J. Astrophys. Astr. (1997) **18,** 465–475

Panel Discussion

N. Mukunda (Chairperson – Panel Discussion)

A panel discussion with the 4 panelists and myself! I was a bit puzzled when Jayant Narlikar asked me to chair this session. Like Rohini Godbole I also felt somewhat of an outsider but now I think I have understood his motive. My perfect ignorance qualifies me to act as a perfect neutral, to function as a detached observer; which by the way is not allowed by quantum mechanics but that is another story. So I hope I will succeed in being that kind of moderator. Permit me to begin by making a few remarks to get things going. About the structure of this meeting – the format – it was planned by Jayant and I think it has been outstandingly successful and I think all of you would agree with this. Starting from the strengths of the standard model, we went on to the constraints on the model and then to alternatives. The discussion speakers and the general discussion session – both turned out extremely stimulating and effective and I believe there is obvious value in bringing out a record of the proceedings of this meeting. If we are fortunate this could turn out to be a truly, catalytic meeting. So this is why it is important for us to have the questions and answers recorded, written out on paper so that the proceedings can be brought out as soon as possible.

Now for this two-hour panel discussion, which can address questions at several levels. We could make comparisons with similar situations in other areas of science. We can look at problems of the philosophical or sociological nature and we can also turn to more detailed questions, technical questions. The discussions over the past two and a half days have highlighted several profound, though familiar, issues and let me mention some of them. They have shown us how every experiment has some unavoidable theoretical underpinning and to that extent it is committed in advance. The discussions have also shown us how even the collection and organisation of data can be subtly influenced by theory and it has shown us how difficult it is to disentangle the two. This is true in every area. There have also been remarks of reliability and credibility of specific experimental results and even specific experimentalists. All these overlap with general epistemological problems of scientific progress, the inertia against changing one's view points and so on. The difficulties with a theory or an idea are not all foreseen initially, they emerge as understanding grows. Many participants in this meeting and some of them on the panel here come with a vast experience acquired over a life-time of science. I believe they can speak to us with a sense of history and also from deep conviction. They can also try to tell us how some old masters might have looked to the present situation and for these reasons I think it is important for the younger people here, myself included, to listen to them.

I would now like to present a few general remarks and ask for your permission to do so and make a few comparisons with other fields of science and also present some random thoughts about theories, experiments and their inter-relationships. I will recall some episodes involving theory, some involving experiment. My aim being to try to learn some lessons from history. Hopefully this will set the stage for later discussion and lend a sense of perspective and I also wish to stop things from getting deadly serious. There has to be a sense of humour all along in these discussions.

So first let me recall a few episodes from particle physics on the experimental side; these are the things that came to my mind as I was listening to many of you yesterday and day before. There have been examples where in an experiment you see what you want to see or you don't see what you don't want to see. A beautiful example of this, as you all know, concerns the positron. You know that Dirac predicted the positron in his famous paper in 1931 on the magnetic monopole and soon after his paper the positron was discovered by Anderson, who apparently was not aware of Dirac's prediction. But it seems, even before this, there had been evidence for positron in cloud chamber photographs of charged particle trajectories bent in a magnetic field. In these photographs if electrons are emitted from a radioactive source they would bend in one direction but in one photograph there were tracks bending in the opposite direction and the interpretation given at that time was that they are also electrons except that they are coming from outside the cloud chamber which may have hit the source at just the time when the photograph was taken. Today it may seem like a joke – it is only because it was so long ago and I must say I have heard this story told by Dirac himself at a colloquium.

The case of the weak neutral currents is another more recent and outstanding example. As you know these neutral weak currents were discovered in CERN in 1973 stimulated by the Electroweak theory; but the data in earlier experiments had this evidence. But nobody recognised them, nobody saw them and the main reason was that at that time the prevailing theory had no place for this. So this needed the theory to guide the experimenters to recognise the existence of the neutral currents and I believe not long ago there was a whole book, examining this episode from the psychological and sociological point of view, about how the scientific community functions.

