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La conférence est résumée par un Copernicien radical : nous pouvons mieux comprendre le monde lointain à partir de la compréhension que nous avons ici. Trois vastes questions sont incluses : le programme de Hubble, étude du flux cinématique des galaxies, principalement à bas décalages vers le rouge, afin d'évaluer les constantes à $z > 1/4$, avec une attention spéciale aux déviations à la simplicité, et au rayonnement de corps noir ; la physique des décalages spectraux en général.

Conclusion : aucun cas cependant contre Copernic, Newton, Maxwell, Alfvèn, et le monde classique Einstein-Friedmann jusqu'à $z \simeq 1$, peut-être au-delà. Il se peut que les galaxies crachent de petits objets par des processus classiques, les grands nuages radio le font surement mais sans signe <u>prima facie</u> de nouvelles physiques.

I don't know about the universe, but I have now produced a rather good first observation about colloquia of this sort: namely, they are organized as a hierarchy; there are summaries, summaries of summaries, and reviews. I'm afraid you're going to hear another one. I will not, obviously, be able to mention either by name or in substance every paper that we've heard but only, so to speak, those points which a fairly reflective observer with his own prejudices and interests finds salient. The record will contain a summary; as I say, many of the papers themselves are summaries upon summaries. It is fair to say that there are schools of view over a wide range. We have our prejudices as pointed out eloquently by a couple of speakers today. I

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would like to make a few remarks which may help you to calibrate my own views, so that you understand which way I am neglecting or over-emphasizing something that you may have heard. That's all I can hope to do.

I would like to pronounce myself a radical Copernican; Of course, if you say the world is at large exactly like here, that is very premature and quite wrong. The sun is not at all like the earth; to assume otherwise would be an absurdity. So obviously I have no sure criteria for action at all; you must have a degree of judgment. Science has a double quality: that is intensely conservative, because something that is firmly known must be kept. Yet sometimes we are quite willing to throw away something which contradicts one well-defined observation. With this familiar dialectic, I should like to view the universe as though it were based on laboratory physics up to the point where there enters a new length, or a new concept of nature. I know one length which I guarantee makes trouble for me: That's the length measured in time units at ten billion years. When I reach lengths of ten billion light years, or times of ten billion years, I know from experience, that something is very different about the world. Maybe the laws we know are right even beyond that--that's a great hope--but it is not sure. From the length of Galactic distances or out to binary stars, of the order of a few kiloparsecs, up to that 10^{10}-year distance, I will be loath to see a new length introduced in any way. I think this is a fair picture of a conservative view of a Copernican attitude towards cosmology.

I'd like to justify this from the history of the greatest of all scientists from the point of his impact on the modern world and the modern mind, Charles Darwin. Of course, he did not study cosmology, but established the ten Gyr better than most people who specialized in that sub- in his time. As a young man he was tutored by a well-known geologist, no famous person, but expert, productive and cheerful, a man called Henslow. One summer vacation, full

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of paleontological enthusiasm, which in the 1820's was about like cosmology today, a big thing in all the papers, Darwin on vacation came across a claypit not far from his home. The workmen were working quite far down in the stratum which yields clay for the bricks of London. One of the workmen recognized that the onlooker was a young man of good dress and proper bearing, obviously with more income than the workmen. He came to Darwin to say that he had found a beautiful object 60 feet down in the claypit, which he would sell to Darwin, if he were interested, for a small sum. Darwin coughed up the shillings and got in return a beautiful large pink shell, like one of those conches you see in the West Indies. When school began he rushed back to display this spectacular result: a modern tropical shell in the London clay.....You know the answer--this workmen had a small income from watching people come by who wanted paleontological specimens. He was prepared to fill orders for any variety he could pick up in the market. This would demonstrate forever to Darwin that it is difficult to attack a well-established body of material representing much experience by one spectacular fact! This is the kind of lesson which occupies us here. That is not to say that we insist on retaining any grand structure; on the whole, the present structure is very imperfect, and we can see that plainly. But we do not for light reasons change the forms to which we are established and thats the situation. It is the weight of the new reason that counts, of course. As far as I can make out, there is a superficial argument that a Newtonian view of the world dominates astronomy for very many kiloparsecs around. Of course I allow special relativity, and I allow quantum mechanics as tremendously well-tested laboratory generalizations now in the same general scheme with those, not to mention input from Maxwell, Alfvén, Dirac, still within the same framework, we can explain whatever we can explain, which is not everything. We can't quite explain pulsars but at least we can try. Only if Cygnus X-1 is--which I don't

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believe--a black hole, will I be faced with something that goes beyond the lab.

Cosmic rays and light involve us essentially in special relativity, but they've been subsumed pretty well into this picture. Yet astronomy is not physics, and I am pleased that it is not. We are dealing with excessively limited data; we can't even go around to the other side of a galaxy to look there. It is plainly a very worthwhile thing to do, but impossible. A physicist committed to looking at only one side of his apparatus would really be in very bad shape. He just wouldn't know at all what is going on. Our poor substitute is to understand New York by studying the south end of Moscow and the north end of Tokyo. We hope by looking at lots of examples and averaging we can get something, usually not showing the Gaussian distribution or even the presence of finite moments of the distributions we use.

