LANGARA COLLEGE

Philosophy 1101 – Introduction to Philosophy

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The Argument from Induction for Rationalism

1. The logical gap between evidence and theory

In our discussion of rationalism vs. empiricism, we have so far looked at one rationalist (Descartes) and one empiricist (Locke). So it might seem that both sides have had a chance to present their best arguments, but that isn't the case. For one thing, Descartes wrote at a time when rationalism was assumed by everyone to be true – it was the only game in town. So Descartes didn't take much trouble to argue for rationalism, and certainly didn't defend rationalism from the arguments of Locke. To find arguments for rationalism, therefore, we must look later in the history of philosophy, to authors such as Leibniz and Kant.

The most important argument for rationalism is the argument from induction.[1] This argument starts from the belief that *scientific knowledge exists*. (It is perhaps no accident that both Leibniz and Kant were physicists.) And what is scientific knowledge like? What kinds of facts about the world has science revealed to us? In general we can say that *scientific knowledge goes far beyond direct observation*. Think about some of the claims made by well-known scientific theories, such as the belief that carbon is made of atoms with six protons per nucleus, or that the sub-continent of India used

[1] Induction, or inductive inference, is the kind of reasoning used in science, to infer theories from data.

to be joined to south-east Africa. These claims are of course *based* on observation, in the sense that there is observational evidence for them, but they are not themselves directly observable. We cannot see carbon atoms with our eyes, and of course no human was around at the time of the supercontinent Pangea.

In science, facts that are directly observed are known as (empirical) *data, observations*, or *evidence*. Claims that go beyond what is observed are called *theories*, or *hypotheses*. (I will treat 'theory' and 'hypothesis' as meaning the same thing, although often the word 'theory' is reserved for claims that are considered very likely to be true, and 'hypothesis' for claims that are more speculative.) In general terms, a hypothesis is a claim about objects and structures in the world that cause the events we observe, even though they themselves cannot be seen. In the language of appearance and reality that Plato used with his Cave allegory, empirical (data) statements describe appearances, while theories attempt to describe the reality that lies behind the appearances.

The difference between theory and data is illustrated by the case of gravity. Is gravity something that is observed (i.e. is gravity *data*?) or is gravity a *theory*? Well, it depends on what you mean. The fact that some apples dropped vertically from trees is something that we have observed. This is data. But when we ask *why* they fell, i.e. what *caused* the fall, we move into the realm of theory. For example, in 1687 Newton claimed that the earth exerts a 'force' on the apple that pulls it downwards. Previously, Aristotle said that the apple is made of 'earth', a dense material whose natural place is at the centre of the universe. Now the ball of earth we're standing on has already reached the centre, so the apple naturally gravitates down to the earth (no force is needed). Later, in 1916, Einstein gave a different theory of gravity called general

relativity. Note that none of these theories can be directly observed to be true or false. For example, we cannot observe Aristotelian natures, or Newton's forces. (In fact, Newton claimed that there is a gravitational force of attraction between *every pair* of bodies in the universe, e.g. between you and a particular elephant roaming West Africa right now. Has anyone observed that?) All we see are the motions of bodies – we don't see what *caused* the motion.

To summarise: scientific beliefs include *hypotheses* as well as *data*. The data are facts that we observe, whereas hypotheses concern the causes of the data, and these causes are not themselves observable. In other words, we can say that science is *trans-empirical*. ('Trans' here means 'beyond'.)

Rationalist philosophers see the trans-empirical nature of science as putting it in conflict with empiricism. They argue that, from a purely logical perspective, experience by itself cannot give information about matters that lie beyond experience. So, if the empiricists are right that all human knowledge comes from experience, then there cannot be any scientific knowledge of theories, but only of data. Laurence BonJour writes, for example:

> "... if the conclusions of the [scientific] inferences genuinely go beyond the content of direct experience, then it is impossible that those inferences could be entirely justified by appeal to that same experience. In this way, *a priori* justification may be seen to be essential if extremely severe forms of scepticism are to be avoided. ..." (Laurence BonJour, *In Defense of Pure Reason*, 1998, p. 3)

Note that rationalists like BonJour see *a priori* knowledge (i.e. innate knowledge, or knowledge prior to experience) as the solution here. Since scientific knowledge does exist, there must be

some human knowledge that doesn't come from experience. Scientific theories do not logically follow (even with probability) from empirical data alone, so additional premises are needed, and these must be *a priori*. Rationalists say that *a priori* knowledge bridges the logical gap between data and hypotheses, or between appearance and reality.

2. The 'many possible causes' problem

So far we've seen that rationalists say that science is trans-empirical, so that there is a logical gap between data and theory, or between appearance and reality. But are they right? After all, some philosophers (notably Francis Bacon, in his *Novum Organon*, 1620) have said the exact opposite: that good scientists set aside any prior beliefs they may have, and base their theories on the data alone. In this section we'll look at how scientific reasoning actually works, and see that Bacon was wrong. More than mere data is needed to support a particular theory.

