CONCLUDING REMARKS

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Unfortunately I was unable to attend the first two days of this meeting, owing to another long-standing engagement, and hence I cannot give a fair summary of the entire symposium. Instead, I shall try to put the subject matter of this symposium into perspective, and to discuss what seem to be some of the key issues we face at the present time.

I think this is one of the most enjoyable of the IAU symposia that I have attended. The number of people working in the field of dense matter is still relatively small, and therefore this symposium has not been crowded with the large number of papers that is all too often a feature of IAU symposia. We have been able to proceed in a relaxed and orderly manner, and I have enjoyed this feature of the symposium very much.

In particular, this has been a symposium of IAU Commission 35 on Stellar Structure. It is a measure of the newness of this field of dense matter calculations that probably the majority of the people presenting papers are not members of the IAU. A large number of the participants are physicists who have entered the field very recently because of their interest in the properties of neutron stars. Probably Commission 35 should co-opt many of you in order to give dense matter a greater representation within the deliberations of the IAU.

In many respects the discussions of this symposium have run parallel to those which were held at the Aspen Workshop on Neutron Stars in August, 1971. It has been interesting to me to see to what degree the field has changed during that intervening year. There have been some important new advances, but in many respects the situation is much as it was then. There have been some aspects of this meeting which also touch upon the Aspen Workshop on The Physics of the Early Universe, held in June, 1972. I will mention some relevant results from that workshop in the course of these remarks.

There are two aspects of dense matter: hot dense matter and cold dense matter. Most of the discussions here have dealt with cold dense matter, with applications mainly to neutron stars. It is unfortunate that the other aspect, hot dense matter, has hardly been touched upon except for the exceedingly important talk by Omnès. One expects to find hot dense matter mainly in the early history of the Universe, and I would first like to speak regarding that subject.

There are two basic approaches to a discussion of the physics of the early Universe. In one approach one takes a completely symmetric Universe, in which there are equal numbers of baryons and antibaryons, and in which the baryonic number is therefore zero. We have heard Omnès describe his approach, involving a phase separation between the baryons and antibaryons at high temperature, and probably this is the only feasible scheme which may be capable of producing a large-scale separation of matter

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from antimatter as is required in the later stages of development of the Universe.

The alternative is to assume an asymmetric universe, in which the number of baryons exceeds the number of antibaryons. Here again we can consider two approaches. In one of these, the earlier stages of the Universe involved exceedingly high temperatures, so that the Universe is filled with baryon-antibaryon pairs, with only a very small excess of baryons over antibaryons at the earliest times. If there is no phase separation between baryons and antibaryons at high temperature, then gradually there will be a complete annihilation of the antibaryons with the baryons, leaving the small excess of baryons as ordinary matter to fill the expanding Universe in its later stages. On the other hand, if there is a separation between baryons and antibaryons at high temperature, then this picture is insignificantly different from the picture presented by Omnès, and one would expect to find large-scale separation of patches of matter and antimatter in this case as in his. Thus the whole question of the reality of the phase separation is obviously of immense importance, and deserving of much further work.

The other approach to the asymmetric universe was first proposed by Hagedorn. In this approach matter reaches a finite limiting temperature as it is squeezed to indefinitely high densities, and hence space becomes filled with baryons, but with baryons of increasingly higher masses well up on the scale of an exponential mass spectrum. We have not seen equations of state of dense matter of this type applied to the cosmological problem at this meeting, but we have seen them applied to discussions of neutron stars.

Let us follow the Hagedorn approach as we go backwards in time in the universe. The radiation background will increase in temperature, and eventually when the temperature rises to 3000 K, matter will become ionized in the Universe, and as we go still further back, when the temperature passes through 10^9 K, all of the nuclei which may exist in the Universe will be broken down by photodisintegration. Further back, we create electron pairs, then muon pairs, and eventually some pions. At that point the baryons present start to be transformed into various baryonic excited states, higher up on the mass spectrum. As the density becomes higher, the characteristic mass of the baryons present will continue to increase, but the temperature will approach an asymptotic limit which is about equal to the pion rest mass, as Craig Wheeler discussed. Hagedorn suggests large variations in baryon number are possible.

