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THE UNIVERSE: PAST AND PRESENT REFLECTIONS[1]

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Defining the Universe to be everything there is, manifestly we cannot be expected to understand it exactly, since to do so would require a complete command of the laws of physics and the fantastic calculating power to work through the detailed properties of assemblies containing very large numbers of particles. What then are astronomers doing in their studies of cosmology? Obviously, we are beavering away to give an imperfect answer to an imperfectly defined problem. The issue is the quality of our approximations. I suppose nine astronomers out of ten work on the presumption that our approximations are quite meritoriously good. In this essay I hint that it may be otherwise.

Let me begin autobiographically. I did my undergraduate work in mathematics and my graduate work in theoretical physics, the latter in the closing stages of the golden age of quantum mechanics and in the opening phase of the golden age of nuclear physics, a lucky time in which to receive an education. I was lucky too to have spent five wartime years at what was then the frontier of electronics. So a little by deliberate choice, but mostly by accident, I found myself tolerably equipped in 1945 for tackling some of the rewarding problems with which astronomy was then strewn, like the treasures of Ali Baba's cave. I hesitated to plunge ahead into cosmology, however, for reasons substantially the same as those explained above. It seemed highly unlikely that the knowledge-of-the-day could possibly be adequate to deal with the whole Universe; and yet if one did not proceed as if it was, it would be necessary to think *outside* what was already known, and how is it possible to think outside one's knowledge?

To think completely outside is of course impossible. Nevertheless, one can go beyond the detail of what is currently known, provided one maintains the

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"style" of physics, and indeed some of the most profitable adventures in science have come in just this way. The special theory of relativity had been in the air a long time before Einstein in 1905, certainly from the Michelson-Morley experiment of 1887. There had been contradictions between the old details of Newtonian theory and new details contained in work by Fitzgerald, Lorentz, Poincaré, and Planck. What Einstein did was to put an issue of style—all observers in uniform motion measure the same speed of light—ahead of the confusion of detail. When this was done consistently, a lot of the old detail went out of the window, to be replaced by deductions that were subsequently shown by experiment to be correct. Of course, physicists never admit to "style," because the word brings an image of Beau Brummel to mind. Instead they talk loftily about "principles," a stuffed-duck posture. Style it is, and principle it is not.

Two more bits of autobiography. In 1938–39 I had the good fortune to become a research student of Paul Dirac, who impressed on my young mind the preeminence of style over detail. And in 1939, when I first became a Fellow of St. John's College, Cambridge, I had Ebenezer Cunningham as a colleague. Ebenezer was of the older generation of Cambridge mathematicians. He had been young himself at the time of Einstein's work of 1905. He told me that immediately after Einstein's paper was published, he had sat down one afternoon to read it, and in only a few hours he passed from an acute perception of the previous difficulties to seeing that Einstein had resolved those difficulties. I think it is generally agreed that of all British scientists in those days Cunningham was the nearest to discovering the special theory of relativity himself. I asked him if he had been disappointed to find another young man had got there ahead of him. He replied in the following general terms: "In some degree, naturally, especially as the mathematics itself was within my powers. But the disappointment was compensated by an extreme sense of clarity. In any case, I could see that I had just not been ruthless enough."

So it came about, again by good luck, that I had learned the importance of style from Paul Dirac, and the importance of ruthlessness from Eb Cunningham, that most Christian of gentlemen. But what could style and ruthlessness do for one in cosmology? This was the question I asked myself in the immediate postwar years.

To answer this question I must turn for a moment to the game of chess. Expert players assert that in a high-level game only two or three moves really matter, the rest are more or less automatic. In one case known to me this was certainly true. In 1972, Bobby Fischer wrested the world championship from Boris Spassky by a rather overwhelming margin. Yet the outstanding moment of the match belonged to Spassky. In the 11th game, in a variation extensively analyzed by Fischer, Spassky on his 14th move played the queen's knight backward from c3 to its home square of b1. Overlooked for many years by the whole international chess fraternity, the move instantly converted a probable win for Fischer into an essentially certain loss. So much can turn on a single move, not only in chess but also in cosmology.

The corresponding decisive move in cosmology is given by a yes-or-no answer to the following question:

> Did the whole Universe come into being, all in a moment, about ten billion years ago?

In 1947, when I first began to ponder this question, the geologist Arthur Holmes was asserting with immense courage that the measured age of the Earth was greater than the age of the entire Universe as then given with confidence by Edwin Hubble. One therefore needed little in the way of an enquiring mind to start pondering. Although astronomers have since increased Hubble's age estimate about fivefold, much the same situation exists today. Thus the currently favored value of about 100 km s^{-1} mpc^{-1} for the "Hubble constant" runs foul of the age of our galaxy, whether the latter is determined by nuclear methods or from the lifetimes of the oldest stars.

The importance of age estimates is to force the above question seriously into the mind, not to determine the answer to it—the age estimates are too uncertain to be relied on for so crucial a decision. By considering the question seriously I mean contemplating the possibility that the answer to it might be *no*. Many believe there is a given answer, like a problem in a student examination, and that the *ex cathedra* answer is yes. It then seems axiomatic that discrepancies of the kind mentioned in the previous paragraph must inevitably be resolvable, a point of view that invites the shading of calculations and the slanting of data. Besides which, the attribution of a definite age to the Universe, whatever it might be, is to exalt the concept of time above the Universe, and since the Universe is everything this is crackpot in itself.

I would regard the need for the Universe to take precedence over time as a knockout argument in favor of a negative answer to the above question, if we could be certain our ever-present idea of particles of various sorts existing in four-dimensional space-time was correct. One could then dismiss cosmologies of finite age because they were offensive to basic logical consistency. But one has to contemplate surprises; indeed, the following speculation would be a surprise if it were true.

Nowadays, the particles of the 1930s, which we blissfully thought to be just four in number—electrons, protons, neutrons, and neutrinos—are known to be complex aggregates of more elementary particles—quarks, God save us. The wave function of a quark has a triple hierarchy: the "spin" multiplicity of Dirac, a multiplicity of five or more called "flavor," and a multiplicity of three or more called "color." Each multiplicity has its own group of mathematical transformations, with "spin" determined by general complex transformations in two dimensions (unimodular), and with both "flavor" and "color" determined by higher-dimensional complex transformations that are, however, less than general—they are restricted to be unitary in order to maintain conservation laws among the particles.

It was my intention on leaving Cambridge in 1972 to try removing the separated categories of these three groups of transformations to permit them to become mixed together by complex transformations. One would then be unable in general to speak about such-and-such a form of particle existing in space-time, because space-time (contained in the two dimensions of a general complex space) would be amalgamated with the spaces determining the nature of the particles. As things fell out I did not carry through this program. Publicly, my reason was that I became involved in other things. Privately, I became scared by the mathematical difficulties.

On such a point of view, the Universe would be described by a single abstract space of high dimensionality in which the general transformations were permitted. Our everyday description of the world as a set of particles "in" space-time would be applicable only to a subspace, or subspaces, of the general space. One could then suspect that it is the special projection into a particular subspace of the state-vector of the portion of the Universe we observe that determines the detailed numerical values of the so-called coupling constants of physics.

When astronomers refer to the "age" of the Universe, could it be that the "age" is really the time interval since the state-vector went into such a subspace? If so, it would be invalid to ask what happened "before" in a temporal sense, because the state-vector would belong to the general complex space to which the space-time concept could not be applied. To be sure, one might still give meaning to "before" in an abstract sequential way, but not in the usual way with respect to "time" as a real parameter.

I find this concept attractive, but I do not find it attractive to suppose that the portion of the Universe we observe experienced such a drastic transformation only ten billion years ago. To explain my position I must turn to biology. To most astronomers the thought of information crucial to cosmology being derived from biology will, I suppose, appear ludicrous. But the Universe is *everything*, and to omit information from any source, especially biology with its vast store of information, would be truly ludicrous.

In *Steady-State Cosmology Revisited* (University College Cardiff Press, 1980) I estimated (on a very conservative basis) the chance of a random shuffling of amino acids producing a workable set of enzymes to be less than 10^{-40000}. Since the minuteness of this probability wipes out any thought of life having originated on the Earth, many whose thoughts are irreversibly programmed to believe in a terrestrial origin of life argue that the enzyme estimate is wrong. It is—in the sense of being too conservative.

Could the vast store of information necessary for the development of biology have been accumulated in only ten billion years? If you are inclined to think that it could, take a look at what we know of the most recent four billion years, and what many people believe to be the beginning of the Universe in a big-bang cosmology. Such a beginning occurs in a holocaust of radiation little suited to harboring the delicate organization of biology, while the past three to four billion years on the Earth have yielded no change in the intricate biochemical complexity of life. The enzymes go essentially unchanged from the cells of a human to the most primitive single cells, which are thought to be typical of life as it existed in the early days of the Earth. Hence we have a situation without a promising beginning and with no change of the crucial aspects of the life system over the last one third to one half of the ten billion year time interval. Where then did the miracle of information contained in biological systems arise? How does one deal with a probability as small as one part in 10^{40000}? In my view only by giving the Universe a very long history, much longer than ten billion years.

Using this 1980 argument, one arrives at the same rejection of a short age for the Universe that I started from in 1947–48, a position which meant changing the details of cosmological theory but not changing the style (a condition demanding that one keep to Einstein's general theory of relativity). Einstein's theory equates a set of quantities (tensor) determined by the geometrical structure of space-time to another set of quantities of a physical nature known as the energy-momentum tensor. Nobody had ever pronounced an edict as to exactly what the energy-momentum tensor must be, except that it be derivable from an "action principle." Hence it followed that whatever one could do to cosmology had to be done in the action formula.

Although several kinds of "field" appeared in the action formula, nobody to that time had introduced into cosmology the simplest field of all, a scalar function of position. So it was obvious that a scalar should be tried (the C-field as I called it). There was little freedom in how a scalar could be so employed, either as a pure field term in the action or in coupling to the particles (classical). So the theory more or less ran itself.

The consequences were surprisingly far-reaching, like those of Spassky's apparently simple move against Fischer. But unlike Spassky's move, which immediately drew approval, the new theory was soon in trouble with both astronomers and physicists. The new equations required matter to be created, which was said to be impossible. Although this opinion did not impress me unduly—I thought it only a guess, which it was—the criticism persuaded most astronomers to treat the theory in a cavalier manner. On the pretext that any stick is good enough to beat a dog with, the theory was assailed by observations with low signal-to-noise ratios that were claimed to be disproofs. Yet throughout this criticism one could hold fast to the critical point that, without departing from the style of physics, the short "age" of the Universe had been banished; the Universe was everlasting, into the past as well as into the future.