Let's start with a very simple example of a scientific inference. A geologist is hiking in the mountains and sees a valley that has a U shape, as shown in the photo below.

The geologist concludes that thousands of years ago the valley was gouged out by a vast river of ice (i.e. a glacier). Of course the glacier in question cannot be seen, since it melted long

before the birth of civilization: it is a hypothesis. So the question arises why the geologists believes that a glacier was responsible? Is that the *only possible* cause of a valley like the one seen?

It doesn't take much creativity to realise that the glacier hypothesis isn't the only possible one. For example, perhaps some ancient civilization carved out the valley using primitive tools. Perhaps extra-terrestrials were responsible, or some unknown geological process that no longer exists on earth? Here, as always, we face what we might call the 'many possible causes problem'.[2] For any given set of data, there many possible causes of it – causes that, if they existed, could produce the effect in question. How do scientists choose between the possible causes? Not by mere personal choice, or feeling, surely? Is there a rule?

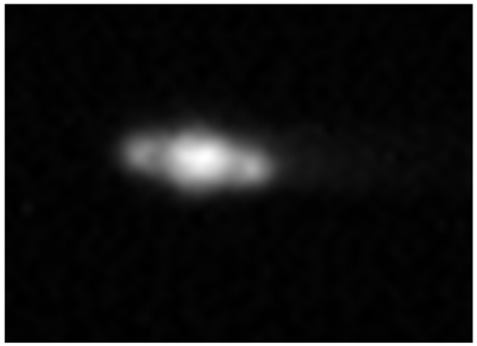
One important rule is to prefer a theory that predicts the data *strongly*, in the sense that if the theory is true, then the data should *definitely* exist, and not merely have some chance of existing. This rule indicates the glacier theory over the other two. This is because (for certain reasons) glaciers *always* create U-shaped valleys, whereas ancient civilisations and UFOs could (as far as we know) create valleys of any shape whatsoever.

The rule to prefer theories that make stronger predictions is an important and useful one, but it doesn't always tell us which theory to accept. What do scientists do when there are two or more theories that predict the observed evidence equally well?

Here's an example that illustrates this. Galileo was the first person to look at the stars and planets with a telescope, in around 1610. When he did, he saw bright discs of course, until he looked at

[2] Philosophers usually call this problem the 'underdetermination of theory by evidence'.

Saturn and was shocked to see what looked like handles on either side of the central disc. The image below shows roughly what Galileo saw.



"What the heck am I looking at?" Galileo no doubt wondered. In his notebooks, he did write that Saturn appeared to have 'ears', or 'handles'. But he rejected these options. In his announcement of the discovery to his financial backer he wrote:

> ". . . the star of Saturn is not a single star, but is a composite of three, which almost touch each other, never change or move relative to each other, and are arranged in a row along the zodiac, the middle one being three times larger than the lateral ones, and they are situated in this form: oOo." (Letter to the Duke of Tuscany, July 30, 1610.)

So he figured that the side bits, that looked like handles, were more likely to be smaller spheres flanking the main one. (The idea of a *ring* didn't occur to him.) For some reason (maybe an optical illusion?) they just look like handles.

Galileo's inference here raises the question: Why did Galileo prefer small flanking spheres to handles? The induction rule above, to prefer the hypothesis that predicts the data more strongly, would tell him to prefer handles. After all, while it's true that spheres *might* look like handles, under some conditions, it's surely more likely that handles will look like handles. From a purely

empirical standpoint, the handle theory seems the better one. If Saturn were shaped something like a soup tureen (see below) then surely that would predict the data better than Galileo's hypothesis?

I'm sure that any respectable scientist, faced with these two options, would prefer the three spheres to the giant soup tureen. But why is that, given that the soup tureen predicts the data better? Well, the soup tureen idea is just ridiculous! Or, to put it more scientifically, it is "not physically sensible". You have to recall that, for thousands of years (going back at least to Plato) all heavenly bodies were thought to be perfect spheres. If we're going to change that, then the change should be as modest as possible. In other words, we see that Galileo's theory about Saturn arose not *just* from the data, but *also* from an existing set of beliefs about the heavens.

In the next section I will argue that this pattern is generally the case in science, and that this is part of how the "many possible causes problem" is solved in practice. Competing hypotheses are evaluated on *two* grounds, not just one. In addition to preferring theories that predict the data more strongly, we also prefer theories that fit better with our background beliefs.

3. Science needs background knowledge

Philosopher of science Thomas Kuhn (developing some ideas of Michael Polanyi) coined the term *paradigm* to mean the framework of background beliefs that scientists use, and need to

use, to infer theories from data. In Kuhn's view, therefore, a scientific inference has more than just empirical data as its premises. The paradigm is also needed as a premise. A scientific (or inductive) inference therefore has this general pattern, according to Kuhn:

1. Empirical data
2. The paradigm

∴Hypothesis

In Kuhn's view, science cannot function without a paradigm. The paradigm operates like a lens, through which the world is interpreted and understood. The paradigm determines which theories are 'reasonable', or 'sensible', and which ones are 'crazy' or 'impossible'. So how can changes of paradigm, which Kuhn calls 'paradigm shifts', or 'scientific revolutions', ever be rational?

