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Questions and Comments that followed the Panel Discussion

J. V. Narlikar (edited)

One point is that, like Geoff, I am also passing through moments of depression. It is depressing when you find that new ideas are not taken up – even as a challenge to disprove them – by the younger generation. What Paddy mentioned, for example – Paddy said this is a vicious circle in which we find ourselves. If there is an alternative (I will state in mathematical form) which is subscribed to by "n" people where "n" is a finite number less than or equal to four (you can work out who these four are) then before an n + 1th person gets into the field his condition is that the theory has to be worked to a sufficient detail which will compare with the existing standard model. Now obviously if "n" is a small number (these three or four whatever number of people) they are not expected to work the theory in greater detail. In the case of Big Bang if you start with the history of Big Bang cosmology you had Friedmann, Lamaitre, Einstein these people who had started working. But eventually more people started working and now you see how many people are working in Big Bang. So you obviously should not expect the Alternative Cosmologists to produce the same level of sophistication which the standard model has produced. So if the n + 1th person says that he did not find anything comparable to be able to get interested in it, this process is not going to advance further. This is my theorem which I have proved I think with the example or attitude which Paddy himself has expressed.

My second point is that certainly COBE found some fluctuations but I remember attending an IAU symposium in Los Angeles in 1979 where Chip Arp and Burbidge were present and one of the observers of microwave background experiment had asked Jim Peebles that "we have gone to the level of $\delta T /T$ below 10^{-3} and we did not find fluctuations you are predicting: so can you tell us to what level we have to go before you can give up your theory?". There was no answer from Jim Peebles.

Now there is also the fact that as you improve your experiment technically you are bound to detect something which appeared to you smooth earlier. To give you an example take a remote sensing satellite which has a certain resolution. It looks at the desert and it finds it very smooth. Then the satellite improves its resolution and it begins to detect ridges. If the satellite improves its resolution further it may be able to detect even tiny particles of sand. So now coming to Cobra-Sambas – I am certain it will detect more than COBE. I am willing to make a prediction that its resolution and ability to detect is higher so it will certainly go further or improve on COBE. There is no question about that. But what I want from Big Bang theorists today is a specific statement that if Cobra-Sambas does not find what you are predicting today (with wavelength dependence or angular dependence) based on a precise statement in a theoretical paper today, you will give up the theory. People will today say "Yes I have written this paper, I have predicted this." But the same people, when Cobra-Sambas comes out with the results which are in disagreement with their predictions, will simply modify the parameters. Because, going back to the desert analogy, in the desert there are infinite number of sand particles – you can have infinite number of parameters and try to simulate it to the level of your remote sensing satellite's resolution. So you can always fit a theory to data by adding a large number of parameters. This is my 2nd point. Initially in the history of astronomy, which is also the history of science, people make correct predictions (and the continental drift was an example) – but not at the correct time. The scientific community is not prepared for these predictions. Ultimately, by the time the prediction comes true the originator of that prediction is often no more. The same thing happened with microwave background. When the Nobel Prize was given, Gamow was not alive. Or when the Nobel Prize was given for stellar models, Eddington was not alive. I have no comment but this is the way it goes.

N Mukunda: I think the student would like to answer his teacher –

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T. Padmanabhan (edited)

This is a debate which goes on – as you must have guessed – between me and Jayant. So I am just reiterating some things which we have been discussing in private on several occasions.

First of all, I cannot be held responsible for what Jim Peebles says or David Schramm says but can only reiterate what I have said in the past (and also in response to one of the questions): There was an occasion when Jayant asked exactly this question to me in 1990 When will I give up Big Bang? And I gave him the answer that if COBE does not see ($\delta T/T$) at the level of 10^{-6} I will give up Big Bang – I am sure he remembers it. We even have a bet on this issue. I was vindicated last time. There could be others who will change the parameters, fiddle around and will keep changing it but eventually the self-correcting methodology of science which Mukunda mentioned will come into operation and they will be ignored. So there is no need to panic about that. As far as Cobra-Sambas is concerned the reason I am excited about it is because when COBE went up we only had very nebulous kinds of predictions which we accepted at some level. The predictions are being made beforehand to extremely accurate levels. Of course there are observational uncertainties, background substraction – all these things need to be handled but I think the theoreticians are doing their jobs very well. On this topic, I really don't know what more to add because I thought Tarun made a very good point about that. If the entire parameter space is ruled out there are definitely quite a few people, including Ravi and me, who will rethink about this entire scenario and will probably give up Big Bang dogma. If these predictions are not true there is no need to worry about that side.

