How to Describe Physical Reality?*

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Because cosmology touches our ideas about what lies "beyond" as much as about what went "before", it will always raise controversy and even cause bitterness. Although I am a classical astrophysicist, I reckon that even in the solar spectrum, some elements† indicate some general principle of the universe. At the same time, another universe than that of the astrophysicists is offered by mathematicians who conceive all sorts of objects and geometries. But there is too great a temptation to consider these constructions "real", as soon as they are plausible.

As a fellow astrophysicist, I see the universe in the same way as Professor Rees. His description of the modern progress of astronomy and cosmology cannot be more balanced, subtle and precise. I would certainly not try to contradict him on the facts, even though some astronomical observations such as "abnormal" redshifts cannot help

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† Such as the displacement of solar spectral lines with respect to their ideal laboratory vacuum, and gravitation-free wavelengths.

but remain in question. Instead, I would like to discuss the more controversial, because speculative, notions he discusses, those of cosmological facts, the Big Bang and the anthropic principle.

The cosmological facts

As Professor Rees makes clear, one fact has determined our cosmological concepts: Hubble's discovery of relationship between the spectral redshift of a given extragalactic object and its distance from us. Modern determinations of distances allows to say quantitatively that expansion is apparently going on at a rate‡ of $H_o = 50$ to 100 km/sec per Mpc. The most likely value for the deceleration parameter q_o is ½, which corresponds to a flat universe. But all measurements are in fact quite dispersed around this value, and seem now to indicate an open universe.

Two other cosmological facts are related to the Big Bang theory. The second cosmological fact often quoted and extensively discussed by Rees is that on earth we are immersed in a cosmic microwave background radiation, like being in a furnace kept at a temperature of $\tau_0 = 2.7$ Kelvin. This accidental discovery seemed to confirm the so-called Big Bang model for theorists. The third cosmological fact depends on the measurement of elementary abundances in the universe, and it seems that the proportions of hydrogen, deuterium, helium, lithium, *etc.*, have changed little since their formation at the time of the Big Bang.

Needless to say, when one discovers several facts which fit within the same theory, one is tempted not to go beyond and to relegate all other facts to secondary importance. And theory begins to act like one is led to be satisfied by Occam's famous razor that seemingly cuts through all the difficulties. It seems to me however that we have gone

‡ The megaparsec is equal, in round figures, to 3 million light-years.

a bit too far in this direction, and that many other facts should also be reconsidered as of cosmological importance. I have referred only to the solar spectrum, but they are many other examples. Hence, the selection of those observed facts which could be labeled "of cosmological significance" is somewhat arbitrary. Could not another set of choices have led to confirm some other cosmology?

The hierarchical universe

A fact which can be considered as one of the most important ones for cosmology, and which has been known since Kepler (although mathematically formalized only by de Cheseaux, and named after Olbers) is that the starry night is black and not luminous like the solar surface. As a matter of fact, "Olbers paradox" (Jaki, 1969) shows that, within a Euclidean universe that is supposedly uniformly filled with stars, and where the apparent brightness of a light source decreases with the inverse of the square of the distance, the sky should be uniformly bright at all times, and as bright as the surface of an average star like the Sun.

The Big Bang model has been considered to solve Olbers paradox. Integration ad infinitum would not be legitimate, since we have to take into account a "horizon" which is in fact the one existing in the epoch of the Big Bang, before which there was no star or galaxy to be seen. In other words, the observable universe can be considered as necessarily finite.

But this cannot be quite correct. The inflationary universe, meant to explain the quasi-isotropy of the background radiation, has introduced a horizon which encompasses a much larger mass than the standard Big Bang model does; and the horizon is pushed back to times where sky brightness was indeed very high.

