# *Scientific explanation*

PAPERS BASED ON HERBERT SPENCER LECTURES GIVEN IN THE UNIVERSITY OF OXFORD

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# 1 *Thematic presuppositions and the direction of scientific advance*

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'I wish to preface what I have to say by expressing to you the great gratitude which I feel to the University of Oxford for having given me the honour and privilege of delivering the Herbert Spencer Lecture.'

With these words, surely echoed by every speaker in this series, Albert Einstein opened his lecture on 10 June 1933. By that time he was a man without a country, passing through this haven as a refugee from Fascism, as so many others, illustrious or unknown, were to do after him. Like them, he retained a warm and thankful memory of the hospitality here.

Philipp Frank, his biographer and colleague, called Einstein's lecture the 'finest formulation of his views on the nature of a physical theory'.[1] The published version[2] has been rarely analysed or even adequately understood. Now that we have access to so many more of Einstein's published and unpublished documents, the essay turns out to be a very appropriate entry for a study of scientific explanation, both of Einstein's own contribution to the subject and of more recent approaches.

## The 'eternal antithesis'

Einstein's choice of 'the method of theoretical physics' as his topic was by no means casual. In fact, for much of his life he seems to have been almost obsessed by the need to explain what he called his epistemological credo. From about 1911 to the end, he wrote on it again and again, almost as frequently as on physics itself. On occasions great and small, he reverted to his self-appointed task in his remarkably consistent way—with the single-minded patience of a hedgehog, and the glorious stubbornness that characterized him from his boyhood on, when his family watched him at one of his favourite activities, making with infinite concentration fantastic houses of cards that had as many as ten levels.

His home-made philosophical system of the practising scientist, of which he wrote so often, seemed to his philosophical commentators something of a house of cards too, a patchwork of pages from Hume, Kant, Ernst Mach, Henri Poincaré, and many others. Indeed, Einstein himself cheerfully acknowledged once that he might appear 'as a type of unscrupulous opportunist', appearing by turns as a realist, idealist, positivist, or even Platonist or Pythagorian. Yet the method he preached and practised turned out to be remarkably robust. Many of today's physicists, without knowing its origin, have adopted a style of attempting fundamental and daring advances that owes a great deal to Einstein's credo, even as Einstein's dream of finding a unification of the forces of nature has, in its modern form, turned out to be the stuff of which Nobel prizes are made.

In his own day, however, Einstein had good reason to suspect that few physicists and philosophers understood what he was saying about scientific methodology, or even could describe clearly what they themselves were doing. And so, rather like Galileo, he took his epistemological message to the wider public. He opened the formal part of his Herbert Spencer lecture with the famous sentence: 'If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds.'

Here he objects to scientists who speak about the products of their imaginations as if these were 'necessary and natural'—not 'creations of thought' but 'given realities'. To expose their mistake, he invites us to pay 'special attention to the relation between the content of a theory' on the one hand, and 'the totality of empirical facts' on the other. These constitute the two 'components of our knowledge', the 'rational' and the 'empirical'; these two components are 'inseparable'; but they stand also, Einstein warns, in 'eternal antithesis'.

To support this conception, Einstein now gives a very brief sketch of a dichotomy built into Western science. The Greek philosopher-scientists provided the necessary confidence for the achievements of the human intellect by introducing into Western thought the 'miracle of the logical system', which, as in Euclid's geometry, 'proceeds from step to step with such precision that every single one of its propositions



was absolutely indubitable'. But 'propositions arrived at by purely logical means are completely empty as regards reality'; 'through purely logical thinking we can attain no knowledge whatsoever of the empirical world'. Einstein tells us that it required the seventeenth-century scientists to show that scientific knowledge 'starts from experience and ends with it'.

It seems therefore that we are left with a thoroughly dualistic method for doing science: on the one hand, Einstein says, 'the structure of the system is the work of reason'; on the other hand, 'the empirical contents and their mutual relations must find their representation in the conclusions of the theory'. Indeed, virtually all of Einstein's commentators have followed him in stressing this dualism—and leaving it at that. For example, F. S. C. Northrop summarized the main content of Einstein's Oxford lecture in these words: An 'analysis of Einstein's conception of science shows that scientific concepts have two sources for their meanings: The one source is empirical. It gives concepts which are particulars, nominalistic in character. The other source is formal, mathematical and theoretical. It gives concepts which are universals, since they derive their meaning by postulation from postulates which are universal propositions.'[3]

This is a view of science (even of Einstein's science) of which there are many versions and variants. I would call it a two-dimensional view. It can be defended, up to a point. All philosophies of science agree on the meaningfulness of two types of statements, namely propositions concerning empirical matters that ultimately boil down to meter readings and other public phenomena, and propositions concerning logic and mathematics that ultimately boil down to tautologies. The first of these, the propositions concerning empirical matters of fact, can in principle be rendered in protocol sentences in ordinary language that command the general assent of a scientific community; I like to call these the *phenomenic propositions*. The second type of propositions, meaningful in so far as they are consistent within the system of accepted axioms, can be called *analytic propositions*. As a mnemonic device, and also to do justice to Einstein's warning about the 'eternally antithetical' nature of these propositions, one may imagine them as lying on a set of orthogonal axes, representing the two dimensions of a plane within which scientific discourse usually takes place.

Now it is the claim of most modern philosophies of science which trace their roots to empiricism or positivism, that any scientific statement has 'meaning' only in so far as it can be shown to have phenomenic and/or analytic components in this plane. And indeed, in the past, this Procrustean criterion has amputated from science its innate properties, occult principles, and all kinds of tantalizing questions for which the consensual mechanism could not provide answers. A good argument can be made that the silent but general agreement to keep the discourse consciously in the phenomenic–analytic plane where statements can be shared and publicly verified or falsified is a main reason why science has been able to grow so rapidly in modern times. The same approach also characterizes the way science is taught in most classrooms, and is 'rationalized' in most of the current epistemological discussions.

## Problems for the two-dimensional view

Nevertheless, this two-dimensional view has its costs. It overlooks or denies the existence of active mechanisms at work in the day-to-day experience of those who are actually engaged in the pursuit of science; and it is of little help in handling questions every historian of science has to face consciously, even if the working scientist, happily, does not. To illustrate, let me mention two such puzzles. Both have to do with the direction of scientific advance, and both will seem more amenable to solution once the dualistic view is modified.

1. If sound discourse is directed entirely by the dictates of logic and of empirical findings, why is science not one great totalitarian engine, taking everyone relentlessly to the same inevitable goal? The laws of reason, the phenomena of physics, and the human skills to deal with both are presumably distributed equally over much of the globe; and yet the story of, say, the reception of Einstein's theories is strikingly different in Germany and England, in France and the United States. On the level of *personal* choice of a research topic, why were some of Einstein's contemporaries so fatally attracted to ether-drift experiments, whereas he himself, as he put it to his friend de Haas, thought it as silly and doomed to failure as trying to study dreams in order to prove the existence of ghosts? As to skills for navigating in the two-dimensional plane, Einstein and Bohr were rather well matched, as were Schrödinger and Heisenberg. And yet there were fundamental antagonisms in terms of programmes, tastes, and beliefs, with occasional passionate outbursts between scientific opponents.