On the aspect of crucial experiments influencing theory in a deep way is the case of parity violation which was predicted in 1956 and verified in 1957. This is an outstanding example and the important feature of this example is that it was a clear, clean and maximal effect. Though there has to be a lot of theory behind the interpretation it was as direct as one can hope the experiment would be. We also note that this was the first shocking realisation of the breakdown of symmetry in nature and we also know how violently Pauli reacted to this event. The somewhat later case of CP violation in 1965 again is an unambiguous experiment and in some sense uncontaminated as far as its interpretation goes – though one must confess that a satisfactory understanding of it is still not with us. It can be accounted for in a phenomenological way. As for experiments which one cannot depend upon, I think some of us remember the case of polywater that was in the early 70 s and more recently cold fusion. These are very interesting examples and hopefully here we see the self-correcting method of science at work even if it sometimes takes a long time.

I would like now to turn to theories in physics and what I may call as epicycle phenomena. Here is an interesting example. In the middle 1950s Heisenberg and Pauli set out to build what was called a nonlinear spin of theory for elementary particles and their interaction and this was supposed to be a master theory which would tell us all about fundamental, elementary particle interaction. But very soon after this project was begun with great enthusiasm, Pauli lost faith in it and withdrew. However, Heisenberg and his student, Hans Peter Duer, continued working with this theory well into the era of the fruitful SU3 in particle physics. This was in the early

60 s and even I, as a student, remember a phase when for every new SU3 sum rule or SU3 prediction that came from the fruitful SU3 theory very soon the nonlinear spin of theory would find a way to produce it from its starting point. It involved modifying the theory, twisting it and turning it to meet every new demand or every result which SU3 produced.

Ultimately it did not get very far. Well it is for these reasons that I am recalling these examples.

There are instructive examples in other fields also; for example, in life sciences, illustrating that you are not alone. In molecular biology there is a central dogma, a ruling dogma, and that is DNA helical structure is right-handed. For many years one of our young colleagues at IISc, Professor V. Shashisekharan argued that in some situation the DNA helix were left-handed but he faced very stiff opposition to publication and propagation of this idea. I believe that there are some experiments now which show that in some cases the DNA is indeed left-handed and I might also mention a couple of years ago a thesis for the Ph.D. was written at the IISc examining this episode from the point of view of the social and psychological forces that work. In theory of evolution also there is a central dogma and as you all know, a leading authority said sometime ago "Nothing in biology makes sense except in the framework of evolutionary theory". The basic concept here is that mutations at the gene level occur spontaneously or autonomously; they cannot be influenced by anything from outside. These changes are then expressed in pheno type in the individuals that arise and it is only later, that the interaction with the environment, the idea of fitness and the idea of survival all come in. They are not supposed to be present at the primary level. However, now there is some evidence that there exist environment-induced mutations so there are attempts these days to think through the whole question to extend and to adjust Darwinian principles so as to accommodate these new findings. I would imagine that these are also somewhat controversial but it does appear that there are experiments which show environment induced mutations.

Turning to standard models, there are several in science. Some years ago Salam had given a lecture where he talked of the three major ones – first in cosmology, second in particle physics and the third in geophysics – all in physical sciences. In the case of particle physics it is refreshing that the community is generally embarrassed at the continuing success of the Glashow-Salam-Weinberg model and there are constant searches to see whether it breaks down. May be some of the particle physicists here may say something about this aspect later on. But I must warn them in advance of something Jayant Narlikar said not long ago with some justification and I read out those paragraphs and repeat the justification he gives – "In a bizarre combination of interests the particle theorists say that their theories of high energy particle will be vindicated if we take the Big Bang theory seriously and argue that such energy prevailed in the universe when it was a billion, billion, billion, billionth part of a second old while the Big Bang Cosmologists insist that because particle theorists say so, such non baryonic particles must exist; though joining of two highly speculative conjectures should be interpreted as a confirmed certainty defies all scientific rationale. The trouble is that this subject is being taken very seriously and is now dignified by the name Astroparticle Physics". In discussing the Big Bang and Alternatives are we accepting the model or are we on the verge of revolution of the Copernican scale? Can we guess how long it might take for a revolution? May be 25 years! At this point Prof. Burbidge's phrase spoken yesterday "old radicals versus young conservatives" is relevant. But this seems just the opposite of what Max Planck had in mind when he said something in a somewhat cynical mood. He said a new scientific truth does not triumph by convincing its opponents and making them see the light but only because its opponents eventually die and the new generation grows up being familiar with it. So we should consider in which sense could Planck's statement be applied and be true in the present context. I would end this random set of remarks at this point. I now invite the panelists in alphabetical order – Arp, Burbidge, Cowsik and Padmanabhan – each, to make an extended statement of ten to fifteen minutes duration. You can discuss general questions of philosophy, sociology, epistemology – the roles of all subjective and objective. If you wish you can also turn to matters of detailed suggestions to the younger members of the audience – what directions to look for. But I only request you to remember that this session is really not an occasion to present new or additional results – it is more an occasion for reflection with wisdom and insight. After the panelists speak, the audience can join in with questions to each other or to the panelists. Remember again the unifying theme is Big Bang versus Alternatives. Let me now invite Professor Arp to present his comments.

