'...the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres' – Ludwig Wittgenstein. *Photo by Getty*

Getting it right

Truth is neither absolute nor timeless. But the pursuit of truth remains at the heart of the scientific endeavour

Michela Massimi

Think of the number of scenarios in which truth matters in science. We care to know whether increased CO2 emission levels cause climate change, and how fast. We care to know whether smoking tobacco increases the risk of lung cancer. We care to know whether poor diet exposes children to the risk of developing obesity, or whether forecasts of economic growth are correct. Truth in science is not esoteric dilly-dallying. It shapes climate science, medicine, public health, the economy and many other worldly endeavours.

That truth matters to science is hardly news. For a long time, people have looked to science for truths about the world. The Scientific Revolution was nothing if not the triumph of Galileo's scientific truth – hard-won through his telescopic observations – over centuries of dogma about the geocentric system. With its system of epicycles and deferents, Ptolemaic astronomy was at once sophisticated and false. It served to, at best, 'save the appearances' about how planets seemed to move in the sky. It did not tell the truth about planetary motion until the discovery of the Copernican explanation. Or consider the Chemical Revolution at the end of the 18th century. We no longer, after all, believe in phlogiston – the fictional imponderable fluid that Georg Ernst Stahl, Joseph Priestley and other natural philosophers at the time believed to be at work in combustion and calcination phenomena. Antoine Lavoisier's scientific truth about oxygen prevailed over false beliefs about phlogiston.

The main actors of these scientific revolutions often fostered this way of thinking about science as an enquiry leading to the inevitable triumph of truth over past errors. Two centuries after Galileo's successful defence of the heliocentric system, this idea of the course of scientific truth continued to inspire philosophers. In his *Cours de philosophie positive* (1830-42), Auguste Comte saw the evolution of human knowledge in three main stages: 'the Theological, or fictitious; the Metaphysical, or abstract; and the Scientific, or positive'. In the 'positive', the third and last stage, 'an explanation of facts is simply the establishment of a connection between single phenomena and some general facts, the number of which continually diminishes with the progress of science'.

Comte's positivism was soon decontextualised from its political and social context (after all, Comte started his career working for the social theorist Henri de Saint-Simon, and positivism was inspired by the Industrial Revolution). By the end of the 19th century (and in its early 20th-century reappearance, which I do not have the space to discuss here), the word 'positivism' had become – to the ears of many – synonymous with an unfailing optimism in the power of science and technology, and their steady progress toward truth.

In some scientific quarters, this Comtean notion of how science evolves and progresses remains common currency. But philosophers of science, over the past half-century, have turned against the representation of science as a ceaseless forward march toward truth. It is just not how science works, how it moves through history. It flies in the face of the wonderful and subtle historical nuances of how scientific revolutions have in fact occurred. It does not accommodate how some of the greatest scientific minds held dearly to some false beliefs. It wilfully ignores the many voices, disagreements and controversies through which scientific knowledge has often advanced and progressed over time. Simple faith in the 'Whiggish' narrative of science naively presumes that progress is marked by some cumulative acquisition of 'more true beliefs'.

However, many (and legitimate in their own right) criticisms against this naive view of science have committed a similar mistake. They have offered a portrait of science purged of any commitment to truth. They see truth as an inconvenient and disposable feature of science. Fraught as the ideal and pursuit of truth is with tendencies to petty doctrinairism, it is nonetheless a mistake to try to purge it. The fallacy of positivist philosophy was to think of science as coming in stages of some sort, or following a particular path, or historical cycles. The anti-truth trend in the philosophy of science has often ended up repeating this same misstep. It is important to move beyond the sterile dichotomy between the old (quasi-positivist) view of truth in science and the rival anti-truth trend of recent decades.

Let us start with some genuine *philosophical* questions about truth in science. Here are three: 1) Does science *aim* at truth? 2) Does science *tell* us the truth? 3) Should we *expect* science to tell us the truth?

In each of these questions, 'science' is a generic placeholder for whichever scientific discipline we are interested in questioning. Question one might strike us as otiose but, in fact, it triggered one of the liveliest debates of the past 40 years. Bas van Fraassen launched this debate as to whether science aims at truth with his pioneering book *The Scientific Image* (1980). Does science aim to tell us a true story about nature? Or does it aim only at saving the observable phenomena (namely, providing an account that makes sense of what we can observe, without expecting it to be the true account about nature)?