Or, again, how to understand the great variety of different personal styles? The physicist Edwin C. Kemble described his typical mode of work, with some regret, as the building of a heavy cantilevered bridge, each piece painstakingly anchored on a well-secured base. Robert Oppenheimer, on the other hand, one might think of as a spider building a web; individual extensions were achieved by daring leaps, and the resulting structures were intricate and shimmering with beauty, but perhaps a bit fragile. Enrico Fermi, whom many regard as the inventor of teamwork in modern physics, ran his laboratory like a father who had assembled around himself a group of very bright offspring.



And then there is the scientist who moves through his problem-area alone, as the fur trapper did through Indian territory. Bernard DeVoto described it in his book *Across the wide Missouri*. The trapper 'not only worked in the wilderness. He also lived there. And he did so from sun to sun by the exercise of total skill'. Learning how to read formal signs was of course essential to him, but more important was 'the interpretation of observed circumstances too minute to be called signs. A branch floats down a stream–is this natural, or the work of animals, or of Indians or trappers? Another branch or a bush or even a pebble is out of place–why? . . . Buffalo are moving down wind, an elk is in an unlikely place or posture, too many magpies are hollering, a wolf's howl is off key–what does it mean?'

What indeed does all this variety of scientific styles mean? If science *were* two-dimensional, the work in a given field would be governed by a rigid, uniform paradigm. But the easily documented existence of pluralism at all times points to the fatal flaw in the two-dimensional model.

2. A second question that escapes the simple model, and to which I have devoted a number of case studies in recent years, is this: why are many scientists, particularly in the nascent phase of their work, willing to hold firmly, and sometimes at great risk, to a form of 'suspension of disbelief' about the possibility of falsification? Moreover, why do they do so sometimes without having any empirical evidence on their side, or even in the face of disconfirming evidence?

Among countless examples of this sort, Max Planck, responsible for the idea of the quantum but one of the most outspoken opponents of its corpuscular implications, cried out as late as May 1927 'Must we really ascribe to the light quanta a physical reality?–and this four years after the publication and verification of Arthur H. Compton's findings. On the other hand, when it came to explaining the electron in terms of what Planck called 'vibrations of a standing wave in a continuous medium', along the lines proposed by de Broglie and Schrödinger, Planck gladly accepted the idea and added that these principles have already [been] established on a solid foundation'–and all that before Planck had heard of any experimental evidence along the lines provided by Davisson and Germer.

## 'I do not doubt at all . . .'

Einstein was even more daring. As I have documented elsewhere, straight after the publication of his 1905 relativity paper there appeared what purported to be an unambiguous experimental disproof of it by the most eminent experimentalist in the field, Walter Kaufmann. If Einstein had been a naïve believer in such notions as falsification criteria or regressive research programmes, he would have had to accept this widely noted disproof from that undoubted source, and turned to other things. For the published data showed that the electrons' motion fitted ether-based theories far better than Einstein's. Yet Einstein paid no attention whatever, and continued to publish as if nothing had happened. When the young man was finally persuaded to respond to the challenge, he dismissed the supposed disproof with a characteristic declaration: The ether-based theories 'have a rather small probability, because their fundamental assumptions concerning the mass of moving electrons are not explainable in terms of theoretical systems which embrace a greater complex of phenomena.' (It took ten years for it to be fully realized that, for once, the prominent experimenter had been working with quite inadequate equipment. By that time, the matter had been settled on other grounds, as it is so often.)

Later, when the gravitational red shift, predicted by general relativity theory for the spectral lines from stars with large masses, turned out to be very difficult to test, and the experimental results were neither systematic nor of the predicted amount, Einstein again simply waited it out. To Max Born he wrote later that, even in the absence of all three of the originally expected observable consequences of general relativity, his central gravitation equations 'would still be convincing', and that in any case he deplored that 'human beings are normally deaf to the strongest [favourable] arguments, while they are always inclined to overestimate measuring accuracies'.

To be sure, if one looks hard, one can find in Einstein's voluminous writings a small number of statements of the opposite kind. An example of this sort, written shortly after the triumphant announcement of Eddington's results late in 1919, is one sentence in the 1920 edition of Einstein's popular exposition, *Relativity, the special and general theory*: 'If the red shift of spectral lines due to the gravitational potential should not exist, then the general theory of relativity will be untenable.' Sir Karl Popper, in his recent *Autobiography*, indicates that his own falsifiability criterion owed at its origin much to what he perceived to be Einstein's example, and he cites this specific sentence, which he says he read with profound effect when he was still in his teens.



Those of us who have admired Sir Karl's work can only be grateful that he came upon Einstein's sentence in the 1920 edition that helped set him on his path. In its earlier editions and frequent printings of 1917, 1918, and 1919, Einstein's book had ended very differently. There, Einstein acknowledged that his general relativity theory so far had only one observable consequence, the precession of the orbit of Mercury, whereas the predicted bending of light and of the red shift of spectral lines owing to the gravitational potential were too small to be then observed. Nevertheless, Einstein drew this conclusion, in a sentence which concluded his book in its first fifteen printings: 'I do not doubt at all that these consequences of the theory will also find their confirmation.'

## Suspension of disbelief

To illustrate that Einstein is not so different from other scientists when it comes to the willingness to suspend disbelief, it will be worth making an excursion to watch how an experimentalist of great skill went about his business in much the same way, but in the privacy of his laboratory. Some time ago I came across the laboratory notebooks of R. A. Millikan, containing the raw data from which he derived his measured value of the basic unit of electric charge, the electron. Millikan's earlier attempts in this direction had been quite vulnerable, and had come under bitter attack from a group of research physicists at the University of Vienna, chiefly Felix Ehrenhaft, who believed not in a unitary but in a divisible electron, in subelectrons carrying charges such as one-fifth, one-tenth, or even less of the ordinary electron. Now, in gearing up his response in 1911–12, Millikan had two strong supports for his counter-attack. One was his unflagging preconception that there is only one 'electrical particle or atom', as he put it, a doctrine he believed to have been proposed first and convincingly by Benjamin Franklin. His other support was the kind of superb skill described in the passage quoted from Bernard DeVoto's book.

Millikan's publication came in the August 1913 issue of the *Physical*

*Review*, and effectively ended the scientific portion of the controversy. It contains data for 58 different oil drops on which he has measured the electric charge. He assures his readers, in italics: '*It is to be remarked, too, that this is not a selected group of drops, but represents all of the drops experimented on during 60 consecutive days.*' Four years later, in his book *The electron*, Millikan repeats this passage, and all the data from the 1913 paper, and he adds for extra emphasis: 'These [58] drops represent all of those studied for 60 consecutive days, no single one being omitted.'

