PHILOSOPHY

I see no atoms

DAVID PAPINEAU

Bradley Monton, editor

IMAGES OF EMPIRICISM

Essays on Science and Stances with a reply from Bas C. van Fraassen

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Bas C. van Fraassen

SCIENTIFIC REPRESENTATION

Paradoxes of Perspective

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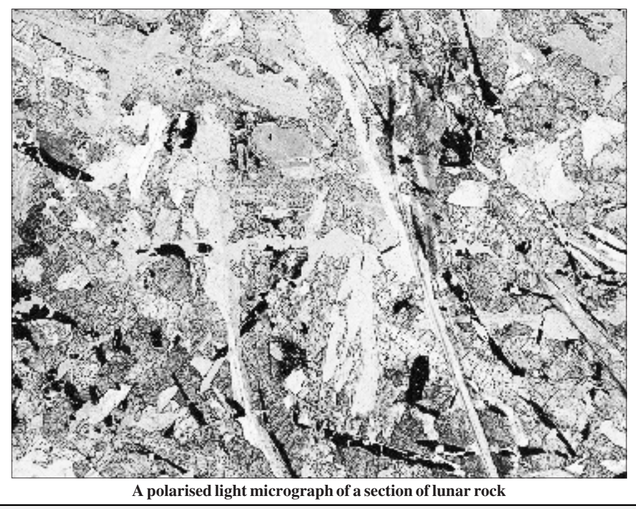
The architects of the scientific revolution shared a vision. According to Galileo, Descartes, Gassendi and Boyle, the natural world is nothing but a giant machine with microscopic parts. At bottom there is only cold matter in motion, and all the beauties of nature arise from the many ways the tiny pieces can be arranged into intricate patterns. This vision has since been amply confirmed. We may now give a slightly different account of the underlying components, but modern science has no doubt that everything is determined by basic physics. Contemporary biochemistry and computer technology are just two of the spin-offs that testify to the validity of the mechanical vision.

The funny thing is that it took over two centuries for the fathers of the scientific revolution to be proved right. It is one of the great curiosities of modern intellectual life that the vision which so inspired the scientific revolution was emphatically rejected in the eighteenth and nineteenth centuries and only fully resurrected in the twentieth. The trouble was that the scientific revolution wasn't much good at microscopic science. The natural philosophers of the time gained a great deal of precise mathematical knowledge about the gross behaviour of observable phenomena like planets, projectiles and gases, and the newly invented microscope lifted one small corner of the veil covering the microscopic world. But when it came to identifying the hidden mechanisms responsible for the general run of observable phenomena, the seventeenth century got nowhere. To take one example, the seventeenth-century scientists knew that air exerts pressure. But what was responsible for the "spring of the air" as they termed it? Was the air packed with bouncy particles like coiled up pieces of wool, as Boyle supposed? Was it simply some kind of porous fluid? Or did it consist of scattered particles in rapid motion, as we now believe? In the seventeenth century all these hypotheses were consistent with the macroscopic data and there seemed little hope of telling them apart. And so it went, across the board.

Plenty of microscopic theories were entertained, but none graduated beyond the status of speculation, and it came to seem that the scientific method was impotent to penetrate beneath the surface of things As a result the whole idea of microscopic theorizing fell into disrepute. "Hypotheses non fingo" declared Newton, by which he meant that hypothetical speculation about underlying mechanisms was unnecessary for his precise mathematical descriptions of gravitational motion. "No science of bodies [is] within our reach" echoed his acolyte Locke, explaining that "whilst we are destitute of senses acute enough to discover the minute particles of bodies, and to give us ideas of their mechanical affections, we must be content to be ignorant of their properties and ways of operation". The upshot was that ideas about "minute particles" ceased to be part of science. Both Newton and Locke allowed that speculation had some utility as a source of intellectual stimulation. But it had no place in experimental philosophy. Proper science should chart the behaviour of observable phenomena and not waste time on hidden mechanisms.

Part of the issue for Newton and his contemporaries was how to determine the appropriate standards for scientific knowledge. Should science satisfy the same requirements of demonstrative certainty as mathematics and logic? Or were lower standards of everyday proof enough, as when we require criminal verdicts to be established "beyond a reasonable doubt"? At first the pioneers of the scientific revolution aimed high. Descartes's first foray into public life was a vehement attack in a prominent Parisian salon on the modish sceptical view that there can be no demonstrative knowledge of nature. And much of Descartes's mature philosophy was designed to show how science could be placed on absolutely secure foundations. However, by the end of his life, Descartes realized that micromechanical theories posed a problem. No matter how much the evidence supported some specific hypothesis, alternatives could not be conclusively eliminated. Even so, Descartes felt that once the evidence for a given theory piled up, we could at least be "morally certain" of its truth, even if not absolutely certain, in the way that we are morally certain, for example, that the Romans once ruled England. Except that the evidence never did pile up in the seventeenth century, and so Newton and Locke gave the whole thing up as a bad job. One upshot of banishing micromechanical theorizing from science was to allow a reaffirmation of absolute certainty. Knowledge could once more mean demonstrative knowledge, information produced by methods that leave no logical room for error. Of course absolute certainty isn't really possibly even for the kind of science Newton and Locke approved of, since we can't even be completely sure that the observable patterns we observe today will continue tomorrow. But it was easy enough to sweep this philosophical quibble under the carpet once blatant speculation about hidden mechanisms had been eliminated from science.



