Philosophy of Science

A Very Short Introduction

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This is a book intended to give a quick idea to an impatient reader what the subject is all about. Consequently one may not expect any depth in the treatment, nor much of a sophisticated approach, still one would expect that the presentation would be fair and not misleading, whether intentionally or haplessly. However, as to the latter, one has to conclude that this is unfortunately not the case.

The author makes from the outset a distinction between deduction and induction. In the words of the historian R.G.Collingwood, deduction compels conclusions, while induction allows them. In induction you go from a few special cases to the general, while in deduction you derive the special from the general. Thus in deduction you need some rather powerful premisses to get you going. In that case it is a top-down approach, rather conducive to an idealistic point of view. While in induction you try to infer powerful truths from rather scant material. It is thus a bottom-up approach, to use standard terminology, and one which is associated with a materialistic approach, although one should not make those statements in too categorical a spirit. Why does induction work? As Hume pointed out, it works precisely because we assume that the universe is uniform (referred to as the UN hypothesis in the booklet). And why do we believe that? Because experience bears it out? But as Hume pointed out this is circular reasoning, any attempt to validate the UN hypothesis presupposes that it is valid. Now, Hume believed in the UN hypothesis as much as any man, and in his daily life he no doubt made constant use of it. His point was that it was a matter of faith beyond proof, and hence that scientific truths could never have the same certainty as deductive truths in mathematics. This is nowadays, if you think about it, a rather uncontroversial opinion, to which most if not all scientists adhere, at least unthinkingly. (And in fact was it ever a contested opinion?) It is in the light of this fact that the demarcation criteria of Popper should be understood. Poppers basic point of departure is that we cannot prove things inductively, because it would involve an infinite process, on the other hand refutation only needs one counterexample. Now if you take this literally, it is of course nonsense, because any refutation is of course a proof of its negation, and thus there is a perfect symmetry between refutations and proofs. However, the symmetry is only apparent, and in fact intuitionistic mathematicians do not accept it, because of its reliance on 'the excluded middle'. It is true that a refutation does implicitly involve statements about an infinite number of cases, but then only on the assumption of some general principle. A counterexample in mathematics shows that an infinite number of putative proofs must all be incorrect, even without having to check a single one of them. Or a far more elementary example, finding your wife in your bedroom excludes

the possibility of an infinite number of alternate locations for her[1]. The general principle we are invoking in the first case is of course that false theorems cannot be proved, and in the second that your wife is unique and does not exist in identical copies spread all over (which cannot be empirically verified). Thus even in a deductively encodable discipline such as mathematics, one also needs to in addition to the truth of the basic axioms assume that those form a non-contradictory system. This cannot be proved, but has to be part of the tacit assumptions. One may think of this as an analogue of the UN hypothesis in a deductive setting. The great insight of Gdel is of course to make this precise, in a way that Humes remark is not amenable to. Thus even in mathematics uncertainty dwells.

Now Popper simply makes a precise way of saying that the findings of Science are all tentative, that future empirical discoveries may overturn present convictions. This is of course something that history has borne out. The shakier the foundations of a science, the quicker its findings are being overturned. The mathematical propositions, with few exceptions and none serious, of the old Greeks are as true today as then. Thus Popper is not so much concerned about falsifying theories as to 'proving' them. The objection stated as

> *The weakness of Popper's argument is obvious. For scientists are not only interested in showing that certain theories are false...her arch-rival's theory perhaps...But much more likely she is trying to convince people that her own theory is true. And in order to do that, she will have to resort to inductive reasoning of some sort. So Popper's attempt to show that science can get by without induction does not succeed.*

is very common[2], but reveals an egregious misunderstanding of Popper. For one thing any strong and striking conclusion we make in everyday life is invariably accompanied with a certain doubt, and we search for ways we may have been mistaken, not so much by examining our ways at arriving at our conclusions, as to what possible ramifications it would involve. If this is true, we ask ourselves, it would mean that this and that would also be the case, and that seems unlikely, so let us check it. A mathematician who is excited by a discovery he has made, does not go through the deductive steps over and over again, hoping not to find a mistake, but instead checks how this fits in with other facts of mathematical life. Conviction of a mathematical reasoning does not lie so much in checking the inevitability of the logical chains of deductions, but seeing how it beautifully meshes with other things, maybe even showing unexpected light on other phenomena in mathematics. It is only if a statement has gone through a forbidding gauntlet of tests that we may have confidence in accepting it. This is the essence of Popper's conception of inductive reasoning, meshing beautifully with Collingwoods characterization quoted above[3]. In particular he does not put down any quantitative criteria for when to accept an inductive argument. He in particular disdains probabilistic criteria, such as anything

[1] Assuming of course the infinity of the Universe

[2] In particular Martin Gardner (private communication) disposed of Popper in the same way. Gardner was of course an acolyte of Popper's 'arch rival' Carnap.