The term in the action formula that coupled particles to the C-field, and the pure C-field itself, contained an ambiguity of sign. This was not unexpected because choices of sign arise with other widely accepted terms in the action. For example, the term giving rise to ordinary gravity would, if its sign were switched, make gravity a repulsive force instead of an attractive one. The choice required by the new cosmological theory made the energy density of the C-field negative, a condition which in my student days I had come to think impossible (at least when there was a field-to-particle coupling, as there was in this case). The argument went as follows: The creation of particles with positive energy would make the field energy more negative, causing the strength of the C-field to increase. This would have the effect of creating more particles at an increased rate, making the field energy still more negative and the C-field still stronger. And so on, into a catastrophic instability.

The argument is a poor one, however, because it assumes space-time to remain flat. A field of negative energy density acts like negative gravity, causing expansion, or explosion if the situation is sufficiently drastic. It was indeed the negative energy density of the C-field that produced the recession of the galaxies in the new cosmology, thereby explaining the expansion of the Universe in physical terms instead of assuming it *ad hoc,* as was done in other theories.

In the early days of the new theory, 1950 or thereabouts, we knew little yet of the violent local explosions that are all the rage in astronomy nowadays. Otherwise it would have seemed natural to attribute them to creational instabilities caused by local condensations of the C-field, and it would have been hard then for the older theories to have survived in the face of such evidence. True, astronomers today have convinced themselves of other ideas for explaining the now-observed local explosions, but these ideas are also unattractively *ad hoc.*

New ideas have never come to me by winging their way down from the clouds, but from calculating orthodox positions, and then finding that situations did not work out as they were supposed to do. The following is an example that began in a very innocent way.

In the 1950s astronomers thought the interstellar grains to be water-ice. Nothing at all was riding on this issue for me, and I would gladly have believed the conventional point of view if calculation had shown it to be viable. The trouble was that interstellar grains are constantly changing their positions in relation to the stars, and even the briefest sojourn of a water-ice grain in a region where the temperature rises to, say, $-150°C$ will cause evaporation. I did not find that, once evaporated, water-ice grains would recondense by themselves under any conditions that seemed plausible. I mentioned this difficulty in a general astronomy book[1], suggesting the grains must consist of a more refractory substance than ice, as for instance graphite. The matter rested so lightly with me, however, that I did nothing about it until some five years later when I had a research student (Chandra Wickramasinghe) in need of a problem.

One encouraging indication was that the physical properties (optical constants) of graphite happened to be such as would give a reasonable approximation to the observed $1/\lambda$ law of extinction that the grains produce in the visual light of distant stars. Furthermore, the behavior of the optical constants with respect to frequency enabled us to predict that graphite would produce enormous extinction in an ultraviolet waveband centered at about 2000 Å. When this predicted large extinction was actually discovered approximately one year later from rocket firings, it seemed certain that there must be something right with the graphite idea.

There are those who are so uncomfortable with new situations that their practice, on hearing a new idea, is to search for an immediately overriding objection to it. I work in the opposite way. To begin with, I search for the good things to be said about a new idea. If some emerge, and especially if they look strong, I then turn to criticism. And the stronger an idea becomes the more relentlessly I search for objections to it. In accord with this methodology, the time had come by 1965 to put the graphite idea through the wringer. By this time, enough was known of the reflectivity of interstellar grains for Wickramasinghe and I to see that the reflectivity of graphite was too low. Graphite was too absorptive, too black. There were also difficulties with the technical details of what is known as the "polarization" produced by the grains. It seemed therefore that while there was something right about graphite, the graphite theory could not be totally correct.

Water-ice has a very strong infrared absorption band near 3.1 micrometers, but attempts to observe this band had not yet shown in the mid-1960s that water-ice is almost completely absent from grains in the general interstellar medium. We were not debarred therefore from considering a composite grain model—grains with graphite cores and water-ice mantles. Of course there was still an evaporation problem for the ice, but at least it was more tolerable to have water-vapor condensing around already existing graphite cores than to have ice grains condense *de novo*.

This composite core-mantle theory was a parameter-fitting enthusiast's delight. The shapes and sizes of the particles could be varied, as well as the

[1]*Frontiers of Astronomy*, Wm. Heinemann, 1955.

relative proportions of graphite and ice. By now, the Institute of Theoretical Astronomy at Cambridge had a fair-sized IBM computer on which Wickramasinghe calculated an immense number of cases. With so many parameters available, a moderate correspondence with all the data was inevitable and could not therefore be considered much of an achievement. The important thing was to obtain a really good correspondence, and this holy grail eluded us with maddening persistence. Starting from a moderate agreement with all the data, we would tune-up the parameters to get some particular feature almost exactly right, only to find the correspondence with the rest of the data had become worse. Gradually it was born in on us that we had a wrong theory on our hands. To add to our troubles, observations were showing grain properties to be remarkably uniform over the whole galaxy, and it would be impossible to obtain uniformity with the many floating parameters we were using.

So it came about that in the later 1960s we began thinking of what other kinds of particles there might be. As the search intensified we became increasingly desperate, to a point where we even tried grains of solid hydrogen, although at best such grains could exist only in the coolest dense clouds and not at all in the distributed interstellar medium. From this escapade something important emerged, however. Certain details, which had never been right before, fitted immediately into place for solid hydrogen. This limited success proved to be a consequence of the very low refractive index of solid hydrogen, much lower than we had thought to use before. Just as the ultraviolet extinction showed there was something right about graphite, so these later calculations for solid hydrogen showed there was something right about a low refractive index.

Accepting that the ultraviolet extinction near 2000 Å was caused by graphite particles, the observations were now good enough for the sizes and shapes of the particles to be inferred. The result was another shock. The particles had to be spheres with diameters not greater than a few hundred Ångstroms. No method of forming graphite particles we could then think of would produce such particles—we expected either long whiskers or sooty plate-like structures.

We considered grains composed of magnesium oxide (MgO) and silica (SiO_2), and when about a year later infrared observations near 10 micrometers indicated the existence of circumstellar "silicates" the moment again seemed right for hats to be thrown into the air. But true to our previous experience there were troubles. Silica crystals have two enormously strong infrared features (absorption coefficient $\sim 30{,}000\ cm^2\ g^{-1}$) and neither was seen, even as minor effects. While these strong narrow bands could be broadened into more indefinite features by combining SiO_2 with some other molecule, as with MgO in enstatite ($MgSiO_3$) or forsterite (Mg_2SiO_4), we could not believe this would be the case for every last scrap of silica, such as would be necessary for the strong features not to show up at all. Besides which, I was convinced that the astronomical community had been misled by a wrong calculation, namely a calculation for a strictly thermodynamic situation, when $MgSiO_3$ and Mg_2SiO_4 are slightly more stable than MgO and SiO_2 taken separately. But grains are not formed in a strictly thermodynamic situation. Such grains were thought to form in outward flows of gas from stars, and such flows are so markedly nonthermodynamic that MgO and SiO_2 would remain separate, if indeed SiO_2 forms at all.

In 1969, E. M. Purcell published an interesting calculation that showed how the minimum amount of matter required to produce the observed extinction of starlight could be calculated from general physical principles. Purcell's work showed that, relative to the observationally determined amount of interstellar gas (mostly hydrogen and helium), the condensable materials are remarkably efficient. Provided the condensed grains are of optimum shapes and sizes, the amount of the condensable materials is sufficient to explain the observed extinction of starlight, but not by any great margin, and then only if an appreciable fraction of interstellar carbon, nitrogen, and oxygen is condensed. If one were to omit the C, N, and O from the grains, the amount of the rest, such as MgO, SiO_2, CaO, and Fe would be insufficient to explain the extinction by a factor of at least 3.

Precisely because of the far-reaching implication of this result, there is a disposition among astronomers to deny it. Indeed, one should deny it, but not in the sense that many would like. The factor 3 by which such materials as MgO, SiO_2, CaO, and Fe fail to explain the observed extinction is calculated using the assumption that the grains into which these materials are condensed are optimum in their size distribution for producing extinction over the whole sweep of wavelengths from one micrometer to 1000 Å. For inorganic grains, which have no strong size-determining property, this is an implausible condition. It becomes even more implausible when one recalls the remarkable uniformity of the extinction over the whole galaxy. The prudent conclusion is that were C, N, and O excluded, the grain-forming materials in the interstellar medium would be deficient by a factor of at least 5. A corollary is that the grains responsible for most of the interstellar extinction must be largely composed of C, N, and O, with the possibility that hydrogen is associated with these elements. Since inorganic solids built from H, C, N, and O evaporate much too readily, the inference is that the grains are organic, an inescapable conclusion unless some serious mistake has been made in estimating the total quantity of interstellar material. Wickramasinghe and I reached this conclusion with an initial sense of bewilderment, for how could organic grains be formed in great quantity throughout the interstellar medium, and similarly everywhere with a size distribution centered at about 0.7 micrometers?

After leaving Cambridge in 1972 I had other things beside interstellar grains to think about, and it was not until 1976 that I returned to this question. Meanwhile, Wickramasinghe had considered polyformaldehyde as a possible grain-forming material, $(CO\,H_2)_n$, built from the two commonest molecules in the Universe, H_2 and CO. A more complex, but more stable, substance built from the same elementary ingredients is obtained by forming rings, usually from five or six COH_2 groups, with some adjustments of atoms between the groups, and by then linking the rings through oxygen atoms into a linear chain, with the elimination of an H_2O molecule at each link, rather than having the carbon atoms joined directly one to another along the chain. This is the difference between polyformaldehyde and a polysaccharide.

My father was a wool merchant and in my early youth he taught me a simple way to distinguish a strand of real wool from imitations. Imitations burned leaving a trail of ash; wool burned by shriveling, with a little ball of free carbon accumulating at the burning end. I remembered this observation from days long gone by. Here at last was a way to obtain the small carbon spheres demanded by the ultraviolet data, from the degradation of a suitable organic polymer, as for instance the keratin in wool.

Wickramasinghe dug out from the literature an infrared transmittance curve for the common biopolymer cellulose. A glance at the curve showed it to have properties of greater interest in the infrared than anything we had seen before. At its longwave end the curve was like that which astronomers had christened "Trapezium material," while at the shortwave end there was a broad absorption similar to that due to water. A. H. Olavesen kindly obtained a carefully calibrated cellulose spectrum for us, and he also showed that a representative sample of other polysaccharides all had spectra very much like cellulose.