☆ ☆ ☆ ☆ ☆

H. C. Arp

In my rush to present to you as much as possible of the observational results I did not get time to express anything at all about theory, or what I thought it all meant. So I was particularly sensitive to, and almost jumped, when Paddy said "Well, we do not know what it all means and there is no possible explanation".

Therefore I want to show a brief outline of what I think it all may mean. Though I might not call it a theory it may be a logical connection, or a possible logical connection, between the empirical results which we have been talking about. The initial point I would make here is that if we start from the conventional Big Bang solution we start from this equation involving the energy (T) terms on one side and the space curvature terms (G) on the other side. You will recall that Friedmann in 1922 made what I think should be called the special solution – one in which the particle masses were assumed constant. Of course you get scale expansion, and you get a Hubble constant which is embarrassingly large for all but the lowest redshift objects. Most embarrassing for physics, you get singularities at $t = 0$ and high densities. All this makes one wonder whether the assumption that led to the redshift being only a scale expansion is tenable. Now, if as some of us believe, the quasars are proved not to show recession velocities then this unconditionally violates the requirements of the Friedmann, Big Bang solution. We have to find a solution of the the general equations which gives intrinsic redshifts.

Something which I did not get to say and which I think is extremely important is that this intrinsic redshift is just not found in quasars but is characteristic of most extragalactic objects, objects that are like quasars like compact galaxies, like active galaxies, like normal galaxies and, in fact, even of very normal galaxies like companion galaxies and even to youngest stars. For example the brightest, youngest super giants in the nearby Magellanic Clouds and our own Milky Way Galaxy, are all systematically redshifted. As the diagram indicates, this means you come up against the brick wall of observational reality. In other words the conventional Big Bang can't work. Empirically I think what it means is that this redshift is intrinsic redshift which is, in turn, a function of the age of the object. That is an empirical conclusion.

So we have to go back up here to the beginning of the diagram to see where the solution has gone wrong. It is my impression that a typical anthropomorphic misassumption is that the whole universe is exactly like the little slice of space time that we experience. Particle masses do not necessarily stay constant over large expanses of space and time. A general solution of the Friedmann equation should not assume this. I would say that even mathematically the correct way to solve this equation is not to make the approximation and then a solution but to make a general solution and then make the approximation if it is appropriate. So I am attracted to the 1977 solution of Jayant Narlikar which, amazingly, brings us to an intrinsic redshift which is a function of time, or age of the particle masses. I would maintain that this passes the empirical test, that this is the logical connection between the observations and the theory. I wish to straightaway point out that this more general solution gives the observed Hubble constant of H = 50km/sec/Mpc (larger Hubble constants arise from including higher redshift galaxies which are younger and have higher intrinsic redshifts). Our more general solution predicts for galaxies all formed at the same time an exact Hubble relation (through look back time) with very little dispersion. The observed Hubble relationship should show more dispersion than it does if redshifts are interpreted as peculiar velocities rather than age differences. Although this non-expanding universe is totally compatible with all local physics by means of a conformal transformation between local and universal time scales, the important part for me is that the general solution is Machian. The conventional general relativistic solution is local and I cannot do cosmology with a local theory. It is like assuming the universe is governed by a physical law but that there is no connection between its parts.

What we are suggesting in place of this is a Machian Universe where the masses of elementary particles depends on their age and therefore the number of other particle masses which they communicate with within their light horizon. This leads to a consistent interpretation of the observations as matter being episodically born in a low mass, high redshift state and then continually evolving from highly energetic, compact objects through to the old, relaxed, low redshift galaxies we are so familiar with. Basically, as I have said, this is simply an empirical description of the observations.

I would like to close with the most important point and say that if the observations are valid they obviously rule out the current cosmology theory. Then we face a really serious crisis. Either the conventional theory is correct or it is catastrophically wrong. This has such serious consequences that any tenured scientist has to think carefully as to what value his production is, what he is achieving with his work. Perhaps even more importantly, any younger, aspiring scientist has to ask what does science mean to him, can he carry out what he considers meaningful work. I present these observations and comments so that each person can make this very important decision for himself.