Eventually we will come to a critical era in the early Universe, at an expansion time of around 10^{-43} s, at which the typical Hubble radius would be about 3×10^{-33} cm. We expect that quantum effects will come into general relativity under these conditions. The typical mass of the baryons which will exist at this time is of the order of 10^{-5} g. Only masses of this high order will have Compton wavelengths small enough to fit into the Hubble radius of the Universe at this time. Indeed, if we multiply this mass by the expansion age of the Universe, the result is a number of the order of $\hbar$; this is simply an indication that we are dealing with an epoch in the expansion of the universe at which the Heisenberg uncertainty principle is approximately fulfilled by the general relativistic quantities, and hence if there is any unity in general relativity and quantum mechanics, it does not make sense to ask what happened in the universe at an earlier time, because large fluctuations in the energy make earlier time scales lack any meaning. We would certainly like to know whether this type of picture has any validity, since it represents such an enormous extrapolation from current knowledge of the baryonic mass spectrum and of baryonic particle theory.

If we take the Omnès approach, and have a symmetric Universe, then the picture at 10^{-43} s would be rather similar, except of course that we would have equal numbers of baryons and antibaryons present. Presumably these baryons and antibaryons would have to have extremely high masses, in order that they can fit into the Universe at this time.

The reason I am stressing the epoch 10^{-43} s, is that some very interesting results concerning this epoch were presented by C. Misner and his colleagues, and by L. Parker of the University of Wisconsin (at Milwaukee), at the 1972 Aspen Workshop on The Physics of the Early Universe, which pertained to this characteristic time of 10^{-43} s. Parker, in particular, has been examining the properties of an anisotropic cosmology associated with that characteristic time. As the Universe expands, the Hubble radius encompasses larger and larger amounts of matter, and we must ask the question why different parts of the Universe should be synchronized in time when the Hubble radii centered upon those parts start to overlap and bring the two different parts into communication. If they are not synchronized, then one expects large variations in the local metric at the places where the Hubble radii start to overlap, and these large variations in the metric may be represented by anisotropic expansion rates. It is best to describe this general situation as representing gravitational chaos.

Now if we had a very strong electric field under such circumstances, then this electric field would undoubtedly be able to produce pairs of baryons and antibaryons which would be accelerated in different directions by the strong field strength, acquiring energy at the expense of the field strength. Parker has shown that strong gravitational potential gradients can perform a similar role. These can produce pairs of baryons and antibaryons, causing them to move in different directions, and acquiring energy at the expense of the gravitational potential gradient. Indeed, the greater part of the gravitational potential gradient associated with anisotropic expansion will be eliminated through the creation of rest mass in baryon-antibaryon pairs. Furthermore, his calculations show that the dimensional extent of the baryons and antibaryons created extends to about twice the distance of the associated Hubble radii. This is a hint that this pair creation process has a physical extent that extends beyond the universal horizon, and thus it is promising that this may provide a mechanism for producing a large-scale homogeneity in the structure of the Universe, thus synchronizing the rates at which different patches of the Universe join onto neighboring patches when their associated Hubble radii become large enough.

If this large-scale synchronization and homogeneity should be achieved, then this would be an argument against the appearance of Yuval Ne'eman's white holes, which he has discussed at this conference. If it is not achieved, then perhaps we should take the white holes seriously.

This work of Parker is certainly philosophically very attractive, and it produces the consequence that we should expect a symmetric Universe, with equal numbers of baryons and antibaryons. If it is correct, then we should certainly prefer the Omnès approach. I was very impressed by the progress which Omnès has achieved in working out the details of the symmetric theory, and apparently being able to separate matter and antimatter at least on a galactic scale. I hope he should eventually find, upon more precise calculation, that the separation can be done on the slightly larger scale of the mass in clusters of galaxies, because otherwise I think we would probably have trouble with too much matter-antimatter annihilation arising from interactions between galaxies in clusters. But he has certainly come sufficiently close to that point that the difference between mass separation on the scale of galaxies or of clusters of galaxies is not a very significant difference.

One of the main issues, mentioned by Omnès at the end of his talk, is the lack of any cosmological nucleosynthesis of helium, or of deuterium and tritium, in the symmetric cosmology which he has presented. The attractiveness of cosmological nucleosynthesis in a Hagedorn-type theory arises from the fact that the amount of helium produced is just of the right order of magnitude, about 25% by mass, of the amount which appears to exist in stars made both early in the Galaxy and late in the Galaxy, and also in other galaxies. This hydrogen-helium ratio appears to be a very universal function, and discussions in recent years from the observational point of view have concluded that the hydrogen to helium ratio is essentially universal. This seems to require a pre-galactic production of helium.