At the Millikan Archive of the California Institute of Technology, the laboratory notebooks are kept from which the published data were derived. If we put our eye to that key-hole in the service of the ethology of science, we find there were really 140 identifiable runs, made over a period of six months, starting in October 1911. Anyone who has done research work in a laboratory cannot help but be impressed by the way Millikan handles his data, and by the power of a presupposition shrewdly used.

To prepare for the proof from Millikan's laboratory records, let me remind you of the chief point of Millikan's oil drop experiment. In a simplified form that nevertheless retains the scientific essentials as well as its beauty and ingenuity, it is now a standard exercise in the repertoire of school physics. A microscopic oil droplet is timed as it falls through a fixed distance in the view field. It will have some net electric charge to begin with, if only owing to the friction that acted on it when it was initially formed and expelled from the vaporizer. Other electric charges may be picked up from time to time as the droplet encounters ionized molecules in the gas through which it falls. Neither of these charges influences the droplet's motion, so long as it falls freely in the gravitational field. But when an electric field of the right sign and magnitude is suddenly applied, the drop will reverse its course, and will rise the more rapidly the larger the electric charge on it. Comparing the times taken for falling and subsequent rising allows one to calculate the net charge owing to friction on the droplet, $q_{fri}$, while comparing the times for alternate risings yields the net charge owing to the encounter with gas ions, $q_{ion}$.

As one watches the same droplet over a long time, through its many up and down excursions, one can accumulate a large number of values for $q_{fri}$ and $q_{ion}$. Now the fundamental assumption Millikan makes throughout his work is that $q_{fri}$ as well as $q_{ion}$ are always some integral multiple of a unit charge equal in magnitude to the charge of the electron,



$e$. Conversely, from the full set of data, he can determine the magnitude of $e$ which is common to all of the values obtained for $q_{fri}$ and $q_{ion}$, both being assumed to be always equal to 1, or 2, or 3 . . . × $e$. These assumptions become plausible when the scatter of values for $e$ turns out to be small when computed from either $q_{fri}$ or $q_{ion}$—and when the mean values of $e$, so differently based, are nevertheless closely equal for a given droplet.

This is just what happens for the 58 'runs' or droplets discussed in the August 1913 paper of Millikan. One of the runs made on the Ides of March, 1912, and recorded in Millikan's laboratory notebook, is typical.[4] The difference between the values of $e$, computed on the two different bases, is only about 0.1 per cent, and not far from the limits set by the apparatus itself. The page on which both the data and the calculations appear records Millikan's exuberance and pleasure in the lower left corner: 'Beauty. *Publish* this surely, *beautiful*!'

Millikan continued immediately to take data on another oil droplet, entering the data on the next page. This time things did not go well. It was now a heavier drop, hence its time of fall was shorter. The numbers of charges it picked up as it went along were not greatly different, and it did not stay in view as long as one would have liked. Now the difference between the average values of $e$, calculated from $q_{fri}$ and $q_{ion}$ respectively, were 1 per cent apart, instead of 0.1 per cent. So Millikan notes in his private laboratory book on that page: '*Error high* will not use',—and indeed it does not appear among the 58 droplets that made it into the final paper. From Millikan's point of view, it was a failed run, or, in effect, no run at all. The magnitude of the difference in the values of $e$ obtained in those two ways was awkwardly large, although not so surprising as to threaten Millikan's fundamental assumptions. Instead of wasting time, he simply went on to the next set of readings with another droplet.

But the discarded set of observations—and many others like it in the same laboratory notebook—would have appeared very differently if examined from another set of presuppositions. Thus, the entries make excellent sense if one assumes that the smallest charge involved in the oil drop experiment is not $e$, but, say, $1/10\,e$. In that case, the number of charges on a given droplet would not have been, in succession, 11, 13, and 14, as Millikan had to assume, but could have been 109, 129, and 139; and correspondingly, the difference between the (now smaller) elementary charges obtained in the two ways would be of the order of 0.1 per cent, instead of Millikan's 1 per cent. The 'high' error was the direct result of Millikan's assumption that the smallest charge in nature could not be a fraction of the charge of the electron $e$, as also determined (although more indirectly) by different methods in many other branches of physics.

Millikan's decisions seem to us now eminently sensible; but the chief point of the story is that, in 1912, Millikan's assumption of the unitary nature of the electric charge was by no means the only one that could be made. On the contrary, a chief reason for his work at the time was to perfect his method and support his claim against the constant onslaught of Felix Ehrenhaft and his associates who, for a couple of years, had been publishing experiments in support of their own, precisely opposite presupposition, namely in favour of the existence of *sub*-electrons.

It is also part of the historical setting that, at the time, Millikan was really just beginning belatedly on his career as a research physicist, whereas Ehrenhaft–at a venerable and much better equipped university –had begun to be widely recognized and rewarded years earlier as a fast-rising star in experimental physics. It was only after losing the argument with Millikan, and probably as a result of it, that he began a rapid decline as a scientist. When Millikan was doing his experiments, the matter was still in the balance. If Ehrenhaft had had access to Millikan's notebook, he would have found for his purposes precisely those runs most valuable which, for Millikan, were 'failed'.

Conversely, Millikan's own presupposition helped him to identify difficulties of the usual experimental nature which he did not feel were worth following up. For many of those he entered a plausibility argument on the spot (e.g. that the battery voltages must have changed, convection interfered, the stop-watch might be in error). The laboratory notebooks record Millikan's frank comments in such cases. The most revealing of the lot–revealing both of Millikan's insights that dust particles might intrude in the observation chamber, and of the willingness to take risks on behalf of his presupposition–is a marginal note entered for a long run that yielded a value of $e$ far outside the expected limit of error: '$e = 4.98$ which means that this could not have been an oil drop.'

Like the trapper in Indian country, he was advancing on dangerous territory, but with a framework of beliefs and assumptions within which judgments are possible. The chief gain was the avoidance of costly interruptions and delays that would have been required to pin down the exact causes of discrepant observations. Obviously, this is not a method we recommend to our beginning students. But obviously also, any discussion of the advance of science that does not recognize the role of the suspension of disbelief at crucial points is not true to the activity.[5]



## Towards a third mechanism

Einstein would not have been surprised by Millikan's notebook. Perhaps because of his experience with Kaufmann, he took a dim view of new experiments that, like Ehrenhaft's, made strong claims not explainable in terms of theoretical systems which embrace a greater complex of phenomena. Very early in his career, Einstein had, it seems to me, formed a clear view about the basic structure of nature: at the top there is a small number of eternal, general principles or laws by which nature operates. These are not easy to find–partly because God is subtle, and partly because they do not stop at the boundaries between fields that happen to be occupied by different theories.

