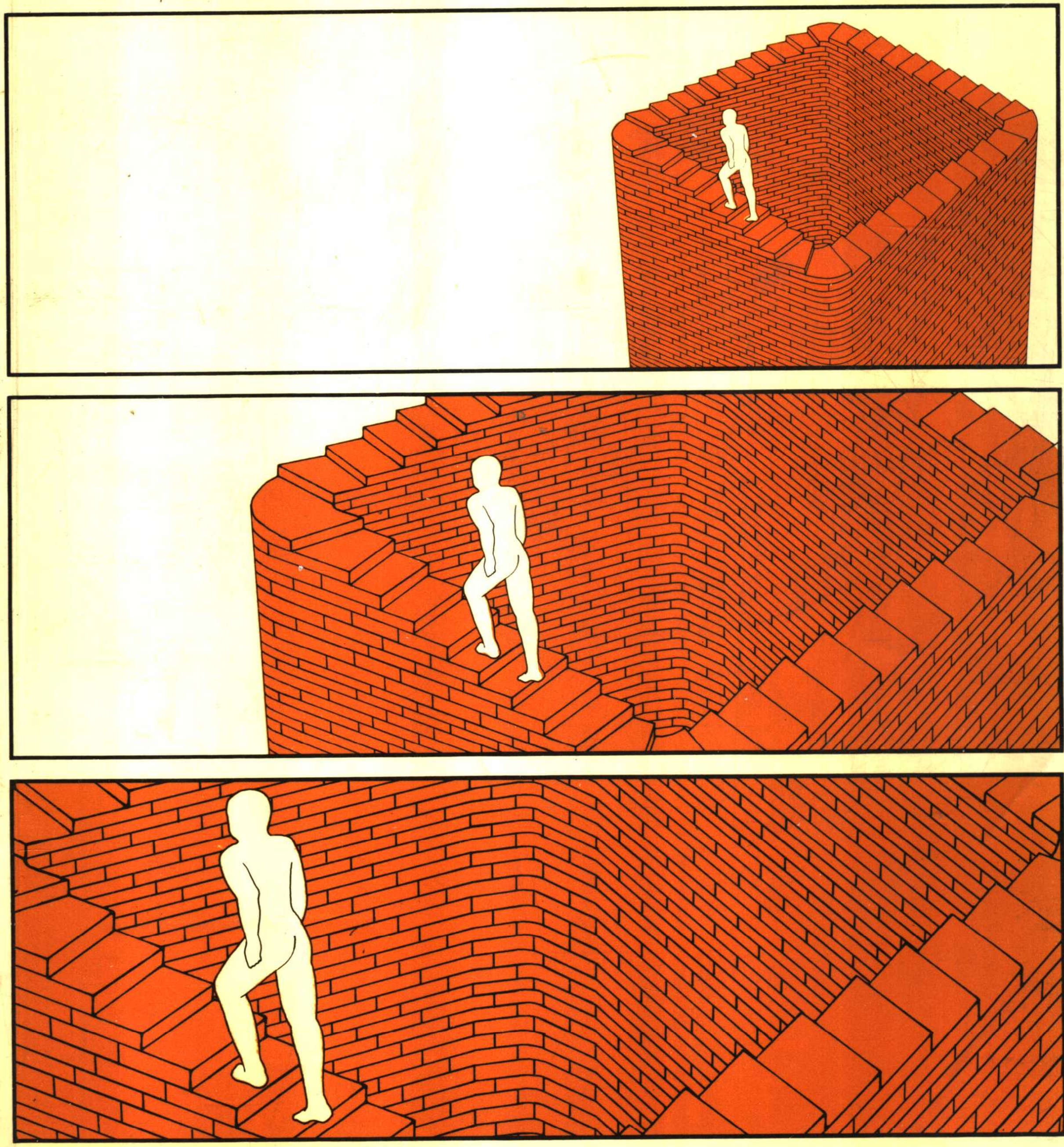
The Encyclopaedia of Ignorance

Everything you ever wanted to know about the unknown



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THE ENCYCLOPAEDIA OF IGNORANCE

Everything you ever wanted to know about the unknown

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EDITORIAL PREFACE

Compared to the pond of knowledge, our ignorance remains atlantic. Indeed the horizon of the unknown recedes as we approach it. The usual encyclopaedia states what we know. This one contains papers on what we do not know, on matters which lie on the edge of knowledge.

In editing this work we have invited scientists to state what it is they would most like to know, that is, where their curiosity is presently focused. We found that this approach appealed to them. The more eminent they were, the more ready to run to us with their ignorance.