Recently there has also been much discussion of the amount of deuterium which we have in the solar system. The deuterium to hydrogen ratio in the solar system seems, at least in the primitive solar nebula, not to be the terrestrial ratio present in sea water but something of the order of 0.1 to 0.3 times terrestrial. This primordial deuterium would be about 50% greater than the amount of primordial ^{3}He. In a cosmology in which early nucleosynthesis occurs, these amounts of deuterium and ^{3}He would be produced in an open Universe with something like 10% of the critical closure density. This makes a very attractive picture. Some recent work by Hubert Reeves has gone further, and has suggested that in addition we can get the right amount of ^{7}Li produced in cosmological nucleosynthesis if we live in a Universe in which there is a negative lepton number, that is, an excess of antineutrinos over neutrinos in the background neutrino radiation.

Now it appears that none of this cosmological nucleosynthesis will take place in an Omnès-type symmetric cosmology. However, it does seem possible that an opportunity may exist for the production of large amounts of helium in the pre-Galactic stage. After matter recombines at a temperature of 3000 K, the Jeans length in the expanding matter encloses only about 10^6 solar masses of material. As the matter continues to expand before halting and recollapsing, the Jeans length continues to decrease, enclosing only a few thousand solar masses of material at the time of maximum expansion, where the Jeans length approach to fragmentation is probably of maximum validity. Further opportunity for fragmentation of the matter may occur during the collapse phase of the matter, so that it is entirely possible that stars in the mass range

10^2 to 10^3 solar masses may be formed as a first pre-galactic generation in space.

In a symmetric cosmology, it would appear that these stars would be entirely composed of hydrogen. In the asymmetric cosmology of the Hagedorn type, they will be composed of a mixture of hydrogen and helium. These different types of stars will behave somewhat differently, particularly during an initial collapse phase, unless angular momentum effects produce flattened disks first from which the massive stars form. In either case, a great deal of supernova explosions with accompanying formation of heavy metals can be expected to occur, and the thing that we must look into carefully is whether huge amounts of helium can be formed at the same time, particularly in the stars which would be composed of pure hydrogen. Such helium production might take place as a result of collapse, conversion of hydrogen to neutrons, production of helium, and re-explosion of the matter into space. Helium might also be produced in large quantities if the massive stars which are formed are vibrationally unstable, leading to mass shedding in which large amounts of helium are ejected. All of these processes will have to be carefully examined before we can answer the question as to whether the Omnès cosmology can acquire enough helium at the pre-galactic stage to satisfy the essential universality of the helium to hydrogen ratio which is observed throughout many galaxies.

There appears to be no opportunity to make deuterium and ^{3}He in the pre-galactic stage, at least not in the proportions appropriate to the early solar system. However, Stirling Colgate has recently suggested a supernova mechanism in which deuterium and ^{3}He might be produced during the normal course of galactic evolution. In this process a strong supernova shock wave, becoming relativistic as it approaches the outer fringes of the star, accelerates electrons forward, creating a strong electric field, which then accelerates protons and alpha-particles at different rates, leading to collisions at several tens of MeV of energy, and consequent spallation production of deuterium and ^{3}He. It is clear that this process will also require a great deal of scrutiny, because it may remove the need for cosmological production of these two light isotopes.

It is thus evident that a great deal of exciting work is in store for those working on the theory of hot matter and its applications to cosmology. Let me now turn to the question of cold dense matter.

The characteristic picture which we have for the neutron star consists of an outer crust, the lower part of that crust containing some superfluid neutrons as well as nuclei, under that a mixture of superfluid neutrons and protons, and then at the center a core which may or may not be a crystalline solid. There are a number of important basic issues to face here, among them the important question as to whether this basic picture usually assumed for the neutron star may be entirely wrong. However, let us proceed for the moment on the assumption that the general picture is right.

There now seems to be fairly general agreement that the nuclei in the crust, including those at the base of the crust which interact with the free neutron gas, tend to be on the small side rather than on the large side. There seems to be some disagreement still as to how one should go about calculating the precise character of those nuclei, but there seems definitely to be agreement that those nuclei should be rather small.