Below this upper layer of a few grand laws lies a layer of experimental facts–not the latest news from the laboratory, but hard-won, well-established, aged-in-the-bottle results, many going back to Faraday and Fresnel, and now indubitable. These experiences or key phenomena are the necessary consequences of the visible compliance with the general laws.

But between these two solid levels is the uncertain and shifting region of concepts, theories, and recent findings. They deserve to be looked at, but sceptically; they are man-made, limited, fallible, and if necessary, disposable. Einstein's attitude was perhaps best expressed in a remark reported to me by one of his colleagues in Berlin, the physical chemist Herman F. Mark: 'Einstein once told me in the lab: "You make experiments and I make theories. Do you know the difference? A theory is something nobody believes except the person who made it, while an experiment is something everybody believes except the person who made it".'

What, then, must one conclude from Kaufmann's fatal predisposition for the ether; Max Planck's predisposition for the continuum and against discreteness; Robert Millikan's predisposition for a discrete rather than a divisible electron; Einstein's predisposition for a theory that encompasses a wide rather than a narrow range of phenomena–all in the face of clear and sometimes overwhelming difficulties? These cases–which can be matched and extended over and over again–show that some *third mechanism* is at work here, in addition to the phenomenic and analytical. And we can find it right in Einstein's lecture on the method of theoretical physics: the two-dimensional model in it, which first strikes the eye, gives way on closer examination to a more sophisticated and appropriate one. In addition to the two inseparable but antithetical components there is indeed a third—not as clearly articulated here as in some others of Einstein's essays, but present nevertheless. The arguments for it float above the plane bounded by the empirical and logical components of the theory.

Einstein launches on it by reminding his audience, as he often did, that the previously mentioned phenomenic-analytic dichotomy prevents the principles of a theory from being 'deduced from experience' by 'abstraction'—that is to say, by logical means. 'In the logical sense [the fundamental concepts and postulates of physics are] free inventions of the human mind', and in that sense different from the unalterable Kantian categories. He repeats more than once that the 'fundamentals of scientific theory' are of 'purely fictitious character'. As he puts it soon afterwards, in the essay 'Physics and reality' (1936), the relation between sense experience and concept 'is analogous not to that of soup to beef, but rather to that of check number to overcoat.' The essential arbitrariness of reference, Einstein explains in the Spencer Lecture, 'is perfectly evident from the fact that one can point to two essentially different foundations'—the general theory of relativity, and Newtonian physics—'both of which correspond with experience to a large extent'—namely, with much of mechanics. The elementary experiences do not provide a logical bridge to the basic concepts and postulates of mechanics. Rather, 'the axiomatic basis of theoretical physics . . . must be freely invented.'

But if this is true, an obvious and terrifying problem arises, and Einstein spells it out. He writes: How 'can we ever hope to find the right way? Nay, more, has this right way an existence outside our illusions? Can we hope to be guided safely by experience at all when there exist theories such as classical mechanics, which do justice to experience to a large extent, but without grasping the matter in a fundamental way?'

We have now left the earlier, confident portion of Einstein's lecture far behind. The question raises itself whether the activities of scientists can ever hope to be cumulative, or whether we must stagger from one fashion, conversion, or revolution to the next, in a kind of perpetual, senseless Brownian motion, without direction or *telos*.



At that point, Einstein issues a clarion call: 'I answer with full confidence that there is, in my opinion, a right way, and that we are capable of finding it.' Here, Einstein goes suddenly beyond his earlier categories of empirical and logical efficacy, and offers us a whole set of selection rules with which, as with a good map and compass, that 'right way' may be found. Here, there, everywhere, guiding concepts emerge and beckon from above the previously defined plane to point us on the right path.

The first directing principle Einstein mentions is his belief in the efficacy of formal structures: The 'creative principle resides in mathematics'—not, for example, in mechanical models. On the next page, there unfolds itself a veritable hymn to the guiding concept of simplicity. Einstein calls it 'the Principle of searching for the mathematically simplest concepts and their connections', and he cheers us on our way with many examples of how effective it has already proved to be: 'If I assume a Riemannian metric [in the four-dimensional continuum] and ask what are the *simplest* laws which such a metric can satisfy, I arrive at the relativistic theory of gravitation in empty space. If in that space I assume a vector field or anti-symmetrical tensor field which can be derived from it, and ask what are the simplest laws which such a field can satisfy, I arrive at Maxwell's equations for empty space'; and so on, collecting victories everywhere under the banner of simplicity.

And over there, at the bottom of another page, we find two other guiding concepts in tight embrace: the concept of parsimony, or economy, and that of unification. As science progresses, Einstein tells us, 'the logical edifice' is more and more 'unified', the 'smaller the number [is] of logically independent conceptual elements which are found necessary to support the whole structure.' Higher up on that same page, we encounter nothing less than 'the noblest aim of all theory', which is 'to make these irreducible elements as simple and as few in number as is possible, without having to renounce the adequate representation of any empirical content'.

Yet another guiding concept given in Einstein's lecture concerns the *continuum*, the field. From 1905 on, when the introduction of discontinuity in the form of the light quantum forced itself on Einstein as a 'heuristic' and therefore not fundamental point of view, he clung to the hope and programme to keep the continuum as a fundamental conception, and he defended it with enthusiasm in his correspondence. It was part of what he called his 'Maxwellian programme' to fashion a unified field theory. Atomistic discreteness and all it entails was not the solution but rather the problem. So here, in his 1933 lecture, he again considers the conception of 'the atomic structure of matter and energy' to be 'the great stumbling block for a unified field theory'.

One cannot, he thought, settle for this basic duality in nature, giving equal status both to the field and to its antithesis. Of course, neither logic nor experience forbade it. Yet, it was almost unthinkable. As he once wrote to his old friend, Michel Besso, 'I concede . . . that it is quite possible that physics might not, finally, be founded on the concept of field—that is to say, on continuous elements. But then out of my whole castle in the air—including the theory of gravitation and most of current physics—there would remain almost nothing.'

We have by no means come to the end of the list of presuppositions which guided Einstein. But it is worth pausing to note how plainly he seemed to have been aware of their operation in his scientific work. In this too he was rare. Sir Isaiah Berlin, in his book *Concepts and categories* [p. 159], remarked: 'The first step to the understanding of men is the bringing to consciousness of the model or models that dominate and penetrate their thought and action. Like all attempts to make men aware of the categories in which they think, it is a difficult and sometimes painful activity, likely to produce deeply disquieting results.' This is generally true; but it was not for Einstein. There are surely at least two reasons for that. It was, after all, Einstein who realized the 'arbitrary character' of what had for so long been accepted as 'the axiom of the absolute character of time, viz., of simultaneity [which] unrecognizedly was anchored in the unconscious', as he put it in his *Autobiographical notes*. 'Clearly to recognize this axiom and its arbitrary character really implies already the solution of the problem.' (Giving up an explicitly or implicitly held presupposition has indeed often had the characteristic of the great sacrificial act of modern science; we find in the writings of Kepler, Planck, Bohr, and Heisenberg that such an act is a climax of a period that in retrospect is characterized by the word 'despair'.)