Fifty years ago Hubble's program was under way. Fifty years ago also quantum mechanics burst upon the order of physicists overturning every fundamental principle that they then held. But of course it gave a new enormous power to the description of nature. I remind you that fifty years before that, we were at the level of Hertz demonstrating the universality of electromagnetic radiation throughout the spectrum, and of Michelson and Morley and of Maxwell and Boltzmann. There is an enormous gap between those two times. If I look now at the Hubble program, there is much less gap, in spite of the prodigious success of our experiments. We still have the intellectual program of Hubble right on the agenda at this conference, just as it was 50 years ago, with most of the same points, most of the same disputes, without much change: candles, rods, and the universal flow. Skillful and acute observers had first to show our Galaxy was not transparent at all; therefore the outer galaxies look anisotropic. To understand the north polar and south polar Galactic caps was a big discovery. Perhaps it was the fundamental discovery that enabled us to understand that the universe was not a universe of one galaxy including a lot of green and funny white nebulae. Isotropy and homogeneity in some degree were pressed hard by Hubble with special samples. He tried to do log N-log S and made some progress, more or less the kind of progress we have made since in radio: pretty good, but how good is not clear from direct observation of any objects.

Then we found that red shifts, by then utterly familiar in the stars, persisted to these other objects with two or three orders of magnitude increase, so that there was no argument really about the difference. The argument was by analogy, a very strong analogy; a typical star redshift multiplied by three orders of magnitude gives the redshift of the kind of distant galaxies that we now study, which Hubble just touched towards the end of his work.

The galaxies showed peculiar motions. They were not flowing out precisely uniformly. The Hubble line was a band; the nature of that band seemed dynamically understandable. Finally, the scheme to get numbers out of this was a sucession of calibration steps made on the objects nearby, governed by internal and external arguments about the validity of the samples, small samples, impossible to make larger because there are no more objects of the right kind close by at your disposal. I could cite these points out of The Realm of the Nebulae. I think most of the optical people would agree it's pretty much that way still. Only radio (and X-rays perhaps) add other dimensions, a true enrichment.

It was an extremely acute remark on the panel this morning by Roberts, that Hubble left one thing out. Most of us too have left it out. In the last few years graphic pictures of crazy galaxies and computer models which simulate some of these crazy galaxies have shown us one novelty: namely, the notion that most galaxies manage all their lives independently is probably wrong. At least some galaxies in large samples are bound to be affected with severe gravitational interaction with their neighbors; maybe all, or most, conspicuous galaxies are heavily affected by unnoticeable perturbations because they have had small neighbors which they digest. Unlike the python they don't show a bulge in the middle (just a bulge in brightness). I think Hubble would find only that new, apart from the new channels, the care, the deeper physical understanding; the general intellectual level of our program hardly looks different from his. We have heard of a series of admirably careful and painstaking stories, the efforts to establish the constants. I have to say a little bit about that because so much time was devoted to it-- not that I can sum it all up in a single best parameter, yet enough to make me unclear about quantitative astrophysical models of extragalactic objects. It does not make a lot of difference, to the cosmologist, properly speaking, whether 40 kilometers per second per megaparsec or 120 kilometers megaparsec is H_o. It makes some difference, but when we get to the ages of the stars it becomes urgent. Of course, we might have to reconcile the extrapolated starting point compared to the curvature, but there are so many errors in all of these quantities that at the moment that isn't the primary problem. The problem is to fix the distance scale of the cosmological problem so that we gain a better understanding of the astrophysical hardware that furnishes out this grand theater. We cannot understand that hardware if we don't know its volume, its mass and its energy content to a factor of ten. That is why I think H_o is valuable, much more than for the neat result. I would not apply that to the problem of q_o, even though q_o clearly has profound implications whether the universe repeats in some sense or doesn't, very important to the philosophical ideas that we all have.

In principle for me, the stars shine and move from the Newtonian point of view. The stars are characterized by $\underline{r}$, and $\underline{v}$ and $n(\underline{r},\underline{v})$ just like points in a real interacting gas, albeit a terribly unstable one, whose Debye length is imaginary, which loves to fragment like crazy in all scales instead of

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radiating to chill itself off, like a reasonable gas whose density you can measure. These motions are real; to some extent we are prisoners of our language especially in the domain within 100 megaparsecs, the general domain where we can really see some detail, without quasars. For we talk about expansion and expansion motions. If I understand the theory of general relativity correctly, I submit it is not possible to distinguish an expansion from an ordinary motion. The parts are just flying apart in space under gravitational interactions. It's true that it is not possible to prove the case; they could also be gravitational potential changes, and so on, but the theory says, I think, that it depends on which framework you choose to be in. So there is no particular reason not to regard these things in the way which is most familiar: namely, as an extension of flinging a ball into the air. It is not different from that. We speak of hydrodynamic flow because it is easy to compute, but the number of stars observed in the universe as a whole falls short of being a millimol, and a millimol of hydrodynamic fluid is a tough thing to work with on a chemical-hydrodynamical basis, especially if the density is such that the mean free path is gigantic compared to the spacing between the objects. We see some kind of a Knudsen flow but with strong interactions between individual particles. There is not, I think, any mysterious property besides. After you measure these flows, you hope to see some broad patterns of the flow, but they're a limited set of objects; they do have internal motions; they do have individual differences.