This was sufficient for a number of interesting calculations to be done, with more satisfactory results than anything achieved in the 1960s. The way ahead seemed to be to press the calculations to finer and finer limits. Conscious that there might be a charring of organic material toward the inner regions of our sources, with a consequent variation of transmittance properties forcing rather complex calculations, I felt the need for a more sophisticated computer than my little hand-held Hewlett-Packard. I therefore applied to the Science Research Council for a modest grant wherewith to purchase a suitable minicomputer. It is a matter of history that the application was refused, not just once but for a second time upon appeal. I mention this affair not to suggest that the Science Research Council be summarily dismantled (which, of course, it should be), but to explain why the line of research pursued so far had to be abandoned. It was now necessary to adopt what Americans call an end-around play.

An interesting question forced itself on one's attention. With the realization that the interstellar grains are largely organic, one sees that the material of the early solar system must have contained an enormous quantity of organics, at least 3,000 Earth masses. Much organic material would be destroyed by the heat of the solar nebula, but much would survive in the comparatively cool outer regions, especially in the regions of the distant comets. And since at subsequent times a fraction of comets have developed orbits of high eccentricity, bringing them to the inner regions of the solar system, with a part of their evaporated material enmeshing the terrestrial atmosphere, there was a known process for transferring organic material from the outer distant regions of the solar system to the Earth. Could this potentially very large and continuing source of organic material have formed the basis for the origin of life, rather than the comparatively trifling quantity of organics generated in terrestrial thunderstorms and other small-scale events?

Wickramasinghe and I answered this question affirmatively, and so arrived at a temporary equilibrium point in our thinking: the organic basis of life was interstellar, a position that others are now favoring. It was at this stage that we began our technical readings in biology, fully expecting the usual picture of the terrestrial origin and evolution of life to be amply confirmed by the facts. Unlike the situation in astronomy, where one has to struggle against a paucity of facts, in biology one has to struggle against being swept away in an avalanche of information. However, if one can avoid being overwhelmed, and if the many facts can be fitted into a consistent picture, then one can have considerable confidence in the result, a major advantage over many situations in astronomy.

This first resting point did not survive our early readings in biology, as it was quickly apparent that the facts point overwhelmingly against life being of terrestrial origin, which would require happenings every bit as miraculous as the views of religious fundamentalists. Although released from this conceptual millstone, my brain made no leap to freedom. It plodded its way, small step by small step, first to the comets. Because comets must have experienced break-up and reformation, with material interchanged between them, and because the material would be organic, it was possible to think of the whole ensemble of comets as a life-generating unit. And because a few comets are breaking-up and scattering their contents all the time, the process was not relegated to the remote past. This was a big plus, since theories that relate to current events stand much above those concerned only with situations long dead and done with. There was much of interest to be worked through in this first shift from the Earth to comets, and the investigation of a number of side issues deluded us for a while into thinking that the main problem had been faced.

It had not, of course. One does not face the factor 10^{40000}, discussed above in connection with the enzymes, simply by going from the Earth to the comets, a move which yields a gain factor of about 10^{6}. Nor does one face 10^{40000} even by venturing from the solar system to all the other star systems of our galaxy, a further step that yields an additional gain factor of 10^{11}. Yet such was our next move, to a galaxy-wide ensemble in which life originated and evolved.

A few scientists had speculated in the nineteenth century on life as a galaxy-wide phenomenon, and in the early years of the present century Svante Arrhenius had added flesh to the bones of those earlier speculations. Our views had come by a different route, with many later facts to guide them, and so with differences from Arrhenius. But there was also much that was the same.[2]

Lack of a suitable computer had forced me out of the interstellar grain problem into a different line of thought, but now the new line suggested a renewed attack on the grain problem. Could the interstellar grains be biological cells, some perhaps alive and others in various stages of degradation, with the graphite particles needed to explain the ultraviolet extinction coming at the last stage of the degradation process? I don't know how others go about dealing with questions like these, but I suspect that many cough violently, go purple in the face, and that by the time breathing returns to normal the question is safely gone. I myself go off for a long walk on the mountains, preferably over a route with the harsh reality of a bit of crag on it, so that, if I don't fall off the crag, I can still have a little assurance that my wits are still within earshot.

Unlike inorganic cells, which can have any size at all, biological cells have well-defined size distributions, as for example the diameter distribution of spore-forming bacteria shown in Figure 1. Notice the clustering around 0.7 micrometers. This value, typical for bacteria, is the same as the known sizes of the interstellar grains (in the case of rod-shaped particles, it is the rod-diameter that matters, not the rod length). Under freeze-dried conditions, as in interstellar space, bacteria normally develop cavities within themselves, and small partially hollow particles scatter light according to a volume-averaged value of the refractive index. Hence bacteria in interstellar space would scatter radiation in the visible spectrum like solid particles of unusually low refractive index, the property we had found from the investigation of solid hydrogen to fit certain tricky details of the observations so well.

When hard-pressed by adverse conditions some bacteria break-up into smaller, more-or-less spherical wall-less cells called mycoplasmas. The curve of Figure 2 is a calculation due to Wickramasinghe of the extinction of starlight produced by a mixture of mycoplasmas, of graphite spheres produced by the degradation of a fraction of the mycoplasmas, and of the bacterial distribution shown in Figure 1. The points of Figure 2 are observations, with both observations and the curve normalized to a value of 1.8 magnitudes per kiloparsec at a wavelength of 0.55 micrometers. Unlike the complex calculations of the late 1960s, which went on for years, the calculation leading to Figure 2 was carried through in a few days. Einstein is reported to have

[2]This situation is discussed at length in *Space-Travellers, the Origins of Life*, University College Cardiff Press, 1981.

remarked that, while God may be subtle, He is not malicious. If the grains were not organic, it would surely be incorrigibly malicious to have given us such poor results in the 1960s with basically the correct theory and such an excellent result now with a wrong theory!

But this was only an entertaining diversion from the main issue. How is the factor 10^{40000} really to be faced? Not by a galaxy-wide ensemble of living cells. Not even by adding other nearby galaxies to the ensemble, or even the totality of galaxies observable with the largest telescopes. To face 10^{40000} the ensemble of life must be hugely cosmological in its scale, and our cosmology has to extend into the past by a time interval exceeding ten billion years by an enormous factor.

So we are back to the starting point, but now with more substance to the argument. It will of course be in the reader's mind to ask if 10^{40000} is really inevitable. The answer is yes, if life is to originate by what are called the "blind" forces of nature, which is to say without initial information. Nothing is to be gained by attempting to shake the calculation of 10^{40000}. The issue you will recall was the probability of a set of amino acids randomly falling together into a workable aggregate of enzymes. Certainly it is easy to frame a deceitful argument, in the following way for example. Start with much simpler, much smaller, enzymes that are sufficiently elementary to be discoverable by chance. Then let evolution in some chemical environment cause the simple enzymes to change gradually into the complex ones we have today. The first retort to this mental deception is that an appeal to initial simplicity has been

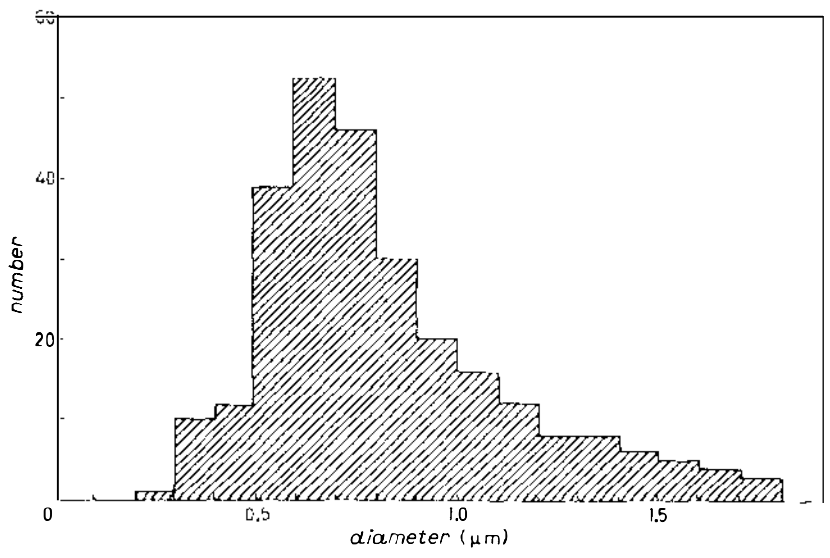

Figure 1 Size distribution of spore-forming bacteria.

allowed for already. Thus the number 10^{40000} was obtained from a calculation in which less than twenty amino acids were required to be in specific sequential positions for each of two thousand enzymes. If the calculation is to be criticized it should be on the grounds of being much too conservative. But the real deceit comes from ignoring the problem of what it was in the environment that caused simple enzymes to evolve into complex ones. If the environment contained information, what was its source? If not, then an improbability of the order of 10^{-40000} has been concealed in the behavior of the environment.

To face 10^{40000} one must think unthinkable thoughts, which means any thought with a chance greater than 1 in 10^{40000} of being right, a condition that permits a wide class of possibilities! One such possibility is that the enzymes were put together in accordance with instructions. Given a knowledge of the appropriate ordering of amino acids, it would need only a slightly superhuman chemist to construct the enzymes with one hundred percent accuracy. It would need a somewhat more superhuman scientist (again given appropriate instructions) to assemble a living cell, but not a level of skill outside our comprehension. Rather than accept a probability less than 1 in 10^{40000} of life having arisen through the "blind" forces of nature, it seems better to suppose that the origin of life was a deliberate intellectual act. By "better" I mean less likely to be wrong.