Having recognized and overcome the negative, or enslaving, role of presuppositions, Einstein also saw their positive, emancipating potential. In one of his early essays on epistemology ('*Induction and deduction in physics*', 1919), he wrote: 'A quick look at the actual development teaches us that the great steps forward in scientific knowledge originated only to a small degree in this [inductive] manner. For if the researcher went about his work without any preconceived opinion, how should he be able at all to select out those facts from the immense abundance of the most complex experience, and just those which are simple enough to permit lawful connections and become evident?'



In essay after essay, Einstein tried to draw attention to this point of view, despite—or because of—the fact that he was making very few converts. The Herbert Spencer lecture can be seen as part of that mission. A decade and a half later, in his 'Reply to criticisms', we see him continuing in this vein. Thus, he acknowledges that the distinction between 'sense impressions' on the one hand, and 'mere ideas' on the other, is a basic conceptual tool for which he can adduce no convincing evidence. Yet, he needs this distinction. His solution is simply to announce, 'we regard the distinction as a category which we use in order that we might the better find our way in the world of immediate sensation.' As with other conceptual distinctions for which 'there is also no logical-philosophical justification', one has to accept it as 'the presupposition of every kind of physical thinking', mindful that 'the only justification lies in its usefulness. We are here concerned with "categories" or schemes of thought, the selection of which is, in principle, entirely open to us and whose qualification can only be judged by the degree to which its use contributes to making the totality [*sic*] of the contents of consciousness "intelligible".' Finally, he curtly dismisses an implied attack on these 'categories' or 'free conventions' with the remark that 'Thinking without the positing of categories and of concepts in general would be as impossible as is breathing in a vacuum.'

## The thematic dimension

His remarkable self-consciousness concerning his fundamental presuppositions throughout his scientific and epistemological writings allows one to assemble a list of about ten chief presuppositions underlying Einstein's theory construction: primacy of formal (rather than materialistic or mechanistic) explanation; unity or unification; cosmological scale in the applicability of laws; logical parsimony and necessity; symmetry (as long as possible); simplicity; causality (in essentially the Newtonian sense); completeness and exhaustiveness; continuum; and of course constancy and invariance.

These ideas, to which Einstein was obstinately devoted, explain why he would continue his work in a given direction even when tests against experience were difficult or unavailable, or, conversely, why he refused to accept theories well supported by the phenomena but, as in the case of Bohr's quantum mechanics, based on presuppositions opposite to his own. Much the same can be said of most of the major scientists whom I have studied. Each has his own, sometimes idiosyncratic map of fundamental guiding notions—from Johannes Kepler to Steven Weinberg and his contemporaries.

With this finding, we must now re-examine the mnemonic device of the two-dimensional plane. I remove its insufficiency by defining a third axis, rising perpendicularly out of it. This is the dimension orthogonal to and not resolvable into the phenomenic or analytic axes. Along it are located those fundamental preconceptions, often stable, many widely shared, that show up in the motivation of the scientist's actual work, as well as in the end-product for which he strives. Since they are not directly derivable either from observation or from analytic ratiocination, they require a term of their own. I call them themata. While the scientist generally is not and need not be conscious of them, the historian of science can chart the growth of a given thema in the work of an individual scientist over time, and show its power upon his scientific imagination. Thematic analysis, then, is in the first instance the identification of the particular map of themata which, like the lines in a fingerprint, can characterize a scientist or a part of the scientific community at a given time.

Most of the themata are ancient and long lived; many come in opposing diads or triads that show up most strikingly during a conflict between individuals or groups that base their work on opposing themata. I have been impressed by the small number of thematic couples, or triads; perhaps fewer than 50 have sufficed us throughout the history of the physical sciences: and of course I have been interested to see that, cautiously, thematic analysis of the same sort has begun to be brought to bear on significant cases in other fields.[6]

With this conceptual tool we can return to some of the puzzles we mentioned earlier. Let me point out two. If, as Einstein claimed, the principles are indeed free inventions of the human mind, there should be an infinite set of possible axiom systems to which one could leap or cleave. Virtually every one of these would ordinarily be useless for constructing theories. How then could there be any hope of success, except by chance? The answer must be that the license implied in the leap to an axiom system of theoretical physics by the freely-inventing mind is the freedom to make such a leap, but not the freedom to make *any leap whatever*. The freedom is narrowly circumscribed by a scientist's particular set of themata that provide constraints shaping the style, direction, and rate of advance of the engagement on novel ground. And in so far as the individual maps of themata overlap, the so-called progress of the scientific community as a group is similarly constrained or



directed. Otherwise, the inherently anarchic connotations of 'freedom' could indeed disperse the total effort. As Mendeleev wrote: 'Since the scientific world view changes drastically not only from one period to another but also from one person to another, it is an expression of creativity. . . . Each scientist endeavors to translate the world view of the school he belongs to into an indisputable principle of science.' However, in practice there is far more coherence than this implies, and we shall presently look more closely at the mechanism responsible for it.

A second puzzle was where the conceptual and even emotional support comes from which, for better or worse, stabilizes the individual scientist's risky speculations and confident suspensions of disbelief during the nascent phase. In case after case, as in the example of Millikan, we see that choices of this sort are made often on thematic grounds. Millikan was devoted to the atomistic view of electricity from the beginning, while his chief opponent, probably under the influence of Ernst Mach and his school, came to look for precisely the opposite evidence, e.g. subelectrons that in principle have no lower limit of charge at all. Similarly, Einstein and his opponents such as Kaufmann were divided sharply on the explanatory value of a plenum (ether), and on the range of fundamental laws across the separate branches of physics.

### The Ionian enchantment

But of all the problems that invite attention with these tools, the most fruitful is a return visit to that mysterious place, early in Einstein's 1933 lecture, where he speaks of the need to pay 'special attention to the relations between the content of the theory and the totality of empirical fact (*Gesamtheit der Erfahrungstatsachen*).' The *totality* of empirical fact! It is a phrase that recurs in his writings, and indicates the sweep of his conscious ambition. But it does even more: it lays bare the most daring of all the themata of science, and points to the holistic drive behind 'scientific progress'.