As the various disciplines have become increasingly specialised, they have tended to invent a language, or as we found in the computer field, a jargon almost incomprehensible to anybody outside that subject. We have tried to curtail this parochialism and have aimed this book at the informed layman, though possibly at university level, in the hope that he will be encouraged to read papers outside his own subject.

Clearly, before any problem can be solved, it has to be articulated. It is possible that one or two of our papers might direct research or stimulate it. In so far as it succeeds in stating what is unknown the volume will be of use to science historians. A decade hence many of the problems mentioned in these pages will have been solved.

It could be said that science has to date advanced largely on the elbows and knees of technology. Even the concept of relativity depended on technology to prove its validity. In some disciplines we have already reached the point when the Heisenberg principle applies and the observer alters the object observed. And it may well be in cosmology especially, in our attitudes to space and time, that our concepts are our limiting factor. Perhaps imagination is a part of our technology? Perhaps some answers depend only on asking the correct question?

Ronald Duncan
Miranda Weston-Smith

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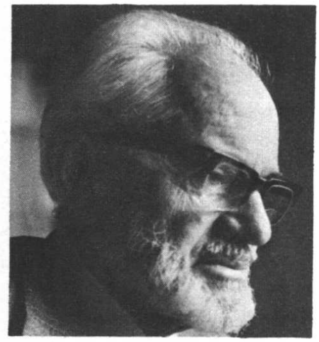

Photograph Bertl Gaye, Cambridge, U.K.

Otto R. Frisch, O.B.E., F.R.S.

Jacksonian Professor of Natural Philosophy, University of Cambridge, 1947-72, now Professor Emeritus.

He is best known as the physicist who in 1939 collaborated with Lise Meitner to produce the first definite identification and explanation of the phenomenon of "nuclear fission", a phrase which he himself coined.

Carried out research in many parts of the world, including Berlin, Copenhagen, Oxford, Los Alamos, Harwell and Cambridge, and is the author of many publications on atomic and nuclear physics.

WHY

Some 15 years ago, WHY was a magic word, used by a small boy to keep Daddy talking.

"Daddy, why does the sun go down in the West?"

"Because West is what we call the place where the sun goes down."

"But why does it go down?"

"It doesn't really; it's the Earth that turns round."

"Why?"

"Because there is no friction to stop it."

"Why . . ."

But what do we mean when we say WHY? We expect some answer; what kind of answer? "Why did Jones break his leg?"

"Because his tibia hit the kerb" says the surgeon.

"Because some fool dropped a banana skin" says Mrs Jones.

"Because he never looks where he goes" says a colleague.

"Because he subconsciously wanted a holiday" says a psychiatrist.

For any event there are several styles of answering the question why it happened. (For further confusion, see Schopenhauer's essay "Die vierfache Wurzel des Satzes vom Grunde".) But when we ask why something is so, then we are on different ground. It would be defeat for a scientist to accept the answer "because God made it so". But often there is another answer which perhaps comes to the same thing. To the question "why do intelligent beings exist?" it seems legitimate to reply "because otherwise there would be nobody to ask that question". Many popular WHYs can be answered in that manner.

Let us try another question. In my car, why does the spark plug ignite the mixture at a particular instant? There are two answers:

(1) because the cam shaft causes a spark at just that instant;

(2) because a spark at that instant gives good engine efficiency.

Answer (1) is what a physicist expects. Still, he may ask why the cam shaft has been made to cause a spark at just that time; then (2) is the answer required. It introduces a new character: the designer, with intelligence and a purpose (in this case the design of an efficient car engine).

The teleological explanation, (2), is here certainly the more telling one (except to that mythical personage, the pure scientist). To the question "why is John running?" the reply "in order to catch the bus" is satisfactory; the reply "because his brain is sending the appropriate messages to his leg muscles" (though basically correct) would be regarded as a leg-pull.

A couple of centuries ago, physical laws were often formulated in teleological language; that this was possible appeared to show that the laws had been designed to fulfil some divine purpose. For instance, a beam of light passing through refracting media (as in a telescope) was shown to travel along the path that requires the minimum time, according to the wave theory of light. But the basic law of refraction can be deduced without reference to that parsimonious principle and, moreover, allows light to travel equally on a path that requires not the least but the most time (at least compared to all neighbouring pathways).

Today such minimum (or maximum) principles are regarded merely as pretty (and sometimes useful) consequences of more basic laws, like the law of refraction; it was anyhow always obscure what divine purpose was served by making light go the quickest (or sometimes the slowest) way. Teleological explanations are not accepted nowadays; not in physics.