A positive answer to the first question defines the philosophical stance known as 'scientific realism'. Scientific realists maintain that the best scientific theories aim at truth as their final goal. But Van Fraassen pointed out (following the French philosopher Pierre Duhem), that often in science truth is not needed, and 'saving the phenomena' (or appearances) suffices. For example, Ptolemy's astronomy could account for the appearance of planets moving in loops in the sky over months. This was called 'retrograde motion', and Ptolemy's theory accommodated it by introducing a series of geometrical contrivances (epicycles and deferents) around which planets were supposed to move. In fact, there are no such circles of circles, and retrograde motion is just an illusion caused by the relative speeds of planets all orbiting the Sun. As Copernican astronomy realised several centuries later, Ptolemaic astronomy was false, despite its century-long success at 'saving the observable phenomena'.

There are philosophers today who embrace the view that science does not need to be true in order to be *good*. They argue that asking for truth is risky because it commits one to believing in things (be it epicycles, phlogiston, ether or something else) that might prove false in the future. In their view, 'empirically adequate' theories, theories that 'save the observable phenomena', are good enough for science. For example, one might take the Standard Model in high-energy physics not as aiming at the truth about whether the world is really carved up into quarks, leptons and force carriers; whether these entities really have the properties that the Standard Model says they have; and so on. A more prudent approach aims for empirical adequacy. Insofar as the Standard Model saves all the observable phenomena (which are not about invisible particles, of course, but about macroscopic observable situations that we can see with our naked eye), the model is empirically adequate. Why ask for more?

When it comes to the second question – does science *tell* us the truth? – scientific realists and anti-realists of various stripes have debated it. Leaving aside the aim of science, let us concentrate on its track record instead. Has science told us the truth? Looking at the history of science, does it amount to a persuasive story of truth accumulated over the centuries? Philosophers, historians, sociologists and science-studies scholars have all challenged a simple affirmative answer to this question. They have in particular challenged the notion of factual truth implicit in the question. In this debate, philosophers take a well-defined set of facts about nature as responsible for making scientific claims true or false, and scientific progress consists in unveiling more of these facts until they are all finally revealed.

A crisp illustration of the view can be found in the *Tractatus Logico-Philosophicus* (1922), where Ludwig Wittgenstein right at the outset announced: ‘The world is the totality of facts’; and ‘the totality of facts determines both what is the case, and also all that is not the case’. Wittgenstein gave a deceptively simple view of how language maps on to reality and, more importantly, on to what he called ‘atomic facts’. For decades (without taking Wittgenstein as a direct target nor as an interlocutor in this specific debate on science), anti-realist trends in philosophy of science have called into question both the possibility of atomic facts and their ability to make scientific claims true or false. Many think these apparently simple matters face some formidable challenges.

This decades-long, multi-pronged, disenchantment-with-truth trend in philosophy of science starts by rejecting the idea that there are facts about nature that make our scientific claims true or false. Fact-constructivism is only one aspect of this multi-pronged disenchantment-with-truth trend. Outlandish as this might sound, its defenders claim that there is not a single, objective way that the world is; there are rather many different and ‘equally true descriptions of the world, and their truth is the only standard of their faithfulness’, in the words of the philosopher Nelson Goodman. For example, he claimed that we do make facts, but not like, say, a baker makes bread, or a sculptor makes a statue. In Goodman’s view, we make facts any time we construct what he called a ‘version’ of the world (via works of art, of music, of poetry, or of science). For example, Camille Saint-Saëns created a version of the world by composing his violin sonata no 1 in D minor, which in turn inspired Marcel Proust to imagine the Vinteuil sonata in the first volume of his novel *À la Recherche du Temps Perdu* (1913-27). Anyone who has read Proust cannot help thinking about the love story of Odette and Swann unfolding in the Verdurin salon when they hear Saint-Saëns’s mesmerising violin sonata.