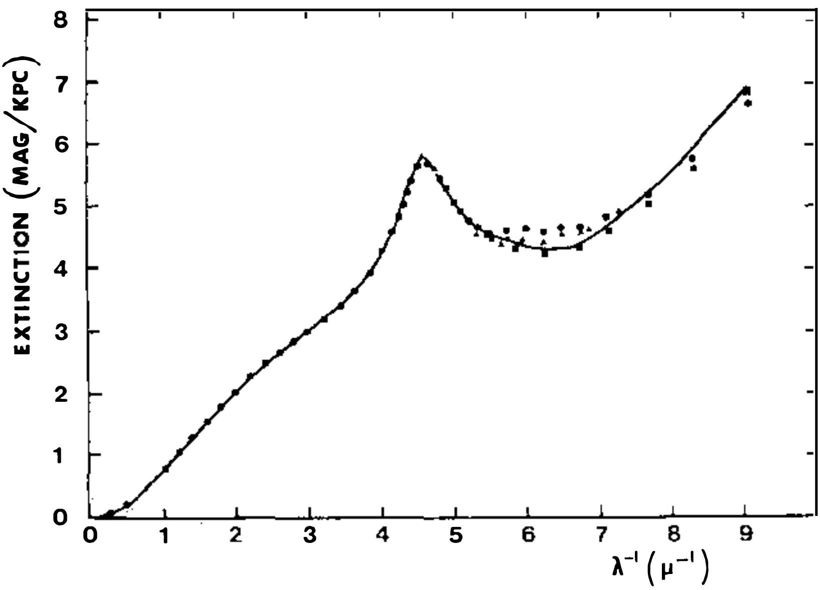

Figure 2 Wavelength dependence of interstellar extinction normalized to 1.8 mag/kpc at $\lambda^{-1} = 1.8\ \mu m^{-1}$. Points are astronomical observations; curve is for grain model. (Circles are average extinction compiled from many sources by Sapar & Kuusik 1978. Triangles are ESA data given by Jamas et al. 1976. Squares are from Bless & Savage 1972.)

A spaceship approaches the Earth, but not close enough for its imaginary inhabitants to distinguish individual terrestrial animals. They see growing crops, roads, bridges, and a debate ensues. Are these chance formations or are they the products of an intelligence?

It is not at all difficult to formulate examples of events with exceedingly low probabilities. A roulette wheel operates in a casino. A bystander notes the sequence of numbers thrown by the wheel over the course of a whole year. What is the chance that this particular sequence should have turned up? Well, not as small as 1 in 10^{40000}, but extremely small nonetheless. So there is nothing especially remarkable in a tiny probability. Yet it surely would be exceedingly remarkable if the sequence thrown by the roulette wheel in the course of a year should have an explicit mathematical significance, as for instance if the numbers turned out to form the digits of π to an enormous number of decimal places. This is just the situation with a living cell, which is not any old random jumble of chemicals.

Taking the view, palatable to most ordinary folk but exceedingly unpalatable to scientists[3], that there is an enormous intelligence abroad in the Universe, it becomes necessary to write blind forces out of astronomy. Interstellar grains, living cells, are to be regarded as powerful tools, every bit as purposeful if you like as a garden spade. We know from astronomical studies that the grains are mysteriously connected with a whole range of phenomena: the rate of condensation of stars; the mass function of stars; magnetic fields; spiral arms of galaxies; and quite probably with the formation of planetary systems. Not one of these phenomena has been explained in better than fuzzy terms, just as the views of the imaginary travellers in the spaceship would be fuzzy if they attempted to explain terrestrial fields, walls, and ditches as products of the blind forces of nature.

It would be necessary to calculate in full detail the properties of complex biopolymers in order to obtain the information required for the construction of a living cell. Such a project would be quite beyond our practical ability, but not beyond our comprehension. Indeed we are nearer to understanding what would be involved in it than a dog is to understanding the construction of a power station.

Edward Blyth, who wrote on natural selection as early as 1835–37, remarked that when the idea first occurred to him "a variety of important considerations crowded on the mind." So it is here. Suppose you were a superintellect working through possibilities in polymer chemistry. Would you not be astonished that polymers based on the carbon atom turned out in your calculations to have the remarkable properties of the enzymes and other biomolecules? Would you not be bowled over in surprise to find that a living

[3] Because, of course, scientists delight in seeing themselves as the only Johannes Factotums in the whole Universe.

cell was a feasible construct? Would you not say to yourself, in whatever language supercalculating intellects use, "Some supercalculating intellect must have designed the properties of the carbon atom, otherwise the chance of my finding such an atom through the blind forces of nature would be less than 1 part in 10^{40000}." Of course you would, and if you were a sensible superintellect you would conclude that the carbon atom is a fix.

From 1953 onward, Willy Fowler and I have always been intrigued by the remarkable relation of the 7.65 Mev energy level in the nucleus of ^{12}C to the 7.12 Mev level in ^{16}O. If you wanted to produce carbon and oxygen in roughly equal quantities by stellar nucleosynthesis, these are the two levels you would have to fix, and your fixing would have to be just where these levels are actually found to be. Another put-up job? Following the above argument, I am inclined to think so. A common sense interpretation of the facts suggests that a superintellect has monkeyed with physics, as well as with chemistry and biology, and that there are no blind forces worth speaking about in nature.

This problem of the energy levels of ^{12}C and ^{16}O is by no means the most puzzling I have accumulated in a lifetime of research. One particular problem proved so difficult that I had long since put it aside as hopelessly intractable. With an entirely new outlook now available, however, I took a fresh look at this old problem, with results described technically in the Appendix, and in more colloquial terms below. As always, I introduce this further topic with a bit of autobiography (permitting myself an idiosyncratic run-to-the-wicket), beginning with a remark or two on the educational process.

Like entropy, which perpetually increases, educational standards perpetually worsen. And like entropy, which increases inevitably because of the policies of physics, education worsens inevitably because of the policies of educators. Instead of teaching being properly confined to the rote-learning of facts and well-proven techniques, pupils are confused nowadays by the teaching of meanings that they cannot comprehend.

You can see what I mean by attending a few rehearsals of amateur theatricals. You will find the players attempting to perform the play long before they know their parts. They strive to give meaning to their lines while still reading from script, and they even strut the stage while still reading from script. The outcome is that, despite an unconscionable number of rehearsals, the players reach the actual performance still insecure, just as nowadays the school population reaches the age of college entrance in a considerable measure unable to read efficiently and insecure in elementary mathematical processes.

There was none of this in the old days of rote-learning, preferably done by chanting. Well-proven techniques acquired by chanting stood their recipients in good stead for a whole lifetime. And the higher the standard one seeks to achieve, the more necessary does rote-learning become. The situation is the same in mathematics and science as it is in learning a musical instrument, or learning to play difficult parts like Macbeth and King Lear on the stage. The golden rule is to learn the lines, or the scales, or the mathematical processes, to a point where they can be reproduced with extreme facility. Only then should one worry seriously about meanings and interpretations.

I have never learned the lines of Macbeth or King Lear, but I will bet that those who have will have gone through something like the following experience. After continuing for days, weeks, or even months thinking they knew the part thoroughly, a sudden perception of several lines will have hit them all in a moment, and they will then exclaim in delight to themselves "Well, surely *this* is what old-man Shakespeare must *really* have meant!" This is certainly what happens in science and mathematics. True understanding comes from just such perceptions. Since these perceptions involve the fine-processing of highly ordered information in the brain, carefully learned technique is an essential prerequisite. True understanding cannot be picked-up casually from a lecturer on the rostrum, any more than one can learn to ride a bicycle by watching someone else riding a bicycle. Facts, techniques, and meanings thrown higgledy-piggledy into the brain produce as flat a situation as all kinds of food thrown at random into a stewpot.

Worse, there is a somber aspect to the matter. Experience shows that teachers giving instruction in well-proven techniques stay honestly within their understanding, but experience also shows that teachers—encouraged or required by educational policies to discuss meanings with their pupils—inevitably go much outside their own understanding. This explains why so much old-style school teaching, especially at sixth-form level, was good, and why nowadays so much teaching at universities, especially in the humanities, is bad.

The reader will properly ask in what respect this present article differs from the system I am condemning. It differs in that I exercise no blackmail over the reader, such as is given by the teacher-pupil relationship, and by public and university examinations. The reader is free to take it or leave it, which is exactly the way things should be with all discussions of meaning and understandings. This was the way it was in the old days when undergraduates in the ancient universities "read" their subjects literally, and where obtaining a "first-class" in university examinations did not depend on regurgitating the opinions of lecturers. The situation would not be half as sinister as it is if rubbish came easily out of the brain. The trouble is that, whereas rubbish easily goes in, like a Japanese harpoon into the body of a whale, rubbish comes out not at all.

The fine-ordering of technical information into the brain carries penalties as well as rewards. The greater the perceptions, the greater the stress. Perceptions imply a reordering in the brain, not just of a few details, but of considerable blocks of information, and the larger the blocks the bigger the mental disturbance. Nothing of this kind can happen when the storage is of the higgledy-piggledy stewpot kind, so that a modern education may be said to give protection against "brainstorm" situations. This is presumably why the advocates of modern education assert that it produces "rounded" personalities, which is a little like arguing that by taking a sledgehammer to the engine of a car one ensures that the car will not be involved in an accident.

I had two "brainstorms" in my student years, the circumstances of which I can recall rather precisely. The first occurred about a month before my final undergraduate examinations in late May, 1936. True to my belief in rote-learning, I could reproduce, like the lines of Macbeth or King Lear, most of the details given in Eddington's *The Mathematical Theory of Relativity*. One day, while pondering the relation of certain of the mathematical symbols to actual physical measurements of space and time, I decided my understanding of the meaning of "clocks" and "measuring rods" was defective. I had come upon the difficulty in the general theory of relativity, but it was soon apparent that the same problem occurred also in the special theory, which to this point I had thought easy meat. To my horror, I now found I couldn't understand the apparently simple special theory. So how could I possibly take an examination in the far more difficult general theory? I seriously contemplated postponing the examination for a further year (an option open to me), but I managed to resolve the crisis before the month was out.

The second crisis occurred in the spring of 1938, and this one I was not able to resolve within a month, a year, or even a generation. I was sitting on the banks of the River Cam when it happened. I had just finished swimming and was waiting for the Red Lion in Granchester to open for afternoon tea. Until this moment I believed I understood quantum mechanics. By now I had won a sought-for research prize, the subject of my entry for it being in quantum mechanics. Besides which, I was then in the process of becoming a research student of the great Paul Dirac. So what terrors could quantum mechanics hold for me? Plenty. As I sat, waiting for the afternoon tea, I suddenly realized that I didn't understand it at all.

To this moment I had accepted without critical appraisal the explanation of uncertainty in quantum mechanics which had been suggested by Werner Heisenberg and Neils Bohr—the Copenhagen school as it became called—according to which uncertainty in a quantum system is taken to be a necessary consequence of interference by the observer with the system. I now saw that this explanation was inadequate, and quite possibly incorrect. Unknown to me, Erwin Schrödinger had run into the same situation and had been so appalled that he had been led to exclaim "I don't like it (quantum mechanics), and I'm sorry to think I ever had anything to do with it!"