The distribution of the properties of stars and galaxies with respect to any parameter is in general uncertain, hard to measure. Such limitations almost always affect the end of the distribution where the high moments come from. Small-sample errors are very heavy. On top of this, of course, the selection resulting from our instruments and our position are intense. We can rely on der Herr Gott not to make a sample depend absurdly upon a parameter which you

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can from first theoretical principles, deny as relevant because it depends on your local position, but you can't quite as easily be sure that it depends upon phenomenological internal parameters in such a way as to give it a reasonable look--a linear or a logarithmic curve--and hope to use that for corrections, without thereby risking error. That error is not easy to estimate unless non-parametric tests work for it, and that is not always the case. It is plausible that HII region diameters, HII rings and supergiant groups all have a relationship to the luminosity or maybe to the mass, or the type of the galaxies that they sit in. But if I don't know physically why that is, and if the regularity goes on for some range in the parameter and then turns around a little bit, I would not be surprised. It's nice to say that it goes on in a nice simple way, but maybe it doesn't. That generates an error which it is very hard to cope with. Until you can bracket, or get real distributions, or understand it in some physical way, it seems to me that the best you can do is have hope to use the regularity while it's here, but not insist that it remain forever as a cornerstone of the theory. This affects not only cosmology but all of astronomy. It is all based on model-dependent assumptions. Usually the assumptions most easily at hand are the ones that give the greatest sense of regularity, but very often they're wrong. When I was a graduate student I went to consult a quite able astronomer. We had a problem with a certain star he had measured. It had such a big redshift that it was ascribed to gravitation. But it couldn't possibly satisfy the theory, and endangered my thesis quite a lot. I complained a little about this; could that redshift be something else? For example, could it be an unusual radial velocity? And the astronomer explained to me that since we knew the proper motion of the star was already quite large, we didn't have anything left over for the radial motion, for then it would escape from its group. This argument is plainly wrong; I think the star has been ascribed

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to a field to which it does not belong. Again a small hopefully-treated sample leads to trouble. On the first day (happily today moderated) we encountered a very gloomy moment when the two most authoritative reviewers of the Hubble constant situation each described, in response to acute questions from the audience, a domain outside of which it was unreasonable to expect the Hubble constant to lie. But their two domains had only one point in common! Ordinarily that would enable me to fix the Hubble constant with good precision according to mathematics, but I refrain from the conclusion. It is clear that this problem of external and internal sample irregularities, the problem of choosing among subsets of possible calibrators which ones are the best ones, gives a very serious problem for those who are not involved in it. I suppose that the pedagogical value of a careful discussion of all the sources of this difficulty in some simple way, and the establishment of wider but more robust limits, which has been tried by several people in this discussion, would be a very valuable thing for us all to read. Dr. Tamman once said he could not see any way to fix the lower limit of the Hubble constant, and I had a dreadful sense of that universe of zero Hubble constant which I couldn't understand at all. I think that there must be some way, because galaxy velocities are seldom positive and it's going to be very hard to show that the universe is actually free of all regular motion. This problem clearly is something not ended. On the other hand, it would be equally fair to say that the factor of 10 difference between today and Hubble's day is a factor we will not wholly relinquish to future errors. I am satisfied as to that with a high probability, if not perfectly: it seems to me that the nature of the detail, the nature of the examination of crude errors, precludes us from ever going back by a factor of 10, which is a great help. For a factor of 10^3 in object energy frightens a theorist! We will have some answer, perhaps not terribly soon or terribly generally, simply because of the

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limitations that are inescapably placed upon all astronomy which works on the margin of available skills.

