Philosophy of Science

Edited by Anthony O'Hear

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Preface

This volume consists of the lectures given in the Royal Institute of Philosophy's annual lecture series in London in 2005-06. Both the topics and the contributors testify to the liveliness of the philosophy of science currently, and to the range of interests within the area. They also testify to the ways in which issues and positions in the philosophy of science bear on central questions of philosophy more generally, such as realism, natural kinds and epistemological progress.

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Anthony O'Hear

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Scientific Progress: Beyond Foundationalism and Coherentism[1]

HASOK CHANG

1. Scientific progress and epistemic justification

Scientific progress remains one of the most significant issues in the philosophy of science today. This is not only because of the intrinsic importance of the topic, but also because of its immense difficulty. In what sense exactly does science makes progress, and how is it that scientists are apparently able to achieve it better than people in other realms of human intellectual endeavour? Neither philosophers nor scientists themselves have been able to answer these questions to general satisfaction.

For conveying the importance of scientific progress as something we need to understand, the person I always look to is not a philosopher, but a historian of science: namely George Sarton, who has been called the 'father' of the academic discipline of history of science. In the preface to his monumental *Introduction to the History of Science*, Sarton announced:

> no history of civilization can be tolerably complete which does not give considerable space to the explanation of scientific progress. If we had any doubts about this, it would suffice to ask ourselves what constitutes the essential difference between our and earlier civilizations. Throughout the course of history, in every period, and in almost every country, we find a small number of saints, of great artists, of men of science. The saints of to-day are not necessarily more saintly than those of a thousand years ago; our artists are not necessarily greater than those of early Greece; they are more likely to be inferior; and of course, our men of science are not necessarily more intelligent than those of old; yet one thing is certain, their knowledge is at once more extensive and more accurate. *The acquisition and systematization of positive knowledge is the only human activity which is truly cumulative and progressive.* Our civilization is

[1] This is an updated and expanded version of the lecture given in the Royal Institute of Philosophy seminar series in London on 28 October 2005.

> essentially different from earlier ones, because our knowledge of the world and of ourselves is deeper, more precise, and more certain ...[2]

For Sarton, and many others in the general tradition of the Enlightenment, scientific progress was also inextricably linked with social and political progress. In his autobiography, the behaviourist psychologist B. F. Skinner recalled attending Sarton's lectures on the history of science at Harvard University in the early 1930s:

> Sarton was convinced that the world was also moving cumulatively in the direction of a better moral order, and to make his point he described the public torture of the demented [Robert-François] Damiens, whose misfortune it had been to attack the King of France [Louis XV, in 1757] with a pocket knife. Sarton gave us all the gruesome details: the platform in the public square, the huge crowd, the royal family with their children on a balcony, window space sold at a high premium, and then the stages of the torture, the man's hand plunged into a bowl of burning sulfur, time taken to allow him to recover when he fainted, and at last his limbs attached to four horses and torn from his body. "Gentlemen", Sarton said, "we have made progress".

To this recollection Skinner dryly added a brief parenthetical remark: 'Hitler would become Chancellor of Germany a year and a half later.'[3]

To the casual observer, it may seem that Sarton was correct about the inexorable progress of science, though he may have been too optimistic about the progress of society. The philosopher of science who has thought about the question carefully can agree with this view only with considerable difficulty and unease. One of the reasons for the philosopher's difficulty is that the question of scientific progress is intimately linked with the question of epistemic justification, which is itself a very tricky issue. Progress means that something is getting better. If scientific knowledge is getting better, it must mean that our current scientific beliefs are more justified than our previous beliefs (or at least that we have a larger number of beliefs, which are as justified as the beliefs we used to hold). Therefore, in order to *judge* progress we need to be

[2] G. Sarton, *Introduction to the History of Science*, I (Baltimore: Carnegie Institution of Washington, 1927), 3–4; emphasis original.

[3] B. F. Skinner, *The Shaping of a Behaviorist* (New York: Alfred A. Knopf, 1979), 57.

able to assess the degree to which beliefs are justified. In order to *make* progress, we should know how to create better-justified beliefs.