As regards the sociology because of which the $n + 1$th will not join the unconventional modelling, I have a feeling that it is only a conjecture rather than a theorem; if you look back into history of science this problem is something which has been faced by everyone who has been coming up with an unconventional theory; we should also remember that in the 50 s and 60 s there were two camps: the Big Bang one and the Steady State one; there were debates and eventually, for some reason – one camp became the band wagon. The bandwagon might have been started for completely wrong reasons, say, fake observational evidences which were totally spurious and might have fallen wayside. I certainly think that if a good theory is put forward by someone at some level, I am sure people will take it seriously. This has happened in alternate theories of gravity, this has happened any number of times in particle physics. Guth's inflation which we all have been criticising on and off is another classic example. The one paper by Guth was so well accepted by cosmologists and it went up in popularity very quickly. So I do not think that there is such a dogma that unconventional theories will not be accepted if presented properly.

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Geoff Burbidge (edited)

I am not going to comment on the discourse which goes on in Pune but I do want to add one point which is very important. You are talking about bandwagons developing and opposition developing and so on without taking into account what is really happening in modern science. Modern science is driven by money. Money is driven by agencies and it is very hard to get support for things that are not bandwagon propositions. Not only that but there is a large amount of discrimination both in getting funding for theorists who want to do other kind of things, and for observers – as Chip very well knows; to get observing time. This is biasing things far more than they have ever been biased before. I will give you a good example associated with COBE. COBE was not sent up to look at the background radiation. It was sent up to prove that the Big Bang is right and of course, claims were made from the very beginning. For. future satellites even more specific statements are being made by what they are going to discover. They are going to discover how galaxies were formed, they are going to discover when they were made and when quasars were made. Now there is a large amount of money going into this and a large number of people are being supported in this venture. If, on the other hand I want to get a postdoc to work on some alternative cosmology I shall not be funded. There is a Centre for particle astrophysics in Berkeley which is run by a Frenchman whose name I always forget (deliberately, I think sometimes) and he is a particle physicist. In that Institute supported by the NSF no-one there is allowed basically to work on anything other than Big Bang cosmology. You simply cannot get funding to do anything else. So there is this tremendous pressure and in the observational field it is also very hard. The kind of things that Chip was talking about this morning have been done all for 20/25 years intermittently and in the face of tremendous obstacles. And you are all very well aware that Chip's access to telescopes was removed because of what he was doing. Nowadays you cannot get time with the space telescope or the 10-meter telescope(s) to work on these projects. If you submit a proposal along those lines it would be rejected. It would be rejected by the Telescope time allocation Committee which is made up of your peers – young people who are quite convinced that they know the answers but who know nothing about the evidence for non-cosmological redshifts. You see there is a driver in all of this which goes as follows. These instruments cost a vast amount of money. To get the Space Telescope – you have to go to the American Congress and plead the case. And you have to essentially overpromise that you will find answers to all the questions. We *will* find the value of the Hubble constant, we *will* find how the universe began, we *will* find the blackhole in M-87, literally you have to do this kind of thing. So you promise and what you don't ever tell them is that research is a very inefficient business. Half the time [I mean (with tongue in cheek) in all research except cosmology] you are wrong but here you are dealing with people who don't understand this i.e. the Hubble Telescope is a great success because it has found a Blackhole in M-87. In fact I am waiting for the day when NASA announces that they have found a nebula fairly close to us with a very strange structure, and that they have decided to call it "The Crab". These factors are really entering in. They are giving a great deal of bias to the situation in which bias is already present, and this makes it ten times worse.