The problem of isotropy has also been discussed, with a pretty good demonstration that at most galaxies display a small anisotropy, not large compared to peculiar motions. For cosmological purposes that is pretty good. If the inhomogeneities which are implied by some interpretations of Hubble constant variability, inside and outside some nearby region, turn out to be real, I would take it that we can understand them; it is not a problem, it seems to me, which lies in any fundamental way before cosmology. It is the physics of the motion, of the flow, of fragments into pieces and pieces of pieces. The values of n, r, and p are sure to have, it seems to me, correlations and regularities, on a small but not on a gross level. Even the masses, velocities and types of galaxies can correlate, in this conservative, Newtonian point of view. I strongly suspect the galaxies are made of old gas, and that the local density, velocity and angular momentum, and the local interactions of that gas, determine the types and motions of the galaxies today. It is very unlikely that some sign of this does not persist in some way to the present day. I don't think that it's extraordinary to find such things. Of course it's very hard to recognize them in view of the two-dimensional projection, the inability to recognize correlations between p and r simply, selection and so on. These are complex but dynamical problems, problems of unstable gravitating systems in an expanding background, that is, in a fluid whose general overall motion is expansive. We're now beginning and only beginning, to make some progress towards these problems, and we're very far from forming galaxies of a certain type.

All this talk has been about low z; but the excitement tends to come at high z. Here too we saw that the simplicity of the Hubble law, this wonderful line, which I observe is always on log log paper. (Cosmology can be defined as that science which always expresses its data in log-log coordinates.) Once you do that, you've concealed a great deal. It was made eloquently clear that the heavy problems of selection affect the intrinsically-brightest end of the Hubble line more than the nearby inner end. Looking for deviations from the simplest law by these means is on selection grounds alone a very hard problem, which might yet be approached and is being approached by some workers. We heard a still more pessimistic but persuasive account that this effect, heavy as it is, is dwarfed by a new problem. Looking at high z from here is not looking merely in distance from 100 megaparsecs to 10 thousand, but is looking back in time by tens of billions of years. We have strong reasons to believe that the universe as a whole was then quite differently constructed. Look back in time means we have a problem. Even if the galaxy does not interact, it would age. That aging is not easy to separate, though brave nuclear-energy producing theorists can be found to do that. But they were themselves convinced that if galaxies interact then the clump of matter you identify now and the clump of matter you identify then are not guaranteed to be the same; in fact,they are unlikely to be the same. Until we have a better understanding of that, or a new way to select objects, it looks as though the program of Hubble seen as a straightforward kinematic program, is close to its end. That is the view I take. It is not quite at an end because we don't have the number, we don't have the slope of the line, we don't have limits we would like to have, I believe, for these near objects with the corrections we need. But it looks to me as though the 50-year scheme has come to an end with technical virtuosity, with much power, but not quite the ability to complete the program, not enough to realize the old program for H_o and q_o. The enthusiasts of general relativity in the 20's and 30's, unencumbered by much knowledge of galaxies and their behavior were able to convince Hubble to try it, with the result we know much more about the universe than we could have otherwise, though what we

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know is more qualitative than numerical. What made this most striking to me was that indeed we have a new powerful Hubble program, going on in 21-cm astronomy! I found this extraordinary and splendid. Indeed I was delighted by the deep agreement between the 21-cm scheme and the others not put off by the discrepancies which appeared between one and another Hubble constant. The 21-cm scheme by the way is being calibrated exactly by what seems to me the most dangerous kind of calibration, namely an internal sample correlation which you can't really understand, between gross features of a rotation curve, corrected out for inclination, and the behavior of a certain small number of nearby galaxies which can be calibrated in the same way. I think it would be very valuable to try to think through that physics. Are there better ways to select galaxies, maybe by inclination (I don't know) or a high-resolution picture? We need really serious effort to improve this whole situation so we can work on something a little bit better than frequency cutoffs which seem so arbitrary. If the sample really works that way, you can't really complain, but you feel uneasy because the self-consistency of any single method is the very problem we must fight against in all this business. For self-consistency does not relate data to the meter stick and the second, and that's what we have finally to do.

But it's certainly very important, I would say that the careful insight and understanding of the optical observers plus the new 21 cm possibilities give the last vigor I think to Hubble's original program. When they agree on a good number and on the difficulty or the lack of difficulty in the kinematic study of q_o then that will probably be the close of the program 50 years old in science, which I think is remarkable.

Now I would like to come to the problem which I would first like to call new forces, or new processes in physics, in the cosmological domain. This I think dominated here the second couple of days. The introduction of new forces

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in cosmology is a traditional and time honored procedure and without studying history very much, I can assert that it goes back at least to Einstein, who in my view, and he too said it later on, made the biggest mistake of his career by insisting that a constant of the motion from the integration of the field equations for the homogenous case could lead to the lambda constant, which produced a force field of arbitrarily great strength, attraction or repulsion at will, so to speak, out of nothing, out of a constant of integration. Now we agree that initial conditions might do that, might give you a constant of integration, but can they give you a genuine force field? This is really very unnewtonian and un-field like, and made everybody a little uneasy. That's not to say that I know it's wrong, but it is certainly a big step. It is no bigger and no smaller a step probably than the kind of steps people now make towards a new cosmology; we should remember that it was done traditionally. On the other hand, a Copernican view would certainly say to struggle hard against it; we only admit it either if the data forces it inexorably, with no easy way out after a decade of striving. That has not come. Or some laboratory experiment with greater penetrance into the problem might give us a clear interpretation. I think that guide is still appropriate for all the other novel cosmologies.