In biology it is otherwise; nobody doubts that many features of animals serve a purpose; claws serve to kill, legs to run, wings to fly. But is it a divine purpose? The great debate has not altogether ceased, but the large majority of scientists are agreed that natural selection can account for the appearance of purposeful design, even though some of them find it hard to imagine how such a marvellous instrument as the human eye (let alone the human brain) could have developed under the pressure of natural selection alone.

The power of artificial selection is, of course, well known to any plant or animal breeder. Admittedly, natural selection lacks the breeder's guiding hand. But it has acted through millions of years and on uncounted billions of individuals, and its power of favouring any improved adaptation to the life a species has to live is inexorable. The dug-up skeletons of horses show the development, over some millions of generations, of a rabbit-size creature to the powerful runner of today whose ancestors survived pursuit by ever faster predators. Natural selection still works today: of two varieties of moths of the same species, the darker variety predominates in smoky cities where it is well camouflaged, as his lighter cousins are in birchwoods where they in turn predominate.

As to the human eye, any light-sensitive organ, however primitive, is useful, and any improvement in sensitivity, resolution and mobility is strongly favoured by natural selection. But what about feathers? Even if a very unlikely mutation caused a reptile to have offspring with feathers instead of scales, what good would that do, without muscles to move them and a brain rebuilt to control those muscles? We can only guess. But let me mention the electric eel. It used to be a puzzle how his electric organ could have grown to its present size when in its early stages it would have been quite useless as a weapon. We now have an answer: even a feeble electric organ helps with navigation in muddy waters, and its gradual improvement has led, as it were, from a radar to a death ray.

Much about the process of evolution is still unknown; but I have no doubt that natural selection provides the justification for teleological answers.

Finally, let us go back to physics and ask a question to which, it seems, there is no answer: Why did a particular radium nucleus break up at a particular time? When the theory of atomic nuclei was young it was suggested that their complexity provided the answer: an alpha particle could escape only when all the others were in a particular configuration, as unlikely as twenty successive zeros in a game of roulette. Even with the configurations changing about 10^{20} times a second, it could take years before the right one turned up. That theory has been given up; for one thing there are much less complex nuclei with similar long lives.

Probability theory started as a theory of gambling. The apparent caprice of Lady Luck was attributed to our unavoidable ignorance of the exact way a dice was thrown; if we knew the exact way we could predict the outcome. Sure, we would have to know exactly how the dice was thrown and every detail of the surface on which it fell much more accurately than we could conceivably hope to achieve; but "in principle" it would be possible.

From those humble beginnings in a gamblers' den, the theory of probability grew in power until it took over large parts of physics. For instance, the observable behaviour of gases was accounted for by the innumerable random collisions of its molecules. Just like computing the profitability of a gambling house or an insurance company, this could be done without predicting the behaviour of single molecules. It might still be possible in principle to predict where a given molecule would be one second later; but to do that we would have to know the positions and velocities of millions of other molecules with such precision that to write down, not those numbers themselves but merely the number of decimals required, would be more than a man's life work!

With that in mind, you might find it easier to accept that quantum theory uses the concept of probability without justifying it by ignorance. Today most physicists believe that it is impossible even "in principle" to predict when a given radioactive nucleus will break up. Indeed it is only a few properties (such as the wavelength of light sent out by a given type of excited atom) for which the quantum theory allows us to calculate accurate values; in most other cases all we get is a probability that a particular event will take place in a given time.

To some people this idea of probability as a physical attribute of, say, an unstable atom seems distasteful; the idea of inexorable laws, even if we can never follow their work in detail, has not lost its appeal. Einstein felt it was essential; "God does not play dice with the world" he said. Could not the seeming randomness of atomic events result from the activities of smaller, still unknown entities? The random movements of small particles (pollen grains, etc.) in a fluid, observed in 1827 through the microscope of a botanist, Robert Brown, were later understood as resulting from the impact of millions of molecules, whose existence was merely a matter for speculation in 1827. Perhaps we shall similarly explain the random behaviour of atoms, in 40 years or so?

Such entities, under the non-committal name of "hidden variables", have been speculated on; so far they have remained hidden. Should they come out of hiding they would probably do no more than restore the illusion that the behaviour of atomic particles can be predicted "in principle". On the other hand, they may possibly predict new and unexpected physical phenomena, and that would be very exciting. I have no serious hope of that, but I can't foretell the future.

Sir Hermann Bondi, F.R.S.

Chief Scientific Adviser, Ministry of Defence and Professor of Mathematics at King's College, University of London.

Formulated the Steady State Theory of the origin of the universe with Thomas Gold.