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H. C. Arp (edited)

I am very amused by the conversations from two Big Bang proponents here about how their theory is on a more developed level and therefore that their theory is more convincing. I thought we could all agree in Science that you could never prove a theory; you can only disprove a theory. So why then are we talking in the current theory about differences between 10^{-5} and 10^{-6} when we observe incontrovertible evidence for quasars of redshift $z = 2$ physically interacting with galaxies of $z = .003$?

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R. Cowsik

The issue here is that one has seen a quasar whose redshift has been measured by looking at its spectral lines. It is seen close to another object whose redshift is more close in angular space but necessary in physical space. Statistically one may say that the likelihood of its accidentally falling is very small but as we all know given a very specific situation the probability that it will occur in that way, I mean estimated in any particular fashion would be very small. That is the issue Professor Arp is alluding to.

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Arati Chokshi (edited)

Let me do a follow-up on what Ramnath said and then a couple of comments. In the field of normal galaxies, papers by Koo and Ellis *et al.* found beautiful periodicities a few years back. The redshift distribution of these galaxies were derived from a faint pencil beam survey on a small piece of sky and they found remarkable periodicities on lengthscales or redshift scales that left a lot of us, who do conventional astronomy, mystified. This led to a lot of papers or lot of cranking of computers to see how likely such an accidental phenomenon was and the same group of observers then decided the way to do this was to do a pencil beam in the opposite direction, to do pencil-beam surveys around those, increase the field of few and eventually, as they got more and more data, one found that the data became more granular and what was left was the granular structures with voids that one sees in normal galaxy distribution which has a particular scale length describable by gravitational structure formation to a certain level. But the fact that periodicities occur in other branches of astronomy or galaxies or in high redshift universe has been taken seriously and people do spend time worrying about interesting observations.

So that aside, another comment related to what is funded and what is not funded in astronomy for whatever reasons. The non-massaged data is there for anyone to grab-HST data or what Arp showed came from data collected from satellites that might have been sent up there with a scientific justification which does not ascribe to your scientific justification but the availability of data is remarkable and may be taken in that spirit.

Finally I can think of one set of interesting observations that might be relevant in this context, which we have not touched upon. These are the observations of gravitational lensing of clusters, of galaxies where you find a consensus building up, at least amongst the observers, that if you look at dynamical masses of rich clusters of galaxies you get a value of omega, at least if you believe in Newton's law (I mean at a certain level you have to believe something and you believe that the simple law which you teach first year undergraduates works). Then, if cosmological redshifts are true, there would be a certain effect of lensing or ring like effect that is seen in background galaxies, and finally, if you go to a 3rd way of trying to confirm this – what do the X-ray photons, which are coming from totally different physics tell us? Just count all the photons coming from these free free emission and one again gets a consistent result – sort of all leading to adverse observations coming to similar mark – constraints which I think fit into place within the conventional Big Bang.

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J. C. Pecker (edited)

As to the discussion related to the "pencil beam surveys", one should note that (it was not mentioned by Dr. Chokshi!) that the main peaks in the redshift distribution appear whatever the area of the sky under study, at the same values of z; and they are statistically significant in the quasar studies, for $0 < z < 3$. This has been shown by many authors – independently. I believe that one of the last papers on that distribution, by Depaquit *et al.* displayed a very complete statistical study of the value of the sampling. And this has nothing to do either with the cellular structure observed in the galaxy distribution for $z < 1$, about which Dr. Chokshi was commenting, or with the small scale periodicity (37.5 km/s) discovered by Tifft, and extensively studied by Napier and Guthrie.