Two things have happened in cosmology since the Hubble program, it seems to me. First, the rich growth of our understanding of the cosmic furniture, so one can begin to make corrections and understand what you are doing. That is a very large piece of astronomy. Secondly, one discovery of a decade ago: namely the three-degree background. This was a kind of uninvited guest here in Paris; but it is of dominating importance, and we cannot afford to overlook it. I myself have feared for many years, in the light of the increasing elegance with which one fitted the thermodynamic predictions of the blackbody spectrum and the increasingly refined measures of better and better isotropy, that we

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were headed for a possible crisis. That crisis would be that the blackbody radiation turned out absolutely isotropic. We would be sitting at the center of the velocity space of the universe, very bad for a professed Copernican! It made me very, very uneasy. I have welcomed repeated rumors and gossip and even some publications, which claimed they had found the effect, but it always proved a disappointment. I am happy to hear that one of the three groups who by the end of the year 1976 are likely to have made independent measurements has found a result--mentioned here--so we are at least for the moment free from the unpleasant contemplation of ourselves at the center, not in position space where there is no center, but in velocity space where there definitely is a center.

We seem to have a few 100 kilometers per second of motion with respect to that; by the way, that is not the motion of the rotation of the galaxy but goes against it. The Galaxy is moving five or six hundred kilometers per second with respect to the photon gas; we here are moving 300 kilometers/sec in such a direction as more or less to cancel with some 300 km/sec left over. That looks like a rather sizable peculiar motion for a Galaxy that belongs to a small group; only perhaps generally to a larger neighborhood. Still, I find that very satisfactory. It seems to me that confirms the view that the flow is not perfect but has all kinds of interactions in it, and all kinds of chance initial conditions. We ought to expect anisotropies as well as peculiar motions and disturbances of the flow by that sort of amount, but not by an amount 10 times larger, because then it becomes uncomfortable. It is a happy situation when we look at the present state of the data. On the other hand, I now have to say something a little less textbook-like. The red shift z seems to have plausible validity as an extension of Newtonian mechanics, out to z at a few tenths or even more. There is some problem with quasars, but that is the domain we are really

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talking about. The spectacular success of the Friedman GR universe reaches beyond $z \cong 5$ to the order of $z \cong 1000$ if we are correct in so interpreting the black body radiation--so far without successful challenge--this is extraordinary. Then I am led to a quandray. For, as I made clear, I am suspicious of wide extrapolations beyond what we know. We have now seen z up to the order of 1000. Beyond lies the rather large domain from $z \sim 1000$ to $z \sim$ infinity. For that we use the full "theological", naive cosmology of the day: that's to say the singularity, the big bang, the white hole, the machinery as stated in the textbooks since Friedman's work of 55 years ago. There is a lot in it; I am the last person to deny that. It seems to me the most powerful generalization in cosmology we have ever made. But still I think infinity is a long way beyond even 1000. I'm not prepared to accept it, without more evidence than the fraction of deuterium that may or may not be there, in this or that object. After all, n_D/n_H is one number, subject to alot of assumptions before it's reluctantly dragged out of the interacting list of nuclei. We have a plausible point of view about the beginning of the universe and the big bang. It might be right for all we know, but it goes beyond anything we have a right to call a tested part of the physical domain. That's just an opinion; of course, if there really is a black hole in Cygnus X-1, and somebody can show that, or if there are lots of little black holes floating around evaporating away their radiation, that will be very interesting. I want to see it. I don't think the meteor that struck Siberia was a black hole and I don't think that the black hole will solve the energy crisis or a number of other things that have been claimed for them. Maybe their existence is real, maybe Nature has something to do with the singularities of general relativity. It may all turn out to be true. I think that's a serious problem, not part of this conference. I don't think that the demonstration that there was a hot gas before the universe fragmented, which

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has a very well-defined temperature and is quasi-isotropic, is a sure demonstration that we live in a hot big bang according to the book. We might, or we might not. One of my reasons is that the story itself is not quite consistent. It is not possible yet to give a clear account of why the pieces of the black body radiation know how to match so well in T. While that anomaly remains I am prudent. It can be blamed on various initial conditions or various mixings, without great success, but it is quite possible that it lies in the inordinately large extrapolation from z=1000 to z=infinity which is required. I would say a new length may well enter, not 10^{-33} cm, but somewhere between 10^{-16} where we know something, and 10^{-33} where dimensional arguments convince us that the metric is destroyed utterly by quantum energy fluctuations. But that is a long distance, a much greater extrapolation than most of us accept without substantiation.

