Why Study the Philosophy of Science

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It's possible to give technical justifications (such as the essays here) for our studies, but let's instead start from the very beginning. Suppose, like Galileo, we stand near the top of the leaning tower of Pisa and drop simultaneously balls of differing weights of roughly the same size. What, before we let go, is the point of this experiment? It's intuitively obvious that the heavier will hit the ground first, so why do it in the first place? Indeed, it may be at least partly because it *was* (and usually still is) so obvious that few people actually checked. Even so, what does this first theory ("the heavier will land first") *mean*?

- The heavier piece will land before the lighter piece of the same size if both are dropped at the same time from the leaning tower of Pisa.
- The heavier piece will *always* land before the lighter piece of the same size if both are dropped at the same time from the leaning tower of Pisa.
- The heavier piece will *always* land before the lighter piece of the same size if both are dropped at the same time from *anywhere*.
- The heavier piece will *always* land before the lighter piece of the same size if both are dropped at the same time and *under the same conditions* from anywhere.
- The heavier piece will *always* land before the lighter piece of the same size if both are dropped at the same time and *under the same conditions* from anywhere and *at any time*.

Already we can see that the meaning of our theory is not immediately clear and that even these few alternatives are very different. Also, they tell us what we *expect* to happen if we actually tried the test (that is, they *predict*), but not *why* (that is, they don't *explain*). Here we have another question to ask of science before we go any further: what are we *aiming at*? That is, what goal do we have in mind, excluding the remark "just throw things at Hugo"?

- A theory that tells us what to expect and hence allows us to predict the consequences of our actions.
- A theory that tells us why one thing should happen instead of another.
- A theory that describes what *happened* but says nothing about what might happen in the future, or why.
- Some combination of the above, or something else.

Again, these aren't the same at all. The first reminds us of the practical person who says "I don't care how it works; I just want to know how to use it." The second seems to be looking deeper, but it of course depends on the context—after all, what do we want the theory for in the first place? The third manages to capture what happened in a description but tells us nothing further. It seems, at this stage, that a little of all would be a better prospect.

Galileo—to get back to the story—had different ideas. He proposed a different theory, according to which both would land at the same time. In fact, the Aristotelian thinking he was opposing was very complex indeed and to check his theory he decided to try the test that was supposed to give an obvious result: he climbed the tower and started dropping things. He found, of course, that they *did* land at the same time, so we have two theories, a test and some results. What can we say now?

- Galileo's theory is correct.
- The first theory is wrong.
- Both of the above.
- Galileo's theory is correct under certain conditions but may still be wrong under others.
- The first theory is wrong under certain conditions but may still be correct (or useful) under others.
- Galileo's theory is more likely to be correct than the other.

... and so on again. The conclusion we're entitled to make is not so obvious; perhaps Galileo cheated to prove his idea, meaning we'd be wrong to reject the first idea? Alternatively, perhaps he *was* right after all but still cheated in his experiment? What could we say then? It could also be that the test was flawed in some way, such that although Galileo was honest in his approach he in fact didn't show anything. Moreover, perhaps the theory is a good one for Pisa, but are we justified in claiming that it'll work anywhere? Here we are up against the *problem of induction* again.

Some people are aware that what Galileo *actually* found was a good deal more complicated. On some occasions the heavier object fell *slightly quicker*, striking the ground just before the lighter. At other times the lighter fell quicker, a result also obtained by Borro using lead and wood. Galileo was *not* inclined, however, to reject his theory because he thought there may be ways to account for the puzzling results that weren't quite as he expected. Indeed, a recent paper by Settle has managed to solve this mystery: it's actually impossible to release two objects from the hands at *exactly* the same time; instead, and without meaning to, an experimenter will invariably let the heavier one go first. Thus we see that the experiment when taken literally seems to be confusing without some notion of how to interpret it; we have to be very careful when asking what it all *means*. Galileo used *experiment* to test his theory, but when it didn't quite work out he nevertheless kept his theory because of some still more theoretical reasons. It's about time, then, that we looked at how science is supposed to proceed—the scientific method—and what philosophy has to say about it.