I suppose I should have gone to talk to Dirac, but like most students I had not learned to put my thoughts coherently into words, and with Dirac you had to be clear in your use of words. So I tried discussing the difficulty with my fellow students, only to find them unable to see any problem. This caused me a year later to quit theoretical physics for astronomy. My brain patterns were seriously disturbed, and I could not see how to reshuffle them in a satisfactory way. Instead of accepting open defeat, I followed the military strategy of retreating, hopefully to fight another day.

Almost twenty years later, the same problem resurfaced in an article by Hugh Everett III (*Reviews of Modern Physics*, Vol. 29, 1957, pp 454–62). Since the lay-out of Everett's argument is to my knowledge the clearest to have appeared, I have adopted it in the Appendix. To avoid technicalities here I content myself now with a more informal discussion, involving an example I constructed in 1938–39 (an example similar to the so-called cat paradox of Schrödinger).

Everybody who becomes involved with quantum mechanics is likely to be uncomfortably aware that the dynamics of the theory leads to a spreading vagueness in the world. If you begin with a reasonably well-condensed particle wavefunction, a so-called wavepacket, the packet spreads with time, and pretty soon the thing is all over the Universe. Yet our observation of the world does not suggest any such spreading vagueness. Hence one infers that in some way there must be a compensating sharpening of the picture. Let us see from the example of 1938–39 how this sharpening occurs.

A city has an inner citadel that can be sealed-off completely from the people of the city, who in turn can be sealed-off by a wall from the surrounding countryside (in the fashion of ancient Troy). The citadel contains a nuclear bomb[4] with a trigger controlled by a device with quantum uncertainty, constructed in the following way. Count n similar radioactive nuclei with a reasonably long half-life τ. Over a specified time interval τ/n, electric power is supplied to the system; otherwise the power is off and nothing can happen. The trigger is arranged to be tripped if one or more of the n nuclei disintegrate in the specified interval. No experimental physicist would have difficulty in actually making such a device, and nobody would doubt that for sufficiently large n, the probability of the trigger being activated would be $\frac{1}{2}$.

Now consider the grand-ensemble wavefunction for the whole city, people and all. Some physicists, foreseeing trouble ahead, have sought to deny this step, claiming that quantum mechanics cannot be applied to macroscopic systems. But this is worse than a brainstorm. It is a kind of scientific nervous breakdown, for it would require a maximum number of particles up to which quantum mechanics was applicable and beyond which it was not—an absurd position. Although the set of base states for the grand-ensemble wavefunction is huge, the base states can be separated into two mutually exclusive catego-

[4]The possibility of constructing a nuclear bomb was apparent as early as 1939.

ries, one in which the city goes about its normal business, and the other in which everything goes up in a mushroom-shaped cloud. At the beginning of the specified time interval the amplitudes of the states in the mushroom category are zero, but as the time interval proceeds the amplitudes become nonzero, and by the end of the time interval the sum of the squares of the moduli of the amplitudes of all the base states in the mushroom category add to $\frac{1}{2}$. And of course the sum of the squares of the moduli of the amplitudes of the base states in the normal category falls from unity at the beginning of the time interval to $\frac{1}{2}$ at its end. This is just a complicated way of saying that the chances of the city surviving and of it being annihilated are even steven.

By good fortune you, the observer, are not incarcerated in the city. You are out in the surrounding countryside at a safe distance, from which position you will be able to see the mushroom cloud, if it goes up. However, because you are unbearably fretted by the situation, you take a stiff dose of a drug that causes you to sleep through the critical time interval, and for long enough afterward so that you cannot tell what happened from the condition of the sky. Nor can you search for radioactive fallout, because you don't have a Geiger counter. But with cunning you have arranged for a camera to take pictures throughout the critical interval. You retrieve the film from the camera, thinking that a decision on the fate of the city is contained in the emulsion of the film. Yet according to quantum mechanics, the grand-ensemble wavefunction is such that the chances of the silver grains in the emulsion being arranged to form a mushroom cloud and of them being clear of such an arrangement remain even. There is no way in which the camera could have made a decision on the fate of the city.

Now proceed to develop the film. In the dark room as you apply developer and fixer there is no light. Satisfied at last that you have done a good job, you take the developing tray outside and hold the prints up to a light. And then at last you know what happened to the city. By "know" I mean you condense the wavefunction for the city. Instead of continuing any longer with amplitudes giving even chances for the two categories of states, you now set all the amplitudes of one category to zero, and the other category takes unit probability. All subsequent experience will be consistent with this drastic shift in the wavefunction. If you make a journey to the site of the city you will either find the people alive there, going about their business, or you will find a scene of woeful devastation. It will all fit exactly to what you decided in your first glance at the prints in the developing tray.

I will not pretend that I saw all this in a flash on the spring day in 1938. To that moment I had followed standard texts in which quantum systems were thought of as minute in comparison with the observer, and so it appeared reasonable to think of a huge hulking observer interfering with, and introducing uncertainty into, quantum systems. What I now saw was that quantum uncertainty could occur in a system huge compared to the observer, and indeed that one could have situations in which the perturbations of the quantum system by the observer were quite negligible. It was so for most cases of decaying radioactive nuclei. From the time of Rutherford's experiments in the early years of the century, it had always been emphasized that decaying nuclei went their own sweet way, irrespective of the experimenter (this was before the era of particle accelerators). What I could not understand, as I sat in 1938 on the banks of the River Cam, was how, in view of this known situation in nuclear physics, scientists had nevertheless thought of interference by the observer as the cause of uncertainty. The uncertainty was inherent, and yet it was somehow the observer who contrived to *resolve the uncertainty*. How was this done from a theoretical point, from within quantum mechanics itself?

This was the question I failed to resolve, the question which led me in 1939 to leave theoretical physics for astronomy, thinking with youthful idealism to be entering a more rational subject. Although an almost forbidden question in 1939, it has been asked more often by the younger postwar generation of physicists. But apart from the article by Everett, not much has been published on it, the general attitude being summed up by a distinguished younger physicist who remarked "This is a matter on which we must each have our own private thoughts."

I have returned twice to the question; in the years 1964–1970, when for the first time I felt I could see a chink of light, and now, very recently. Let me begin with the 1964–1970 period. It was then that I became a dyed-in-the-wool believer in the time symmetry of basic physics. Of course there are aspects of our experience that are not time symmetric—thermodynamics, the past-to-future propagation of radiation fields, and certain features of particle physics. In my view such asymmetries are cosmological manifestations, however, not basic physics. Here I have space only to discuss the past-to-future propagation of the electromagnetic field. In a famous demonstration, Wheeler and Feynman showed more than thirty years ago that one could have a time-symmetric electrodynamics augmented by a cosmological response from the future that reproduced exactly the same results as the classical Maxwell-Lorentz theory. For some years it was thought that a similar demonstration could not be given in quantum physics, but in the late 1960s Jayant Narlikar and I showed that, just as in the classical case of Wheeler and Feynman, it was possible to have a time-symmetric local quantum theory augmented by a cosmological response from the future that reproduced exactly all the practical results of normal quantum electrodynamics. Although there was no difference at all in its statistical predictions, the time-symmetric theory was interestingly different in its details. Unlike normal quantum mechanics, no pure-amplitude theory could be formulated because the cosmological response involved both the wavefunction and its conjugate complex. This I saw as an advantage. The pure-amplitude aspect of normal quantum mechanics involves a redundancy, because information is discarded in passing to practical results. The time-symmetric theory yields the practical results without redundancy.

While these considerations did nothing directly to answer the quantum puzzle, they served to lift a dark cloud upon it. One of the unremitting struggles of my life had been to read *Mathematical Foundations of Quantum Mechanics* by J. von Neumann (for the full flavor try the Springer edition, Berlin 1932, but if you want it in less excruciating form try Princeton University Press, 1955). In the later chapters of his book, von Neumann claims to demonstrate that no theory with a greater measure of predictability than present quantum mechanics can be found, which would seem to close-out any possibility of answering the critical question posed above. Everett makes a courageous attempt to accept this position, as I describe in the Appendix, but the better route seemed to me to contemplate the possibility that von Neumann might have been wrong in his assertion. From someone with such slender mathematical ability as myself this might seem a great conceit, but I was encouraged in it by the conviction that in his challenge to Dirac over the mathematical validity of the famous delta-function, von Neumann had been mistaken. What I saw in 1970 or thereabouts was that von Neumann had been concerned with a finite local system. If cosmology was involved, with a response from the future, the dynamical variables in the system could be infinite, and the situation could then be different. This was the chink of light.

Even so, the problem remained acutely puzzling. The future imposes a condition on a local system because a signal goes out from the local system to other material systems in the future, which respond with a return signal on account of the time symmetry. One would like a situation in which the return signal imposed a deterministic reality on the local system, forcing an explicit decision to be made in all situations of an A or not-A kind, as in the example discussed above (mushroom cloud or no-mushroom cloud). The trouble is that so long as one calculates the return signal from within quantum mechanics this does not happen, just as von Neumann claimed. One is faced by a chicken-and-egg situation. The initial local system does not have deterministic reality because the systems in its future with which it interacts do not have deterministic reality, and this is because the systems in the further future with which the second systems interact do not have deterministic reality, and so on along an infinite chain of interactions. Yet somewhere the gordian knot has to be cut—it must be, since our everyday experience tells us that it is! The mathematical loophole lies at the limit of the infinite chain of interactions. True, we cannot establish deterministic reality by starting within the chain and by attempting to argue in a past-to-future direction toward the limit. But if we were to start with deterministic reality at the limit, arguing backwards from future to past, there would be deterministic reality at every link of the chain.

In other words, the trouble may well come from arguing the problem back-to-front instead of front-to-back.

This was the stage of my thinking following the work of the 1964–1970 period, before it became apparent (from the arguments given earlier) that an enormous intelligence must be abroad in the Universe. As the Americans say, this instantly created a new ball game. An intelligence of a strictly finite kind, such as might calculate the properties of the enzymes, would not suffice to resolve the quantum mechanical dilemma, however. For this, it would be necessary to control the infinite limit discussed above, or some other process of equivalent significance. At first, one might think the tremendous scope of such a thing would take it entirely outside the range of our comprehension. But remarkably this is not so. It is possible to see in rather precise mathematical terms how such a control could establish intelligence throughout the Universe by imposing information sequences on finite material systems. The technical details of how this might be done are given at the end of the Appendix. Here I will jump the issue of *how* it might be done, to ask *is it actually done?* Is information impressed in our brains from outside? Obviously yes, from the five senses. But is there a subtle further component arising from an external control of quantum uncertainty? The evidence is not of a kind that one is obliged to consider compelling, but it is not negligible either.

