Interestingly, later heliocentric models (e.g. due to Kepler, and then Newton) became progressively less *ad hoc*.

I said above that this argument for heliocentrism provides a good illustration of how rationalistic arguments are not purely logical. Recall that Copernicus was a Catholic priest, and like all theists he was a creationist in the broad sense, i.e. he thought that the universe was engineered by a super-intellect, a master craftsman. Suppose that such a craftsman decided to build a universe according to the Ptolemaic blueprint. Would this be logically impossible? Surely not – while getting the orbits all synchronised perfectly might pose technical challenges, even human clockmakers have overcome similar difficulties, so a divine watchmaker could certainly pull it off. So why did Copernicus think that God didn't in fact do that? In *De Revolutionibus*, Book 1, Chapter 10, Copernicus wrote that in making his model "We thus follow Nature, who producing nothing in vain or superfluous often prefers to endow one cause with many effects." In other words he believed that the creator, a wise engineer, would use the neatest, simplest, most economical mechanism available. As with Leibniz, Copernicus's *a priori* judgements were based on the wisdom of the creator, who never acts without a sufficient reason, rather than being based on logical necessity.

6. The thirst for rational explanation

This inductive argument of Copernicus highlights an important element of scientific reasoning that we have not yet discussed. Science may be roughly characterised as the search for *explanations* of natural phenomena, i.e. the search for *causes* of what we observe. In this vein, we might say that Copernicus exemplifies the key assumption of science that patterns and coincidences call for an explanation. Rather than accept patterns (such as the six-fold repetition of the solar orbit) as ultimate facts, we should attempt to derive them from a single, simple cause.

The method called "Inference to the best explanation" (IBE) is based on the prejudice (if you will) that there is a satisfying rational explanation for observed patterns. IBE is therefore biased, right from the start, against *ad hoc* explanations, claims that the pattern is due merely to chance, and so forth. In this way, some philosophers, notably David Armstrong and Laurence BonJour, claim that inductive inferences are rationally justified in that they use the method of IBE. When we see some patterns in nature, the best explanation of them is the proposed cause that predicts them with the fewest *ad hoc* assumptions.

It is clear enough that invoking IBE in this way, to justify inductive inference, assumes that there is some *a priori* knowledge. How could we know, except *a priori*, that simpler explanations are more likely to be true (more on this question later).

Copernicus's inductive inference discussed above aims to uncover the real and objective structure of the universe from facts about how it appears from our perspective on earth. Hume, on the other hand, considers inductive inferences that go from the past to the future. Let us therefore consider the problem of *predicting the future* from a rationalist Bayesian perspective.

The collision problem is a useful case to consider here, as collision laws can be used (together with observations of the present) to predict the future motions of particles. The point to note here is that the collision law itself is a statement about the *whole history* of the particles: past, present and future. Hence, if the collision law is probable (as a result of *a priori* arguments and supporting evidence) then it can be used to derive statements about the future that are also probable.

In this way a rationalist Bayesian can also show that the sun will probably rise tomorrow. From the wisdom of God we infer that physical space is symmetric under rotations (space is 'isotropic'), and then the conservation of angular momentum follows by a mathematical theorem proved by Emmy Noether[1]. This law says that spinning bodies must continue to revolve at the same rate, unless acted upon by an external force. Hence the earth will continue to rotate, and the sun will rise.

7. Empiricism and Inductive Inference

Finally, let us consider inductive reasoning from the perspective of Bayesian empiricism. I claimed in Section 4 that:

> (iii) If empiricism is true, then the priors cannot be justified at all.

If this is true, then *empiricism must lead to inductive scepticism*. Without rationally justified prior probabilities, there is no rational method to infer theories from observations.

Given that Bayesian empiricists cannot justify their prior probabilities by purely logical considerations, or by appeals to matters that could only be known *a priori* (such as the wisdom of God), *the priors must be determined by experience*. But such empirically-determined priors seem impossible in principle, as the following argument shows.

The argument involves what we might call *Goodman laws*,[2] which are say things like, "Newton's laws are followed up to Feb. 27, 2023, but after that <some other law> holds". The difficulty posed by such laws is that they are flawless, from an empirical perspective, until the fateful moment of switchover. They make all the right predictions until that point, so it seems that they can be

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[1] Noether's 1st theorem: Every differentiable symmetry of the action of a physical system with conservative forces has a corresponding conservation law.
[2] Named after Nelson Goodman, who introduced the idea of such non-uniform laws in order to discuss inductive inference.

dismissed as improbable only on *a priori* grounds (e.g., to paraphrase Einstein, "God would be making a big mistake" in creating such a law). Yet if we assign the same prior probabilities to uniform laws as to their Goodman alternatives, inductive inference is impossible.

Is this argument too quick, however? Perhaps we can argue against Goodman laws on the empirical grounds that we have never found such a law to hold, in all our experience? After all, astute readers may have noticed that, now and again, I've thrown in a little *empirical* argument for rationalist principles, as follows:

1. *A priori* arguments have often anticipated new data.
2. If *a priori* arguments were mere sophistry and illusion then this empirical success would amount to a miracle.

    ∴*A priori* arguments are not illusions

If this argument is successful, however, then perhaps these rationalistic principles no longer need *a priori* support, as the empirical support is now sufficient? Early scientists were perhaps merely lucky that their preference for uniform laws (i.e. non-Goodman laws) turned out well. But hundreds of years later, after seeing that simple, uniform laws have a great track record of success, our preference for such laws has a strong empirical grounding, which is reflected in the prior probabilities we assign.

Unfortunately, this response will not work. Hume himself considered this response in a slightly different form, and showed the flaw in it. Hume suggested an empirical argument that nature is uniform, based on the fact that it has been uniform in the past. Hume pointed out that the argument is circular, since to project past uniformity into the future *assumes* the very uniformity in question. From an empiricist's perspective, the future (from right now) is a place that no one has ever observed, and so we can have no empirical knowledge of it. To form beliefs about the future, based on observations about the past and present, would requires knowledge that the past and future are at least likely to be similar in certain respects. But how could experience provide such knowledge, since our experience is entirely of the past and present?

A Bayesian empiricist can offer one final argument, by appealing to a mathematical consequence of Bayes' theorem which is called the 'washing out of the priors'. Here's what the math says. Suppose there are two scientists, and two possible hypotheses, H_1 and H_2. One scientist initially favours H_1 fairly strongly, while the other finds H_2 much more plausible. Thus, their priors are very different from each other, but let's suppose that they both receive a piece of evidence that favours H_1. In that case, $P(H_1)$ will increase, and $P(H_2)$ will decrease, for *both* scientists, although their probabilities will still be different.

If the evidence stream continues to favour H_1, and both scientists update their probabilities according to Bayes' theorem, then over time their posterior probabilities will essentially converge, i.e. the difference becomes insignificant. The difference in their priors has now been "washed out" by the stream of data.

This result shows that, in favourable cases where there is plenty of data that all parties can agree on, there is a reduced sensitivity to prior probabilities. Does this show that inductive inference can occur, in the Bayesian framework, without rational prior probabilities?