But is the Big Bang the only solution? Fournier d'Albe, in a strange but far-reaching construction, and later Charlier (1908, 1922), introduced, in order to solve the Olbers paradox, the idea of a hierarchical and infinite universe, where the density averaged over a volume of radius R would *decrease* with R. If it decreases quicker than a law in R^{-1}, the divergence of the brightness integral does not occur. This can be expressed by requiring that the mass distribution in the universe be a fractal distribution of index $N < 2$ rather than the index 3 of a uniformly filled volume (the relation M proportional to R^3 being then valid; in a fractal distribution of index N, the relation between M and R is M proportional to R^N, according Mandelbrot, 1977).

A classical static model, quasi-Euclidian and obviously open, considering the black sky as a fact, allows us to predict that mass distribution in the Universe is sufficiently hierarchized. What do we observe? Strangely enough, as shown by de Vaucouleurs (1970) the observable part of the Universe is indeed hierarchized and can be described by a fractal distribution of index 1.3, that accounts well for Olbers paradox.

From white dwarfs to the Lick stellar counts, over about 25 orders of magnitude in distance, this hierarchy satisfies Charlier's condition. The Big Bang imposes an end on this hierarchy practically at the point we have now reached. It is certainly possible that it continues further, but how to be sure? We thus have here an observed fact that was predicted by the steady-state theory, and verified forty years later. But, as true as it seems, we must still admit that the Big Bang explains the facts just as well!

We meet similar difficulties with another type of paradox, derived from a nineteenth century paper by Seeliger. If the observable mass of the universe is infinite, tides produced on Earth by the most remote part of it would completely overcome lunar tides, and put us in an exploding condition. Fortunately, nothing of that sort happens! It may mean that the Universe is finite, in agreement with the Big Bang theory, but it puts a strong constraint on it. Notably, it may force us to doubt present-day views about the inflationary universe.

Other "facts" are the existence of relations between redshift and angular size, or between redshift and magnitude, or the counts of galaxies with varying magnitudes, or counts with different various radio brightnesses. Such relations were considered by Hubble and Tolman as physical tests of expansion, and they have been taken up again recently by Pecker (1986) and LaViolette (1986). The latter finds that they indicate tired-light mechanisms more than an expanding model. Within the reference frame of expansion, the tests could probably be interpreted in terms of evolution, with highly redshifted objects (*i.e.*, representative objects of a young universe) being possibly affected by the evolution of the universe as a whole.

But should we go along with a logic that starts with the Big Bang as if it were indisputable? I remember a sentence from one of the Perry Mason mysteries I read when travelling in California:

> *Mason said nothing, kept on pacing the floor. Suddenly he whirled: -We're all making the most asinine of all fundamental errors! -What's that, Chief? Della Street asked. -We're looking at the thing from the stand point of the Prosecution. The Prosecution reconstructed the crime and we're falling right in with their reconstruction. Let's go back to first principles. Let me see those photographic exhibits, Della...*
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> *(Gardner, 1952).*

So let us return instead to Einsteinian logic, although we could as well comment on other cosmologically disturbing facts, such as quasar distribution (Fliche, *et al*., 1982, Depaquit *et al*. 1985) and the arguments for abnormal redshifts (see Pecker 1976; Chu *et al*. 1984; Arp 1972, 1976).

Static cosmology

After Einstein developed General Relativity in 1916-1917, he decided to solve the GR equations in order to derive a model of the Universe. At the base of his model lies, however, the *a priori* idea of a stationary universe that had existed forever and would continue to exist; Pecker and Vigier (1976) described this universe more recently as "statistically stable and locally fluctuant" and as never in equilibrium, but everlasting. But Einstein could not know of Hubble's observations.

The only way to solve the GR equations appeared then to introduce a cosmological constant that would imply a repulsive action of gravitation at large distances. The static solution was then possible, with the cosmological constant explaining some aspects of the structure of the Universe. Truly enough, the Einstein's Universe is not stable unless empty, as several people have shown, Eddington in particular. But our statement about hierarchy does not rule out average emptiness; if the hierarchical law described above were universally valid, the average density of the universe would be equal to nought and the universe would be flat on a large scale. Moreover, assuming Einstein's point of view, the horizon would be infinitely remote. Such a Universe can account for background radiation, assuming a long-time equilibrium between matter and radiation; and it can account for observed chemical compositions, although at the expense of local mechanisms, only after considering the long but finite lifetimes of particles.