I have always thought it curious that, while most scientists claim to eschew religion, it actually dominates their thoughts more than it does the clergy. The passionate frenzy with which the big-bang cosmology is clutched to the corporate scientific bosom evidently arises from a deep-rooted attachment to the first page of Genesis, religious fundamentalism at its strongest. A little should be said in favor of this mania. Let us think of every animal as a computer terminal equipped with a certain measure of backing storage, which has been established partly through the animal's genetic heritage and partly through inputs from the five senses. Each computer terminal receives vestigial signals arising from the phenomenon of the condensation of the wavefunction (see the end of Appendix for details). The information content of these signals, ranging from the simple to the complex, has to be interpreted against the available backing storage. Where the information falls well within the capacity of the backing storage we have a clear consistent picture, as in science. Where the information falls at the limit (or outside) of the backing storage we have a muddled illogical picture, as in religion. In both cases the signals are valid enough. Limitations arise in the interpretation, not in the signals themselves. A dog cannot understand the operation of a power station because of limitations in the scope of its backing storage, and in a like fashion we have trouble with problems of religion, even if one is incorrigibly attracted to them like moths to a candle (as scientists are).

Perhaps because it remains a cultural thread in the Yorkshire valleys where

I was born and brought up, I have always been an admirer of the music of *Messiah,* latterly in the form in which Handel actually wrote it. Yet in my earlier years I could make little or nothing of most of the words. A particularly obscure passage comes from the bass soloist in the third part, just before the famous trumpet passage: "Behold, I tell you a mystery: we shall not all sleep, but we shall all be changed, in a moment, in the twinkling of an eye, at the last trumpet. The trumpet shall sound . . . ."

It is curious to contemplate that there could be a connection between quantum mechanics and this apparent gibberish. Nevertheless, the persistent religious conviction that the pattern of our lives is stored in the future looks as if it could quite well be correct. At the mathematical limit discussed above. At the last trumpet! What an extraordinary way to describe the outcome of a sequence of arguments involving the condensation of the wavefunction, the need to avoid von Neumann's mathematical result for finite systems, and time-symmetric electrodynamics. Of course one can argue that the correspondences are fortuitous. Notice, however, that in time-symmetric theory influences are indeed felt "in (less than) a moment, in (even less than) the twinkling of an eye," and that all finite events are brought together at the mathematical limit in the future. Fortuitous or not, it is curious that so many people without scientific knowledge have believed in the idea, as if they had caught a glimpse of a difficult message that they could only express in terms of an everyday analogy.

Religion is an interesting but not really convincing example of the computer terminal data. Some years ago I had a graphic description from Dick Feynman of what a moment of inspiration feels like, and of it being followed by an enormous sense of euphoria, lasting for maybe two or three days. I asked how often had it happened, to which Feynman replied "four." We both agreed that twelve days of euphoria was not a great reward for a lifetime's work.

Actually, Feynman was lucky with his four times. Only once have I had a similar experience. The circumstances were extraordinary and far outside any other perceptions I have ever had. Rather as the relevation occurred to Paul on the road to Damascus, mine occurred on the road over Bowes Moor. The time was the late 1960s, when Narlikar and I were struggling with the problem of the quantum mechanical signal from the future. We were tackling the nonrelativistic theory at that stage, and were seeking a way to evaluate a multiple integral with a complicated integrand determined by a system of equations.

A small party of summer visitors to the Institute of Theoretical Astronomy at Cambridge was spending a few days in the Scottish Highlands. Because of a committee meeting I was late in joining them. I started alone from Cambridge, driving north by way of Scotch Corner, Penrith, Carlisle, and Stirling. As the miles slipped by I turned the quantum mechanical problem mentioned above over in my mind, in the hazy way I normally have in thinking mathematics in my head. Normally, I have to write things on paper, and then fiddle with the equations and integrals as best I can. But somewhere on Bowes Moor my awareness of the mathematics clarified, not a little, not even a lot, but as if a huge brilliant light had suddenly been switched on. How long did it take to become totally convinced that the problem was solved? Less than five seconds. It only remained to make sure that before the clarity faded I had enough of the essential steps safely stored in my recallable memory. It is indicative of the measure of certainty I felt that in the ensuing days I didn't trouble to commit anything to paper. When I returned to Cambridge ten days or so later, I found it possible to write the thing down without difficulty.

Many will smile if I say that such an incident was triggered by the deciphering of a cosmic signal. It will be agreed that a sudden reordering of substantial blocks of information in the brain must have been involved, but it will be said that the initiating signal happened by chance, from a random firing of neurons. Perhaps. I have no means of calculating the probability of random brain processes just happening to trigger so complex an affair, but if I had, I suspect I would arrive at 1 part in 10^{40000}, or less.

My last example is not exposed to this criticism, since it involves an output too vast and too long sustained to be attributable to chance. Before the late works of Beethoven became a fashion they were thought difficult, which they are. My friend Leo Smit explained to me one of the difficulties, namely that Beethoven was apt to combine two works into one, two sonatas in one, two symphonies in one. There is no difficulty in following the famous Ninth Symphony on a bar-by-bar basis, the difficulty lies in the overall structure. While Beethoven admirers will tolerate no words of criticism against the Ninth, critics, including Giuseppi Verdi, have described its fourth movement as a failure. After Leo Smit's remark, I realized that "failure" is the wrong word; "misjudgment" possibly, but not "failure." The point is that the first and third movements together form a symphony of cosmic proportions and grandeur (akin to the two movements of the Opus 111 piano sonata), while in the second and fourth movements Beethoven is down-to-earth, addressing us more or less in our own terms. So one should think of two symphonies interlaced with each other. For my example I want the first and third movements taken as a unity.

On a visit to the Kitt Peak National Observatory, I made the acquaintance of the big J. B. Lansing loudspeakers, which will put out 100 watts without distortion. I want those speakers, a good hi-fi amplifier, a room large enough to propagate the lowest register, and a first-rate modern recording. The volume should be set so that the long-sustained drumroll in the middle of the first movement sounds as if Homer himself had caught the thunder of Zeus on Mt. Olympus.

But before we listen, let us enquire a little into the history of the composer.

A poor boy, tough, hard-working, determined to force himself to the top. Add great ability to determination, and no crackpot educators to deflect him from "specializing." With these formidable advantages we find Beethoven in his early thirties as the greatest keyboard artist yet known, an artist gradually establishing a name as a composer. Now disaster strikes. Although still a comparatively young man, Beethoven begins to go deaf. His deafness is a long drawn-out affair, with loud hissing in the ears that must have introduced distortions in the aural memories of earlier years. The situation was surely much worse than if Beethoven had gone deaf all in a moment. Long before the Ninth was written, however, Beethoven's hearing had become negligible, so that at its first performance he could hear neither the orchestra nor the applause of the audience.

Now listen and ponder how those sounds were conceived. Did Beethoven simply permute and combine memories for sound he had acquired in his youth? At best, discounting distortion, those memories represented a stage of development illustrated by the First and Second Symphonies, a universe apart from the Ninth. Remember too that it is hard to find anything in the past evolution of our species where the ability of a deaf man, beyond the prime of life, to rearrange patterns of sound from far-distant memories would have conferred a significant selective advantage.

The alternate view is that the deaf Beethoven, decisively cut-off from the distractions of the world of men, equipped as a terminal with unusual backing storage, was able to receive a particular component of the cosmic signals, and with sharply increasing clarity as the years passed by. This view would be my choice, but each of us must listen and decide. Perhaps the decision turns on whether we ourselves hear the thunder of Zeus on Mt. Olympus.

Appendix: On the Condensation of the Wavefunction

Let ϕ_i, $i = 1, 2, \ldots$, be a complete orthonormal set of wavefunctions, each satisfying exactly the dynamical equations of a quantum mechanical system in the absence of interaction with the observer, who to begin with has a grand ensemble wavefunction denoted by Φ. Suppose for the moment that the system is initially in the particular state ϕ_i. So long as system and observer remain independent of each other the total wavefunction for system plus observer is the simple product $\phi_i\,\Phi$.

An interaction between system and observer, operating over a specified time interval, in general, produces a complicated entangled total wavefunction at the end of the interval,

$$\phi_i\,\Phi \to \sum_j a_j\,\phi_j\,\Phi_j,$$

where not only have states of the system at other suffix values appeared, but the initial observer-state Φ has "branched" into the set of observer-states Φ_j, $j = 1, 2, \ldots$.

The key aspect of the article by Hugh Everett III (*Reviews of Modern Physics,* Vol. 29, 1957, p. 454) lies in restricting the discussion to interactions that are less than general, namely to interactions with respect to the base set ϕ_i, $i = 1, 2, \ldots$, that have the effect over a time interval of giving

$$\phi_i \, \Phi \rightarrow \phi_i \, \Phi_i, \; i = 1, 2, \ldots,$$

where each observer-state Φ_i involves only the corresponding system-state ϕ_i. Interactions with this special property are said to be "good." By means of an example due to von Neumann (from the latter's book *Mathematical Foundations of Quantum Mechanics),* Everett shows that a good interaction occurs in a particular special case. He then goes on, in what seems to me a gap in the argument, to assume the possibility of a good interaction whatever the quantum system and whatever the observer. The idea is that the observer is free to adjust the interaction by an appropriate design of experiment in such a way that it is good. To establish whether or not this is true, one would need to examine each case separately in detail. But because this issue is mathematical, not conceptual, let us proceed (for the moment) taking it as axiomatic that the observer can indeed find a good interaction whenever it is required for the argument.

Suppose next that the system-state before interaction with the observer is mixed with respect to the base set ϕ_i, $i = 1, 2, \ldots$, viz $\Sigma_i \, a_i \, \phi_i$. The coefficients a_i before the interaction are constants, since each state ϕ_i satisfies the dynamical equation of the system in the absence of interaction. The total wavefunction before interaction for observer + system is thus $\Phi \, \Sigma_i \, a_i \, \phi_i$. For a good interaction over a specified time interval, this initially separated total wavefunction is changed to the mixed state $\Sigma_i \, a_i \, \phi_i \, \Phi_i$. Although the system and observer have become entangled, it is the simplifying property of a good interaction that the initial coefficients a_i have been preserved, and that each Φ_i refers only to the corresponding system-state ϕ_i. Each term satisfies the coupled dynamical equations of system + observer, with each initial $\Phi \, \phi_i$ evolving separately to $\Phi_i \, \phi_i$, $i = 1, 2, \ldots$.