☆ ☆ ☆ ☆ ☆

Geoffrey Burbidge

This is a panel discussion. I am not going to make a connected set of remarks, but first of all I want to make some comments on one or two things the Chairman said. In giving the first examples about physics of course he is quite right but what you learn from this mostly is that people are not prepared to accept observational or experimental results unless you have a theory ready to accommodate them. This is a very depressing aspect of this whole situation in cosmology. I must say that I am pretty depressed about the situation, and coming to this meeting and arguing about it has not particularly improved my morale. It has upset me not because this point is clear but it is also clear that people of this generation have decided that they can only learn something about astronomy by applying the known laws of physics; they are not taking into account history which says that you learn quite a lot about physics from astronomy. To give you one example – in the discussion of the very early history of the universe. Particle theorists are talking about timescales -10^{-43} seconds on the assumption that the laws of physics are the only items in the universe that never evolve but remain constant. I think this discussion of physics in a situation where it is completely untestable is purely a matter of taste and has nothing whatever to do with science as we are all brought up to understand it. Another remark that came to mind as the Chairman was talking is that there is some hope because there is at least one major change in the direction in physical science which I am aware of in this century, which came about without the theory to support it, and that is continental drift. In 1910 Wegener, who was an Austrian meteorologist, looked at the globe and he saw that in a rather simple way you could fit all the continents together and that observation is a very simple observation. I would like to put it on a parallel with some of the phenomena that Chip Arp was showing this morning where you see two objects with very different redshifts which are physically associated. Wegener was also treated extremely badly. The geo-physical community from the time he proposed this idea treated him in the same way as some astrophysicists like Arp and Tifft have been treated. He could not get funding, and his graduate students were sent away from him. The geophysical community shunned him completely. He died in 1936 on the Greenland icecap and his theory was still being trashed. It took until the 1950s when from a completely different direction, namely attempts to measure the direction of magnetic field on the ocean floors, a project instigated by a (wrong) theory proposed by Blackett, the idea was proved to be correct. It was realised in a space of about 5 years that indeed there was a good case for Continental Drift. Now what is interesting in this connection is that the theoretical objections that were being raised to Wegener's idea relied on the leading theoretician of the day namely Harold Jeffreys, who was the Plumian Professor before Fred Hoyle in Cambridge. Jeffreys wrote the famous book called "The Earth", and his objection to the theory of Continental Drift was that there was no energy source which could move the continents around. Theoretically it failed – we did not have a theory, so it could not be right! My understanding from geophysicists today is that they all believe in continental drift, but they still do not understand why it happens. They all believe in the fact but they do not understand why it happens. They still have no good theory. One of the amusing aspects of this is that Harold Jeffreys went from being the leader of all of the people who disbelieved this idea to being the only one left – since he lived until he was a very old man who did not believe in the theory.

I am depressed about this situation because it is clear to me that there are literally two universes that we are talking about. One is the universe that most people believe in who have accepted the Big Bang as the way to go, are working on it and try to work out the details, and they are very excited about what they can get out of the microwave background and the infinite amount of detail through which they are going to settle one issue after the other about the kind of expanding universe, the kind of evolving universe that they live in. They accept from the beginning that all the discrete objects in the universe arise from fluctuations which evolve this way. We are providing observational evidence that this is *not* the way discrete objects are made, and what I am told is that we should really produce a complete theory of this before attention need be paid to it. The other problem which does worry me is the fact that most people are not aware of what has gone on in this field over the years, and the few things that Chip can show or I can show for that matter which are only the tip of the iceberg of the data that has accumulated over many years are not known to large numbers of people who are working on evolution of the universe using for example quasars and absorption lines and all these things. QSOs, etc. are probably not relevant if you take seriously any of the effects we are talking about.

Now is there any way out of this? The way out for many years for the people who do not want to hear, or do not believe in these data, has been to say that they are all accidental. One of my friends on the panel says these things are accidental. All I can say is that you have to study all of the evidence and decide to what extent this is really true.