The Scientific Method

Why do we need to worry about what we mean by scientific method? It's true that your humble narrator is inclined to talk to himself on the matter, but what difference does that make? Well, suppose we look at the history of science, particularly those episodes that—with the benefit of hindsight—we consider to have contained good ideas or decisions, such as supposing that theories should be tested by experiment or that the earth isn't flat. Are there any features in common that could account for the success? If so, we could perhaps say something like "if you want to find a good theory, you should do x", or at least "... you shouldn't do y". This way, both good and bad moves made in the past can inform us today. On the face of it, this seems like a good idea, so let's see what suggestions for methods were offered historically.

Induction

It's often held that early scientists didn't approach their work with the same sophistication as we do today, but we've already seen that Galileo was both *doing* experiments and considering what the implications were for knowledge. In the early seventeenth century, on the other hand, Bacon was advocating for science an *inductive* method: the idea was to gather as much data as possible about the world and infer general theories therefrom, all the while taking care not to allow any assumptions or theories to influence the finding of information in the first place. We already know about some problems with the former; the latter we'll come to in more detail soon, but for the time being we can at least note that stopping ourselves from having *any* prior thoughts on what we expect to find is a tall order. Lakatos also pointed out the logical impossibility of deriving a general law from facts.

Hypothetico-deductive

Although he didn't call it so, this method was conceived by Newton late in the seventeenth century. The principle is as follows: first, we have an idea or suggested theory (the *hypothesis* part) that we come up with for some reason or other; then, we try to figure out what the consequences of it would be (the *deduction* part). The final stage is to test for these expectations and, by so doing, *verify* whether the theory is a good one or not. In this method it doesn't matter *where* the theory comes from, but only how well it's confirmed by experiment.

Unfortunately there's a significant problem here that becomes clear when we set the method out in logical form, as we saw in the earlier article. We want to say:

P1: If theory T is true, then we would expect to see a set of facts or results F;
P2: We see F;
C: Therefore, T is true.
This is a logical fallacy called *affirming the consequent*; the flaw is that although T *may* be true, F might instead be due to something else entirely. Look at this argument, for example:

P1: If rain dances are effective, we would expect to see rain after a dance;
P2: Rain is found to follow rain dances.;
C: Therefore, rain dances are effective.
In fact, it could be that the rain is caused by something other than the dancing (we would say that it is) and the dance leader has a fair idea of what signs to look for, only starting a dance at such times. If that's so, no amount of wiggling is likely to open the floodgates. Hence, the conclusion doesn't follow from the premises. The flaw in this argument is a difficulty for the hypothetico-deductive method.

Abduction

The generally overlooked philosopher C.S. Peirce wrote a good deal on this method that dates back to Aristotle. It's often called *inference to the best explanation* and reasons thus:

P1: Facts of the form B have been observed;
P2: The statement, "If A, then B" can explain B;
C: Therefore, A.
This is much the same as the previous method but the important distinction for Pierce was that A is the *best* explanation for B and therefore is the *probable* explanation. In our example of the rain dancing, then, it would seem that this isn't the *best* explanation of the rain, unless your dancing is quite something.

One problem with this theory is what we mean by the "best" explanation. Another is how it can cope with Hume's problem (it can't). A third is that making a statement like "A is the most probable explanation" has proved very difficult indeed and prompted a great deal of (highly technical) work in inductive justification.

According to the philosopher Hilary Putnam, however, it would be a *miracle* if a false hypothesis was nevertheless as successful as some of our scientific theories are and many people consider this a decisive objection. Of course, it isn't; one of several objections is that this scheme uses inference to the best explanation to justify inference to the best explanation—a decidedly unsatisfactory situation.