[3] One may even bring in the philosopher and logician C.S.Pierce, who referred to so called ablative reasoning, not going backwards but forwards and exploring ramifications.

with such and such level of certainty should be accepted[4]. To Popper inductive 'proof' means putting an assertion or more ambitiously a theory to exacting tests, and only when it has survived those texts are we allowed to accept it, but only provisionally. What those tests are depends on the circumstances and the imagination of the testers and are not laid out beforehand. Popper is not out to provide a manual for doing science, his ambition is ultimately meta-physical, in particular suggestions whether his own theory was testable threw him into a rage of frustration[5]. His demarcation criteria are simply not meant to be scientific. As Popper emphasizes, not being scientific is not necessarily 'bad' but could have important uses nevertheless, even in scientific pursuits. This is one aspect, and maybe the most important, in which Popper differs from the logical positivists, who never really could understand why he was distancing himself from them[6]. To bring the non-scientific nature of his criteria in focus, Popper claimed that science is a collective enterprise, an individual can never on his own produce scientific results (as little as you can play chess with yourself), simply because the individual imagination is not powerful enough, nor sufficiently motivated, to produce the necessary exacting array of tests. As he has pointed out, if you want to have your arguments and conclusions checked for weaknesses, you should not give it to a friend, but submit it to an adversary. Arch-enemies play a crucial role in the collective enterprise that constitutes science.

Now there are more subtle objections to be levied against Popper, such as the possibility, very much admitted by him, to engage in special pleading (referred to in passing by the author, by citing the example of the irregularities of the orbit of Uranus) as well as what constitute a test, and the problem of simple observations being so called theory-laden. I will postpone this discussion until I bring in Kuhn.

The next topic Okasha tackles is the apparent symmetry between prediction and explanation as proposed by Hempel, and the intuitive asymmetry between cause and effect. I am not familiar with the work of Hempel, so I cannot assess to what degree he has been misunderstood, but the case of Popper makes me less assured that he is being fairly presented. However, this is irrelevant to the discussion to follow. First, as the author rightly points out, there seems to be a strong difference between prediction and explanation. Now he does not really make that explicit, but as a reader one thinks of explanation as a subjective phenomenon. What may be convincing and constitute an explanation to one may not be to another. Will we ever be able to 'explain' consciousness? In what sense can it be 'explained'. If a single individual refuses to have his consciousness 'explained' to him, does that not mean that any putative explanation is failing? Prediction on the other hand seems far more objective. It is a matter of doing a test. Either the prediction turns out to be true or not. (This is of course a bit naive, but will suffice for the moment, until we will tackle it later on.) Taking the example of the author knowing the height of a flagpole and the height of the sun, as well as some basic trigonometry and its real-life implementation (sun-rays traveling in straight lines etc) we may easily predict the length of

[4] Of course in practical matters as to the effectiveness of various medicine and vaccinations, such more or less arbitrary criteria are inevitable. But then we are not dealing with Science proper.

[5] As reported by John Horgan in his 'The End of Science'

[6] As Carnap noted. The distance between me and Popper is small, but the distance between Popper and me is apparently huge.

its shadow. Or conversely knowing the length of the shadow we may predict the height of the flagpole. Now, why should we think of the first as an explanation and not the second? Why should we say that the height of the flagpole causes the shadow to be a certain length and not that the length of the shadow causes the flagpole to be of a certain height? It may be a bit counter-intuitive, but such objections are usually beside the point. Hume also famously denied causality, one thing happens and then another thing. What causes what? Thus should one not think of explanation and causes only to have a meaning within the context of a theory? Only within a theory it becomes meaningful to refer to them at all. In fact explanations (and logical arguments) proceed by chains, in which one link causes the other. Still even in the context of a theory, one should be wary of giving causation any really intrinsic meaning. In a mathematical theory it is often a matter of convention which facts one consider fundamental and those whom are merely derived. It is a sign of mathematical familiarity to be able to go back and forth at will, thus a mathematical theory becomes a 'space-filling web' with any two parts interconnected, and not just a long string of derivations. The latter may be inevitable when it comes to learning and 'encoding', but once that part is truly assimilated one takes a more 'holistic' point of view.