In the epistemology of justification, there are two main theories: foundationalism and coherentism. Both have had their influences in the debates on scientific progress. Historically speaking, foundationalism was the doctrine more readily enlisted in the enterprise of understanding and making scientific progress: if we can build a firm foundation of knowledge, then the rest of science is a straightforward process of building on that foundation. Particularly in the seventeenth century, there was a prevalent dream shared by a broad spectrum of European thinkers ranging from Descartes to Bacon: sweep away unreliable systems of knowledge inherited from the past, propped up by authority and tradition; find elements of knowledge that are free from doubt; and build up a new system on the basis of the secure elements. The rationalists attempted to find the correct first principles, and deduce the rest of knowledge from these principles. This enterprise is generally considered to have failed when it came to the development of empirical science, and I will not go into that history here.

The empiricist variety of the foundationalist project had a longer life in science: science would progress by continually accumulating secure facts gained through observations untainted by groundless assumptions, and building up theories by generalization from the facts. But this vision, too, has faded, most importantly through the recognition of the theory-ladenness of observation: since observations rely on theoretical assumptions, observations are only as reliable as the theories involved in making them. If we were to seek observations that do not embody theoretical interpretations, we are reduced to the level of 'sense-data': not anything like 'The photon emitted by a distant star has the wavelength of 8000 angstroms', not even 'I see a red star', but 'Red here now', whose desperate incorrigibility is entirely useless for building scientific knowledge.

Although the theory-ladenness of observation is usually associated with the post-positivist trend in philosophy of science starting around 1960 and most famously represented by the works of Hanson, Kuhn, Feyerabend and the like, the basic problem was already noted by the logical positivists. The Vienna Circle itself was split by the so-called protocol-sentence debate, in which the leading positivists differed sharply over the privileged epistemic status of observational statements. In the course of that debate, Otto Neurath made famous a metaphor against foundationalism, which we now remember as 'Neurath's boat':

> We are like sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry-dock and reconstruct it from the best components.[4]

Even Moritz Schlick, Neurath's main opponent in the debate, acknowledged that pure observation statements could not serve as a foundation on which to build knowledge, since 'they are already gone at the moment building begins.'[5] Kuhn and Feyerabend only took all these thoughts to their logical conclusion, when they concluded that there could not be any completely decisive and objective empirical testing of theories.

What we are left with after these assaults on foundationalism is coherentism, which is a doctrine that the only form of epistemic justification we can ultimately have is the coherence of a belief with all the other beliefs we hold. This is the idea that Neurath's boat metaphor expresses so well: our task in making scientific progress is to plug up the holes in the leaky boat; what that amounts to is making the various planks in the boat fit together well with each other. But that image is simply inadequate for capturing the nature of scientific progress, which is not just a matter of making our beliefs more consistent with each other. Nor is that simply an inadequacy of Neurath's metaphor. The problem is with the coherentist doctrine itself, which can only understand justification in terms of consistency. This problem of coherentism manifests itself most acutely as the threat of relativism: if we follow the standard coherentist doctrine, any internally consistent system of knowledge is equally justified. One of the greatest consequences of accepting such relativism is that we can no longer make sense of progress as we know it, because there will be no grounds for saying that we have done better by replacing one system of knowledge with another, as long as both are internally consistent. For some, this renunciation of the idea of progress is only an honest response to the poverty of foundationalism. But I think we can do better than that, because foundationalism and coherentism are not the only alternatives. As I will try to explain in the rest of this paper, I

[4] O. Neurath, 'Protocol Statements (1932/33)', *Philosophical Papers 1913–1946*, R. S. Cohen and M. Neurath (eds.) (Dordrecht: Reidel, 1983), 91–99, on 92.

[5] M. Schlick, 'On the Foundation of Knowledge [1934]', *Philosophical Papers*, II (1925–1936), H. L. Mulder and B. F. B. van de Velde-Schlick (eds.) (Dordrecht: Reidel, 1979), 370–387, on 382.

think we can best make sense of scientific progress by transcending the false dichotomy and dilemma between foundationalism and coherentism.

2. The problem, and the solution, in brief

Before I get on to the details, I will give a more concise and definitive statement of the problem, and sketch very briefly the solution I want to offer. The root of the problem is that foundationalism is inadequate as an account of how scientific knowledge is justified. I am using a standard definition of foundationalism here: 'According to foundationalists, epistemic justification has a hierarchical structure. Some beliefs are self-justifying and as such constitute one's evidence base. Others are justified only if they are appropriately supported by these basic beliefs.'[6] The problem is that we have failed to find enough self-justifying propositions that are useful in building scientific knowledge. As most of the apparently self-evident propositions are shown to be really in need of justification, we find ourselves mired in an infinite regress. Metaphorically, we end up like the ancient people who reportedly explained that the ground was firm because it rested on the back of large elephants; the elephants stood on a very large turtle. And what about the turtle?