But what about Hubble's law? In a stationary universe, either disappearing mass is immediately replaced - the steady-state model of continuous creation conceived by Hoyle and his co-authors—or the redshift is not at all produced by motion but by some other as yet undiscovered cause. It seems obvious, then, that such static models are not suitable, in that they do not easily account for Hubble's law. Or don't they? Adequate models are the tired-light models that have been introduced several times in history. They never lasted long because of a lack of detailed physical mechanisms that could be checked in laboratory and that could describe all the interactions, whenever the Doppler-Fizeau effect is a well-known laboratory fact. But recent work by LaViolette (1985, 1986) using subquantum kinetics, and by Pecker & Vigier (1986) through studying interactions between photons and a Dirac vacuum, might well revive the consideration of tired-light mechanics.

Another very interesting idea is Segal's highly developed "chronogeometry" (1976). Segal believes that a causally oriented space-time must be the basis of the definition of time sequences. To introduce into it such notions as observer, clock and rod, which generalize Special Relativity, it is natural to assume and exploit group invariance theories. This broad definition of time sequences allows one to define two times—the locally valid *t*, and *t*, valid at large scales, and their introduction is necessary to impose a causally asymmetric space-time. Although I can hardly describe this theory in all its mathematical aspects, its consequences can be tested. The strangest consequence is a "square law" redshift-distance relation. Redshift is a measurement of the difference between the two times of a galaxy and of the observer, and Segal's "universal cosmos" is locally tangential to the local Minkowski space-time. Nicoll and Segal (1980) have shown that this seems indeed to be the case, but a strong opposition claims that the statistical samples were not valid. It seems actually that local motions within the local group and the local supercluster are by far too large to allow this criticism to be unambiguously valid. Moreover, distance determination methods implicitly assume the linear law, to which the results must adhere.

But at least Segal's prediction is more compatible with the observed distribution of quasar redshifts than are classical theories. In addition, Segal explains background radiation as the result of interaction between the light from distant galaxies and the medium traversed by it. We should say that the location of various solar apexes and the distribution of velocities towards it give the background radiation a quasi-local character, which is justified both by Segal's theory and by tired-light mechanisms. I should mention recent work by Segal and by Roscoe (unpublished) as new developments in chronogeometry.

A cosmology based upon a variation in the physical constants with time was proposed in the late thirties by Dirac (1937) and was renewed, amongst others, by Canuto and Hsieh (1979, 1980, 1981). Notably, the variation of G with time would induce a redshift and its value could allow computation of Hubble's rate of expansion. Some years ago the analysis of Earth and Moon motions (strongly depending upon the astronomical theory of perturbations) gave an apparent basis to this theory, but was later shown to be in error. It was also shown that up to a large value of redshift, the constants e, h, c, m_e are actually constants; the fine structure constant is constant in the totality of the observed universe. Thus, there is not much to connect the theory to; but we might mention that it predicts the background radiation just as well as any other theory.

The value of proofs

Background radiation has been a strong argument for the standard Big Bang cosmology. It is true that in the years 1948-54, Gamow, independently and then together with Alpher and Herman, developed the idea of a hot Big Bang, which explained the chemical composition of the universe, as the result of a sort of quasi-metallurgic tempering, that could freeze-in the chemical composition of the Universe at the time (roughly speaking) of the decoupling of matter and radiation. As a by-product, the three authors predicted the existence of fossil radiation and gave various estimations, which themselves were wrong because based on incorrect values for the main parameters.