I would like to make two points. The first, in reply to Dr. Cowsik. The problem is that the number of "abnormal redshifts" observed by Arp or others is not small but indeed very large. In the very beginning, when Chip started to produce his first examples, I was doubtful, as is still Dr. Cowsik. But not only are there many cases, but in each case, the geometry of the case is quite unlikely (alignments connected with jets, . . .). There is even one case when you see a quasar of high z appear in front of a large galaxy of low z. There are even "abnormal redshifts" in the solar system, where the question of distance is not to be taken into consideration: for example, there is a redshift affecting, during the time of their passage near the Sun, the radiation of all sources occulted by the Sun (Tau A at 21 cm, an OH source at 18 cm and Pioneer VI probe, at 6 cm); these redshifts are consistent each with the other, at these different radio wave lengths. We are not allowed to rule out all those cases as spurious; I regret the fact that people just criticize, – but they do not really work on all these cases. Sometimes, they even refuse to do so. And they are, I think, to be blamed for that undue ostracism.

My last remark, I think, is still more important.

I keep my strong doubts about the possibility now to agree on any particular type of cosmology. Why? Let us face the fact that, in the beginning of this century, we knew nothing about the evolution of stars. The only theory we had was that of Sir Norman Lockyer; it was nice, and completely wrong of course; but it was a good beginning. It took only (!) a century to reach a satisfactory theory of stellar evolution, fitting well the observed data, except perhaps at some very special moments of the evolution. One century! . . . As to the galaxies, we know almost nothing on their evolution. They contain billions of stars, a lot of dust, of gases, nebulae, clusters of stars. . . They take extremely diverse shapes. We do not know which galaxy is young, which one is old. . . We do not even know whether a quasar is a young galaxy, or perhaps an old one. We do not even know if all galaxies are going through the stage of being a quasar as only whether they always have had an AGN, or if all quasars are passing at a time or another in the stage of being a "normal" galaxy. We do not even know what can be considered a "normal" galaxy. . . . There is a great deal to learn before we reach the stage in understanding the evolution of galaxies, i.e. relatively close-by objects! . . . Half a century perhaps? This is optimistic! . . . . And we would pretend to understand everything about cosmology, which concerns the whole Universe? We are not even ready to start to do that. All that we can do is to enter in the field of speculations. So far as I am concerned, I would not comment myself on any cosmological theory, on the so-called "standard theory" less on many others. Actually, I would like to leave the door wide open. But as a consequence of this attitude which reflects only the facts of life, I would also leave wide open the pages of the journals, and the doors of the big astronomical agencies. It is a very absurd and sterile attitude on their part to close their eyes and their doors to unconventional ideas. Thank you very much.

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Tarun Souradeep (edited)

I want to comment on the remarks regarding the (decreasing) levels at which $\Delta T/T$ was predicted at various times. One should not adopt a purely historical perspective. It simply reflects the scientific process of model building without modifying the basic framework.

At some stage cosmologists did expect $\Delta T/T$ to be observed at the level of $\approx 10^{-4}$. But in those models the matter content of the universe was entirely baryonic and density perturbations could grow only after recombination. The fact that $\Delta T/T \approx 10^{-4}$ was not observed led to models which included cosmological non-baryonic dark matter as an additional free parameter. Before the COBE detection, the theoretical predictions for $\Delta T/T$ was above $\approx 10^{-6}$ for these revised models. A null result from COBE could have brought about a major revision of structure formation models.

This is an ongoing process. The results of future experiments such as COBRA/ SAMBAS (now renamed the PLANCK Surveyor) and MAP are at least five to ten years away. But there are a lot of other experiments which are expected to provide more data on CMB anisotropy at higher angular resolution. At present we have a model with around 9 or 10 parameters to play with. The PLANCK Surveyor will provide thousands of data points for the power spectrum of CMB fluctuations. If the universe is radically different from what we have built into our models, I think it will be difficult to recreate the observed power spectrum by playing around with parameters. In that case, the whole cosmology community will start exploring alternative scenarios. The other possibility is that we will have an ugly theory which fits the observed power spectrum of CMB fluctuations by fixing many of the parameters. This case would be akin to the present situation in particle physics where you have standard theory which is working well but it is ugly in the sense that you have too many free parameters. I believe that in this case too, many theorists will take a more serious look at alternative ideas and look for deviation from the standard theory. For example, the controversy regarding quasar redshift may come under more widespread scrutiny. But unless there is a standard theory which has been developed to some maturity and has been tested out against the available observations one should not expect the whole community to look for alternatives.