Should a good observation of the system again be made, the total wavefunction at the end of the second interaction period would be $\Sigma_i \, a_i \, \phi_i \, \Phi_{i,i}$. No additional mixing between the system-states and the observer-states would occur, but the latter would again acquire information that served to identify the corresponding system-state. If we were to think of Φ_i, $i = 1, 2, \ldots$, as wavefunctions belonging to a set of "subobservers," each subobserver would consider himself as being identified with a definite state of the quantum-mechanical system, Φ_i with ϕ_i for each value of i. Every additional good observation made by a particular subobserver on the system would confirm that the system was "in" the corresponding system-state. Thus a series of observations by good interactions would lead subobserver Φ_i to $\Phi_{i,i\,\ldots}$, which would seem to the subobserver in question as repeated confirmation that the system was really "in" the state ϕ_i.

Now let the outcome of the first good interaction of system and observer, $\Sigma_i\, a_i\, \phi_i\, \Phi_i$, experience interaction with a second identical quantum system. That is to say, each subobserver Φ_i experiences a good interaction with $\Sigma_j\, a_j\, \phi_j$, giving $\Sigma_j\, a_i\, \phi_j\, \Phi_{i,j}$, with the subsubobserver-state $\Phi_{i,j}$ containing, in sequence, records of both ϕ_i and ϕ_j. The total wavefunction would be $\Sigma_i \Sigma_j\, a_i\, a_j\, \phi_i\, \phi_j\, \Phi_{i,j}$. Generalizing to a long sequence of good interactions between the observer and a set of initially similar quantum systems, the total wavefunction at the end of the interactions would be $\Sigma_i \Sigma_j \ldots a_i\, a_j \ldots \phi_i\, \phi_j \ldots \Phi_{i,j,\ldots}$, where each multi-subobserver $\Phi_{i,j} \ldots$, has a record in sequence of the states ϕ_i, ϕ_j, ... Now choose a particular $\Phi_{i,j} \ldots$, subject to weights given by the squares of the moduli of the coefficients $a_i\, a_j \ldots$, but otherwise at random. After the choice has been made, let this particular subobserver count how many records he has of ϕ_1, of ϕ_2, and so on, in the sequence $i, j, \ldots$. For a sufficiently long sequence of interactions, and for most choices of $\Phi_{i,j,} \ldots$, the resulting counts have the same ratios as do the squares of the moduli of a_1, a_2, . . . (see mathematical note at end).

We have arrived at a somewhat amazing situation. Although theory requires the observer to be associated with the totality $\Phi_{i,j,\ldots}$ for all $i, j, \ldots$, the properties of just one typical subobserver-state corresponds to subjective experience. A subobserver with wavefunction $\Phi_{i,j,\ldots}$ would find the first system to be repeatedly "in" the state ϕ_i, the second system to be "in" the state ϕ_j, and so on. Moreover, for a sufficiently long sequence, the subobserver would find ϕ_1, ϕ_2, . . . to be recorded with frequencies in the same ratios as the squares of the moduli of the coefficients in the initial system-state, $\Sigma_i\, a_i\, \phi_i$, and so the subobserver would arrive at the usual statistical results of quantum mechanics.

This brings us to the critical issue. Starting with the single wavefunction Φ, the observer's experience has gone through a series of branching, first $\Phi \rightarrow \Phi_i$ $(i = 1, 2, \ldots)$ then each Φ_i to $\Phi_{i,j}$ $(j = 1, 2, \ldots)$ and so on. This generation of an enormous tree through repeated branchings is the increasing vagueness of quantum mechanics discussed in the main article. In the opposite direction, it is the correspondence of subjective experience of a subobserver wavefunction, $\Phi_{i,j}, \ldots$, that represents the condensation of the wavefunction. The critical question is whether the Universe constitutes the whole enormously complex tree of quantum mechanics, or is the Universe confined to the particular route through the tree represented by a particular choice for the subobserver state $\Phi_{i,j}, \ldots$?

Everett considered that it is the whole tree that constitutes the Universe (see footnote on page 459 of his article). We would then have no means within the theory of specifying the particular route of which we are consciously aware. To treat all routes equally, one would need to postulate an ensemble of *alter egos* who are consciously aware of the other routes. Since each route satisfies the dynamical equations (including the interactions) quite independently of the other routes, there would be no way to compare notes with an *alter ego*, so at least in this respect the situation would be free from contradiction.

The *alter ego* concept was not a new topic in Everett's article of 1957. I recall debating it with my fellow students in the years 1937–39, and no doubt it had been speculated about from the earliest days of quantum mechanics. Opinion was, and I think still is, largely against it. One can object, not very cogently, that it assumes an ensemble of existences of which we have no evidence. More to the point, to make sense of the idea it would be essential that the many routes through the tree be uniquely defined, and while the total wavefunction $\Sigma_i \Sigma_j \ldots a_i a_j \ldots \phi_i \phi_j \ldots \Phi_{ij} \ldots$ is unique, the individual terms within the multiple summation, which determine the individual routes, are not unique. Thus one could replace ϕ_1, ϕ_2 by $(\phi_1 \pm \phi_2)/\sqrt{2}$ in the set of base states of the quantum mechanical system and then all routes involving suffix values 1 and 2 would be changed.

To have any hope of countering this difficulty one must return to the axiom according to which good interactions are always considered to exist at the behest of the observer. In the actual universe there are only specific interactions, which may or may not be good. Instead of forcing the property of "goodness" with respect to a preordained base set ϕ_i, $i = 1, 2, \ldots$, one might attempt an inversion of the situation. Take the interactions as they actually are, and try choosing a base set with respect to which the interactions are good. However, unless the interactions happen to be of special forms, with the observer wavefunction Φ chosen to have special properties, such a project fails. Indeed the conditions needed for interactions to be "good" are mathematically so remarkable that one wonders how they could ever occur in practice. My own explanation, given in the main essay, is through controlling signals from the future. The concept in Everett's argument (as in von Neumann's theory of measurement) is that interactions are made good through the observer's decision to make them so, but this surely begs the question, since conscious decisions by the observer are not themselves explained by the theory. Even so, I have agreed in the above to bypass the problem of the nature of the interactions, and therefore press on to another difficulty present in the argument at its very beginning.

While I am ready to admit that base states ϕ_i, $i = 1, 2 \ldots$, exist in an abstract sense as solutions of the dynamical equations of the system, nobody to my knowledge has ever seen such beasts explicitly. What is done is to obtain approximate solutions ϕ_i, $i = 1, 2, \ldots$, by omitting troublesome small terms from the dynamical equations (of which there are plenty quite apart from interaction with the observer!). Usually ϕ_i, $i = 1, 2, \ldots$, are stationary states, which is to say the eigenstates of an approximate energy operator. The omitted terms are then taken into account through a slow change with time of the coefficients a_i in the exact wavefunction $\Sigma_i\, a_i\, \phi_i$. As the coefficients change, one says there are transitions between the states ϕ_i.

Everett's argument can be modified to fit this more realistic situation in the following way. Let there be a time interval short enough for the coefficients a_i, $i = 1, 2, \ldots$, to be taken as constants, but long enough for the observer to establish interactions that are made good in some way, as before by taking this possibility to be axiomatic. The result of applying good interactions progressively to a sequence of similar systems is again given by

$$\sum_i \sum_j \ldots a_i\, a_j \ldots \phi_i\, \phi_j \ldots \Phi_{ij} \ldots .$$

Subjective consciousness now picks out a particular subobserver wavefunction, $\Phi_{ij\ldots}$ say. Subjectively the first system is regarded as being in the state ϕ_i, the second system in the state ϕ_j, and so on. This apparently definitive situation becomes the initial condition for calculating the transitions that occur in a succeeding longer time interval during which the quantum systems and the observer are uncoupled.

As a matter of curiosity, consult any text on quantum mechanics. I will bet (nearly my bottom dollar) you will find the author(s) specifying initial conditions for perturbation calculations by assigning their systems to explicit initial states, but rarely, if ever, will the author(s) tell you how the systems got that way in the first place. Unless one appeals to subjective consciousness in the manner of the preceding paragraph, such calculations are a pretense. Since most authors do not like to appeal to subjective consciousness, or to admit that their work is a pretense, it is understandable that they say nothing!

I will sketch the transition calculation in two ways, first as it is done after specifying initial conditions in the manner of the textbooks, and then according to the general method of Everett.

At the end of the interaction period, the subobserver representing one's conscious state has the wavefunction $\Phi_{i,j} \ldots$. Now let a period of time elapse sufficient for the states ϕ_i, ϕ_j, $\ldots$, to evolve subject to the exact dynamical equation of the quantum system, and let the observer be uncoupled during this interval. Writing $\Sigma_k\, \xi_{ik}\, \phi_k$ for the evolution of ϕ_i, $i = 1, 2, \ldots$, the wavefunction becomes

$$\Phi_{i,j,\ldots} \sum_k \sum_\ell{}' \ldots \xi_{ik}\, \xi_{i\ell} \ldots \phi_k\, \phi_\ell \ldots ,$$

where the subobserver-state $\Phi_{i,j,\ldots}$ evolves over the time interval according to its own now-independent dynamical variables.

Next, let there be a second interaction period, again short enough for the coefficients ξ_{ik} not to change appreciably, but long enough for $\Phi_{i,j,\ldots}$ to be coupled sequentially by good interactions to the sequence of quantum systems. The result is

$$\sum_k \sum_\ell \ldots \xi_{ik}\, \xi_{j\ell} \ldots \phi_k\, \phi_\ell \ldots \Phi_{ik,j\ell,\ldots}$$

in which each multi-subobserver-state $\Phi_{ik,j\ell,\ldots}$ has two suffixes for each quantum system in its record. (We have to contemplate that the details of the interactions in this second period may be different from those of the first period, and that to make them "good" a linear transformation of the base states of the quantum mechanical system may be necessary. If so, the transformation can be absorbed into the coefficients ξ_{ik} without loss of generality.)

As always, we choose a particular $\Phi_{ik,j\ell,\ldots}$ to represent our conscious experience, weighting our choice by the square of the moduli of the products $\xi_{ik}\, \xi_{j\ell} \ldots$, but otherwise making the choice at random. Our judgment is that the first quantum system has changed from ϕ_i to ϕ_k (i can be the same as, or different from, k), the second system has changed from ϕ_j to ϕ_ℓ, and so on along the sequence. Our typical conscious observer counts the numbers of the various transitions, and obtains results which agree (for a long-enough sequence—see the mathematical note) with the usual textbook calculations of perturbation theory.