Paradigm shifts certainly have happened during the history of science. The paradigm that the earth is not a planet, but rooted at the centre of the universe, was replaced by the sun-centred (i.e. 'heliocentric') paradigm in the 17th century. In the 1960s, the 'fixist' paradigm of stationary continents was replaced by the 'mobilist' paradigm of continental drift. We usually think these changes were for the better – they constitute important scientific progress. But if Kuhn is right that all the rules that determine what is 'rational' or 'sensible' are relative to the paradigm, then changes of paradigm can never be objectively rational. They are always rational from the point of view of the new paradigm, but irrational for the old one. In fact, in Kuhn's book *The Structure of Scientific Revolutions*, he seems to argue on these grounds that scientific revolutions themselves are irrational and lawless, and cannot ever

be considered to be progress, objectively speaking. (Kuhn later denied that he meant to say this, however.)

If scientific revolutions (and the choice of an initial paradigm) are not to be irrational, then there must be some background knowledge that is common to all paradigms and so part of the scientific enterprise itself. Fortunately, such knowledge seems to exist.

Chief among these is Plato's belief that the natural world is rational, in the sense that it is structured according to logical and mathematical principles. Recall Plato's theory of Forms, which says that the real world, the reality that lies behind the appearances, is *intelligible* (i.e. understandable) to us even though it is not visible. It is intelligible because it is structured by the Forms, which are objective rational concepts. This means that the world is fundamentally *comprehensible* to human intellects – it makes sense to us. On fundamental matters, the world will tend to conform to our expectations.

Here are some examples. First up is Isaac Newton, who is generally known as an empiricist in his way of doing science. He seems to agree with Francis Bacon's motto, "you for your part must force yourself to lay aside your notions [preconceived ideas] and start to familiarise yourself with facts [data]". Newton is famous for saying, in the General Scholium:

> I have not as yet been able to discover the reason for these properties of gravity from phenomena, and I do not frame hypotheses. For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult

> qualities, or mechanical, have no place in experimental philosophy.

So Newton *says*, but of course he didn't (and couldn't) do this in practice. The "many possible causes problem" means that one can never *deduce* theories from data, and so experimental philosophy is *all about* framing hypotheses.

The problem that Newton is discussing here is that he has no *mechanism* for his famous gravitational force. For example, the earth stays in orbit around the sun due to the force of the sun's gravity, pulling on the earth (just as a tetherball is moved in a circle by the pull of the rope). But *how* does the sun act on the earth, given that there is apparently nothing physical connecting them, like a rope? Between the sun and the earth is just 93 million miles of empty space! Perhaps a possible answer is that there is no mechanism to be found here? Perhaps the gravitational force is just an intrinsic property of matter, so that material bodies act directly on each other, across vast reaches of empty space?

Newton doesn't seem to have any empirical data against this theory that gravity acts at a distance, across empty space, but he firmly rejects it anyway.

> "It is inconceivable that inanimate Matter should, without the Mediation of something else, which is not material, operate upon, and affect other matter without mutual Contact…That Gravity should be innate, inherent and essential to Matter, so that one body may act upon another at a distance thro' a Vacuum, without the Mediation of any thing else, by and through which their Action and Force may be conveyed from one to another, is to me so great an Absurdity that I believe no Man who has in philosophical

> Matters a competent Faculty of thinking can ever fall into it." (Letters to Bentley, 1692/3)

He rejects this theory as an absurdity, something that offends his intellect. It couldn't be true, as a competent thinker could never believe it. It's interesting to ask precisely *what* is wrong with the theory, for it certainly isn't logically impossible. One can imagine, for example, God making the world that way. But Newton, like almost all physicists, believes that nature should obey the principle of locality, which says that a direct cause should be right next to its effects, in both space and time. That just seems to be the right way for causation to work. (And Newton was right about gravity working that way, as it turns out.)

Another common scientific belief about the world is that it is economical, in the sense that nature is as simple as it can be, while still getting the job done. Newton again:

> "To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes."

This belief in economy was also a key premise in Copernicus's argument for his heliocentric universe. His model did not (at that stage) predict the data any more accurately than the old earth-centred model, but it was more economical. Copernicus summarised his argument by saying, in *De Revolutionibus*, Book 1, Chapter 10:

> "We thus follow Nature, who producing nothing in vain or superfluous often prefers to endow one cause with many effects."

This *a priori* preference for a simple, economical world was not only used by scientists in the 16th and 17th centuries. The same practice still guides science today. Earlier I mentioned Einstein's theory of gravity, known as 'general relativity', or GR for short. Einstein based this theory on very little data – basically just two things. One was that Newton's theory of gravity gives very nearly the right predictions in most situations. The other was the strange (and for Newton inexplicable) fact that bodies of different masses will fall with the same acceleration through a vacuum. No doubt there are thousands of theories that could account for these facts, so what guided Einstein toward GR?