Einstein explicitly and frankly hoped for a theory that would ultimately be utterly comprehensive and completely unified. This vision drove him on from the special to the general theory, and then to the unified field theory. In a letter to a biographer, Carl Seelig, Einstein likened his progress to the construction of an architectonic entity through three stages of development. Each stage is characterized by the adoption of a 'limiting principle', a formal condition which restricts the choice of possible theories. For example, in going from special to general relativity theory, Einstein had to accept, from 1912 on, that physical significance attaches not to the differentials of the space–time co-ordinates themselves, as the strict operationalists would insist, 'but only to the Riemannian metric corresponding to them'. This entailed Einstein's reluctant sacrifice of the primacy of direct sense perception in constructing a physically significant system; but otherwise he would have had to give up hope of finding unity at the base of physical theory.

The search for one grand architectonic structure is of course an ancient dream. At its worst, it has sometimes produced authoritarian visions which are as empty in science as their equivalent is dangerous in politics. At its best, it has propelled the drive to the various grand syntheses that rise above the more monotonous landscape of analytic science. This has been the case in the last decades in the physical sciences. Today's triumphant purveyors of the promise that all the forces of physics will eventually melt down to one, who in the titles of their publications casually use the term 'The Grand Unification', are in a real sense the successful children of those earliest synthesis-seekers of physical phenomena, the Ionian philosophers.

To be sure, as Sir Isaiah warned in *Concepts and categories*, there is the danger of a trap. He has christened it the 'Ionian Fallacy', defined as the search, from Aristotle to Bertrand Russell and our day, for the ultimate constituents of the world in some non-empirical sense. Superficially, the synthesis-seekers of physics, particularly in their monistic exhortations, appear to have fallen into that trap—from Copernicus, who confessed that the chief point of his work was to perceive nothing less than 'the form of the world and the certain commensurability of its parts', to Einstein's contemporaries such as Max Planck, who exclaimed in 1915 that 'physical research cannot rest so long as mechanics and electrodynamics have not been welded together with thermodynamics and heat radiation', to today's theorists who, in their more popular presentations, seem to imitate Thales himself and announce that all is ineffable quark.

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A chief point in my view of science is that scientists, in so far as they are successful, are in practice rescued from the fallacy *by the multiplicity of their themata, a multiplicity which gives them the flexibility that an authoritarian research programme built on a single thema would lack*. I shall develop this, but I can also agree quickly that something like an Ionian Enchantment, the commitment to the theme of grand unification, was upon Einstein. Once alerted, we can find it in



his work from the very beginning. In his first published paper (1901), he tries to understand the contrary-appearing forces of capillarity and gravitation, and exclaims in a letter to his friend Marcel Grossmann, 'It is a magnificant feeling to recognize the unity [*Einheitlichkeit*] of a complex of phenomena that to direct observation appear to be quite separate things'—such as capillarity and gravitation, the physics of micro- and macro- regions. In each of his next papers we find something of the same drive, which he later called 'my need to generalize'. He examines whether the laws of mechanics provide a sufficient foundation for the general theory of heat, and whether the fluctuation phenomena that turn up in statistical mechanics also explain the basic behaviour of light beams and their interference, the Brownian motion of microscropic particles in fluids, and even the fluctuation of electric charges in conductors. And in his deepest work of those early years, in special relativity theory, the most powerful propellant is Einstein's drive toward unification; his clear motivation is to find a more general point of view which would subsume the seemingly limited and contrary problems and methods of mechanics and of electrodynamics.

Following the same programme obstinately to the end of his life, he tried to bring together, as he had put it in 1920, 'the gravitational field and the electromagnetic field into a unified edifice', leaving 'the whole physics' as a 'closed system of thought'. In that longing for a unified world picture, a structure that encompasses 'the totality of empirical facts', one cannot help hearing the voice of Goethe's Faust who exclaimed that he longed 'to detect the inmost force that binds the world and guides its course'—or, for that matter, Newton himself, who wanted to build a unifying structure so tight that the most minute details would not escape it.

## The unified *Weltbild* as 'supreme task'

In its modern form, the Ionian Enchantment, expressing itself in the search for a unifying world picture, is usually traced to Von Humboldt and Schleiermacher, Fichte and Schelling. The influence of the 'Nature Philosophers' on physicists such as Hans Christian Oersted—who in this way was directly led to the first experimental unification of electricity and magnetism—has been amply chronicled. At the end of the nineteenth century, in the Germany of Einstein's youth, the pursuit of a unified world picture as the scientist's highest task had become almost a cult activity. Looking on from his side of the Channel, J. T. Mertz exclaimed in 1904 that the lives of the continental thinkers are 'devoted to the realization of some great ideal. . . . The English man of science would reply that it is unsafe to trust exclusively to the guidance of a pure idea, that the ideality of German research has frequently been identical with unreality, that in no country has so much time and power been frittered away in following phantoms, and in systematizing empty notions, as in the Land of the Idea.'

Einstein himself could not easily have escaped being aware of these drives toward unification, even as a young person. For example, we know that as a boy he was given Ludwig Büchner's widely popular book *Kraft und Stoff* (*Energy and matter*), a book Einstein often recollected having read with great interest. The little volume does talk about energy and matter; but chiefly it is a late-Enlightenment polemic. Büchner comes out explicitly and enthusiastically in favour of an empirical, almost Lucretian scientific materialism, which its author calls a 'materialistic world view'. Through this world view, the author declares, one can attain 'the unity of energy and matter, and thereby banish forever the old dualism'.

But the books which Einstein himself credited as having been the most influential on him in his youth were Ernst Mach's *Theory of heat* and *Science of mechanics*. That author was motivated by the same Enlightenment animus, and employed the same language. In the *Science of mechanics*, Mach exclaims: 'Science cannot settle for a ready-made world view. It must work toward a future one. . . that will not come to us as a gift. We must earn it! [At the end there beckons] the idea of a unified world view, which is the only one consistent with the economy of a healthy spirit.'

Indeed, in the early years of this century, German scientists were thrashing about in a veritable flood of publications that called for the unification or reformation of the 'world picture' in the very title of their books or essays. Max Planck and Ernst Mach carried on a bitter battle, publishing essays directly in the *Physikalische Zeitschrift*, with titles such as 'The unity of the physical world picture'. Friedrich Adler, one of Einstein's close friends, wrote a book with the same title, attacking Planck. Max von Laue countered with an essay he called 'The physical world picture'. The applied scientist Aurel Stodola, Einstein's admired older colleague in Zurich, corresponded at length with Einstein on a book which finally appeared under the title *The world view of an engineer*. Similarly titled works were published by other collaborators and friends of Einstein, such as Ludwig Hopf and Philipp Frank.



Perhaps the most revealing document of this sort was the manifesto published in 1912 in the *Physikalische Zeitschrift* on behalf the new *Gesellschaft für positivistische Philosophie*, composed in 1911 at the height of the *Weltbild* battle between Mach and Planck. Its declared aim was nothing less than 'to develop a comprehensive *Weltanschauung*', and thereby 'to advance toward a noncontradictory, total conception [*Gesamtauffassung*]'. The document was signed by, among others, Ernst Mach, Josef Petzold, David Hilbert, Felix Klein, Georg Helm, Albert Einstein (only just becoming more widely known at the time), and that embattled builder of another world view, Sigmund Freud.