Just a year ago David Pines said at Aspen that if it turned out that the nuclei were small, that would invalidate the starquake theory, at least in terms of crustal starquakes. Not long after the 1971 Aspen workshop the second major glitch was observed in the Vela pulsar, and this caused Pines to have additional grounds for distrusting the crust-quake theory, at least as applied to the Vela pulsar. Meanwhile, the work of Canuto and Chitre, and of some others, tended to indicate that probably there was a solid core at the center of neutron stars, and that led Pines and others to suggest that maybe we have glitches in the form of corequakes. I think the whole problem of glitches is still very much uncertain.

People have suggested that the infall of matter can produce such glitches. Personally I find this to be a very implausible suggestion because of the difficulty of arranging to have a suitable supply of matter to do the infalling. Crust-quakes seem not to produce glitches. Corequakes may or may not work. I am a little concerned about the ability of cores to maintain distortions as their rotation decelerates, in the presence of density-sensitive reactions which can convert solids to liquids as the density changes. George Greenstein has mentioned that we have in principle another way to get glitches, although we do not know how to make use of it yet. In any model of a neutron star in which there is a fair amount of superfluid, the superfluid must slow down by frictional processes, and if one can arrange for variations in the friction, one can certainly have what Pines calls the 'restless' behavior of the Crab nebula. If one can have sudden transfers of angular momentum from the superfluid neutron reservoir to the charged particle system in the pulsar, then we may get major glitches as well. Greenstein was suggesting the 'boiling pot theory' of glitches, which he did not indicate that he took very seriously, so I suppose the rest of us will not take it very seriously either. At any rate, some such mechanism for a sudden angular momentum transfer always remains a possibility if we can think of the appropriate physics that could produce it.

Now let me come to the question of the solid core. I think there are some extremely challenging aspects that are associated with the calculations which Canuto presented here, in particular concerning the hyperonic components of the crystalline core in his calculations. The basic problem is that the $^3P_1$ interaction between neutrons and protons is repulsive; it plays a big role in the stability of nuclei, but when Canuto and Chitre computed the interactions that should exist between hyperons using SU(3) techniques, they found that the interaction is as attractive as those of the other interactions, at least until very short distances are reached. The result is that if one makes a plot of the energy per nucleon vs. density, then by far the lowest energies that emerge from the calculations are those in which the crystalline lattice contains only hyperons, such as a Λ^0 lattice or a mixed lattice with Λ^0 and Σ hyperons. Furthermore, the calculations even indicate that such purely hyperonic lattices may form highly bound systems, with negative energies, at densities very much greater than that of ordinary nuclear matter. Such lattices are unstable against shear motions, and the implication is that the liquid state of a mixture of pure hyperons may be even more tightly bound than indicated for the lattice calculations of Canuto and Chitre.

If one takes these calculations at face value, we must realize that we have a fundamental uncertainty concerning our standard picture of the neutron star. A purely hyperonic star would be a more stable configuration. The calculations of Canuto and Chitre indicate that such a star may have a mass as high as 1.1 $M_{\odot}$, and its moment of inertia is certainly great enough to provide the requirements of the Crab nebula pulsar. If the mixture of pure hyperons has a large negative binding energy at a density of a few times 10^{15} g cm^{-3}, then it is even possible that this hyperonic star has an abrupt sharp surface at this density, without even a fringe of ordinary neutrons and protons If Σ hyperons are included within the mixture, then this configuration can maintain a strong magnetic field, so that all of the basic characteristics of the pulsar could be provided by such a model. Even restless behavior in glitches might occur in the lower density regime, near the surface, where the material has fluid properties, if in fact the properties are superfluid, and there is only a small amount of friction between the Λ^0 hyperons and the charged Σ hyperons.

I have tried very hard to think of pulsar observations which might exclude this type of model which emerges if we take the work of Canuto and Chitre seriously, but I have been unable to do so. This indicates the enormous importance for the future development of neutron star physics for particle physicists to determine the character of the $^3P_1$ interaction between hyperons, and between hyperons and neutrons or protons. If this interaction should be attractive between hyperons, as indicated by the SU(3) calculations of Canuto and Chitre, then this pure compact hyperonic star would seem to be a leading candidate for the explanation of pulsars.

This suggests also that further investigations of the physics of cold dense matter are likely to be exceedingly exciting. This is a very challenging field in which to work, and I am sure that there will be many exciting future conferences on the physics of dense matter.

It remains for me to express great thanks on behalf of myself and also on behalf of everyone here, to the organizers of this conference for the very excellent meeting which they have hosted and for the great hospitality which they have shown us. I propose a vote of thanks to the local organizing committee.

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