Einstein was guided by his preference for symmetry and mathematical elegance – specifically a requirement on coordinate systems he called 'general covariance'. This requirement led him to a single possible theory, so he concluded that it must be correct. Three years after the theory was published, observations were made to see if some of the theory's predictions were correct. This is a nervous time for a scientist, as if a theory's predictions are false then it is likely that the theory itself is wrong. Fortunately for Einstein, the observations agreed with the prediction. A journalist at the time asked Einstein what it would have meant if his theory had been wrong, and he replied, "God would have missed a great opportunity". In other words, Einstein believed his theory described the *right* way for gravity to work, and so had God done it differently, *God* would have made a mistake, not Einstein.

In a similar way, the physicist Paul Dirac wrote about GR in 1980:

> One has a great confidence in the theory arising from its great beauty, quite independent of its detailed successes … One has

> an overpowering belief that its foundations must be correct quite independent of its agreement with observation.

Many more examples of scientists relying on *a priori* beliefs about nature could be discussed. Galileo, Huygens, Descartes, Leibniz, Stevin, Kant, Helmholtz, Maupertuis, Euler and Lagrange all developed their theories using symmetry principles, beliefs in locality, the principle of sufficient reason, and so on. Also Darwin, in the *Origin of Species*, makes rationalist arguments such as the following:

> …the simplicity of the view that each species was first produced within a single region captivates the mind. He who rejects it, rejects the vera causa of ordinary generation with subsequent migration, and calls in the agency of a miracle.

4. Leibniz on induction

Gottfried Leibniz (1646 – 1716) was an important mathematician and physicist as well as a philosopher. He discovered the calculus independently of Newton, and developed the familiar dy/dx notation that is used today. Leibniz also was first to solve problems in dynamics using notions similar to potential and kinetic energy (although he called them 'motive force' and 'living force'). One of Leibniz's works in philosophy was his *New Essays on Human Understanding*, a response to Locke's *Essay Concerning Human Understanding*.

In this book Leibniz argues that innate concepts and beliefs must be involved in the justification of laws of nature, since experience by itself would provide very little evidence for them. (Note that Leibniz only argues that laws of nature would be impossible to learn from experience *alone*, not that experience isn't needed in

coming to know them.) The reason for this is that laws of nature are (if true) both *universal* and *necessary*. A universal statement says that something holds in *all* cases – e.g. the statements "all ravens are black", "all US presidents have been men", and "all metals expand when heated", are universal. A necessary statement is one that *must* be the case; it isn't just true as things turned out, but would still have been true, no matter what happened. So the statement that all US presidents have been men is a true universal, but it isn't *necessarily* true since Clinton *might* have become president in 2017.

Universal statements are of course very 'strong' statements, in the sense that they contain a lot of information, and consequently they are very hard to prove. How can you show, for example, that all ravens are black? It seems that you would have to examine all the ravens there are, in the whole world, and check that they are all black. If even one raven escapes your attention, then some doubt must linger as to the truth of the universal claim. Necessary statements are even harder to prove. Even if all ravens are observed to be black in fact, how would you establish that they *must* be black? Looking at a particular raven tells you that it *is* black, but doesn't reveal whether or not it *might* have been another colour, perhaps orange, or pink.

In practice, the only necessities we are sure of are those in mathematics and logic, where a universal statement is *proved* to be true. For example, we know that the angles inside every triangle *must* add to 180 degrees, the area of every circle must be πr^2, etc. since these statements have been proved. Only a rational proof establishes the necessity (and not merely the truth) of the conclusion. Thus Leibniz writes:

> Although the senses are necessary for all our actual knowledge, they aren't sufficient to provide it all, because the senses never give us anything but instances, i.e. particular or singular truths. But however many instances confirm a general truth, they aren't enough to establish its universal necessity; for it needn't be the case that what has happened always will—let alone that it must—happen in the same way. (pp. 2-3 in the Bennett edition)

Leibniz notes that necessary statements can be known only through logical proof, or 'demonstration', and says that these proofs requires as premises 'inner principles' that are known innately. So Leibniz argues in the same general way as BonJour, whose work was mentioned in Section 1. Since scientific conclusions go far beyond the data that are used to support them, some additional (and therefore *a priori*) knowledge is needed in science.

It is important to see that, in using scientific laws to predict unobserved states of affairs (e.g. future events), we require that they hold necessarily (or at least probably), and are not merely true as things turned out. Consider for example the task of predicting the future tosses of a coin, based on some past outcomes that were observed. Of course we tend to use the past as a guide to the future, and so if a coin is observed to land heads many times in a row then we predict that it will land heads again on the next toss. But what is the logic of this inference? It is illogical to reason *directly* from the past outcomes to future ones. For example, if we know for sure that the outcomes are completely random, then the past outcomes have no bearing at all on future cases. Past outcomes become logically relevant only if we think that they reveal that the coin has an inherent bias toward heads. If we can say that the coin landed heads in the past *because* this was

necessitated (or rendered probable) by its nature, then we can take that information about its nature and use it to predict the future. But past facts, by themselves, tell us nothing about other cases.