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Pasupathy (edited)

I just want to make a general comment here. As a particle physicist I do not know much about cosmology. But let me just remind you what the chairman said. Standard model in particle physics is a model which is primarily driven by experimental facts. This explains why the non-abelian gauge theory which was written down by Oscar Klein in the late 1930s received scant attention. So progress in particle physics is based on carefully planned experimental programme. Unlike some cosmologists theorists in other areas set themselves rather limited goals. There are some paradigms which you can learn from nuclear physics and condensed matter physics which are I think relevant to cosmologists and I will mention some. For example in nuclear physics we use collective model in certain regimes to understand certain properties of nuclei like large electromagnetic moments and certain aspects of the spectra while the shell model is used to explain the magic numbers. Although Quantum Chromodynamics is widely accepted as the basic theory of all strong interactions we are a very long way from computing the properties of nuclei from QCD Lagrangian. So people are willing to make models which have limited domain of applicability. So you work with limited experimental data which might have coherent pattern which you want to explain in terms of certain framework and this attitude of nuclear physics is what is mentioned here. The other thing that I want to mention is the much misused term called complexity. Take condensed matter physics for example. Consider the metallic property of aluminium. It has a very simple explanation in terms of nearly free electrons and Bloch waves. On the other hand the metallic properties of the High T_c materials require completely different paradigms to understand so much so that today there is no commonly accepted approach to the problems. In other words although all materials are made of atoms and obey the laws of electrodynamics, the explanation of similar physical quantities in two different substances may call for different theoretical models.

It therefore appears to me that it is worthwhile to explore the possibility that in the matter of structure formation in the universe too, one may need different models to explain various structures.

The other point I would like to make is the willingness of particle physicists not only to change their models to agree with evolving experimental information but also to even replace basic ideas like a quantum field by something like a sting theory if these should eventually prove superior. There is no passionate commitment to any particular idea here. It seems to me in cosmology it is more like religious programme. Something has to be either entirely right or has to be entirely wrong. I think in the next 10 to 20 years more experiments should be mounted on observational cosmology. It would be nice to have here a specific set of experimental goals that you set yourself and test models and theories in limited domains just as we do in nuclear physics or condensed matter physics without worrying about the ultimate theory.

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Rohini Godbole (edited)

I am a particle physicist. I am not even an astroparticle particle physicist. I do not understand either cosmology or astroparticle physics. But I still want to defend a little bit the reputation of particle physicists because I think particle physicists are somewhat maligned in this context.

We have to remember one thing that the particle physicists did not invent a nonbaryonic particle to explain the observed nonbaryonic dark matter in the Universe. If the cosmologists agree at the end of the day that we have non-baryonic dark matter in the universe then particle physicists have some readymade candidates (such as neutrinos, neutralinos) with enough ignorance (both theoretical and experimental) about their properties such that they may or may not provide Dark Matter. I can cite two examples in terms of Majorana Neutrino and Dirac Neutrino which were both postulated in particle physics because we wanted to understand the neutrino mass problem even before we began to worry about Dark Matter. If anything I would say that the results of the direct search for Dark Matter candidates have already ruled out Majorana Neutrinos as a dark matter candidate for certain mass ranges *when combined with results of some accelerator experiments.* This has helped particle physicists refine some ways of going beyond the Standard Model. So in that sense I just wanted to emphasize the statement which Paddy made that the laboratory detection of a weakly interacting non-baryonic particle is perhaps helpful not only for Big Bang cosmology, to disprove or prove it but it is equally important for us to refine our particle physics theories as to how you can go beyond standard model. Because even today in different ways of going beyond standard model one has more than one candidate for the Dark Matter. There is nothing that is going to replace either the accelerator or laboratory experiments. I do not belong to that breed of particle physicists which say that I understand what happened in the billion billionth second but I do think that below 100 GeV scale we do understand what is happening and for me the connection between the two is highly exciting. I do think that particle physics experiments will have something to tell the cosmologists whether or not we have these objects and then you have to try and see where they fit in your theory of cosmology if they exist.