> Some argue that science does not tell the *truth* but does provide important non-factive understanding

Another way of constructing a version of the world is by clustering objects according to a particular shape and giving them a name. We do this all the time, for example, with stars and constellations. As the philosopher Hilary Putnam expresses it: ‘Nowadays, there is a Big Dipper up there in the sky, and we, so to speak, “put” a Big Dipper up there in the sky by constructing that version.’ Goodman’s world-making view has severe implications for truth in science. ‘Truth,’ he wrote, ‘far from being a solemn and severe master, is a docile and obedient servant. The scientist who supposes that he is single-mindedly dedicated to the search for truth deceives himself … He as much decrees as discovers the laws he sets forth, as much designs as discerns the patterns he delineates.’ Goodman’s view is similar in some relevant respects to the French philosopher Bruno Latour’s. In *Pandora’s Hope* (1999), Latour argues that facts about whether or not the savannah is advancing (and the Amazon forest retreating) are the outcome of laboratory practices about sampling soil, rather than independent facts about nature itself.

Fact-constructivism sounds too radical to many philosophers, and alienating to most scientists. So here is another approach against factual truth, well-known among philosophers of science. Over the past 40 years, they have produced an extraordinary amount of work on models in science. The role of abstractions and idealisations in scientific models, they maintain, is to select and to distort aspects of the relevant target system. The billiard-ball model of Brownian motion, for example, represents the motion of molecules by idealising them as perfectly spherical billiard balls. Moreover, the model abstracts, or removes, molecules from their actual environment, which is of course where collisions among molecules take place.

Studying modelling practices in science has led some to argue that science does not tell the *truth* but it does provide important non-factive understanding. Consider, for instance, Boyle’s gas law, which captures the relation between pressure p and volume v in an ideal gas at constant temperature. At best, Boyle’s law is true *ceteris paribus* (ie, all else being equal) in highly idealised and contrived circumstances. There simply is no ideal gas with perfectly spherical molecules displaying ‘atomic facts’ (in a quasi-Wittgensteinian sense) that make Boyle’s law true. Despite being true of nothing real, the billiard-ball model of Brownian motion and Boyle’s ideal gas law do nonetheless provide important non-factual understanding of the behaviour of real gases. For they allow scientists to understand the relation between decreasing volume and increasing pressure in any gas, even if there are no atomic facts in nature about perfectly spherical molecules corresponding to such idealisations.

Anti-dogmatic and anti-monist approaches to science have also questioned the value, as well as the facticity, of truth. From the 1960s, science-studies scholars began to see the word ‘truth’ as evoking unpalatable petty doctrinairism and intracultural battles in the wake of the Vietnamese war, postmodernism and, later on, what became known as the 'science wars'. Many saw the physicist Thomas Kuhn as the forefather of a new historicist trend that dismantled what they perceived as the naive view that science aims at or tracks truth. Kuhn saw himself as 'a fact lover and a truth seeker'. Yet in the final remarks to his classic *The Structure of Scientific Revolutions* (1962), he made a prescient, almost ominous, warning:

> Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to the ultimate goal? … Successive stages in that developmental process are marked by an increase in articulation and specialisation. And the entire process might have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar.

For Kuhn, truth is not an overarching aim of science across scientific revolutions. Nor do scientific revolutions (eg, from Ptolemaic to Copernican astronomy) track truth either. What they do, at best, is to increase our ability to solve anomalies that beset the previous paradigm (as when we eventually discovered that retrograde motion was only an illusion, and not something that needed epicycles and deferents to be explained).

We see the spirit of Kuhn's warning in discussions today. Truth itself is not enough to settle or even guide debates about expertise, trust, consensus and dissent in science. The philosophers of science Inmaculada de Melo-Martín and Kristen Intemann have described the matter well in their book *The Fight Against Doubt* (2018). When it comes to the role of science in policymaking, the key is 'engaging in discussions with all relevant parties about the values at stake, rather than the truth of particular scientific claims'. Policymaking involves politics and values, and 'disagreement about values cannot, and should not, be decided by scientists alone' or by just scientific evidence. Intemann and de Melo-Martín have further maintained that, even if truth were the goal of science, consensus in a disciplinary or subfield community cannot always be 'a proxy for truth, and whether it is necessary for adopting public policy is still questionable'.

To summarise, the history of science, the practice of science, and science policy all provide reasons for disenchantment with truth in science. That is why, in our time, philosophers of science who call themselves realists tend to add some qualifying adjective to the word 'realism' (be it 'structural', 'selective', 'pragmatic', 'perspectival' or 'local'). The net result – the dethroning of truth – has left philosophical discussions about its nature to logicians, philosophers of language and metaphysicians, as opposed to philosophers of science.