We now come to one of the central questions addressed by the author. What is the ontological status of a theory? He pits the so called realists against the anti-realists, by the latter he means not people of an idealistic bent, to whom the world is created by human consciousness (a somewhat absurd yet eminently rational point of view), but those who maintain a fundamental difference between the directly observable and the inferred. Popper is quite clear on this. He calls himself a realist, in the sense of believing that there is an independent world outside human thought. He refers to it was world 1. World 2 is the world of our private thoughts and sensations, and theories belong to world 3, the world of human constructions. Thus in a certain sense, a realist such as Popper subscribes to the view that theory is fiction. He would also claim that mathematics is fiction. However, even fiction has a kind of independent reality beyond its construction. Now the anti-realist position appears rather naive, at least in the way Okasha presents it. To any thinking adolescent the privacy of ones perceptive world of sight, sound and smell must be at some time become strikingly manifest[7]. In fact it is impossible to make a clear distinction between the observable and the unobservable. Even the most mundane of visual perception is a case of reconstruction. And as just indicated, it is impossible to make a direct correspondence between the worlds (In the terminology of Popper, world 2's) as conceived by different individuals (and maybe even of the same individual over time?). My quale of 'red' may be very different from your 'quale' (although it is not so clear in what sense one may even talk about difference, when it comes to non-comparable objects.) In fact the irony is that the more real and directly observable something is, the more private and non-exportable. What we share is not the 'real' world of kick-able stones and chairs (whether by Samuel Johnson or others), but the one of abstract ideas (belonging to World 3?). The 'feel' of two colors may be impossible to convey, but the more abstract fact that two colors are not equal, is easier to communicate. The worlds may be incommensurable as experienced data, but from a more abstract point of view they may be isomorphic. After all this is what makes communication between individuals possible. One may think of

[7] The logical conclusion of which is solipsism

this as nit-picking, after all one is usually able to make continuous deformations between two different phenomena, without compromising their difference[8]. But considering, as the author rightly notes, the amount of philosophical work this distinction is meant to carry, it is devastating to the theory. I am prepared to completely disavow the anti-realist stand as being pointless and superfluous. We make sense of the world through our theories. And this is true on the most basic level as well as the most hypothetical, the difference is simply one of familiarity. We have evolved through evolutionary history as well as unforgiving personal experience, to maintain very robust theories of the everyday world[9]. To summarize. Even if theories are fictions, does not mean that they are not 'true'. In fact they are true in the only sense they can be true, of having identifiable consequences. Fiction is imbued with an intrinsic logic, like any kind of invention, and have emerging features that we never consciously designed. One may dispute their ontological status, but in a sense the more abstract its features, the 'truer' it is. Can we ever observe an electron? We think of it as a particle, but that is nothing but a convenient metaphor to allow us to fix it in our minds. In many contexts it also behaves like a wave. Is the electron a particle or a wave? The question is not really well-posed. It is both and neither, it is an electron. In a sense the abstract representation of an electron as say as described by a system of equations, is in a sense more 'true' than a pictorial representation. This is of course something all physicists know in their bones. As the saying goes, everyone knows what an atom is, except the physicists[10].

Thomas Kuhn is in my opinion a greatly over-rated philosopher of science. He is mostly remembered for the notoriety that excessive misunderstanding of his ideas have lead to, namely the idea that what counts for scientific truth is simply fashion. This is the post-modernist stand-point, that truth is just a social convention. There are no Truths. This may seem as a very deep and radical position, but of course it is logically inconsistent, as the author is quick to point out[11]. Of course, as the author is at pains to emphasize

[8] The author takes the example of a full head of hair versus baldness, either extremes are well-defined, even if either can be transformed into the other through many graduations

[9] In our dealings with the physical world that engages us in daily life, there is no room for big unobserved elephants. That objects fall to the ground is so ingrained that we cannot think of it otherwise, and any violation of such expectations is immediately noticed. But if you are exploring a mathematical reality, we are far from 'seeing' all the elephants. There may be very prominent features of the system of which we are totally ignorant, and as a result of which we are literally fumbling in the dark. In mathematical reasoning even the most accomplished is liable to make elementary errors, equivalent to believing for a moment that objects are not always subjected to gravity.

[10] Atomic theory is of course of an old vintage. The Greeks posited the existence of indivisible matter. They also realized that such matter could not be visualized in any detail, because everything we can so visualize or describe must by necessity consist of even smaller pieces. This remind me of a trick question I was asked by our chemistry teacher in high-school. What is half of the charge of an electron? I made the required division and was rebutted. The charge of the electron is the smallest unit of charge, so half of it does not make sense, I was told. I was about to protest that the charge was measured as a multiple of some units, and by the same argument, those units did not make sense. This is of course somewhat beside the point, but footnotes provide ideal locations for authors to digress.