Now for the general point of view. The total wavefunction before the first interaction is $\Phi\Sigma_i\Sigma_j \ldots a_i\, a_j \ldots \phi_i\, \phi_j \ldots$. After the first interaction period the total wavefunction has become $\Sigma_i\Sigma_j \ldots a_i\, a_j \ldots \phi_i\, \phi_j \ldots \Phi_{i,j,\ldots}$, and after the evolution of ϕ_i to $\Sigma_k\, \xi_{ik}\, \phi_k$, the total wavefunction is

$$\sum_i \sum_j \ldots a_i\, a_j \ldots \Phi_{i,j,\ldots} \sum_k \sum_\ell \ldots \xi_{ik}\, \xi_{j\ell} \ldots \phi_k\, \phi_\ell \ldots .$$

The second interaction period then leads to

$$\sum_i \sum_j \ldots a_i\, a_j \ldots \sum_k \sum_\ell \ldots \xi_{ik}\, \xi_{j\ell} \ldots \phi_k\, \phi_\ell \ldots \Phi_{ik,j\ell,} \ldots .$$

Weighting the choice of subobserver for each set of numerical values of the indices $i, j, \ldots, k, \ell, \ldots$ by the square of the modulus of $a_i\, a_j \ldots \xi_{ik}\, \xi_{j\ell} \ldots$ we can arrive at a typical choice $\Phi_{ik,j\ell,\ldots}$ to represent our conscious state, and we can use just the same counting procedure for determining the transition probabilities. The outcome for a sufficiently long sequence is the same.

Although the general method appears at first sight only trivially different from the textbook case, notice that it does not require a specification of our conscious state for the first interaction period. Of course our actual experience for the first interaction period will define a subobserver, $\Phi_{i',j'}$, . . . say, but there is no requirement for i', j', . . . to be the same numbers as i, j, . . . in $\Phi_{ik,j\ell}$, ... Thus there is no requirement for the state of consciousness in the second interaction period to have evolved from the state of consciousness in the first period. In the second period we may be remembering the experience of an *alter ego* in the first period.

From a practical standpoint one might think that such jumps in our state of consciousness would involve issues of only minute detail, just because it is usual for the observer to be enormous compared to the quantum system under observation. The example discussed in the main article of a city that survives or is annihilated shows such a supposition to be unwarranted. The differences among the $\Phi_{ik,j\ell}$, . . . can be very great indeed. It is probable that every so-called snap decision we make depends on only a few quantum transitions in the brain. In the ensemble of our lives, with repeated switches to the memory sequences of our alter egos there would be all the existences we would have experienced if our snap decisions had been made differently. There could be snap decisions affecting our behavior in moments of danger, the places we visit, the people we meet, and perhaps even the people we marry. There is the possibility of waking each morning beside a different spouse, although our memory each morning will always be consistent with the spouse-of-the-day, and we will therefore be entirely unaware of the other possibilities.

One tends to feel such a point of view is more suited to fiction than science, and indeed I once used it as the basis of a novel[5]. One's instinctive prejudices are not usually a sound guide to what is true or false, however. As Everett pointed out, in the days of Copernicus people argued that the Earth could not be moving around the Sun because we would surely feel the motion if it were. Such opinions, based on prejudicial judgments rather than experience, tend to be self-deceiving.

On the other hand, we must be careful not to attach false weight to the apparent generality of the fantastic multiple-picture just described. For if this multiple-picture is truly general, nothing from outside itself can be permitted. How then is the special subobserver wavefunction representing our consciousness-of-the-moment to be chosen? Subject to certain weighting factors, but otherwise at random, we argue. How then is randomness to be ensured? There can be no external throwing of dice, no external random-number generator. Without anything at all outside the general quantum mechanical tree it is hard to see how to define the particularities of our consciousness. In any case what is our consciousness? The trouble with the generality of the multiple-picture is that its claim for generality promises more than it delivers. Indeed, the most relevant aspect of our experience, our highly explicit consciousness, remains unexplained.

[5]*October the First is too Late,* Wm. Heinemann, 1966

Turning now to the single-picture theory, in which the wavefunction is condensed through the lopping of those branches of the quantum mechanical tree that lie outside our experience, we can say with advantage that it is precisely the lopping process that makes possible the phenomenon of consciousness, an immediately interesting concept that lies outside the capacity of the multiple-picture. What then is the point of the general tree? Is its function merely to be lopped? No. The general tree is really a reference tree, a backcloth, that serves to define statistical relationships that must be preserved in the lopping process.

The single-picture theory frankly admits the need for defining a particular route through the reference tree. The speculation considered in the main essay required the defining process to be through signals that propagate future-to-past, opposite to the branching of the tree itself, which goes past-to-future. It is through this difference of time sense that one can contemplate going "outside" the usual theory. In analogy to a two-stroke engine, quantum mechanics is just one of the cylinders, stroking from past-to-future. The other cylinder serves to condense the wavefunction, and it strokes from future-to-past.

I also suggest in the main article that the cylinder which strokes from future-to-past is directed by a superintelligence, and that through the condensation of the wavefunction our thoughts are controlled. Doubtless this concept may have seemed to the younger generation as the vaporings of an aging scientist, rather like the spiritualism of Oliver Lodge and William Crookes, or the "fundamental theory" of Eddington. So let me counter with a final blow or two.

The outcome, as we saw above, of a sequence of good interactions between the observer and a set of similar quantum systems each with the wavefunction $\Sigma_i \, a_i \, \phi_i$ is to produce the mixed wavefunction $\Sigma_i \Sigma_j \ldots a_i \, a_j \ldots \phi_i \, \phi_j \ldots \Phi_{ij\ldots}$. The condensation of this highly complex expression consists in the choosing of a particular subobserver wavefunction $\Phi_{ij\ldots}$, subject to the choice satisfying a broad statistical requirement that ensures that the numbers 1, 2, . . . , turn up among the indices $i, j, \ldots$, of a sufficiently long sequence in the ratios $|a_1|^2 : |a_2|^2 : |a_3|^2 \ldots$. This requirement ensures that in our consciousness, represented by $\Phi_{ij\ldots}$, we find the ratios of the number of systems with wavefunctions $\phi_1, \phi_2, \ldots$, to be the usual statistical ratios of quantum mechanics.

Subject to this restriction, suppose you were free to choose the numbers appearing in the sequence $i, j, \ldots$. What could you achieve thereby? To understand the extraordinary power such a situation would give you, consider a quantum mechanical system with only two states, ϕ_1 and ϕ_2. Then the sequence $i, j, \ldots$, contains only ones and twos. Think of the ones as dots and the twos as dashes. Although you must maintain a specified ratio of dots-to-dashes, you will nevertheless have the freedom in a long sequence to convey a great deal of information in Morse code. It follows therefore that if the condensation of the wavefunction is written structurally into our brains, information sequences can be implanted in our memory, as I speculated was the case in the last part of the main essay. This is not vaporing; it is a consequence of the nature of quantum mechanics. It is a consequence of the cylinder working from past-to-future being insufficient to establish a completely deterministic theory. There would be no such possibility if this cylinder was Newtonian in its character, since the Newtonian theory, being wholly deterministic, would have no room for the operation of the second cylinder from future-to-past.

It is insufficient for information sequences merely to exist. To be effective they would have to be read and acted upon, just as an unread book in a library is an arid thing. We can contemplate that the information sequences present in a finite material system may or may not be read and acted upon, and we can contemplate that this is the difference between animate and inanimate systems.

Starting from the known biochemical facts, we have grown accustomed to think of animate systems as being somehow connected with DNA and with a highly complex aggregate of biopolymers. But there is no reason why DNA, or hemoglobin, or cytochrome-*c*, should in themselves guarantee conscious thought any more than a lump of rock or metal does, or a silicon chip. Something of a drastically different nature is needed. The difference I suggest lies in the ability to read the sequences $i, j, \ldots$. Whereas lumps of metal and rock are illiterate, biological structures (at least at a certain measure of multicellular complexity) begin to become literate. Various animals are literate in various degrees. A dog has trouble in understanding a power station, and man has trouble in understanding religion. And so back to the concluding passages of the main essay.

Mathematical Note

To discuss the choice of a subobserver wavefunction $\Phi_{ij\ldots}$ from $\Sigma_i \Sigma_j \ldots a_i\, a_j \ldots \phi_i\, \phi_j \ldots \Phi_{ij\ldots}$ begin with the case of a system having only two states, ϕ_1 and ϕ_2. Writing $p = |a_1|^2$, $q = |a_2|^2$, we have $p + q = 1$.

For a sequence $i, j, \ldots,$ with n places there are $n!/(n - r)!r!$ possible arrangements with 1 appearing r times and 2 appearing $(n - r)$ times. Since each such possibility for $\Phi_{ij\ldots}$ has to be weighted by $p^r\, q^{n-r}$, the chance of choosing a subobserver who finds r systems to be in the state ϕ_1, and $(n - r)$ systems in the state ϕ_2, is the rth term in the binomial expansion of $(p + q)^n$. Now the root-mean-square deviation of the binomial distribution from its mean values of np and nq is known to be $\sqrt{npq}$, which becomes negligible as n becomes large. The mean values np and nq are the statistical results of quantum mechanics.

For three states ϕ_1, ϕ_2, ϕ_3, write $p = |a_1|^2$, $q = |a_2|^2$, $r = |a_3|^2$, $p + q +$

$r = 1$. Lumping ϕ_2 and ϕ_3 together, use the previous result. A typical choice of $\Phi_{ik\ldots}$ will thus have np suffixes corresponding to ϕ_1 and $n(1 - p)$ suffixes corresponding to either ϕ_2 or ϕ_3. The ratio of the weighting factors associated with ϕ_2 and ϕ_3 is $q{:}r$, and since ϕ_1 does not appear in $n(1 - p)$ of the suffixes, the weighting factors for ϕ_2 and ϕ_3 in these suffixes are $q/(1 - p)$ and $r/(1 - p)$ respectively. Applying the previous result to these $n(1 - p)$ suffixes, one finds ϕ_2 appearing $n(1 - p) \cdot q/(1 - p) = nq$ times, and ϕ_3 appearing nr times. One can progress in the same way to four states, five states, ... Hence the general result.

The coefficients ξ_{ik} appearing in the discussion of transitions must be calculated from the explicit dynamical equation of the system in question. Once these coefficients have been obtained, the discussion proceeds similarly to the above.

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