Falsification

Before he became the butt of philosophical jokes, Karl Popper claimed to have conceived the method of falsification that in fact—again—dates back to Aristotle. It took several forms (naïve, methodological and sophisticated) as it proved very difficult indeed to stick up for and was battered by a succession of brutal critiques. In its basic form it was an attempt to avoid the problem of induction by suggesting that science could instead proceed in a *deductive* fashion: scientists would propose theories and then try to falsify them (i.e. show them to be wrong). A theory that had stood the test of many such attempts is a good one but may still be wrong; a theory that is falsified is discarded. On the other hand, a theory that *cannot* be falsified at all is thereby not scientific.

An uncharitable way to look at Popper is to ask if—in common with many philosophers of science—he neglected to check how scientists were *actually* working, but in fact he was suggesting a new way in which science was to be understood. Unfortunately his ideas were taken to task because very often theories are proposed that don't specify what would falsify them (perhaps they're at an early stage), or else *are* falsified but still clung to by scientists (Einstein is the paradigmatic example of both)—and why not? It may be that an experiment discovers an *anomaly*, not a falsification; also, what if the experiment was in error somewhere, or its consequences misunderstood? What if the theory *was* wrong but by clinging to it scientists found a way around the difficulty and thereby made it stronger? None of the possibilities that take place throughout the history of science are accounted for by Popper's ideas and hence falsification was eventually treated with some hostility.

Who needs method?

As a result of these difficulties, some philosophers began to wonder if the prospect of a unique scientific method was such a good one after all. (Meanwhile, other philosophers worried that such thinking would swiftly send the world to hell in a hand basket.) Research found that in fact the many sciences were not unified at all and employed different methodologies (for example, compare particle and condensed matter physics, or molecular and organismic biology), very often even within the same field (compare Einstein or Dirac to Ehrenhaft). Nowadays this *disunity* of the scientific enterprise is gaining greater recognition and scientists and philosophers alike are less keen to hold forth on *the* scientific method. Moreover, studies in the *history* of science have shown that no methodological account seems to be able to take in all the twists and turns made by individuals.

The demarcation problem

Perhaps none of this is such a big deal but many people want to distinguish between science and non-science (or pseudo-science), usually to disparage the latter. In that case, we may not be too concerned at the lack of a distinct *method* but it would help if we could say "this *is* science" and, similarly, point out what isn't; sometimes we see "scientific" used as a word meaning "you should accept this", so if it's wrongly applied then people could be deceived. This became especially important to debates on funding (who gets the little money available to try all the ideas out there?) and education (how do we decide what goes in the curriculum as science?), the latter particularly with regard to creationism. Thus the *demarcation problem*: what factors characterise science?

It seemed that the ideal solution would state that science consists of *x*, *y* and *z* but creationism (or whatever) doesn't; therefore, creationism isn't science and shouldn't be on the curriculum. Some philosophers, though, warned either that this wasn't possible (Lakatos and Feyerabend in particular) or that it would backfire (Laudan). Due to the former, the latter is what happened: science was defined according to a few flawed criteria, leaving creationists the task of adapting their ideas to fulfil them and hence giving birth to creation *science*, so-called.

There have been several attempts to propose criterion that would solve the demarcation problem but they were either subject to severe critique (usually by Lakatos) or proved to have no uncontroversial analysis. This led Laudan to declare "the demise of the demarcation problem" and indeed many thinkers have decided to try for something less ambitious.

What can we say about science?

A description of science today is likely in some quarters to consist in a non-prescriptive *list*. For example, a scientific theory is one that has *some or all* of the following factors:

- It makes testable predictions.
- It is falsifiable.
- It predicts *new* facts.
- It unifies already existing ideas.
- It is consistent with what we already know.
- And so on...

However, the point of it being *non-prescriptive* is that even a theory that doesn't succeed in meeting one of the criteria may be a good or useful theory; we need only be a little cautious about those that fail to meet any or only a few.