Let us come to red shift physics. The latter part of the conference is mostly related to that problem. I have already implied that for me there ought to be some kind of correlation between galaxy type, galaxy age, galaxy position, galaxy peculiar motion. I see nothing against that; some day the dynamics will tell us. What, I don't know; no one is prepared to say. Whether we can find any strange regularities in these difficult measures, for example, the velocity type-correlation seen in Coma, or the periodicity seen in the histogram of large red shifts is in my mind very much to be tested by further experiment. It seems to me that all sets of complex measurements are affected by the problem that initial runs take a long time to correct by adding more statistics. All these matters certainly lie in the domain we don't know, and we should frankly admit that we need more information. It's not so hard to give reasons for clustering near this or that redshift; indeed that goes back to the classical fathers of our theory. Balancing off the cosmological constant against initial expansion you can manage to place plateaus anywhere you like. I don't think it would be too hard to have several of them if you wanted to; that would be another story. We ought accept those complications only very reluctantly, when the data have made an unmistakable case for it; that means not at the level of 2σ in some power spectrum, but really unmistakable. Again, the redshift arises if you like from gravitational potential, or if you like from motion--really you can't distinguish these things in the large; that's why it's called cosmological. I would like to think of it, especially nearby, at low z, as something like throwing a stone in the air. It is really no more subtle than that because the stone, too, is following a geodesic curve in space.

Our cosmology, crudely speaking, is relatively well tested in some respects: surface brightness, continuity of object power, and more. These things have to be looked at with more care, but a prima facie case has been made for the validity of this naive idea. Against this view four novelties were described to us in cosmology. They've been discussed a little more at length today; and I will only mention them, in order to give a fair summary. I'll try to characterize them very briefly, but frankly I don't understand them well enough to tell you much you wouldn't learn better from the record.

First, there is a new group-theoretical view of relativity, persuasive in its elegance, which gives a conflicting kinematic result with the simple Newtonian view I am pressing. But it agrees statistically quite well with the data, if you take an extremely detached view of the samples. Do not try to correct the samples very much, but imagine that they're made without selection built in by instrumentation. That's the essential claim, as I understand it. From my point of view I cannot say very much more about this, because so far this theory, which may in the end produce all that is required, has no dynamics. It does not have generation of energy; it can't explain how much energy comes

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out of a given mass. Therefore I'm not able to grasp the probability of evolution of a galaxy in it, over the infinite time which the galaxy may have been in existence. Under those circumstances a theorist who deals with the dynamics of things is left up in the air. We just have to wait for more work to be done in the same direction to see if the beauty remains, beyond the kinematic to the dynamical domain.

Second, we have a group of theories--I'm willing to call them a group by now--which were described very briefly by Professor Narlikar. They represent a sort of remapping of the world which I have described in Newtonian language, with a significant change predicted in some of the constants of nature. We are not in any position to understand what this means at the present time.

The most novel scheme introduced in this meeting was given in the very interesting paper of Professor Pecker. From my point of view it was fully within my idea of laboratory physics--nothing deeply new about it. New particles? We have plenty of new particles, why not have another one? If it works, I'd buy it. But the way to find out will have to be in the laboratory, if it is at all possible. Cosmological results, where small effects are shown, are good for forming hypotheses but in the long run you're going to ask coldly: well, can't you demonstrate that in laser plasma somewhere? We'll just wait until it's done. Certainly it was a very interesting account, an admirable job of criticism and survey over the whole domain of cosmology. I recommend the paper to all.

The fourth proposal is an unknown but pervasive effect, a new cause for redshift, which somehow has to do with the source object, depending on whether a source is first-generation or second generation. (It would seem to me excluded in my country by various legislation!) I found the arguments not very persuasive, but I want to spend a little more time on them as I close.

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I have now five points to make. First point: the famous velocities greater than light. I believe these are illusions; there are lots of possible illusions. The data are not very well seated dynamically, or even in terms of intensity maxima. The radiations are studied monochromatically, and the amount of energy in these tiny, tiny wonderfully resolved volumes is relatively small, even though the luminosity is high. The economy of the object does not much depend upon these interferometrically-revealed bumps. I'm quite anxious to see them done in total intensity, not just for the single Stokes parameter, which in general is the interferometric result. Does the total intensity move the same way? Does it slide around? Are there polarization differences? We'll have to wait sometime to find it out. We will eventually need also the geometrical data which were put on the screen, I think, by Martin Rees. There is a lot of work for this extraordinarily powerful technique in the future. But this is not the first time that we've been confronted by an illusion. The first time I know of was understood by Kapteyn in 1901, the expansion of the shell around Nova Persei 1901. It turned out indeed that was a light shell. If you used the velocity of light you could get a distance. Then it was pointed out quite promptly that if the mirror in which you were seeing the light shell reflected was before or behind the object, not actually at its distance, you got a false result. This illusion was first fully explained I think by Couderc in the Paris Observatory before WWII. It is too often forgotten; you have to rediscover it each time. The fact of the matter is that we don't see what is actually going on in rapidly moving objects. The Andromeda galaxy itself is an illusion; no photographic plates shows the Andromeda galaxy as it was at any instant of time. We know that. The man on the street knows that you see it as of 2 million years ago. That is the transit time; but also in detail you do not see it. For you see the front edge at a time 50,000 years ahead of the far edge. Of course, the galaxy doesn't change much in 50,000 years but I assure you a quasar does. What you are seeing is not the real thing until after you reconstruct the image painstakingly by proper relativistic means. It is wrong to say that an illusion is unexpected; in this situation illusions are to be expected. The only question is what kind of an illusion? If we'd only been alert enough to publish that before the radio people found their apparent motions we'd be better theoretical physicists than we are (and maybe Martin Rees was!).