The third question is whether we should *expect* science to tell us the truth, or is truth (or at least the notion of factual truth) not best left to logicians and metaphysicians?

While critical analyses of factual truth are indeed best left to logicians and metaphysicians, philosophers of science should not abdicate their responsibility to talk about truth in science. The quasi-Wittgensteinian myth of atomic facts as the truth-makers of scientific claims has proved inadequate to even scratch the surface of very complex practices in science. But that is not a good reason (or pretext) for forgoing truth altogether. Nor is it a reason for concluding that science should not be expected to tell us the truth. Philosophers of science have a social responsibility to talk about truth in science. Questions about truth cannot be left unspoken, delegated, or, worse, met with discontent and misgivings. For a serious engagement with the topic of truth in science – thorny and difficult as it might be – must be the starting point of any serious engagement with science in democratic, tolerant and pluralistic societies.

But *whose* truth? By *whose* lights? Some might be tempted at this point by a Jamesian pragmatist theory of truth. American pragmatism has traditionally provided an alternative way of thinking about truth, which some philosophers of science see as more congenial to capturing the complex nuances and the power structure of scientific practice. There are significant and important differences among pragmatists such as John Dewey, William James and Charles Sanders Peirce when it comes to truth (see Cheryl Misak's Aeon Idea for an excellent introduction), and one should avoid assimilating or caricaturing this diverse and nuanced range of philosophical views. In James's words: '"The true" … is only the expedient in the way of our thinking, just as "the right" is only the expedient in the way of our behaving.' Stripped of its rhetorical flourishes, for James to be true is (to a good approximation) to work successfully. A scientific model is true – on a loosely Jamesian view – if it successfully facilitates and enables activities (be they epistemic or not). If the billiard-ball model of Brownian motion helps scientists to predict the behaviour of gas molecules, for example, the model is (pragmatically) true. The falseness of the presumption of perfectly spherical molecules does not matter. Is the Jamesian conception of truth more congenial to science, and more conducive to pluralism in democratic societies?

The risk with a James-inspired conception of truth, as I see it, is that it is too malleable to resist the tides of time and the stresses of social forces endlessly at work in science. A James-inspired view of truth abdicates the expectation that science tells us the truth in the name of a non-better-qualified kind of success of a scientific practice. But how to tell apart cases where success does indeed track truth from cases where it does not? More to the point, when it comes to matters such as climate change, the benefit of vaccinating children, or economic forecasts, we seem to need more than a malleable Jamesian conception of truth for the sake of scientifically informed decisions that do not bow to pressure from powerful lobbies and political agendas (in the name of what 'might work'). But, someone might reply, how can truth and pluralism go hand in hand if not by opting for a Jamesian conception of truth (if we really care about truth at all)?

There is another way of thinking about how truth and pluralism might go hand in hand, without reducing matters of truth to calculations of what is pragmatically good to individuals or communities sharing a scientific perspective at some point in time. First, it is necessary to understand the key term 'scientific perspective' and how it impinges on scientific pluralism. In its original use by the philosopher Ronald Giere in 2006, 'scientific perspective' is akin to Kuhn's disciplinary matrix: a set of scientific models (including the relevant experimental instruments to gather data). In broader terms, scientific perspective is the disciplinary practice of a real scientific community at any given historical time. It includes the knowledge they produce, and the theoretical, technological and experimental resources they use, or that guide their work.

> Truth is a normative commitment inherent in scientific knowledge

Thus, in physics, one can speak of the Newtonian perspective vs the Einsteinian perspective; in chemistry, the Priestley vs Lavoisier perspective; in economics, the Keynesian vs the monetarist perspective; or the classical-genetics vs molecular-genetics perspective in biology, and so forth. What is to be said about the flourishing array of scientific perspectives? One thing is certain: science works through producing a plurality of perspectives. Over time, it leads to scientific progress, a process Comte mistakenly theorised into a monist picture of 'positive science'. But a plurality of scientific perspectives does not solve the problem of truth. On the contrary, it invites worrisome thoughts that truth is either redundant to scientific perspectives; or, worse, relativised to perspectives. One might ask again: *whose truth?* By *whose* lights?