Leibniz also describes a rudimentary kind of induction that even animals do, which is simply to notice a past pattern and instinctively expect it to continue. Animals do not try to explain that past pattern, or distinguish between patterns that occur of necessity and those that happen merely by chance, or due to temporary circumstances. Thus animal induction does not involve reasoning, and is rather unreliable.

4. David Hume: Empiricism strikes back!

At this point, the argument that science needs innate knowledge might look pretty strong, but we have yet to consider the best-known author on inductive inference – the arch-empiricist David Hume. We will look at Hume's work on inductive inference, which is in his *Enquiry Concerning Human Understanding*, Section 4 (Parts 1 and 2) and Section 5 Part 1.

In one important matter, Hume actually agrees with Leibniz and not his fellow empiricist Francis Bacon. Recall that Bacon thought that scientists could rationally infer theories from data alone, by a process of 'induction' (which Bacon often talked about, but he never said how it worked). Leibniz disagreed, saying that data by themselves never give enough information to infer a scientific theory, so that innate 'inner principles' are needed in addition to data. Hume (apart from the last bit) agreed with Leibniz here. Hume writes, for example:

> All that past experience can tell us, directly and for sure, concerns the behaviour of the particular objects we observed, at the particular time when we observed them. My experience directly and certainly informs me that that fire consumed coal then; but it's silent about the behaviour of the same fire a few minutes later, and about other fires at any time. The bread that I formerly ate nourished me; i.e. a body with such and such sensible qualities did at that time have such and such secret powers. But does it follow that other bread must also nourish me at other times, and that the same perceptible qualities must always be accompanied by the same secret powers? It doesn't seem to follow necessarily [i.e. logically].

Nevertheless, Hume recognizes that we do make inductive inferences. Even though there is a logical gap between particular data and general laws, we do somehow come to know some general laws. (For example, Hume mentions the law of conservation of momentum and describes it as "something we know purely from experience".) But now Hume seems to be contradicting himself. On the one hand he says that experience isn't logically sufficient to derive laws of nature, and yet he also says that such laws are known purely from experience! How can he say both things?

The key phrase here is 'logically sufficient'. Since experience is not *logically* sufficient to infer laws of nature (and since there is no *a priori* knowledge either) it follows that inductive inference isn't a *logical* process at all! As Hume puts it, "the conclusions we draw from that experience are not based on reasoning or on any process of the understanding." Leibniz's mistake, as Hume sees it, was to assume that inductive inference is a kind of valid reasoning, so that

the premises need sufficient information to allow the conclusion to be inferred. On the other hand, if induction isn't a process of reasoning, then it isn't constrained by logical rules.

But if inductive inference isn't any sort of logical reasoning, then how does it work? In Section 5, Hume says induction works by means of 'custom', or 'habit', which he describes as a kind of 'natural instinct'. We do many things by our innate human nature, Hume says, such as to love those who help us, and hate those who deliberately harm us. In a similar way, he says, we naturally and instinctively believe that observed patterns will continue into the future.

> Custom, then, is the great guide of human life. It alone is what makes our experience useful to us, and makes us expect future sequences of events to be like ones that have appeared in the past. Without the influence of custom, we would be entirely ignorant of every matter of fact beyond what is immediately present to the memory and senses. (*Enquiry*, Section 5, Part 1)

In other words, scientists form conclusions by the same exactly process of induction that Leibniz attributes to animals, or 'sheer empirics', who spot patterns in nature and instinctively (non-rationally) believe that the same pattern will continue.

This is Hume's challenge to the rationalists' argument from induction. There is actually no need for innate knowledge to enable inductive inference to be rationally justified. For inductive inferences are not rational at all – instead, they proceed by habit, or natural instinct. In the next section we will consider whether or not Hume's response is successful.

5. Is Hume an inductive sceptic?

The main criticism of Hume is that his view seems to threaten the status of scientific theories as *knowledge*. Indeed, Hume is often described as an 'inductive sceptic', which is someone who doubts the conclusions of all scientific inferences, as a matter of principle. To be an inductive sceptic is not merely to doubt some particular scientific theory, on the grounds that the evidence for it seems lacking. It is to doubt *all* scientific theories, no matter how much evidence supports them, on the grounds that theories can *never* be supported (even a little bit) by any amount of evidence. In other words, inductive scepticism is a pretty crazy position to take, but Hume is accused of doing just that.

If you read sections 4 and 5 of the *Enquiry* (which you should) then I think you'll agree that Hume isn't denying or doubting all scientific knowledge. He never explicitly says anything of the sort, and he also refers to one of Newton's laws as "something we *know* purely from experience" (my emphasis). His scepticism is limited to the claim that scientific theories are justified by *reasoning* of any sort. Nevertheless, there is often a big difference between the views that scholars explicitly *endorse* and the ones they're *committed to*, as scholars are often reluctant to accept the logical consequences of their own views. So, we need to ask whether Hume's view of induction commits him to inductive scepticism.