Imre Lakatos used this understanding to develop his <>methodology of scientific research programmes[/i] that made an effort to take into account both the philosophical difficulties we've seen so far and the *history* of what happened to various ideas and the thinking proposed by scientists and philosophers over the years. He wanted to appreciate just *when* it would be appropriate to finally discard a theory or, conversely, whether we should be reluctant to ever do so. This was sparked, at least in part, by some historical cases.

For example, *Atomism* was proposed back in classical Greek times, in particular by Leucippus and Democritus. Since that time it was mooted, supported, refuted or rejected on several occasions until some two thousand years later it finally became a scientific theory, even though in the early part of the twentieth century it was still looked upon with some scorn. This being so, how can we be sure in eliminating a shaky theory that we won't be making a mistake in so doing? If the answer is that we *can't*, how can we instead minimise our chances of error or giving a similar idea every chance to impress us again?

Mill gave a thorough and quite beautiful argument in his *On Liberty* in favour of *methodological pluralism*, the notion of giving even apparently crazy theories a chance and using them to aid our work with others. It can be found here. Feyerabend showed with many examples how such pluralism is indispensable and that very often only another method can illuminate flaws or strengths in one we may support. In combination with the *tenacity* in the face of difficulties that is the lesson of the history of ideas, Lakatos thought he could take these into account with his two concepts of firstly a *negative heuristic*, being the core parts of a theory that we are reluctant to give up (this is what Kuhn looked at in his famous work *The Structure of Scientific Revolutions*), and secondly the *positive heuristic*, being the additional or auxiliary ideas that try to defend the theory against the anomalies and new information that may come up.

He suggested, then, that the distinguishing characteristic of a *progressive, scientific* research programme is that it makes new predictions or discovers new facts; a *degenerating, pseudo-scientific* research programme does not. Nevertheless, the latter case is no reason to reject a theory and we may ask just how new facts are to be found unless we employ a methodological pluralism in the first place and devote time and energy to alternative hypotheses. Lakatos was criticised on such grounds but his terminology has become widely-used today in both science and philosophy.

In the philosophy of science, then, we *have* seen progress; we've learned that a simplistic understanding of science won't suffice and that myriad factors need to be taken into account.

Some concepts in the philosophy of science

It may be useful in closing this article to look at some of the terms that come up often in discussion that are from or related to the philosophy of science. By doing so, we may begin to understand just what the hell your narrator is talking about in the majority of his blustering.

Ockham's Razor

With the exception of the *argumentum ad hominem*, parsimony is probably one of the least understood concepts around. Philosophers and scientists alike are very sceptical of its application and with good reason. The idea is usually given as "do not multiply entities unnecessarily", or that the theory with the least assumptions is to be preferred. Technical analyses of this suggestion can be found here, but the general point is that we are *very* rarely, if ever, in a situation where two theories have exactly the same consequences and content, except for one having more assumptions. A point made with much force by Bohr is that these consequences of the additional assumptions that we're supposed to reject are *never* clear before the fact; they have to be investigated to see if they tell us anything extra, either in the area being looked at or outside. Once they've been studied in any depth the issue of which theory to choose usually ends up being decided by other reasons, but even when we think we *have* considered everything it may still be that at a later date something further comes up. Thus it makes little sense, especially given the many examples from the history of science and ideas we could adduce, to reject a theory on the basis of parsimony unless it meets the very unlikely conditions for use.

The under-determination of theories

In the last article we looked at the example of finding white sheep and asking how reasonable it would be to adopt the theory that the next sheep found will be purple. Given that the already available evidence supports equally this hypothesis and an alternative that the sheep would be white, we couldn't say that one was any more reasonable than the other. This is generally called the *under-determination of theories*: the evidence we have to hand fails to pick out *one* theory when all are equally supported, as in this example. One way around this difficulty is to note that we're rarely faced with an infinity (or even just several) competing theories and when we are (as in this case) there are other reasons why we accept the one and not the other (for example, some information on the possible pigmentation of wool). Nevertheless, and in light of our comments on pluralism, perhaps we should view it as a failing if we *don't* have rival theories to choose between?