[11] The verbal play with such self-referential ideas has a long pedigree. Bolzano showed that there must

Kuhn was no post-modernist. But if so, what was he really that was so radical? I believe that there is not much point in making any fundamental distinction between a paradigm and a theory, even if one concedes that the first also includes all the tacit assumptions. Furthermore paradigm changes are not effected randomly, but because phenomena arise which cannot be properly accounted for in a theory, and a new one has to emerge. This is, as far as I can tell, a clear-cut case of Popperian falsification. It is true that in the course of so called normal science (Kuhns somewhat derisive terminology) the prevailing paradigm is considered paramount, and anything that seems to contradict it, is either considered a quirk and a simple mistake, or one engages in special pleading to get around it. One spectacular example of that is that Newtonian mechanics was so firmly entrenched in the 19th century that the irregularities of the orbit of Uranus were never seriously considered as an affront, but must have other explanations, such as the existence of an unknown planet. Special pleading if any, but of course the subsequent discovery of that missing planet was if anything a further triumph for Newton[12]. This is typical and also sound practice, and sticking to having faith in a paradigm usually pays a lot of dividends. In spite of the fact that theories are in principle under-determined by data, they are excessively hard to come up with. It is always easy to come up with theories tailored to 'explain' a certain data, but as Feynman has observed, any such concoction is bound to have additional consequences which are not observed. It is tempting to think of theories as having evolved, any stray mutation of any, is bound to make it collapse. The conservatism of science is in fact both a consequence and prerequisite for its robustness. It is also somewhat misleading to claim that paradigm shifts involve revolutionary changes. Einstein's relativity theory was of course a revolution of thought, yet as a change of paradigm it appears somewhat less than abrupt. There is no trace of any incommensurability. Einstein's relativity theory is a refinement of Newtonian mechanics, the latter is at least a pedagogical prerequisite. To learn Einstein does not mean to unlearn Newton, on the contrary. And to say that Einstein is more Aristotelean in character than Newton is a statement that means little, and definitely not that Einstein was a step backwards towards the Greek sage.

Now crucial to the whole discussion has been the notion of a test. An objective step of justification in the sense of the logical positivists. Now there are some serious problems with tests. There is no such thing as a test which does not depend on theory. In fact the most mundane of our every-day observations depend on some theory, even if we are not aware of it. Any test is based on some stated or unstated assumptions. Science is a social endeavor according to Popper and a testing of a theory is a social act. Testing involves convincing someone. The more ignorant and unsophisticated the one to be convinced is, the lower the level of a common ground has to be set. The common ground is something that has to be decided from case to case. The more incredulous we are of a phenomenon, the more basic the common ground. To judge the success of an atomic explosion the most

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be truths, because if not, the statement there are no truths is false. He was preceded by St-Augustin, who employed a somewhat more sophisticated argument to show in the same vein that there must be immortal truths, true for all times.

[12] In fact the discovery of Neptune was a bit of luck. Some wrong a priori assumptions about the missing planet were made, such as that it would satisfy Boodes law. But in real life it is not unusual that mistakes cancel each other out, provided that you make enough of them.

convincing way is actually to have one performed. Thus science becomes in principle a democratic enterprise in the sense that anyone has a right and the ability to question the achievements of science, through the possibility of it having ramifications on all levels[13]. In other fields outsiders are rebuffed from criticizing, on the grounds that you need to thoroughly have understood their theory before that. Science is democratic in the sense that human authority is not allowed to rule unopposed. Of course you can never in any kind of test and judgement dispose of authority per se. But Science only recognizes another kind of authority.

In addition to those basic question of philosophy of science, the author discusses some of more marginal interest (if potentially of an arresting kind nevertheless) dealing with philosophical questions pertaining to specific sciences, such as the controversy between Newton and Leibniz on absolute space, the way to classify organisms in biology, concluding with some topical remarks on the isue of religion and science. It is rather remarkable that classical religion has had a much easier task of accommodating modern Physics (after the Copernican revolution) than Darwinian Biology. In fact modern cosmology fits very well with the first book of Genesis, and one of the founders of the Big bang theory was a Catholic priest. There is no movement in the States that alternative physics should be taught as there is one for so called Creation Science.

In spite of what I find obvious flaws in the book, it has some strong points. It is written in an engaging style and makes some good points. The subject is narrowed to science, thus skirting the far more contentious issue (except in passing) about the 'scientific' status of much of Social Sciences. Pseudo-science in the realm of Science proper is easy to identify, but what about in other contexts? The issue is a bit confused because in Anglo-Saxon countries, science means natural science, while in Germanic countries, there is the notion of 'Wissenschaft' which has the ambition to describe all kinds of disciplines characterized by some kind of systematic inquiry. Any penetrating philosophical inquiry into scientific methods and activities needs to take this broader view.

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[13] It is a bit remarkable that Popper for all his emphasis on the democratic project, as illustrated by his volumes on the enemies of the Open Society, he does not make more explicit what I have just sketched. Maybe it is to be found somewhere. I have not read everything he has written, and even if I had it might be easy to overlook, or even more likely, having subconsciously absorbed it as my own, as you tend to do with things you thoroughly understand. This incidentally points to the moral that ideas belong to us all, and are not the possessions of individuals who make claims on them. Of course in the way our society is economically organized, the utopia of totally anonymous contributions is an impossibility.