It was perhaps the first time that Einstein signed a manifesto of any sort. That it was not a casual act is clear from his subsequent, persistent recurrence to the same theme. His most telling essay was delivered in late 1918, possibly triggered in part by the publication of Oswald Spengler's *Decline of the west*, that polemic against what Spengler called 'the scientific world picture of the West'. Einstein took the occasion of a presentation he made in honour of Max Planck (in *Motiv des Forschens*) to lay out in detail the method of constructing a valid world picture. He insisted that it was not only possible to form for oneself 'a simplified world picture that permits an overview [*übersichtliches Bild der Welt*]', but that it was the scientist's 'supreme task'. Specifically, the world view of the theoretical physicist 'deserves its proud name *Weltbild*, because the general laws upon which the conceptual structure of theoretical physics is based can assert the claim that they are valid for any natural event whatsoever. . . . The supreme task of the physicist is therefore to seek those most universal elementary laws from which, by pure deduction, the *Weltbild* may be achieved.'

There is of course no doubt that Einstein's work during those years constituted great progress towards this self-appointed task. In the developing relativistic *Weltbild*, a huge portion of the world of events and processes was being subsumed in a four-dimensional structure which Minkowski in 1908 named simply *die Welt*—a Parmenidean crystal-universe, in which changes, e.g. motions, are largely suspended and, instead, the main themata are those of constancy and invariance, determinism, necessity, and completeness.

Typically, it was Einstein himself who knew best and recorded frequently the limitations of his work. Even as special relativity began to make converts, he announced that the solution was quite incomplete because it applied only to inertial systems and left out entirely the great puzzle of gravitation. Later he worked on removing the obstinate dualities, explaining for example that 'measuring rods and clocks would have to be represented as solutions of the basic equation . . . not, as it were, as theoretical self-sufficient entities'. This he called a 'sin' which 'one must not legalize'. The removal of the sin was part of the hoped-for perfection of the total programme, the achievement of a unified field theory in which 'the particles themselves would *everywhere* be describable as singularity-free solutions of the complete field-equations. Only then would the general theory of relativity be a *complete* theory.'[7] Therefore, the work of finding those most general elementary laws from which by pure deduction a single, consistent, and complete *Weltbild* can be won, had to continue.

There has always been a notable polarity in Einstein's thought with respect to the completeness of the world picture he was seeking. On the one hand he insisted from beginning to end that no single event, individually considered, must be allowed to escape from the final grand net. We noted that in the Herbert Spencer lecture of 1933 he is concerned with encompassing the 'totality of experience', and declared the supreme goal of theory to be 'the adequate representation of any content of experience' (translated in the first English version of the 1933 lecture, as delivered by Einstein, as 'the adequate representation of a single datum of experience'). He even goes beyond that; toward the end of his lecture he reiterates his old opposition to the Bohr–Born–Heisenberg view of quantum physics, and declares 'I still believe in the possibility of a model of reality, that is to say a theory, which shall represent the events themselves [*die Dinge selbst*] and not merely the probability of their occurence'. Writing three years later (*Physics and reality* 1936), he insists even more bluntly:

> But now, I ask, does any physicist whosoever really believe that we shall never be able to attain insight into these significant changes of single systems, their structure, and their causal connections, despite the fact that these individual events have been brought into such close proximity of experience, thanks to the marvellous inventions of the Wilson-Chamber and the Geiger counter? To believe this is, to be sure, logically possible without contradiction; but it is in such lively opposition to my scientific instinct that I cannot forego the search for a more complete mode of conception.

Yet, even while Einstein seemed anxious not to let a single event escape from the final *Weltbild*, he seems to have been strangely uninterested in nuclear phenomena, that lively branch of physics which began to command great attention precisely in the years Einstein



started his own researches. He seems to have thought that these phenomena, in a relatively new and untried field, would not lead to the deeper truths. And one can well argue that he was right; not until the 1930s was there a reasonable theory of nuclear structure, and not until after the big accelerators were built were there adequate conceptions and equipment for the hard tests of the theories of nuclear forces.

Einstein's persistent pursuit of fundamental theory without including nuclear phenomena can be understood as a consequence of a suspension of disbelief of an extraordinary sort. It is ironic that, as it turned out, even while Einstein was trying to unify the two long-range forces (electromagnetism and gravitation), the nucleus was harbouring two additional fundamental forces, and moreover that after a period of neglect, the modern unification programme, two decades after Einstein's death, began to succeed in joining one of the nuclear (relatively short-range) forces with one of the relatively long-range forces (electromagnetism). In this respect, the labyrinth through which the physicists have been moving appears now to be less symmetrical than Einstein had thought it to be.

For this and similar reasons, few of today's working researchers consciously identify their drive towards the 'grand unification' with Einstein's. Their attention is attracted by the thematic differences, expressed for example by their willingness to accept a fundamentally probabilistic world. And yet the historian can see the profound continuity. Today, as in Einstein's time, and indeed that of his predecessors, the deepest aim of fundamental research is still to achieve one logically unified and parsimoniously constructed system of thought that will provide the conceptual comprehension, as complete as humanly possible, of the scientifically accessible sense experiences in their full diversity. This ambition embodies a *telos* of scientific work itself, and it has done so since the rise of science in the Western world. Most scientists, working on small fragments of the total structure, are as unselfconscious about their participation in that grand monistic task as they are about, say, their fundamental monotheistic assumption, carried centrally without having to be avowed believers. Indeed, Joseph Needham may well be right that the development of the concept of a unified natural science depended on the preparation of the ground through monotheism, so that one can understand more easily the reason that modern science rose in seventeenth-century Europe rather than, say, in China.

### Thematic pluralism and the direction of advance

Difference between some themata and sharing of others: this formula in brief seems to me to answer the question why the preoccupation with the eventual achievement of one unified world picture did not lead physics to a totalitarian disaster, as an Ionian Fallacy by itself could well have done. At every step, each of the various world pictures in use was seen as a preliminary version, a premonition of the holy grail. Moreover, each of these various, hopeful but incomplete world pictures of the movement was not a seamless, unresolvable entity (unlike a 'paradigm'). Nor was each completely shared within a given sub-group. Each operated with a whole spectrum of separable themata, with some of the same themata present in portions of the spectrum in rival world pictures. Indeed, Einstein and Bohr agreed on far more than they disagreed. Moreover, most of the themata were not new—they very rarely are—but adopted from predecessor versions of the *Weltbild*, just as many of them would later be incorporated in subsequent versions of it. Einstein freely called his project a 'Maxwellian programme' in this sense.[8]

It is also for this reason that Einstein saw himself with characteristic clarity not at all as a revolutionary, as his friends and his enemies so readily did. He took every opportunity to stress his role as a member of an evolutionary chain. Even while he was working on relativity theory in 1905, he called it 'a modification' of the theory of space and time. Later, in the face of being acclaimed the revolutionary hero of the new science, he insisted, as in his King's College (1921) lecture: 'We have here no revolutionary act but the natural development of a line that can be traced through centuries.' Relativity theory, he held, 'provided a sort of completion of the mighty intellectual edifice of Maxwell and Lorentz'. Indeed he shared quite explicitly with Maxwell and Lorentz some fundamental presuppositions such as the need to describe reality in terms of continua (fields), even though he differed completely with respect to others, such as the role of a plenum.