The only obvious alternative to foundationalism is coherentism. Again, I use a standard definition: 'Coherentists deny that any beliefs are self-justifying and propose instead that beliefs are justified in so far as they belong to a system of beliefs that are mutually supportive.'[7] But coherentism, as normally understood, gives no clear picture of how scientific progress can be made. The solution I offer is called *progressive coherentism*.[8] This doctrine is coherentist in the sense that it rejects the search for an absolutely firm foundation. At the same time, it recognizes the possibility of making progress by first accepting a certain system of knowledge without ultimate justification, and then using that system to launch lines of inquiry which can in the end refine and correct the initially affirmed system.

[6] R. Foley, 'Justification, Epistemic', *Routledge Encyclopedia of Philosophy*, V, E. Craig (ed.) (London: Routledge, 1998), 158–159.

[7] *Ibid.*, 157.

[8] For a more detailed exposition of this doctrine, see H. Chang, *Inventing Temperature: Measurement and Scientific Progress* (New York: Oxford University Press, 2004), chapter 5.

A metaphor will be helpful in generating a vivid image of progressive coherentism. Recall the foundationalist metaphor of progress as building on a firm ground [**Figure 1**]. This metaphor is only as good as the flat-earth cosmology which it presumes. What if we modernize this metaphor? The real situation, according to our current view of the universe, is that we build outward on a round earth, not upward on a flat earth [**Figure 2**]. The round-earth metaphor graphically demonstrates the futility of seeking the absolutely firm ground, the ultimate epistemic justification. The earth is not firmly fixed to anything at all; still, we build on the earth because it is a large, dense body that attracts other objects, and we happen to live on it. The round-earth metaphor also nicely suggests that even in the absence of an absolute foundation, there is a sense of grounding, and a clear directionality according to which building needs to take place.

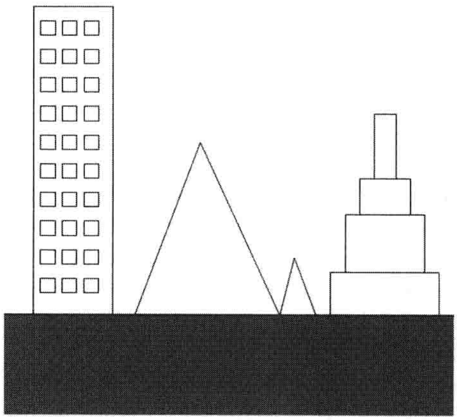

Figure 1. The foundationalist metaphor of building on a firm ground.

3. Progressive coherentism in action (I): justifying and improving measurement methods

3.1. From sensation to instrumentation

Leaving behind the abstractions and metaphors now, I would like to illustrate how progressive coherentism works through some concrete examples. I will present two classes of examples. The first class concern the justification and improvement of measurement

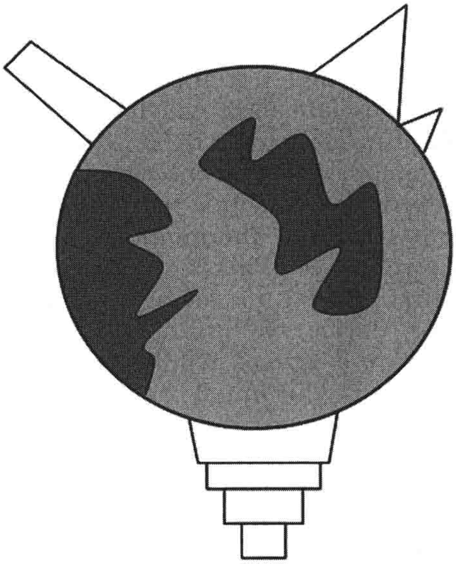

Figure 2. The progressive coherentist metaphor of building on a round earth.

methods. There are several reasons for this focus on measurement. First of all, quantitative measurement is an integral part of modern science, so if we understand how progress is made there, we will understand one important aspect of scientific progress. Secondly, measurement is an area in which science has undoubtedly made unequivocal and lasting progress, so it is an important area to look at when we try to understand progress. (In contrast, there is a sense in which grand theories come and go, and it is very uncertain business debating whether we are approaching the Truth or not.) Thirdly, and perhaps most significantly, I want to discuss the justification of measurement methods because that is where the fatal circularity of empiricist foundationalism is most clearly laid bare for all to see. The examples to be discussed in this section arise from the research I did on thermometry, much of which ended up in my book *Inventing Temperature*.