A polarised light micrograph of a section of lunar rock

Throughout the eighteenth and into the nineteenth century successful science conformed to the Newtonian model. Scientists stuck to the surface of things, systematizing and cataloging the observable world. Important advances were made in mechanics, hydrodynamics, thermodynamics and biological classification. But none of this hinged on any ideas about unobservable machinery. It was only with the development of atomic chemistry that the Newtonian suspicion of hypotheses was called into question. In the early years of the nineteenth century the English chemist John Dalton conjectured that chemical elements are made of characteristic atoms which stick together in fixed ways. This offered a natural explanation of the way elements combine in fixed proportions and of a number of other chemical facts. However the theory was by no means an immediate success. At first it was hard to make the numbers add up. Doubters took this as further evidence for the futility of hypothetical speculation. It was only with the mid-century advent of the kinetic theory of gases that things began to fall into place. The kinetic theory corroborated Dalton's ideas and allowed the details to be filled in. Many different ways of calculating the properties of the posited atoms turned out to give the same answers. By the end of the century most workaday scientists were convinced that the atomic theory was effectively established.

But there remained plenty of hold-outs. The French were particularly sticky. Curiously, the French were far more committed to the Newtonian rejection of mechanical hypotheses than the English. The *philosophes* of the Enlightenment viewed Descartes as a malign influence and had elevated Newton to the apex of their philosophical pantheon.

Descartes's enthusiasm for mechanical hypotheses was associated with the *ancien régime*, whereas Newton's focus on the observable world was seen as the path to the future. There is of course some irony here: we now know Newton was a secret occultist obsessed with alchemy and biblical numerology, while Descartes was as close to a progressive friend of scientific reason as you could hope to find in the seventeenth century. Still, however misplaced the Enlightenment sentiments, they meant that the high priests of French nineteenth-century science were very slow to come round to the atomic view. Figures like Pierre Duhem (1861-1916) and Henri Poincaré (1854-1912) were highly dismissive of "the atomic hypothesis". Duhem attributed the cross-channel enthusiasm for atomism to the crude "English mind". As he saw it, the nineteenth-century English were limited to concrete thinking-he cited Dickens-and so could only understand scientific processes with the help of mechanical models. The English "have little liking for more abstract reasoning and do it poorly". The German Ernst Mach (1838-1916) was even more dismissive. The atomic hypothesis merely added "childish and superfluous accompanying pictures" to a proper understanding of chemistry. Both Duhem and Mach continued to reject the atomic theory until the end of their lives.

Once this old guard had died off, however, scientific resistance crumbed. By this stage atoms were not the only well-established unobservable entities. Maxwell's mathematical tamming of the electromagnetic field was beginning to manifest itself publicly in radio technology. The germ theory of diseases had been extended to cover a range of sub-microscopic viruses. In these and other cases, many lines of independent evidence left no room for reasonable doubt about the reality of these hidden entities. Of course, the evidence was all circumstantial-there was no question of observing atoms, radio waves or viruses directly. But it came to seem silly to continue demanding some higher standard of proof. Why would such a wealth of evidence keep pointing in the same direction, if the posited entities didn't exist? Along with the scientists, most mainstream philosophers were persuaded too, and drew the obvious moral that even in science there can be firmly established knowledge even without absolutely conclusive proof.

Only among one last group did the old Newtonian suspicions linger on. Somewhat oddly, it was the specialist philosophers of science who continued to have doubts about the unobservable world. During the first half of the twentieth century philosophy of science was much influenced by the logical positivism of the Vienna Circle and its attempt to discredit the florid metaphysics of the idealist tradition as "meaningless". It was difficult to prevent these arguments against metaphysical speculation from spilling over to theories about unobservables. As a result, many philosophers of science found themselves arguing that claims about atoms, viruses and radio waves must be meaningless too. If such claims have any virtue, they argued, it is not because they portray some unobservable realm, but because they are useful "instruments" for keeping track of the familiar observable world.