On this model we can understand why scientists need not hold substantially the same set of beliefs, either in order to communicate meaningfully with one another in agreement or disagreement, or in order to contribute to cumulative improvement of the state of science. Their beliefs have considerable fine structure; and within that structure there is, on the one hand, generally sufficient stabilizing thematic overlap and agreement, and on the other hand sufficient warrant for intellectual freedom that can express itself it thematic disagreements. Innovations



emerging from such a balance, even as 'far-reaching changes' as Einstein called the contributions of Maxwell, Faraday, and Hertz, require neither from the individual scientist nor from the scientific community the kind of complete and sudden reorientation implied in such currently fashionable language as revolution, Gestalt switch, discontinuity, incommensurability, conversion, etc. On the contrary, the innovations are coherent with the model of evolutionary scientific progress to which Einstein himself explicitly adhered, and which emerges also from the actual historical study of his scientific work.

Thus, I believe that generally major scientific advance can be understood in terms of an evolutionary process that involves battles over only a few but by no means all of the recurrent themata. The work of scientists, acting individually or as a group, seen synchronically or diachronically, is not constrained to the phenomenic-analytic plane alone, and hence is an enterprise whose saving pluralism resides in its many internal degrees of freedom. Therefore we can understand why scientific progress is often disorderly, but not catastrophic; why there are many errors and delusions, but not one great fallacy; and how mere human beings, confronting the seemingly endless, interlocking puzzles of the universe, can advance at all—even if not soon, or inevitably, to the Elysium of the single world conception that grasps the totality of phenomena.

### Notes

1. P. Frank, *Einstein: his life and times*, p. 217. Knopf, New York (1947). As his correspondence with Frederick Lindemann (kept at the Einstein Archives in Princeton) shows, Einstein was 'particularly pleased' to enter into what he hoped would be 'regular contact' with Oxford, and he seems to have considered this lecture as part of that process. Indeed, Einstein added to the prefatory sentence cited above: 'May I say that the invitation makes me feel that the links between this University and myself are becoming professionally stronger?' At that time, Einstein had made up his mind not to return to Germany. But he had not yet decided, among various possibilities, where to settle.

2. It is of some importance to note here the publication history of Einstein's Herbert Spencer Lecture—a confusing history, although in that respect by no means different from that of many of Einstein's important essays. Einstein read his lecture in English, apparently the first time he had dared to do so at Oxford. As we know from his correspondence and diary of that time, he was studying English, but felt that he had a quite incomplete mastery of the language. The original manuscript of Einstein's lecture was in German, and has been

published in his collection *Mein Weltbild* pp. 113–19, Ullstein Verlag, Frankfurt am Main (1977), under the title 'Zur Methodik [*not* Methode] der theoretischen Physik'. In the English version, as actually delivered, Einstein acknowledged his 'thanks to my colleagues at Christ Church, Mr Ryle, Mr Page, and Dr Hurst, who helped me—and perhaps a few of you—by translating into the English the lecture which I wrote in German.'

Unfortunately, the English translation, as published as a small booklet by Oxford University Press (1933), left a good deal to be desired. Key portions of the original manuscript were rendered quite freely. Perhaps for this reason, a different English translation was prepared (by Sonja Bargmann) when Einstein later published a collection of his essays under the title *Ideas and opinions* pp. 270–6, Dell, New York (1954). In quoting from Einstein's Spencer Lecture, and indeed from his other publications, I have gone back to the corresponding original German essays and prepared my own translations where necessary.

3. P. Schilpp (ed.) *Albert Einstein, philosopher–scientist* p. 407. Open Court, Evanston, Illinois (1949).

4. I have given a detailed analysis of Millikan's work in Chapter 2 of my recent book, *The scientific imagination: case studies.* Cambridge University Press (1978).

5. Lest it be thought that Millikan was only lucky in guessing which of the data were really usable, I hasten to point out that he continued to exhibit his skill under much more difficult circumstances immediately after this work on the electron. He resumed his experiments on the photoelectric effect, for which he became best known. For ten years he worked with a wrong presupposition that light did not exhibit the quantization of energy. But in the end, he proved the quantum hypothesis experimentally—as he said in his Nobel Prize address, 'contrary to my own expectation'.

6. A brief survey of thematic analysis is provided in the Introduction and Chapter 1 of *The scientific imagination*, Cambridge University Press (1978).

7. A. Einstein, Autobiographical notes. In *Albert Einstein, philosopher–scientist* (ed. P. Schilpp) pp. 59–61, 81. Open Court, Evanston, Illinois (1949). Emphases in original.

In the Spencer Lecture, Einstein raises this whole problem only gently and at the end, by saying: 'Meanwhile the great stumbling block for a field theory of this kind lies in the conception of the atomic structure of matter and energy. For the theory is fundamentally non-atomic insofar as it operates exclusively with continuous functions of space', unlike classical mechanics which, by introducing as its most important element the material point, does justice to an atomic structure of matter. He does see a way out: 'For instance, to account for the atomic character of electricity the field equations need only lead to the following conclusion: The region of three-dimensional space at whose boundary electrical density vanishes everywhere always contains a total electrical charge whose size is represented by a whole number. In the continuum theory, atomic characteristics would be satisfactorily expressed by integral laws without localization of the entities which constitute the atomic structure.' In referring to the total electric charge whose size is represented by a whole number, he points of course to the result of R. A. Millikan's work.



8. The case is quite general. Thus, Kepler's world was constructed of three overlapping thematic structures, two ancient and one new: the universe as theological order, the universe as mathematical harmony, and the universe as physical machine. Newton's scientific world picture clearly retained animistic and theological elements. Lorentz's predominantly electromagnetic world view was really a mixture of Newtonian mechanics, as applied to point masses, determining the motion of electrons, and Maxwell's continuous-field physics. Ernest Rutherford, writing to his new protégé, Niels Bohr, on 20 March 1913, gently scolds him: 'Your ideas as to the mode of origin of spectra in hydrogen are very ingenious and seem to work out well: but the mixture of Planck's ideas [quantization] with the old mechanics make it very difficult to form a physical idea of what is the basis of it.' In fact, of course, Bohr's progress toward the new quantum mechanics via the correspondence principle was a conscious attempt to find his way stepwise from the classical basis.

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