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G. Burbidge (edited)

Non-baryonic dark matter is only required for our version of the big bang model. Let us suppose for one moment that the QSS cosmology turns out to be correct. It does not need non-baryonic matter. In fact the dark matter is baryonic. What is the case to be made for you people pursuing the non baryonic case at all?

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Rohini Godbole (edited)

I was trying to make the point that tomorrow if you (cosmologists) conclusively prove for me that there is no non-baryonic dark matter then I would treat this information as a constraint on my particle physics theories. Because the particles which are Dark Matter candidates exist in our theories independent of whether the Universe has Dark

Matter and whether it is baryonic or non-baryonic. So the experimental information from Cosmology gives me constraints on the properties of these particles.

Because I am saying that right now particle physics does not really know how to go beyond standard model and as a matter of fact I am thinking of this issue of the dark matter as an example of the symbiotic relation between cosmology and particle physics. Particle physics takes pointers from cosmology if you wish and cosmology would take pointers from particle physics.

Geoff: So the case for doing experiments would disappear.

Rohini: I am making a case for doing experiments both in low energy detection of these objects and also, I am making a case for accelerator experiments which try to tell me something about particle physics. For example if particle physicists tell you conclusively tomorrow from the accelerator experiments that a heavy Dirac Neutrino or a Neutralino exists, then the onus is on the cosmologists to try and fit it into their observations and theory, the same way particle physicists have already used the negation of a heavy Majorana Neutrino as a Dark Matter candidate to learn about neutrino masses, that is all I am trying to say.

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H. C. Arp (edited)

I just want to reply to your point. What it seems to me is being overlooked in this argument is that particle physicists are studying physics "here and now" and cosmologists are studying physics "there and then".

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Gopal Krishna (edited)

I see ahead of me 'old radicals' and behind me 'young conservatives'. But frankly I find the old generation to be far more conservative and the younger generation to be more open minded and less prone to dividing the issues into rigid paradigms. To make some real progress it is important that observers once again become (unbiased) observers. Recall that the beauty of the Penzias-Wilson discovery of the microwave background was that when it was made, the discoverers were not aware of its cosmological relevance. In the same way, several outstanding issues in cosmology can be forcefully addressed if the observers are not swayed by any pre-conceived notions about their observations. As an example, many ambiguities involving quasars can be circumvented today, since one now has got sample of galaxies going to $z \geq 3$ which incidentally, has led to a very tight *Hubble diagram* (for radio galaxies alone), covering the redshift range up to $z \sim 3$. Likewise, using the most modern optical telescopes like HST and KECK, it may now well be possible to obtain spectra of some of the 'bridges' that have been claimed to physically link some high redshift quasars with low redshift galaxies. Conceivably, an open minded approach can now resolve this long pending issue, given today's observational capabilities. Things are also more doable for the younger generation because of the availability of large data archives (which allows them access to large telescopes, totally bypassing the time allocation committees).

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R. Cowsik

This is just a brief comment.

What one does when one finds an observation or experimental result in contradiction to the existing theory. It is not always that you throw away the baby with the bath water. You may have to do your surgical operations on the theory you have been working on carefully and throw out the sections which are not correct and you have to modify your theory. I don't want to give examples which are well-known from the field of physics. It has happened many many times.

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J. V. Narlikar (edited)

I have two points to make. One is in response to what Gustav said. I can appreciate that there is a lot in astronomy and cosmology that is well understood or explained by conventional physics, otherwise we would not be attracted to the subject. And the fact that the adjective anomalous is used for certain redshifts indicates that these are exceptions to the conventional explanation. There are certain features about these cases which stand out. So certainly one is recognising that a lot of astronomy and astrophysics is understood in terms of conventional physics but history of astronomy tells us that new inputs to physics have come from astronomy. So we should not close the door on something that looks anomalous and one should try to understand it. The second point is in response to the particle physicists who spoke. I want to tell them that they should not think that very high energy particle physics is any longer tied down with standard hot Big Bang. The Quasi Steady State Cosmology also has problems dealing with very high energy particle physics. So if you people feel you are standing on the sidelines looking at cosmology and you are not committed to the Big Bang cosmology I would invite you to take some interest in this alternative idea. Unlike the standard Hot Big Bang which happened long time back, which nobody actually can observe and which occurred once only; in the Quasi Steady State Cosmology it is a frequently repeated phenomenon and it is observable. So astroparticle physics in this cosmology is a very real science.