As an initial example of scientific progress in the absence of indubitable foundations, consider how thermometers can correct and overrule our sensations of hot and cold, much as the initial justification for their adoption had to be based on their agreement with the senses. There is a simple experiment designed to illustrate the unreliability of our temperature sensation. Take three buckets, each filled with hot, cold, and lukewarm water. Plunge your right hand in the hot bucket and your left hand in the cold bucket. After a while, take them out and put them both immediately in the lukewarm bucket. Your left hand will feel warm, and your right hand will feel cold. But inserting a thermometer into various parts of the lukewarm water reveals that the temperature in that body of water is quite uniform. We conclude that our sensations are faulty.

But why do we trust the thermometer, over and above the testimony of our own senses? This is quite puzzling when we think about how we first agreed to measure temperature with the thermometer. If a device we invented with the intention of measuring temperature disagreed wildly with the evidence of our senses, we would reject it. Our initial confidence in something like a column of mercury as a trustworthy indication of temperature comes from the correlation between its behavior and our own sensations: seeing it rise when we put it into a place that feels warm to us, seeing it rise rapidly when we put it near a fire, seeing it drop when we wet it and blow on it, etc. But once we adopt the thermometer as a reliable standard, then we begin to use its readings not only to confirm but also to correct our own sensations.

So here we start with the unjustified initial assumption that our sensations of hot and cold are basically accurate indications of temperature. Then, on the basis of sensations, we then adopt a certain mechanical device to measure temperatures more reliably and precisely than our sensations can. The relation between our sensation and the thermometer is quite subtle, based on what I call the *principle of respect*. Our use of the instrument is made with a respect for sensation as a prior standard, but that does not mean that the verdict of sensation has unconditional authority.

The actual history of the development of simple thermometers shows three main stages.[9] Initially the judgment of temperature was only qualitative, based on the sensation of hot and cold. Then came what we call thermoscopes, instruments that indicate the increase and decrease of temperatures without attaching numbers to them; certified by their broad agreement with sensations, thermoscopes allowed a decisive and consistent comparison and ordering of various phenomena possessing different temperatures. Afterwards numerical thermometers, arising from thermoscopes, went further by attaching meaningful numbers to the degrees of temperature. In this developmental process temperature evolved

[9] For further detail on these three stages of development, see *ibid.*, 47–48.

from an unquantified property to an ordinal quantity, then to a cardinal quantity. This process of development constituted the *creation* of the quantitative concept of temperature, because each stage built on the previous one, but added a new dimension to it.

3.2. Increasing precision

Once a quantitative concept is created, science does not stop there. There is a continual effort to increase precision. To illustrate that point, let me take a more contemporary example—from the 25th of June in 2004, to be precise. I was doing some work then in a teaching lab in the Chemistry Department at University College London, trying to replicate some curious old experiments. One of the things I was investigating was a report by Joseph-Louis Gay-Lussac in 1812 that the temperature of boiling water in a glass vessel was 1.2°C higher than the boiling temperature in a metallic vessel.[10] When I tried this out, I did observe a difference, though it seemed to be only about 0.7–0.8°C according to the common mercury thermometer I was using, graduated down to 1°C.

When I showed this to Andrea Sella, my sponsor at the Chemistry Department, he said 'You need an ancient thermometer', and pulled out a curious old instrument called a Beckmann thermometer, which was in common use not so long ago in physical chemistry for the precise determination of boiling points. The Beckmann thermometer is a thing of beauty, a mercury thermometer with an enormous 6-degree scale graduated down to 0.01°C. One can adapt this instrument to almost any particular temperature range normally desired in a chemistry lab, because the zero point can be moved around by shifting mercury back and forth between the main reservoir at the bottom and an auxiliary reservoir at the top.