But it is also true that, at the same time, Finlay-Freundlich (1953, 1954) published four papers and Max Born (1954) two others, as comments on the former, and all developed the idea of some as yet undefined tired-light mechanism. They went on to predict the existence of a background radiation of about 2 K; and Born added in a foot-note that this radiation might be observed by the newly developed radio telescopes. Our problem then is that two completely different theories predicted beforehand background radiation before it was detected; hence, background radiation cannot be a bona fide test, no matter what errors might be detected in the analyses leading to the prediction! In other words, for any such theory, the predictive power is not a sufficient argument. Actually, practically all cosmological theories (chronogeometry: or changes in physical constants, or tired-light mechanisms) predict or explain a background radiation of 3 K. One may criticize the artificial character of these deductions; one might not be convinced. At the least, it proves that the existence of background radiation is no “proof” for a cosmological theory; and this is true as much for the hierarchical universe as for background radiation. Some “facts of cosmological significance” can be accounted for by different theories, then they prove nothing!

Difficulties with the Big Bang

Early ideas about the Big Bang originated from two different areas: Hubble's discovery and the Friedmann-Lemaitre non-static solutions of the GR equations for a non-empty universe. The latter gave way at a certain epoch in the past to a singularity that could be determined knowing some observed factors, in particular Hubble's rate of expansion. This singularity creates problems, however. And Professor Rees's paper discusses it. To his expose I would like to add only my concern with the definition of time, because the classical definition underlies all GR equations.

The standard Big Bang theories can no longer be considered satisfactory, even though, at its blooming, it seemed "the best in the best of all the possible worlds." The GR equations, which were conveniently simplified by Friedmann by taking out the superfluous cosmological constant, led to simple models. These models then admit, as a maximum value of the age of the Universe, the so-called "Hubble age," which is derived from a simple extrapolation of Hubble's present and local rate of expansion. One's love of simplicity was in essence well satisfied, but flaws couldn't help but appear.

First of all, Hubble's rate of expansion was disputed then as now. Between the school of Sandage and Tammann who constantly reduced the Hubble rate and hence increased the "Hubble age" to values on the order of 18 to 20 billions of years, and the school of de Vaucouleurs and others, who found a higher rate of expansion that implied a smaller age, say 10 billions of years, the battle has not ended. The age of 18 billions agreed at least with whatever else we knew about the Galaxy. Let us quote Sandage (1975) himself, in the last sentence of the last chapter of the last book of the epoch-making series "Stars and Stellar Systems," edited in the sixties and early seventies by Kuiper and Middlehurst:

> *This, plus the evidence that the Universe evolves (equality of age of other galaxies, similarity of H^{-1} and the age of chemical elements, and the possible observation of the time horizon for the birth of quasars), is consistent with the view that a universal creation event did, in fact, occur.*

This was true, for the older globular clusters were 15-17 billions years old, and everything was still nice and clean.

The trouble is that de Vaucouleurs' value seems now more correct than Sandage's; the age of globular clusters has probably been underestimated, as inferred from progress in the theory of stellar evolution, after taking turbulent diffusion into account. Hence the Friedmann models cannot be considered valid anymore, and the cosmological constant must be reintroduced. The need for explaining the isotropy of background radiation has also led to an "inflationary" universe. So the simple model has moved progressively into a very complicated one, as one *ad hoc* assumption after another was added; and despite an already quite sophisticated construction, many questions remained unanswered. Occam is left to cry in his grave over the need for "too many epicycles."

Of course, this is no problem for those who have faith in the Big Bang. There are ways to squeeze more parameters into the equations, to invent inflationary models, and to keep the Big Bang alive in some modified version. It is not a happy evolution. Starting with the very simple Big Bang, an idea full indeed of metaphysical connotations, one has since elaborated on it in order to justify it at the expense of simplicity. A veritable Procrustean bed! But our suspicion about the quality of proofs leads us to consider that the cosmological problem is still wide open to research and controversy. Although the inflationary version of the Big Bang is likely to be the most widely accepted model nowadays, I would hate to see research limited to its elaboration.