The time for a defence of truth in science has come. It begins with a commitment to *get things right*, which is at the heart of the realist programme, despite mounting Kuhnian challenges from the history of science, considerations about modelling, and values in contemporary scientific practice. In the simple-minded sense, getting things right means that things are as the relevant scientific theory says that they are. Climate science is true if what it says about CO2 emissions (and their effects on climate change) corresponds to the way that things are in nature. For the sake of powerful economic interests, sociopolitical consequences or simply different economic principles, one can try to discount, mitigate, compensate for, disregard or ignore altogether the way that things are. But doing so is to forgo the normative nature of the realist commitment in science. The scientific world, we have seen, is too complex and messy to be represented by any quasi-Wittgensteinian picture of atomic facts. Nor can the naive image of Comte's positive science render justice to it. But acknowledging complexity and historical nuances gives no reason (or justification) for forgoing truth altogether; much less for concluding that science trades in falsehoods of some kind. It is part of our social responsibility as philosophers of science to set the record straight on such matters.

We should expect science to tell us the truth because, by realist lights, this is what science ought to do. Truth – understood as getting things right – is not the aim of science, because it is not what science (or, better, scientists) should aspire to (assuming one has realist leanings). Instead, it is what science *ought* to do by realist lights. Thus, to judge a scientific theory or model as true is to judge it as one that 'commands our assent'. Truth, ultimately, is not an aspiration; a desirable (but maybe unachievable) goal; a figment in the mind of the working scientist; or, worse, an insupportable and dispensable burden in scientific research. Truth is a normative commitment inherent in scientific knowledge.

Constructive empiricists, instrumentalists, Jamesian pragmatists, relativists and constructivists do not share the same commitment. They do not share with the realist a suitable notion of 'rightness'. As an example, compare the normative commitment to *get things right* with the view of the philosopher Richard Rorty, in whose hands Putnam's truth as 'idealised *warranted assertibility*' reduces to what is acceptable to '*us* as we should like to be … us educated, sophisticated, tolerant, wet liberals, the people who are always willing to hear the other side, to think out all the implications'. *Getting things right* is not a norm about us at our best, 'educated, sophisticated, tolerant, wet liberals'. It is a norm inherent in scientific knowledge. To claim to *know* something in science (or about a scientific topic or domain) is to claim for the *truth* of the relevant beliefs about that topic or domain. But, a critic might reply, how can our knowledge – which is perspectival, entrenched in historically and culturally well-defined scientific practices – track the way the world is? How can we be expected to *truthfully* believe the things we believe in science, despite being situated in a plurality of scientific perspectives? To put it in a different way, how can we – historically and culturally situated epistemic agents – reliably build scientific knowledge over time, despite the possible errors and false steps of past (and current) scientific perspectives?

Thinking of truth as a normative commitment inherent in the very notion of scientific knowledge brings some benefits. It overcomes a false dichotomy between atomic facts and non-factive, non-truth-conducive inferences. And it makes realism compatible with perspectivism. Scientific communities that endorse historically and culturally situated scientific perspectives (either across the history of science or in contemporary science, across different fields or different scientific programmes) share (and indeed ought to) a normative commitment to *get things right*. That is a minimum requirement to pass the bar of what we count as 'scientific knowledge'.

Since the 16th century, the method of science has been at the centre of an important debate. The hope was that a method (if any could be identified) could lead to conclusions that ought to be shared, communicated, agreed upon. But we learned that there is no single scientific method, some magic recipe valid always and everywhere, across disciplines. A plurality of scientific perspectives is not however a royal road to some worrisome kind of perspectival solipsism, thanks to the normative realist commitment that scientific perspectives ought to (and typically do) share.

Scientists endorsing different scientific perspectives (and I stress the adjective *scientific* here to undercut a possible rejoinder that might trade on the ambiguity of the word 'perspective') do not construct perspectival facts. To do so would be to possibly commit scientific fraud. Rather, they ought to share a commitment to the same tribunal of evidence. They might adopt different experimental strategies, different models, different theories. They might endorse different values about what really matters in a given field. But it is the tribunal of evidence that they all ultimately ought to respond to. *Getting the evidence right*, in the first instance – via accurate measurements, sound non-ad-hoc procedures, and robust inferential strategies – defines any research programme that is worth being called 'scientific'. The realist commitment to get things right must begin with getting the evidence right. No perspective worthy of being called 'scientific' survives fudging the evidence, massaging or altering the data or discarding evidence.