The standard view of science, known as 'scientific realism', says that the best-supported scientific theories count as knowledge, and are a superior and more certain form of knowledge than non-scientific beliefs, such as those of folklore, tradition, or superstition. (Thus, when science and folklore come into conflict, folklore must yield to science, not the other way around.) But what is knowledge? Traditionally, as we discussed earlier in the

course, knowledge is seen as *justified true belief*, and a justified belief is one for which we possess sufficient evidence. Now 'sufficient' here means *logically* sufficient: a belief is justified to the extent that it is logically supported by the total available evidence. So, if Hume is right about inductive inference being non-rational, then it seems that scientific conclusions aren't justified, and so aren't cases of knowledge.

We should not be too hasty here, however, for we have seen that some philosophers reject this traditional 'internalist' (i.e. JTB) account of knowledge. Externalists deny that knowledge requires justification in this sense. For example, some externalists (called 'reliabilists') say that knowledge requires only that the true belief is produced by a reliable cognitive process. 'Proper functionalists' say that the key requirement of knowledge is that our cognitive apparatus is working properly. Might such externalist views say that beliefs produced by habits and natural instincts are cases of knowledge?

It is clear that not *all* natural instincts and habits produce knowledge. Humans lived (quite successfully) for many thousands of years before science came along, and in that time formed many beliefs in the way that Hume describes. But these beliefs were folklore, not science. They believed that the world was flat, that the sun and moon are gods that fly through the sky before sinking into the underworld each evening, that famine, flood and disease are caused by angry deities, and so on. If scientific beliefs are produced by natural instincts, then they are very special instincts that humans use very rarely (even today).

Let's suppose then that there are such special, knowledge-producing innate instincts – we can call them 'scientific instincts', I suppose. Since there are always thousands of theories that are

logically compatible with the data, this view says that scientific instincts are needed to guide scientists toward the 'sensible' theories that are likely to be true. (For example, Galileo's scientific instincts made him reject the 'soup tureen' theory of Saturn.) Would recognising knowledge-producing 'scientific instincts' allow Hume to avoid inductive scepticism?

If he took this approach, Hume would face a couple of difficulties. First, the way that he describes custom or habit operating, in the *Enquiry*, is very different from how scientists work. As mentioned above, Hume's account of induction by habit is identical to Leibniz's description of animal induction, wherein animals notice a pattern in nature and instinctively expect it to continue. It is however impossible to fit actual examples of scientific inference (by Copernicus, Galileo, Newton, Darwin, Einstein, etc.) into this very crude mould. Scientists are often able, through understanding the rational structure of the world, to predict entirely new phenomena that no one has ever witnessed, which is a feat that no animal can match.

A second difficulty is that the innate 'scientific instincts' involved in this approach to induction are arguably a form of innate knowledge. Think back to Leibniz's response to Locke's rejection of innate knowledge. Leibniz said that innate human knowledge takes the form of "natural inclinations and dispositions, natural habits or potentialities" (*New Essays on Human Understanding*, 1705, Preface).[3] How are these natural inclinations and habits different from what Hume talks about? If 'scientific instincts'

[3] Blaise Pascal, in his Pensées (1669) takes a similar view to Leibniz, claiming that reason by itself is powerless to produce useful knowledge. In addition to reason (and experience) we require a faculty called 'the heart', or 'intuition', which provides direct knowledge of first principles.

guide us toward the truth, as scientific realism requires, then surely they are a form of knowledge, or perhaps proto-knowledge? To avoid giving up empiricism, Hume must emphatically deny that our innate natural habits have anything to do with knowledge. But in that case, it is hard to see how they can produce scientific knowledge. To the extent that Hume clings to empiricism, therefore, he seems to be committed to inductive scepticism, however unwilling he may be to adopt this view.

6. Hume's arguments against *a priori* physics

In the previous section it was argued that Hume's view of induction commits him to inductive scepticism (which, as BonJour says, is an extremely severe form of scepticism). Yet Hume provides arguments for his position that have not yet been addressed. Specifically, Hume argues that *a priori* reasoning cannot contribute anything at all to inductive inference. If reason tries to say anything at all about the laws of cause and effect, Hume says, then it finds itself completely powerless to do so. These are the arguments that lead Hume to his view that induction isn't rational, so a rationalist will have to respond to them. Here are the arguments, in summary.

Argument #1 Laws of nature cannot be proved through reasoning, since laws of nature aren't logically necessary. For example, the negation of a law of nature is perfectly conceivable, and not at all contradictory.

Argument #2 Reasoning is obviously unable to predict the results of complex experiments like feeding bread to tigers, or dropping a spark onto gunpowder.

Argument #3 Even with a simple experiment, such as two colliding billiard balls, Hume issues the challenge of showing how pure reasoning might reveal what will happen. He says that the challenge has not been met, and presumably never will be.