At this stage the reader should remember that any cosmological view is strongly influenced, whether one wants it or not, by metaphysical preconceptions. Einstein wanted a static universe for quasi-metaphysical reasons; but on the other side, as early as 1951, His Holiness Pius the XIIth (1951) said, almost verbatim, that "the Big Bang is the Fiat Lux." Of course, they were both rather poor theologians, and St. Thomas of Aquinus (around 1270) would have smiled at their comments! But it does not alter my point. Hannes Alfven (1976) explicitly considered the Big Bang at most a "wonderful myth", and I am tempted to agree. More recently, Narlikar (1986) stressed the implausibility of a singular point in the evolution of the universe. We are still far from being able to say what happened up there and before, so that all avenues are still open and I can only advise the reader against any dogmatic attitude in the field.

Visit to Flatland and multi-dimensional universes

Meanwhile the mathematicians' imagination is blooming with marvels, and fantastic topologies can always be constructed. Are these fantasies (a fantasy, being "something which can be thought of," does exist of course) suitable for describing the "real physical world"? "Si notre vue s'arrête la, que l'imagination passe outre; elle se lassera plutôt de concevoir, que la nature de fournir". (Pascal)

It was already clear in the last century that non-Euclidian geometries would not be easy to understand using common sense alone. And this difficulty led E.A. Abbott to write his account of the "Flatland" - by a square - even before H. Poincaré's popular "La Science et l'Hypothese." In this fantastic two-dimensional country, the landscapes are strange. If, for instance, three-dimensional objects were to cross this space, they would appear from nothing and disappear into a vacuum, which has been used by science-fiction writers to predict the visits of strangers from another dimension. And it leads logically to the question of how many dimensions has our physical space, the space of the real world?

The usual three-plus-one dimensional space-time has been generally considered suitable for describing the astronomical universe. But for the microscopic universe, it seems quite appropriate to adopt a more complex description. Could it be possible to have a three-plus-one dimensional world imbedded in a three-plus-more-than-one dimensional universe?

I wonder whether this analogy can really be pushed forward too much. Indeed, we *know* that there can be no planar flatland imbedded in our space. Even the flatter animals, such as the stinkbug, are truly three-dimensional; their sight is three-dimensional, as are their sensations, even though they can move only on a plane or even along a given line, or even not move at all as is true of plants and minerals. Truly enough, the projection into a two-dimensional space of some impossible three-dimensional configurations might make them appear real as do Max Escher's suggestive drawings. But seeing these structures on some etching or photograph in two dimensions does not make them more real if reconstructed in a three-dimensional landscape. They are truly impossible! Holograms may also be both real and impossible.

Of course, one has tried to get rid of some of the difficulties in cosmological thinking by constructing strange topologies, *e.g.*, by thinking of the universe as a Riemann surface (of a high order) with loops, holes, tubes, and a multiple connectedness, but I am not quite sure there is any need for these fantasies. That mathematicians can fantasize about these situations does not imply that they can even exist in reality.

But to describe the microscopic universe, it does seem necessary to bring in some multi-dimensional space that implies a whole closed geometry around each point of the three-plus-one dimensional space to represent particle properties. As early as the twenties, Klein and Kaluza (*cf*. Freedmann, Van Nieuwenhuizen, 1985) have introduced similar considerations; and they have been developed further by Green and Schwarz (1984, 1985), among others, leading to the so-called "superstring" models of particles that replace the point-like description of a particle by a multidimensional description. Particles are then associated with vibrations that occur along the strings. The theory behind it all can predict a finite but large number of elementary particles, but it is still being developed. But this multi-dimensional geometry may only be considered as a tool, since it requires a large number of parameters. And it is not identical to a geometrical description in that it still includes three spatial dimensions, one time dimension and six extra dimensions, all of which can be treated together as ten coordinates (in a way similar to general relativity) to represent the coupling of the material content of the universe with its space-time geometry.