In the history of science, we see that it is the younger generation which comes along and overturns the ideas of the older generation. You would think therefore that one or two members of the younger generation will come along and do this. I tend to get depressed because the younger generation seems to be more conservative, but there must be a few people who are interested enough to come along and take up the cudgels and get more data to try to confirm what I believe is already present but may not believe it, and not simply make observations to try and disprove it which is what a few observers have tried to do. But I have talked about this subject in public extensively and talked to a large number of astronomers from all over the world. I find that the younger generation are not only ignorant, but they have been told that it does not matter. They find that their livelihoods require them not to work on it. So this, in summary is why I am depressed. Please cheer me up and do the right thing.

☆ ☆ ☆ ☆ ☆

Ramnath Cowsik

I would like to make some remarks which are not very deeply thought out and not so carefully sculpted as our Chairman has spoken earlier but they do rely on some of the points that he has already brought out.

First thing is the relationship with examples in particle physics and so on and so forth and standard model. The status of the field of cosmology can be discerned by noting the following semantic situation. In particle physics we talk of the standard model and beyond the standard model. In cosmology we talk of the Big Bang Cosmology and non-standard cosmologies. It tells you the status of the field and I hope that in years to come we will have some standard cosmology and we will be able to talk beyond the standard cosmology as a driving force for research.

The second point also relies on the development in other fields. General theory of relativity is broadly accepted as a good theory of gravitation. There have been alternate theories but they were making no headway and even the general theory of relativity was not making any headway until the parametrised post-Newtonian formalism was brought out. In that form, all generalised theories fitted in and there were some parameters which took values like 1 or 0, say and general relativity chose specific values. The observational people or the experimental people went on trying to measure these parameters and these kinds of results have now been established. It gave strength not merely to general theory of relativity but also to alternate theories of gravity. They can all be put on the same platform and experimental and observational data can be used to distinguish among the set of theories. I don't know when cosmology can be put in that kind of an underpinning or umbrella.

These are broad remarks as I mentioned. Then a few specific remarks I would like to make. First of all whenever results are stated in astrophysics or in cosmology they are stated far too vehemently, far too sharply, not giving adequate caveat as to the underlying assumptions that have gone in, which may be justifiable, justified or reasonable assumptions but still many of the results are derived on the basis of these assumptions which have not been tested, which are very difficult to test. One example of such a thing is the following. Normally one thinks that adiabatic fluctuations are well motivated but there are also isocurvature fluctuations – they may not be as well motivated. These fluctuations can be there in the universe. They will lead to a different kind of growth of structures in the universe. The results of these models for all practical purpose fit the correlation function or power spectrum of distribution of matter in the universe roughly equally well. One may fit marginally better than the other but the thing is that there are different kinds of assumptions. One is hot dark matter plus a mixture of isothermal and isocurvature fluctuations and so on and so forth. The specifics do not matter – all that I am trying to say is – this is one example in which we have to be somewhat careful the way we describe the system.

And often there are very simple consequences of the assumptions that we make in trying to make a theory. These simple consequences which do not need elaborate understanding of the background situation or the physics, or the changing physics – they have to be worked out so that it gives a convincing view as to what is happening. For example in the context of particle creation there was serious discussion sometime in the past about whether we are living in a baryon symmetric universe or not. Now that was tested on the basis of how much gamma rays we find from the sky because the particles and anti-particles would annihilate each other and they will generate gamma rays. One would like to be assured that when one has this particle creation phenomenon that such issues do not come into play and that they are consistent with the background radiation. Probably this has been done but certainly in this meeting we have not found emphasis on such direct and very simple kind of checks that can be made. This might have been done or might not have been done. I am merely saying that one should try to look for very simple confirmations of the underlying ideas that are there at a very basic level.

☆ ☆ ☆ ☆ ☆

T. Padmanabhan

There is always an advantage in speaking last because many of the points you wanted to make have already been made by somebody else. So I will just confine myself to a few points which I think have not been made, at least during the opening comments by the panelists