Director-General of the European Space Research Organization 1967-71. Former Chairman of the U.K. Ministry of Defence Space Committee and the U.K. National Committee for Astronomy, and Secretary of the Royal Astronomical Society. Published widely on many aspects of Cosmology and Astrophysics.

THE LURE OF COMPLETENESS

Theories are an essential part of science. Following Karl Popper it is clear that theories are necessary for scientific progress and equally clear that they must be sufficiently definite in their forecasts to be empirically falsifiable. However, it does not follow that it is desirable, let alone necessary, for a theory to be comprehensive in the sense of leaving no room for the unknown or at least the undefined. In many instances it is the very task of the theory to describe the common features of a large group of phenomena, their range of variety necessarily stemming from something outside the theory. Consider, for example, the Galilean theory that in the gravitational field all bodies suffer the same acceleration and that in a suitably limited volume of space (e.g. a golf course) this acceleration is everywhere the same. Yet golf balls fly about in a vast variety of motions (even if one abstracts from air resistance) according as to how, when and where they have been hit by golf clubs.

The theory is explicitly designed to omit any statement of how the bodies were set in motion and indeed gains its importance from this, for otherwise it would not have its vital universality. By concentration on *accelerations*, dynamical theories allow for an input from arbitrary initial conditions of position and velocity. Any dynamical theory not doing so would be condemned to have an absurdly limited *applicability*. It is not that a dynamicist would regard initial conditions in any sense as inexplicable, but he would not view it as *his* business to explain them.

This is a very widespread characteristic of many scientific theories. Thus in Maxwellian electrodynamics the forces making charges (or current-carrying conductors) are explicitly outside the theory (provided they are electrically neutral), in hydrodynamics the position and motion of the boundaries are viewed as an external input to the theory; in the theory of the excited levels of atoms the exciting agency is taken as externally given, in General Relativity the equation of state of matter is viewed as outside the purview of the theory, etc.

In all these fields a theory that had no room for something outside itself as an essential input would be uselessly narrow. Is this a universal characteristic of scientific theories?

It may be worthwhile trying to classify the exceptions. On the one hand we have *historical* theories. Any theory of the origin of the solar system, of the origin of life on Earth, of the origin of the Universe is of an exceptional nature in the sense stated above in that it tries to describe an event in some sense *unique*.

Looking first at the problem of the origin of the solar system we do not, as yet, know of any other planetary systems though many astronomers suspect that they may be fairly or very common. Up to now the challenge is therefore to devise a sequence of occurrences by which the event of the origin of our solar system *could* have happened. This has turned out to be an extremely severe test, and it has been possible to disprove a variety of theories by demonstrating that no planetary system could have formed thus. We have reached the stage of having theories of how *a* planetary system could have formed, but not one with the actual properties of *our* planetary system, nor the stage of having several theories, each accounting satisfactorily for the features of our solar system, so that each *could* be an adequate description, but leaving us in ignorance of which it was in fact. However, it is reasonable to expect that before so very long we will have significant empirical evidence on the frequency of occurrence of planetary systems and perhaps on what are common features of such systems. Such discoveries will add statistical arguments to be considered and may do much to reassure one that one is not dealing with a truly singular event. In the case of the origin of life it may take much longer before there is any evidence on its frequency of occurrence, and we must recall Monod's warning on the indications in favour of its uniqueness, or at least its extreme rarity. Lastly the origin and evolution of the Universe are almost by definition without peers, and thus of intrinsic uniqueness.

The applicability of scientific argumentation to unique historical events is debatable. A *description* of what happened is surely the most ambitious that could be aimed for. A theory wider than this (e.g. one allowing for a whole range for the time dependence of Hubble's constant) simply does not serve the purpose of accounting for the properties of *the* Universe. For what can be the meaning of the set of unrealised universes? What was it that selected the model with the actually occurring time dependence of Hubble's constant from all the others?

But even in cosmology this demand for completeness that looks so sensible for a global feature like Hubble's constant dissolves into nonsense for characteristics on a lesser scale. The bewildering variety of our Universe is surely one of its most striking features. Even very large subunits, like galaxies, have a taxonomy of amazing complexity, varying amongst each other widely in size, in shape, in constituents, in clustering. Would one really ask of a theory of the origin and evolution of our Universe that it should result in a catalogue of galaxies with their individual properties, arranged cluster by cluster? This is surely driving the demand for completeness to absurdity. The best one can reasonably aim for is that one's theory of the Universe should provide a background against which galaxies of the kinds we know of can form. We could not wish for and could not imagine a theory of the Universe telling us why the Virgo cluster formed in one area and our local system in another near to it. We need a separate input for this, and since something external to the Universe is meaningless, our only alibi can be randomness, which fortunately is an intrinsic property of matter.