Once the influence of the Vienna Circle waned, however, it became difficult to take this "instrumentalist" doctrine seriously. If someone maintains that it is meaningless nonsense to assert that "matter is made of tiny particles, too small to see, one kind for each element", the natural response is "which bit don't you understand?" By the second half of the twentieth century instrumentalism had fallen into disrepute even among philosophers of science. But the Newtonian tradition is nothing if not resilient. In 1980 the philosopher Bas van Fraassen gave it a new lease of life with his publication of *The Scientific Image*. Van Fraassen's first move was to distance himself from the semantic doctrines of the Vienna Circle. Far from being meaningless, scientific theories mean just what they seem to mean – that there are unobservable entities of specified sorts, responsible for such-and-such observable effects. And these theories will therefore be straightforwardly true or false, depending on whether the unobservable world is as they say. But – and here is the Newtonian twist – nothing in science requires us to *believe* these theories. Since they take us beyond what we experience directly, argued van Fraassen, there is no rational compulsion for us to embrace them. Moreover, science can manage perfectly well without any such commitment. Theorizing about the unobservable world no doubt plays an important part in directing scientific research. But such theorizing is not aimed at the truth. The real job of science is only to "save the phenomena" by accurately anticipating observable happenings. Whether or not scientific theories also get the unobservable world right is neither here nor there.

The strength of van Fraassen's position was that he separated Newtonian scepticism from the obscurities of the Vienna Circle. He agreed that there was nothing incoherent about the idea of unobservable mechanisms. His complaint was simply that we can't find out about them, given that our limited human constitution leaves us "destitute of senses acute enough to discover the minute particles of bodies". Still, van Fraassen was quickly challenged on this central point. Is it really too hard to find out about atoms and radio waves? They may not be immediately visible, but surely by now we have more than enough evidence to believe in them.

Some of van Fraassen's arguments made it seem that he was objecting to scientific theories on the traditional grounds that we can never absolutely secure when making inferences to hidden mechanisms. But this line, as critics were not slow to point out, threatens too much. In particular, it threatens to rule out any knowledge of the future. Since Einstein's overthrow of classical mechanics, philosophers have been acutely aware that, even when we are dealing with purely observable matters, "inductive" inferences from past to future patterns are never absolutely foolproof either. Karl Popper was prepared to bite this bullet, and so ended up with the absurd view that it irrational to believe, say, that birds will continue to fly or that the next bottle of whisky will make you drunk. But everybody else could see there must be some room for genuine knowledge that is less than absolutely secure. Van Fraassen was no exception. He soon made it clear that he was no Popperian, and that a lack of absolute certainty was not his problem. He had no complaint against future predictions, provided they were restricted to the familiar observable world.

Indeed it turned out that van Fraassen had no definite complaint against unobservables either. In a series of subsequent writings, especially *The Empirical Stance* (2002), he insisted that rationality is a matter of choice rather than compulsion. He contrasted the "Prussian" view of rationality – everything is forbidden that is not explicitly permitted – with the "English" one – everything is permitted that is not explicitly forbidden. In line with the English view, van Fraassen explained that his aim was merely to show that it is permissible to be agnostic about unobservables, not that it is forbidden to believe in them. Epistemology requires commitment. Some may favour a realist stance and embrace belief in scientific unobservables. Others will prefer the "empiricist" stance and avoid any such allegiance. We can of course discuss the relative merits of these options. But there is no neutral ground, prior to any commitment, from which we can definitely show that one is right and the other wrong.

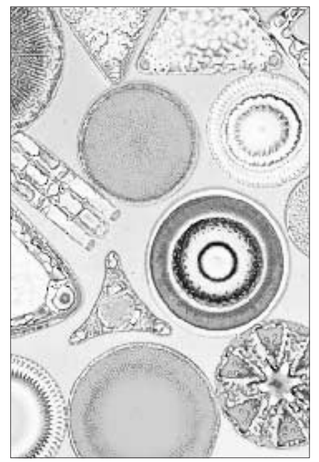
Van Fraassen may be right that the requirements of rationality leave room for different commitments. But this still leaves plenty of room to query his own specific stance.

Why exactly is it reasonable to withhold belief from scientific unobservables? After all, claims about unobservable mechanisms are interesting, useful and-crucially-massively evidenced by modern scientific research. Given all this, it seems little more than stubborn to doubt their existence. Van Fraassen's response was to associate his realist opponents with "metaphysics". The "empiricist" is satisfied with the world as it appears, but the realist hopes to plumb its inner nature. Van Fraassen placed himself in a long tradition of doubts about the power of human reason to penetrate metaphysical secrets, going back to Kant and beyond. He is particularly dubious about the claims of many contemporary metaphysicians to have established a "materialist" or "naturalist" world view. Van Fraassen has an enviable familiarity with many by-ways in the history of thought, and he used this effectively to disparage the more extreme metaphysical ambitions of philosophy.

Still, metaphysics is one thing, science another. Perhaps van Fraassen is right that human reason often overreaches itself when it lays claim to general metaphysical insights (though his attacks on current metaphysical views would carry more weight if he paid as much attention to the details of contemporary debate as to their historical antecedents).