As just mentioned, I belong to the younger generation of cosmologists or structure formationalists or whatever you may call. I started working in this field only from 1984 onwards. The reason I started working in this field is because I personally felt, at that time that cosmology was beginning to become a science. It became a science in the same sense as nuclear physics or condensed matter physics viz., that known laws of physics were being applied to explain observed phenomenon. Of course we still have not reached anywhere near the levels in Nuclear physics or Condensed matter physics or Particle physics but that is the direction towards which cosmology is moving. There are observations and there are models which are struggling to fit these observations. This is why in my lecture, I confined my attention to the period in the universe where the energy is below 100 GeV so that I can, in principle, test it in the laboratory. I also confined myself to an epoch [though I did not stress it] above a redshift of 5 in the conventional universe. This is because below a redshift of 5 – which hosts most of the exciting phenomena which have been presented today – one requires very complex astrophysical, baryonic modelling which we cannot yet do properly within the Big Bang model. I know that there exist strong-minded big-bangers who would make claims which are more than what I have made today but to me this itself is sufficient. The way science progresses, as I see it, is to use known laws of physics in a particular domain and then start extrapolating it in both directions. If you can go higher than 100 GeV there are several untested particle physics models, using which people have been trying to make very interesting predictions in cosmology – Inflation is one of them. Indeed none of them are tested in particle physics. So I will take a point of view that these are very exciting – we should look at them – but one should not put too much faith in them at this stage. Similarly there is model building which goes on in the very hard astrophysical modelling of the systems which we see upto a redshift of 5 and Geoff himself mentioned today morning how difficult it is. But people have ventured into it and one should keep an open mind. In my opinion the way it is going to proceed is something like this. There is a strong lobby of people who believe that we understand 100 GV to 1 keV regime in the universe and people are pushing their models to great levels of detail. I think the talk by Tarun highlighted, for example, the fact that cosmic microwave background radiation is going to be tested to unprecedented accuracy within a span of 10–15 years time and there are very clear predictions within a variety of parametrised Big Bang models where there is a set of about half a dozen parameters which are being used to predict the C_l coefficients which are probably thousands in number. So we should be able to distinguish between these models or possibly even rule out all these models. Either of this can happen.

Now the question before us is the following: suppose that happens – the reason I come for these meetings is because there is always a possibility that the Cobra-Sambas or some such observations will rule out all known models, the entire parameter space of Big Bang and I would very much like to know whether there is a back-up theory which I can fall back on. In a somewhat different sense, I am also going back very depressed (like Geoff) because I do not see a good, viable, alternative model. The reason is because the alternate models have not developed the same level of accuracy at which they can be compared with observations. I think Ramnath made a very good point about that. There are several people who have their own theories of general relativity or gravitation. I know quite a few of them myself; they have their own alternatives to Einstein's theory of gravity. However, most of these people spend time and develop the theory to a certain extent that you can test that against a parametrised post-Newtonian approximation or in some such sense against general relativity. Then either the theory can be put in an acceptable "not-in-contradiction-with-experiment class" or it can be thrown out or it always has a hope of becoming a serious rival to Einstein's theory. I do not see that kind of development in any of the Alternative Cosmologies at the present stage. I am sure people will be working on this and eventually I look forward to a time when it is developed to such an extent that the cosmological predictions from QSSC can be compared against, say, the Cobra-Sambas data.

There are a few other points which I wanted to make which are somewhat aside to the main theme. I was quite impressed by the comment which Jayant has made about astroparticle physics. I have not read that before. Now it is rather amusing to me that most of QSSC is also based on a field theoretical model (which I personally think is quite viable) but is completely untested in the laboratory. I am sure he will have an answer to that but I look forward to that answer.

The other comment which I wanted to make was regarding Mukunda's opening statement about there being a self-correcting tendency in a very large timescale over theories. But I think there is always a drift in the right direction historically. This is important because to every single person who has said the right thing and was completely ignored, and may be died in poverty because the bandwagon will not agree with him, there are hundreds of people who are just cranks whose theories have fallen by the wayside. So just because the theory is unconventional, does not mean it is true.

The last comment I would like to make is what the future has in store, in a true scientific spirit, for the Big Bang model. In 1991, I was writing a book on this subject and in the last chapter I said that there are two things which Big Bang cosmologists should look forward to: first is detection of anisotropics in the microwave background radiation; second is the laboratory detection of a dark matter candidate particle. Now fortunately, by the time the book was in print, the COBE detection showed us that broad paradigm of gravitational instability as a source of structure formation is not incorrect. In fact there was a time, as Jayant was commenting yesterday, when Big Bang cosmologists were very seriously worried whether the basic idea of structure formation is right or wrong and COBE set those doubts aside at least in our minds. The next thing we look forward to is – in 10 years 15 years 20 years time – a laboratory detection of a wimp. That would go a long way in guiding us in the right direction. I think that is all I would like to say right now and if there are specific questions we can take them up later.

☆ ☆ ☆ ☆ ☆