Concerning **Argument #1**, we must grant Hume's premise that laws of nature are not *logically* necessary. One can conceive of alternative laws, without any logical contradiction. But this argument is really attacking a straw person, since no prominent physicist (that I know of) has ever claimed that laws of nature are logically necessary. Consider the examples of *a priori* physics from Section 3, such as Newton's belief in causal locality, economy and simplicity. It is clear that none of these features is logically necessary, as we can consistently imagine a universe that is messy, complicated, wasteful, and contains action at a distance. But if natural laws are not logically necessary, then it what sense are they 'necessary', and how does this necessity help the mind to discover them?

The fundamental entity that logicians study is the relation of logical consequence, which is usually expressed by saying, "it follows that". Using logical consequence, one can define necessity by saying that P is necessary if and only if P follows from something. 'Something'? What is this 'something'? Well, that depends on what kind of necessity you're talking about. For the strongest kind of necessity, logical necessity, the 'something' is actually the empty proposition, one that gives no information at all. P is logically necessary if it can be logically derived from no starting information – from a 'logical vacuum', if you will.

Laws of nature are not logically necessary, so they cannot be inferred from a logical vacuum. What then do scientists like Newton and Einstein use to infer them? According to Leibniz, laws are inferred from 'inner principles' that are known innately, so that the laws are necessary *given those principles*. For example, according to Leibniz, one of these is the Principle of Sufficient Reason. A list of such principles, that I have compiled from various sources, is presented in Appendix 2 below.

Argument #2 is not a very serious argument, as Hume himself recognises. We must agree with Hume that *a priori* reasoning does not inform us about the functioning of complex mechanisms, such as a tiger's digestive system, or provide us with detailed knowledge of chemical reactions. The purported innate principles listed in Appendix 2 are obviously much more general and abstract than that.

Argument #3 is one occupies quite a lot of space in Part 1 of Section 4, but it doesn't have much substance. Here's a sample of what Hume says:

> If we are asked to say what the effects will be of some object, without consulting past experience of it, how can the mind go about doing this? It must invent or imagine some event as being the object's effect; and clearly this invention must be entirely arbitrary. The mind can't possibly find the effect in the supposed cause, however carefully we examine it, for the effect is totally different from the cause and therefore can never be discovered in it. Motion in the second billiard ball is a distinct event from motion in the first, and nothing in the first ball's motion even hints at motion in the second.

In this passage, Hume seems to simply assert his conclusion (that reason is powerless to discover the effect of a given cause) rather

than giving us any evidence for it. That's why, in my summary of the argument above, I expressed it in the form of a challenge – I wanted to be charitable.

The really bizarre thing about this argument is that the 'collision problem' that Hume refers to is one that was carefully studied by physicists (such as Descartes, Huygens, and his student Leibniz) in the previous century. Moreover, the problem was initially solved (by Huygens) using deduction from rational principles[4] rather than by experiment – although the results agreed with experiments. (In fact, since Leibniz learned physics from Huygens, it is likely that Huygens' work on the collision problem influenced his thinking about science generally.) Why does Hume say that reason has nothing to contribute to the collision problem, when reason has already solved it? I will not speculate about this, but it is clear that Hume's argument as it stands utterly fails. In view of Huygens' work on collision (and hundreds of similar arguments in the history of physics) the burden of proof rests on those, like Hume, who say that such arguments are impossible in principle. They must examine the arguments in detail, and show that they do not actually work as they appear to, since (for example) they rest entirely – not just partly – on empirical knowledge that has been smuggled in.

[4] See Huygens, "On the Motion of Bodies Resulting from Impact", completed in the 1650s but first published posthumously in 1703. The three general principles are: (i) Natural motion is rectilinear, at constant speed, (ii) Symmetry is conserved during collisions, and (iii) The laws of collision are the same in all uniformly-moving reference frames.

7. Conclusion

There is general agreement among philosophers that theories cannot be justified by means of empirical data alone, due to the 'many possible causes' problem.[5] Something, in addition to data, is needed to select one theory, or a small set of theories, from among all the possible ones. According to the rationalists like Leibniz this 'something extra' is *a priori* knowledge, whereas for Hume it is innate human instinct. But what do philosophers today say about this question?

Philosophers today are almost unanimous in their rejection of rationalism, and the use of *a priori* knowledge in science. At the same time there is little support for Hume's view of induction, as based only on non-rational instincts, since it seems to render scientific theories unjustified. However, no third account of inductive inference has gathered much support either. Consequently, there is no accepted account of what justifies scientific reasoning, and philosophers talk of the *problem* of induction. Induction is a 'problem', because we know that it *is* justified, but we cannot see how it *can* be justified.