Scientists endorsing different scientific perspectives do not concoct the scientific standards by which their knowledge claims, and their resilience over time, are assessed. To do so would run the risk of partiality, or ad-hoc manoeuvring. Obviously, the notion of 'scientific standard' is complex and one has to tread carefully. Nonetheless, by and large (without any presumption of producing an exhaustive list), scientists ought to share a commitment to common scientific standards. They might disagree about what counts as a 'simpler' theory (eg, introducing new entities vs modifying laws of nature); they might place a different emphasis on the role of consistency vs explanatory power when it comes to modelling at different scales. But they ought to agree that any viable scientific theory must be able to make sense of the available evidence broadly along the lines of those (among many other possible) scientific standards.

> Truth is not a commodity disposable with the old scientific paradigm

Scientists ought to share rules for cross-perspectival assessment. That our knowledge is situated and perspectival does not make scientific truths relativised to perspectives. Often enough, scientific perspectives themselves provide the rules for cross-perspectival assessment. Those rules can be as simple as translating the 10 degree Celsius temperature in Edinburgh today into the 50 degree equivalent on the Fahrenheit scale. Or they can be as complex as retrieving the viscosity of a fluid in statistical mechanics, where fluids are treated as statistical ensembles of a large number of discrete molecules. At other times, scientists might not necessarily possess a rule-book, so to speak, for 'translating' knowledge claims from one scientific perspective into another. In those situations, we need something like what Kuhn called 'bilingualism' (rather than translation).

Even in genuine scientific revolutions, when old taxonomies fall and new, previously undreamt ones, take their place, truth is not a commodity disposable with the old scientific paradigm. Critics of Kuhn – who emphasised continuities during periods of crises and scientific revolutions, and paid attention to the subtle nuances that scientific concepts undergo across centuries – have a point here. Namely, that it is our commitment to get things right across perspectives (indeed across major conceptual upheavals) that allows communities (before and after a revolution) to understand one another and to grow in their scientific knowledge – no matter how different and perspectival the local idioms in which their scientific claims might be couched.

Galileo, trying to escape the Inquisition, arrived at a new concept of *gravitas* that eventually advanced the new lexicon of Copernicanism against the backdrop of century-long criticisms of Aristotle's theory of motion, elaborated by Jean Buridan and Nicole Oresme in 14th-century Paris, and before them by the Medieval Arabic commentators of Aristotle (the so-called Baghdad school, and Abu'l-Barakāt's almost prescient study of accelerated motion). Lavoisier, a tragic victim of the French Revolution, knew how to use the old apothecary's concept of *weight* to advance his critique of phlogiston theory and to usher in the new concept of oxygen and a new role for chemistry. Chinese knowledge about how to use saltpeter for making gunpowder entered Europe through the Silk Road and made possible Robert Boyle and Isaac Newton's highly influential speculative experimentalism about 'true permanent air' being released in explosions, and an entire research programme for the study of fire and electricity in the British and Dutch natural philosophy of the 18th century (in addition to its use in pyrotechnic arts). And as Sandra Harding has argued in *Objectivity and Diversity* (2015), Micronesian knowledge about navigation systems has a lot to teach us today.

Let there be no doubt: scientific knowledge is the product of our *getting it right* across our perspectival multicultural scientific history. Scientific knowledge is not a prerogative of our Western cultural perspective (and its discipline-specific scientific perspectives) but the outcome of a plurality of historically and culturally situated scientific perspectives that, over millennia, have reliably produced knowledge with the tools, resources and concepts respectively available to each and every one of them.

Communities of epistemic agents learn how to *get things right* across time, across historically and culturally situated scientific perspectives. Not because there are atomic facts as truth-makers of atomic propositions, but because the perspectival nature of our scientific knowledge resembles what Wittgenstein called the thread that 'we twist fibre on fibre. And the strength of the thread resides not in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres.' Scientific truths are the resilient and robust outcome of a plurality of scientific perspectives that, over time, have meshed with one another in their (tacit, implicit and often survival-adaptive) normative commitment to reliably produce scientific knowledge *for us as humankind*. That is why, far from being an insufferable hindrance to scientific pluralism, truth is in fact its best safeguard in tolerant, open and democratic societies that are genuinely committed to the advancement of scientific knowledge in the very many faces it comes with.

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