Needless to say, each of these theories will take its place in all future attempts to describe the physics of the early standard universe, if there ever was such a thing, and in the various trends towards a GUT. For this the treatment of complicated gauge symmetry groups [such as SO(32]) is needed. But whatever the issue of this research, it will neither be settled by tomorrow nor necessarily offer any clear-cut choice between cosmological models.

The anthropic principle

Professor Rees's account is interesting in many ways. First it shows that Professor Rees, a prudent promoter of this new avenue of thinking, has been somewhat mellowing his point of view. In their 1979 review paper, Carr and Rees conclude:

> *Even if all apparently anthropic coincidences could be explained in this way, it would still be remarkable that the relationships dictated by physical theory happened also to be propitious for life.*

Now he tells us that: "*the anthropic principle cannot claim to be a scientific explanation in the proper sense.*" No, it does not, indeed. It reminds me of the famous eighteenth century naivety of Bernardin de Saint-Pierre, who seriously claimed that cantaloupes were divided into sections so that they could be eaten in family dinners, or that the fleas were black in order to be more easily plucked off white skin. This goes much beyond anthropomorphism and is linked to philosophical thinking. about "final causes" and "prime causes." The appearance of finality certainly can exist but only on earth when looking at the evolution and adaptation of animals: birds have wings, so that they could fly. No matter what one claims about the anthropic principle, it cannot help but be so deeply affected by prior metaphysics that I will find it hard to agree to its being called scientific fact.

Professor Rees's suggestion is to consider separately, independent of the inherent teleology, the idea that beyond our universe, built as it is, with our values of the coupling constants, there may exist, as suggested by Everett (1957) or Wheeler(1971), other universes in different forms. Are they able to interact with ours? If not, I have nothing to say; I could even claim I could not care less! And if so, then how? This of course would mean that the universe as a whole contains several smaller universes; one easily observable, while the others interact with it. This would also modify completely the thermodynamics of our own universe, no longer an isolated system.

I must say that I am reluctant to get involved in so many subtle possibilities. Indeed, the very fact that we can conceive of a principle of anthropic selection contains in itself a basic contradiction. That we can conceive any thing, along with its contrary, shows the high degree of evolution we have achieved; and this in itself seems to more or less exclude the possibility that the universe has been selected for its convenience for humans. And the same can be said for any other species that has existed or will exist, be it able to observe consciously the universe, or not.

Another idea I am reluctant to agree to is that man can be inserted logically into the chain of hierarchized structures, as has been suggested by Carr and Rees in 1979, for the size ratio of the different elements in the chain is much too simple: *e.g.* man = $(\text{planet} \times \text{atom})^{1/2}$; planet = $(\text{Universe} \times \text{atom})^{1/2}$. Since each element in the chain (why are some elements suppressed?) constitutes a continuum of values (in the animal realm from the flea to the whale), how can these continua be introduced into these magic non-dimensional ratios? This seems to me more a resurgence of a pre-Copernican attitude and its extension to the scale of the presently known universe, than a bona fide scientific discussion.

I nonetheless feel that divergences between Professor Rees and myself are less severe than it may appear. I am perhaps more open than he to cosmological models that do not imply temporal singularity and less tolerant than he of excursions outside the outer boundaries of science: I am somewhat worried when seeing theological thinking entering the scientific realm! But in either case, as long as one is willing to keep to the observed and proven facts, there is no reason why we should not speculate about the world. The only real danger would be in confusing this provisional, imaginary world, which may at any time lead to either good or bad theories, with physical reality. A working hypothesis is not a theory but only a starting point, and much remains to be done afterwards. Meanwhile, and perhaps forever, the basic question remains: how can the "real world" be described, and what is the appropriate mathematics to do it? (I am grateful to Drs. Collin, Demaret, Narlikar, Schatzman, Schneider and Vigier for enlightening discussions, and to Joli Adams for the English).

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