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Pasupathy (edited)

Of course I am quite in agreement with Professor Narlikar. Let me just make a point here. Particle physicists do not take cosmological bounds seriously. That must be clearly understood. For example if Z decays had demanded the existence of four neutrinos and cosmology had allowed only three, the particle physicists would have laid less faith in cosmology than try to correct their theories about partial widths of Z decays to neutrinos and contrive to make the Z width consistent with 3 neutrinos. In the same vein it is not clear to me that cosmologists should take various superparticles and dark matter candidates seriously in their attempt to understand structure formation in the universe.

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T. Padmanabhan (edited)

This is again a brief reaction to some of the comments which are being made especially about this business of 99% to 1% vis-a-vis what Ramnath has been saying. I have friends working in low temperature laboratory physics and condensed matter physics. They have a paradigm, based on quantum theory, which decides the general properties of a solid, let us say. Every once in a while they will find that a set of alloys behave differently. They do not go around saying solids are not made out of atoms or quantum theory is wrong. They try to interpret the new phenomenon by just modifying certain parts of the theory and see whether it can be done. We do not even understand fluid turbulence at a precise level; so while trying to explain very complex astrophysical phenomenon at 1% or 2% or 5% level one has to be little cautious before we throw away everything.

The second comment is about the way I interpret what Jayant was saying – all that particle physicist needed was fairly high energies and whether it comes from Big Bang or mini Bang it does not matter; this comment is very gratifying to me. I see that, over the years, Steady State Cosmology has become Quasi Steady State Cosmology and it is slowly moving towards Big Bang Cosmology and I am sure at some stage it will merge with it. I look forward to that.

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D. F. Roscoe (edited)

I would invite those who are interested/sceptical in the claims for the quantized redshift phenomena to look at the paper published by Guthrie and Napier 1996 "Redshift Periodicity in the Local Supercluster", *Astron & Astrophys* **310,** 353–370. It is perhaps of interest to know that Napier and Guthrie originally approached the claims of Tifft (for redshift periodicity) with great scepticism, and were of the view that the analysis they proposed would almost certainly result in a strong rejection of the Tifft hypothesis.

Make your judgements after reading this. Thank you.

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N. Mukunda

I think it is time to bring this session to a close. It has succeeded far better than I had hoped for. I was not sure that we would be able to use the 2 hours given to us. I think it has been a very good panel discussion and let me invite Professor Narlikar to have the last word and bring this meeting to a close.

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J. V. Narlikar (edited)

The original suggestion for organising such a meeting had come from Amitabha Ghosh who thought of organising a meeting where the conventional and unconventional are put together. There are very rarely such meetings where a free and frank discussion can take place. I had also the benefit of consultation with Geoffrey Burbidge and Padmanabhan and others as to how to organise this meeting and finally it has taken shape. It has gone very much the way I had expected it would go. I certainly did not expect that at the end of the meeting we will have some kind of paradigm shift but would like all of you to think about these issues. May be, we will again have opportunity to meet when more data will have come through – as Chip Arp brings some more pairs or triplets which look very awkward or as Hubble diagram proceeds in a well behaved fashion to fainter magnitudes. Who knows, in some future meeting we may have a blueshifted object with the Big Bang Lobby explaining it away!

I thank all the 4 institutions which have co-sponsored this meeting and in particular I would like to thank Professor Mukunda on behalf of JNC, for looking after the organisation and his colleagues in JNC, especially Mr. Nagaraja Rao, who had been dealing with the infrastructural problems. And I also thank all of you again for participating in the meeting and making it so lively.

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