Thus we see that even the theory of a necessarily unique system like the Universe not only cannot, but must not, be complete. Similarly we would view with the utmost suspicion a theory of the origin of the solar system that necessarily led to just the set of planetary orbits, masses and satellites that we actually find.

Another case, different from the historical one, where completeness of description looks attractive at first sight, is the case of the study of overall systems, as in thermodynamics. In a certain sense a system in thermodynamic equilibrium is fully described by a small set of parameters (volume, temperature, entropy, etc.), a set we like to think of as complete. However, the very power and elegance of the thermodynamic appraisal lies in its *essential incompleteness.* Whatever the interactions between the constituent particles, whatever their character, the system's parameters give a valid and most useful description of its state. It is true that this is a description of the overall state rather than of all the detail that goes on in the micro-scale, but this detail is generally not required. The fact that we can say a great deal about such a system without knowing about it in detail is a source of pride rather than of regret at the incompleteness of our knowledge.

Similarly the existence of systems parameters such as linear momentum and angular momentum derives its value precisely because completeness of knowledge of the interior of the system is not required. No understanding whatever is needed of anatomy, physiology, or the properties of leather to establish that one cannot pull oneself up by one's bootstraps. Indeed one can argue that science is only possible because one can say *something* without knowing *everything.* To aim for completeness of knowledge can thus be essentially unsound. It is far more productive to make the best of what one knows, adding to it as means become available.

Yet in some sense the lure of completeness seems to have got hold of some of the greatest minds in physics; Einstein, Eddington, Schrödinger and most recently Heisenberg have aimed for "world equations" giving a complete description of all forces in the form perhaps of a "unified field theory". A vast number of hours and indeed years of the time of these towering intellects have been spent on this enterprise, with the end result (measured as one should measure science, by the lasting influence on others) of precisely zero.

In my view it is by no means fortuitous that all this endeavour was in vain, for I think that to aim at such completeness of description is mistaken in principle.

Science is by its nature inexhaustible. Whenever new technologies become available for experiment and observation, the possibility, indeed the probability exists that something previously not dreamt of is discovered. To look only at extraterrestrial research in the last quarter century the van Allen radiation belts, the solar wind, Mars craters, radio sources, quasars, pulsars, X-ray stars are all in this class, owing their discovery to space probes and satellites of various kinds, to radio telescopes and to new instrumentation for optical telescopes. Most of these discoveries were totally unforeseen in the antecedent picture though some of their aspects later turned out to be compatible with it. To suggest that at any stage of technical progress in experimentation and observation we have reached such a level of completeness that it is worth a major effort to encapsulate this imagined (not to say imaginary) completeness in an accordingly supposedly harmonious mathematical formulation makes little sense to me. Where there are empirical reasons to join together previously separate branches then this is a worthwhile enterprise likely to lead to important insights but where there are no such indications one is probably only indulging in a mathematical game rather than in science, for one is hardly likely to find testable observable consequences of such a purposeless unification. Of course, the fashion was started by General Relativity which unified inertia and gravitation with great success. But this was based on Galileo's observation that all bodies fall equally fast. Theory followed experiment by 300 years, for his experiment established the equality of inertia and passive gravitational mass. What is there to guide us in attempting to unify gravitation with electromagnetism and perhaps with weak or strong nuclear interactions? There are no experiments beyond those involved in General Relativity that are joining any of these fields. Until a new technology enables us to perform such experiments, the unification is virtually bound to be sterile.

The counter-argument to my scepticism has generally been that one should rely for guidance on a supposed concept of "mathematical beauty". Experience indicates that while an individual theoretician may perhaps find such a concept heuristically helpful, it is not one on which different people can agree, in stark contrast to the unanimity with which the yardstick of experimental disproof is accepted. Hence the failure of the work of Einstein and others on unified field theories to be followed up, hence the total waste of all this effort. To my mind, which is perhaps not very appreciative of the significance of mathematical beauty, the whole concept looks meaningless and arbitrary depending as it does on whether somebody invents a concise notation or whether a similarity with a previously established mathematical field can be adduced.

The aim of this article has been to show that our most successful theories in physics are those that explicitly leave room for the *unknown*, while confining this room sufficiently to make the theory empirically disprovable. It does not matter whether this room is created by allowing for arbitrary forces as Newtonian dynamics does, or by allowing for arbitrary equations of state for matter, as General Relativity does, or for arbitrary motions of charges and dipoles, as Maxwell's electrodynamics does. To exclude the unknown wholly as a "unified field theory" or a "world equation" purports to do is pointless and of no scientific significance.