I did manage to calibrate the Beckmann thermometer in the range I needed, setting its zero at about 98°C. But then the question presented itself: how do I know what exactly the zero point on the Beckmann thermometer indicates, and how do I know whether the instrument is operating correctly anyway? The only clear guide I had was comparisons with the ordinary mercury thermometer, which I can read at best to about 0.1°C, probably only one-fourth of a degree if I want to be safe. So the ordinary mercury

[10] Reported in J. B. Biot, *Traité de physique expérimentale et mathématique* (Paris: Deterville, 1816), vol. 1, 42–43.

thermometer can only tell me that the 0.00° on the Beckmann thermometer is, say, about 97.2°C, but not whether it is 97.24°C or 97.27°C; it also cannot tell me whether the behaviour of the Beckmann thermometer is linear between 97.2°C and 97.4°C (which is about a whole centimeter on the Beckmann scale). My trust in the precise behavior of the Beckmann thermometer must come from other sources, although my *basic* trust in it comes from its consistency with the ordinary mercury thermometer. And interestingly, once I gain enough trust in the Beckmann thermometer, I can use its readings not only to refine the readings of the ordinary mercury thermometer, but occasionally to correct or overrule them.

The situation, then, is quite similar to the first story. The relation between the Beckmann thermometer and the ordinary mercury thermometer is like the relation between a thermometer and human sensation. In each case, the prior standard provides initial justification for the later standard, but the later standard refines and even corrects the prior standard.

The case of the Beckmann thermometer shows a bit more clearly the exact mechanism of progress. If we think again about why we trust the Beckmann thermometer beyond where the ordinary mercury thermometer can give us the verdict, we can see how there are other criteria that guide our judgements. First of all, it seems that higher precision in itself is an attraction. This is like how we are predisposed in vision to take the sharper image as the superior picture; when we put on our glasses and see that objects are seen with a sharper focus, we believe that we are getting a better picture; we make that judgement without understanding how the glasses produce a truer picture according to the laws of optics and the physiology of the eye. But of course the higher precision has to be backed up by self-consistency; I would not trust the Beckmann thermometer if it gave me varying readings under the same circumstance; I would also have worries if two different Beckmann thermometers gave me mutually inconsistent sets of readings. In addition, coherence with other things we believe also guides our thinking. If I am heating a body of water with a steady heat source, then it is reassuring to see that the mercury column in the Beckmann thermometer climbs up smoothly and steadily, not in a jerky fashion; conversely, if there are sudden changes in the situation, it is reassuring to see sudden changes in the thermometer readings. These are the kinds of factors that give us confidence that the new instrument actually constitutes an improvement, while it is not within the capacity of the old instrument to produce that judgement.

3.3. Handling recognized sources of error

One more story of temperature measurement will illustrate another dimension of progress in the absence of assured foundations. Daniel Gabriel Fahrenheit made some important early experimental contributions to the study of specific heats, by mixing measured-out amounts of different fluids at different initial temperatures and observing the temperature of the resulting mixture. In these experiments he was clearly aware of an important source of error: the initial temperature of the mixing vessel (and the thermometer itself) would have a distorting effect on the outcome. The only way to eliminate this source of error was to make sure that the mixing vessel *started out* at the temperature that the mixture would take, but that final temperature was unknown to Fahrenheit, since that was just what he was trying to find out by experiment. In a modern setting we would try to correct for this error by calculating the amount of heat exchange between the vessel and the liquids contained in it. But this would involve knowing the specific heat of the vessel and the liquids, and even the concept of specific heat was not available to Fahrenheit. Indeed, the concept could not have been established until after Fahrenheit's type of experiments started delivering stable results.

The solution adopted by Fahrenheit was both pragmatic and profound. In a letter of 12 December 1718 to Herman Boerhaave, he wrote:

> (1) I used wide vessels which were made of the thinnest glass I could get. (2) I saw to it that these vessels were heated to approximately the same temperature as that which the liquids assumed when they were poured into them. (3) I had learned this approximate temperature from some tests performed in advance, and found that, if the vessel were not so approximately heated, it communicated some of its own temperature (warmer or colder) to the mixture.[11]

[11] P. van der Star (ed.), *Fahrenheit's Letters to Leibniz and Boerhaave* (Leiden: Museum Boerhaave; Amsterdam: Rodopi, 1983), 80–81.