A question which I find important is whether there really are or are not starry galaxies around quasars or semi-quasars such as BL Lacertae objects. Here there's one case of a well classified object about which there is no disagreement concerning its galactic cortege of absorption-line stars. Correct me if I'm wrong: it is AP Libra. One case is not everything; but it is sure one case! I would very much like to see more work on 3C120, and I can ask a question: what are the controls, how hard is it to see the absorption lines in what kinds of objects? We ought to know something more about that. When Christian some years ago presented his argument that the quasars had galaxy fuzzes around them, Geoffrey Burbidge made a tightly logical response that I cannot disagree with: namely, that the definition of a galaxy is by no means a visible fuzz of a certain diameter. An optical glow can come from many causes. Can we look for stars? To what level and in what kind of bright-nucleus objects? Has that search been made and nothing found, or is it simply not possible to look? Some discussion in relation to that would be very valuable.

Third, the energy problem. It was not discussed here explicitly, but I think that to some extent the reviewer was unfair when he said that nothing had changed since the time when the energy of quasars was first seen a) to raise the danger of a Compton catastrophe of very bright, compact objects and b) to limit the mass which must be contained in an object that over all its life gave off 10^{62} ergs in

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relativistic particles and in fast bulk motion. What has changed is very straightforward. We have seen a couple of hundred pulsars and a number of other related objects. As far as I know it is still true that we cannot explain the pulsars by incoherent synchrotron emission without the catastrophe, but yet we have not moved the pulsars forward; it is not likely we can understand the quasars, still more complex objects, any more simply than we do pulsars; they may involve quite related relativistic electrodynamics. So I find that the argument has not been strengthened. The result with Markarian pairs, with Seyferts, and a number of other examples, seem to me to show that while there are anomalies here and there in every class, one or two outstanding cases which are left out by people who describe the bulk of the class, the classes as wholes do not seem anomalous. They ally reasonably well with galaxies in all the properties that we ascribe to them.

The tough problem is the existence of--once I would have said M82 but no one now need believe that M82 has an active galactic nucleus--NGC5128, Centaurus A. I don't see any way of changing the distance of Centaurus A without making a lot of trouble. (Take a factor of 2, if you like, or more!) Centaurus A has megaparsec radio lobes; it is definitely not a compact local object of any kind. It may have an active interior, but that seems to me to demonstrate that there is something else going on. If I have to believe that some quasars are local, I will have to come back to the position that there might be two kinds of quasars, but certainly the kind of quasar that has giant radio lobes of large dimensions is sitting there pretty nearby in 5128; it's pretty hard to change it by jiggling with distances. You would have to show it is not at all like a quasar, with two radio lobes unlike the others. That is not to say that I understand 5128, or that I am sure it will all be solved in an ordinary way. I hope so, but I have no idea. But it is certainly there to be looked at.

Finally, I come to the phenomena, and the phenomenon, of HC Arp. Personally, I am delighted to belong to the same profession, more or less, with him, to see his wonderful pictures, to debate them, to think about them. I walk with fear and trembling into his presence, because I know he's going to pull some new crazy object out of his hat which I can't explain at all. He's actually published a picture (Ap.J.Lett. 207, L147 1976) of a galaxy which looked to me like a cloud chamber photograph! There's nothing else to be said about it; if there isn't some kind of particle ballistics behind that picture, I don't know what it is. His case is very hard to beat unless indeed there are glowing sky writers over Palomar who make right-angle turns at night! It's going to take a bizarre hypothesis to accomodate those things. That much I freely say, personally I believe that it can be done with sling-shots and fission, governed by Newton's laws, in some galaxies. That means they cannot throw out objects of 10^8 sun masses; they are not going to throw out big radio lobes. They're not going to do everything he wants; I won't go that far. But I will say that he has a lot to show us; it is valuable for people to take deep pictures and to look at them closely. But distant alignments and associations, quintets, sextets and what I think I can now call the Bologna Band do not yet convince me utterly. They can be shaken by studies with 21 cm or with higher resolution, showing that you can get confused, because individual objects can always have a chance property. The field of 4151 looked to me roughly speaking like Christmas morning. I couldn't agree with the conclusions at all. I can't say a word about that now; I'm going to wait and see. In the present state I just have to pass. 3C303 does show some coincidences, but they're not smack on, there is a very faint optical object, the whole radio source is small. The matter looks to me as though it might be explained some day by confusion. The B0924+30 set (which I call the Bologna Band) is also too new for me to comment on. But those alignments and associations depend fundamentally on assumptions about background numbers, on how hard you search, on whether the number depends strongly upon

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the magnitude you look at, and so on. Those objects are not the same for me as the more information-rich objects which show straight lines, right angles, things of this sort. The more information in the picture, the harder it is to get away with a theory that explain it statistically. There was a wonderful science-fiction story in which after careful deep plates, exposing and developing better and better, a picture of a person appeared in a distant galaxy cluster, in the image of the galaxy, spectroscopically present with the right redshift. They identified it to be a woman who had died a few years before in Philadelphia. That was the unresolved end of the story! I remind you it was literally put forward as fiction.