But it scarcely follows that reason is overreaching itself when it lays claim to atoms. It is striking that van Fraassen never tries to undermine any specific scientific claims, in the way he does metaphysical ones. Plenty of other philosophers of science do just that, suggesting for example that the poor record of past scientific theories should make us doubtful about present ones. But van Fraassen has always stayed clear of this "pessimistic" line of argument, no doubt because of its limited effectiveness: by no means all areas of science have poor past records. Whatever his real complaint about unobservables, then, it seems clear that it must rest on something more generic than worries about particular scientific claims.

Images of Empiricism is a collection of essays on van Fraassen, published together with his replies. Most of the pieces involve relatively detailed points of critical interpretation, but two in particular are suggestive about possible motivations for van Fraassen's scepticism. Nancy Cartwright focuses on van Fraassen's oft-quoted dictum that "it is not an epistemological principle that one might as well hang for a sheep as a lamb". Van Fraassen's thought here is that beliefs about unobservables aren't worth the candle, since they bring no benefits to compensate the risk of error. But this thought scarcely wears its justification on its sleeve. Why aren't truths about unobservables just as beneficial as any other truths? They certainly seem to give us very useful information about all manner of important things. But Cartwright points out that van Fraassen's position would make sense if we supposed that our experiences matter to us in a way that other things do not. Given this assumption, beliefs about what we will observe will play a special role in our practical deliberations. At first sight this may look unattractive – to privilege experience in this way smacks of crude hedonism. But interestingly van Fraassen does not demur. In his reply to Cartwright he agrees that his epistemology hinges essentially on the distinguished way that experience matters to us.



Diatom frustules seen through a polarising microscope

Ernan McMullin attends to the religious dimension of van Fraassen's writings. It is well known that van Fraassen is an adult convert to Catholicism, and reflections on the nature of religious thought appear in the last chapter of *The Empirical Stance*. In particular, van Fraassen there emphasizes the importance of "encounters" with God. McMullin, himself a Catholic priest, wants to know how far such encounters involve some specific mode of experience. In the end, both he and van Fraassen agree that academic discussion can take us only so far with such questions. Even so, it is hard to avoid the impression that for van Fraassen it is only in lived experience, whether of nature or of something possibly transcendent, that we make contact with reality, and that other modes of intellectual access are impotent to latch onto anything substantial. If this is right, it would explain why van Fraassen is so down on beliefs about unobservables. He simply isn't convinced that there is any unobservable realm for them to be true about. This is further confirmed by some of the other things that van Fraassen says in *The Empirical Stance*. At one point he associates his "antirealism" with the sense of loss that comes with secular "objectification", and explains how from his point of view "a theory can at best replace real life by a phantasm". A few lines later he credits the antirealist with "a continuing sense of wonder not alleviated" by the successes of any new scientific theory.

This is a far cry from the position originally on offer in *The Scientific Image*. In the earlier book, unobservables were perfectly respectable denizens of reality, just rather hard to find out about. But now it turns out that for van Fraassen they are "at best . . . a phantasm". This looks more like the old Vienna accusation of meaninglessness than a simple lack of good evidence about microscopic structures. This change of direction is confirmed by van Fraassen's latest book, *Scientific Representation*. Based on the John Locke lectures he gave in Oxford in 2001, it is a typically erudite survey of many kinds of representation both outside science and within, ranging from portraits and perspective to maps and measurement. But the account of scientific theories that emerges is unfamiliar and austere. Theories are abstract mathematical structures, not literal descriptions of hidden mechanisms; they are constrained only by the need to accommodate the summaries that scientists make of their observational activities; and even at this level it can be misplaced to ask whether or not those summaries get the facts right.

Van Fraassen's focus here is on actual scientific practice, on the complex and little understood ways in which scientists go about their esoteric business. But one can acquiesce in his insistence that this practice is not to be taken for granted without agreeing that the theories that emerge are merely mathematical devices for regimenting observational databases. At the beginning of the book he says that he is aiming for an account of representation that will be of interest to his realist opponents as well as his empiricist friends. But any realists worth their salt will object that his many admittedly interesting insights into scientific practice fail to discredit the natural view that science is offering a literal story about what lies behind the appearances.

More generally, there seems to be little in van Fraassen's overall view of science to shake his opponents out of their realism. Maybe this wouldn't worry him. As he repeatedly avers, his aim is not to show that realism is wrong, but simply to make rational space for his sceptical alternative. But perhaps it should worry his readers. There are many incidental pleasures to be gained from van Fraassen's writings. But on the central issue of belief in unobservables he offers no positive inducement to adopt his strange sceptical stance. He is an ingenious thinker, and has taken many pains to render his position consistent. But beyond that he does nothing to recommend it to those who do not already share an existential commitment that he scarcely ever discusses.