The theory-ladenness of terms

A much more difficult proposition is given by the idea that the appeal to evidence made by many people is all but empty. In its most extreme (and common) form, the conception is of theories that are tested against the facts that somehow sit in the world awaiting our comparison. Instead, these facts themselves depend on other theories in order to be understood, and they on further facts that are interpreted by other theories, and so on. Theories, therefore, go *all the way down*: there is no evidence free of any theory to appeal to. Another way of saying this is that there's no way to make an observation without relying on theory in some way.

What are the consequences of this strange situation? Well, early (naïve) versions of empiricism were killed because the experience to be referred to is infected by theory. Also, the comment "I don't see any evidence" is to be more carefully considered; if our observations rely on theories then Lubbock was at least partly correct that "what we see depends mostly on what we look for".

There are other more technical points that we won't consider here.

Verisimilitude

When Popper began to look at the possibility of comparing a theory to the truth, in the sense of "what there really is", he conceived the notion of *verisimilitude*: essentially, a measure of how close to or far from the actual truth a theory is. This would be especially useful if two (or more) theories have the same consequences or are both known to be incorrect because we may still care to know which is closer to the truth. Unfortunately this is a notoriously difficult idea to make satisfactory and, as is the sport, Popper came up against some very serious criticism from the likes of Lakatos and Oddie. In recent times Niiniluoto, Tuomela and others have offered more stringent versions but they require a good deal of mathematics to appreciate so we won't cover them here.

The problem of realism

The main concern in the philosophy of science today is the *problem of realism*, which deals with the interpretations of theories. Suppose, for example, that we have a theory that explains in a satisfactory way why an apple dropped outside Notre Dame in Paris falls to the ground, using some form of theory of gravity. Since we can't see or observe gravity with our own senses except by what we suppose to be its effects, should we say that gravity is *real* (i.e. that it really *exists*)? Later on the theory might become more successful, in which case we might be even more tempted to say that it is so *because* the gravity referred to really does exist, although we need to be wary of making the same logical flaw that we saw earlier of affirming the consequent. However, on many occasions in the past our theories have turned out to be wrong, replaced by others. Should we, then, not be a little more careful when declaring what exists and what doesn't?

This debate has grown into many threads and even *realism* is no longer easily defined. Niiniluoto gave six different areas we could be realists about, along with the type of questions we could ask:

- *Ontological:* Which entities are real? Is there a mind-independent world?
- *Semantical:* Is truth an objective language-world relation?
- *Epistemological:* Is knowledge about the world possible?
- *Axiological:* Is truth one of the aims of inquiry?
- *Methodological:* What are the best methods for pursuing knowledge?
- *Ethical:* Do moral values exist in reality?

Some of these areas we haven't yet covered, but we can see that the problem is wide-ranging and the questions important. If we answer "no" (or similar) to any, we call ourselves *anti-realists* with respect to them. Note that we could be realists on some issues but anti-realists on others: for example, we could believe that the world really does exist and can be known more or less, but also that there are no moral values other than those we create for ourselves. Presently the discussions are at something of an impasse on traditional fronts but new perspectives are being tried by many thinkers. Perhaps the most famous case of realist versus anti-realist interpretation is that of the Quantum Theory. At a later date much more will be said on this vibrant and impassioned area of study.

There is one significant problem in the philosophy of science to be avoided: poor philosophical ideas may hold back the practice of science. Unfortunately, rather than this being a concern for *philosophers* (although sometimes it has been), often the guilty parties are scientists who employ uncritical philosophical assumptions in their work without appreciating their basis and their *consequences*. This has very much been the case with the Quantum Theory, where philosophical decisions made deliberately or unthinkingly have influenced the course of subsequent work—some (including the scientists involved) saying negatively so. Thus it is that whatever our feelings on the philosophy of science, it cannot help but remain relevant and important.