To all these I can only say the following. It is more or less appropriate to close with these remarks because they are Parisian remarks, and therefore they might be apropos. In the calendar introduced by Robespierre, who wanted to break with everything of the past, in the month of Florial in the year 9 (which translates to the first few years of the 19th century--I've forgotten the exact date) a very well known Paris physicist J.-B. Biot, well known for the Biot-Savart law, left Paris to go to Normandy, to a place called L'Aigle, there to inspect a fantastic, remarkable event, of which the news had arrived in Paris only rather recently. The news was that stones had fallen from the sky on this little town; lots of stones. In those days of course--this now cuts at both sides of our controversy--the Establishment denied firmly that anything ever fell from the sky, except hail and rain and so on. Certainly stones did not fall from the sky. Biot heard this story; he was deputed to make the study on behalf of the Academie. He wrote a wonderful monograph on it, a spectacularly good piece. (When I took it out of the library a couple of years ago it hadn't been taken out in 70 years.) It is true that after his visit everybody in the Establishment believed in meteorites; before that, nobody did. Why? Because he had a most fortunately good case at hand. The Academy had made a geological survey of the region a couple of years before. They had made an industrial survey too; they knew all the slag piles, all the brick kilns, all the deposits of rock everywhere around there. Biot went to the museums here, and studied all the objects--there were plenty of them--in the cases marked thunderbolts, more or less with a question mark, stones that fell from the sky. Now the proper Enlightenment scientist who are our forebears did not believe at all in those thunderbolts. And I'll tell you why they were quite right not to believe in them, as a class, because they were largely polished axes of stone. Lightning strikes a tree or a farmhouse; the people go rummage around the ground for something new, and they sometimes find a polished stone axe, which is not all that uncommon in France. After a lot of searching, why here is the axe that Jupiter threw when he threw the thunderbolt! Some real meteorites had been seen once or twice; then they brought back a meteorite instead, not a hand axe. But the collection consisted mostly of hand axes, pieces of slag, and all kinds of odd lots that the people had found when they searched, after something bright had struck from the sky. The mineralogists looked at it and said in no way is this sky material; it is earth material; many are even man-made. The astronomer-physicist Chladni (who also made the Chladni plate) had published a superb thick, volume five years before, showing that in this confused sample there was nevertheless something good; there were irons that had fallen from the sky--they had to be. His case was correct. Nobody much believed it. We had to wait for Biot to travel. He got 3000 samples of stones, he found some still on the rooftops, he saw witnesses, from preachers to peasants, all agreeing to what happened, the neighboring towns confirmed the fall by triangulation. He came back and saw the stones change on his laboratory bench as he watched week after week, as they oxidized. That proved they hadn't been there very long. So he said there can't be any other answer; these stones must have fallen from the sky and Chladni was right. That's my example. I don't think we're going to believe in these things until we see it isn't just statistics.

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Everybody does statistics to his own purpose; the only way to convince a skeptical group of people who have a large well-made framework to defend is to show them the case so complete that it breaks the barrier. That's what J.B. Biot did in the month of Florial long ago; I would not be astonished--I would be delighted--if HC Arp does that, too, in some clear month. Maybe 1097 does it, but not for the redshifts, I emphasize, or for the associations. What it may do is for smallish ejected objects.

I come to my conclusion. It is the death of a science to look only inward, and to complain about its assumptions. It is the life of a science to seek new information, to seek new generalizations, to push hard its instrumental technique, to develop unexpected things, not to engage in endless internal arguments. This has happened in our day in America to geology for example. Geologists recognize it; the geological records became full of arguments concerning the re-definition of stratum, which had been carefully defined in the past. Was the old definition really right? Did this class or group include this one, or not? Half the articles became internal; no new data were put in; reworking data and old conclusions was commonplace. Then of course, marine geology came through, magnetic sea-floor geology, and produced the current revolution in geological science which has inundated the whole subject. Geologists are no longer specialists in this little province or that; they begin to understand the earth as a whole. We too have to avoid looking inward. We can succeed as long as the dishes, the image tubes, and the deep plates are at work. We are on the eve of a post-Hubble period, in which we'll study new problems, by new methods, without ignoring the old ones; clearly, we have a much richer universe to examine than we had 50 years ago.

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