There isn't space here to present the reasons why philosophers today have accepted empiricism. What I wish to highlight is the curious situation we are now in, where the argument for rationalism from induction is hardly even known to philosophers. For example, after summarising this argument BonJour writes:

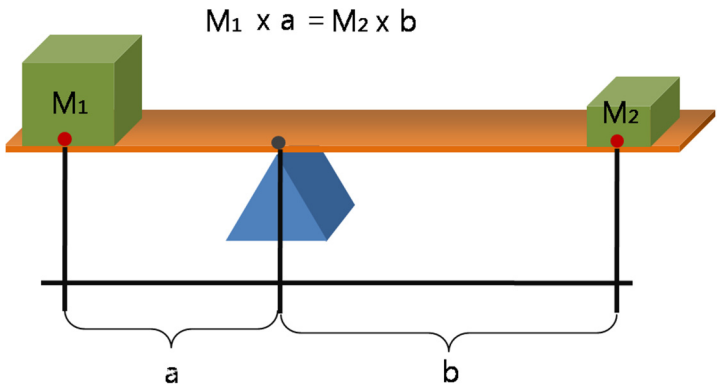
> The argument for this conclusion is extremely straightforward and obvious, so much so that it is very hard to understand the widespread failure to acknowledge it.

[5] Quine, for example, describes the data we receive as 'meager input', and the theories we produce as 'torrential output'. (See "Epistemology Naturalized".)

Also, when discussing the problem of induction, the role of empiricism (in creating the problem in the first place) is often not mentioned, and appealing to *a priori* knowledge is not even listed as one of the possible solutions. Nevertheless, it seems to me that the 'problem of induction' is really a misnomer, since the 'problem' was created by Hume after it had already been solved by Leibniz and others. We should really talk about the argument for *a priori* knowledge from induction.

Appendix 1: A Sample of *A Priori Physics*

The principle of the lever is a physical laws discovered by Archimedes (3rd century BC). The principle says that a heavy object can be lifted by a much lighter one, by putting one object at each end of a stiff beam, and placing the pivot closer to the heavier object, as shown below:

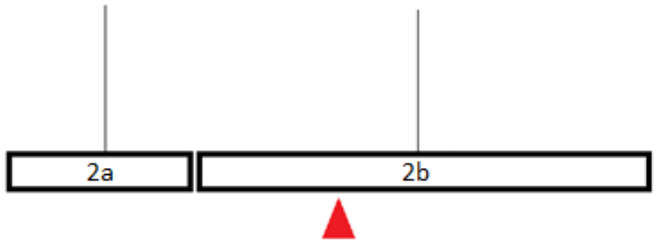


In the diagram shown, the beam is 'in equilibrium' (perfectly balanced) if the equation shown is true – i.e. the large mass, multiplied by its distance from the fulcrum, equals the smaller mass times its distance.

After discovering this 'magic' of force multiplication, Archimedes apparently said, in a rush of excitement, "Give me a place to stand, and I shall move the Earth with it".

In 1586 the Flemish mathematician Simon Stevin gave a rational argument for this principle of the lever, which (as far I can tell, as I couldn't locate the original text) went something like this:

Imagine two metal bars of uniform thickness and density, each suspended horizontally from a thread attached to its centre. As shown in the diagram below, their lengths are $2a$ and $2b$, and they're arranged so that their ends are almost touching.



Since each bar is symmetrical about its thread, with an equal length on either side, the two bars are initially at equilibrium. It is clear that if the two ends (that are almost touching) are now glued together, then the bars will remain at equilibrium. (Since they weren't moving anyway, how will fixing them together make a difference?) After the glue dries, a pivot (shown as a red triangle) is raised until it just touches the new bar, whose length is $2(a+b)$, at its centre.

It's clear, again by symmetry, that if the strings were cut simultaneously then the bar would remain at equilibrium, being perfectly balanced around its centre. Now the left-hand string is exerting an upward force proportional to $2a$, at a distance b from the fulcrum. (This is easily calculated, as half the length of the combined bar, $(a+b)$, minus a.) Also, the larger upward force, proportional to $2b$, from the right-hand string is applied at a smaller distance a from the fulcrum. Multiplying each force

by its distance from the fulcrum gives $2ab$ in each case, proving that such pairs of forces do not disturb the equilibrium of a lever.

Appendix 2: Common Rationalist Principles

The following principles all flow from the one 'master principle' that *objective reality has a rational structure, so that reality is comprehensible.*

1. The relation of cause and effect mirrors the relation of logical consequence:

- Effects can be logically inferred from their causes, i.e. from suitably complete descriptions of the total cause. (Or, at least, the probability of an effect is logically determined by the causes.)
- Every event has a cause. (Objects and events don't appear "from nowhere", spontaneously, all by themselves.)
- Exactly similar causes always yield exactly similar effects (or the same probabilities of effects)
- If a cause is symmetric, in a certain respect, then its effects (or the probabilities of effects) must also be symmetric, in the same respect.

2. *The Separability Principle.* The spatial and temporal parts of a system can be considered as individuals, and will behave independently of each other, unless they exert forces upon each other.)

3. *The Locality Principle*. Forces on a system can only be exerted by the immediate environment, not by distant objects, except indirectly via a chain of intermediaries.

4. *The Markov principle*. The past states of a system cannot act directly on future states, but only indirectly via the states at intermediate times.