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General Discussion after Session I

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General discussion after session I

D. A. Williams (*UMIST, Manchester, U.K.*). I wish to highlight some of the interesting and possibly controversial points that were raised. Professor Tayler gave us a very good introduction to the subject and I expect we shall discuss the questions that he raised on abundance anomalies and in particular the survival of grains. There are two particular aspects that interest me. First there is deuterium fractionation in the interstellar medium and it is, of course, known that deuterium fractionation occurs in meteorite material. That seems to indicate that material was fractionated in cold conditions and that the conditions have remained cold ever since because, if the temperature gets above *ca.* 200 K, that fractionation will disappear. The other point that I found particularly interesting in recent literature is the detection of diamond in the carbonaceous component of certain meteorites and again this seems to indicate a low-temperature régime for that particular material; diamond not being the most stable form of carbon. In Professor Kroto's stimulating talk there were raised a number of questions, but not so much about chemistry of the interstellar medium as on chemistry in the laboratory or possibly chemistry in circumstellar regions; the main question that I would expect to hear discussed today is that of the applicability of what he has done. Very exciting though it is, there is some uncertainty about the applicability of his work to the problems that we are considering in this particular meeting. The conditions that you might find in the circumstellar regions are obviously not going to be quite like the conditions produced in the laboratory. The second important question that I would expect to be addressed in discussion now is the following. As material moves out of the circumstellar regions into the interstellar regions are the structures that Professor Kroto was describing expected to persist or not? He mentioned that C_{60} may be formed in Bunsen burners and he also said that C_{60} is very stable. If that is so why is all carbon on the earth not in the form of C_{60}? There must be some destruction mechanisms applying to these structures. Actually, if one makes amorphous carbon by having a surface in a carbon rich medium then, in fact, one does not get the sort of structures that he talked about. A mixture of diamond-like and graphitic-like regions of fairly small extent, perhaps a few tens of ångströms, is found.

Finally, in Dr Wolstencroft's paper I find it interesting that the radius of dust particles that he finds is greater than that of the interstellar dust and that might suggest incorporation of interstellar material in the dust. To develop the models properly more realistic models of grains do have to be incorporated. Of direct interest to the subject of this meeting, we do need information about the chemical composition of these grains. I would make the connection here between the source of objects that Dr Wolstencroft was talking about and other objects that are extensively studied. These are the hot cores that one finds for example in Orion where molecular anomalies are found in the gas. The conventional interpretation of this is that grains have accreted mantles, that there has been some processing on the mantles, that a star-formation process has gone off nearby and that the mantles have been evaporated and that one sees the effect of that evaporation on the region. Such models do agree with the observations reasonably well, which suggests that by following this line one should be able to determine the chemical composition of grains in regions of star formation where mantles are present on the underlying refractory cores.

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D. Clayton (*University of Durham, U.K.*). I wish to make just one remark on Professor Tayler's keynote address and then put a question to Professor Kroto. At this Royal Society Discussion Meeting it is not inappropriate to remind ourselves that this beautiful theory of nuclear synthesis largely originated with one of the most distinguished members of this Society whose papers in 1946 and 1954 laid the groundwork for almost everything that followed. Secondly, in the problem that Professor Tayler brought up about inhomogeneity or incomplete mixing, there are cases where the English language doesn't exactly tell all that it should scientifically. We really need to distinguish between two types of inhomogeneity. One type is a spacial inhomogeneity in which one region of space is isotopically or chemically different from another. Another type of inhomogeneity is that for which all regions of space are similar but there are structures within each region of space that are chemically and isotopically different from other structures. I prefer the latter type of theory for the source of the isotopic anomalies.

I have a question for Professor Kroto on C_{60}. What happens if there is an oxidizing environment around when this is trying to form? In particular in the outflows from carbon stars, the isotopic composition of the carbon is changing rapidly while the star is changing from an oxygen-rich star to a carbon-rich star so I think that it would be interesting to know whether if there was a Bunsen burner with oxygen around, one could still get C_{60}.

H. W. Kroto. This is one of the most interesting questions because I feel that oxygen-containing stars must have some similarities with the experiments that one does with a Bunsen burner. In fact, comparing the experiment that I talked about with the data of oxygen-containing stars, which I only found in the literature five days ago and which was published about three weeks ago, shows that under those conditions C_{60} gives a very big peak. Under oxygen starvation, but nevertheless in an oxygen environment, one can obtain soot particles. There is a very powerful process corresponding to hydrogen extraction from things like acetylene and di-acetylene. What is happening is the hydrogen is being taken off as H_2O and carbon is left; the important thing is that C_{60} is a survivor of this process. Other species are produced and will be processed and be photofragmented but the empirical evidence, and it is unequivocal, is that C_{60} is much more durable than the others.

The other thing that I would like to say to Professor Williams is that I think our experiments are very relevant to the circumstellar situation. Whenever we look at a sooting flame we find polyacetylene where we see C_{60} and carbon black. Whenever we see infrared objects where one sees scattering, presumably from particles, for a carbon star we also see polyacetylenes. Therefore the link is the polyacetylene signature. There is a very close similarity between the objects that we have just suggested and these embryo structures which, I believe, are the fundamental refractory-core-producing mechanism and a structure on which I think more or less all particles can actually form. Here we believe we have the missing link between atoms and molecules and large particles and that, perhaps, is the most important aspect of what I had to say.

B. Fegley (*Massachusetts Institute of Technology, U.S.A.*). How does an excess of hydrogen (for example, an H/C ratio equal to the solar value of 2396 (Cameron 1982) affect the stability of carbon clusters such as C_{60}?

Reference

Cameron, A. G. W. 1982 In *Essays in nuclear astrophysics* (ed. C. A. Barnes, D. D. Clayton & D. N. Schramm), pp. 23–43. Cambridge University Press.

H. W. Kroto. We have found that carbon clusters do form even in the presence of significant amounts of hydrogen. Perhaps the crucial factor here is temperature. The C—C bond is significantly more stable than the C—H bond and if the gas-phase reactions occur at the correct temperature and pressure, C—C bond formation will occur at the expense of C—H bonds. We are aiming to explore this problem in the next phase of our experiments.

C. T. Pillinger. I would like to ask Professor Kroto a related question, following on from that last one, which perhaps would bring the subject of isotopic anomalies into context with his work. Firstly a small point which I may have missed. Did he mention anything about the dimensions of his different-sized, ball-shaped molecules? And then to carry on from that, has he considered the possibility of using them to trap gaseous species, perhaps a large atom such as xenon, or alternatively as he coils up balls of soot is there space between the layers to trap species which might be of interest to those people interested in isotopic anomalies. As a final point, is there any scope for substituting nitrogen atoms into the structure of his molecules; for example I could think of a situation where three pentagons create a sphere within which one nitrogen atom could be trapped?

H. W. Kroto. The small molecule C_{60}, which I must insist is a minor character in the exercise, is in a cul-de-sac; it doesn't take part in the nucleation so it is about 7 Å in diameter. The particles that I think we are seeing are just soot particles, they will presumably have soot-like sizing and they will be coiled and grow in three-dimensional shells. Yes, I am absolutely convinced that you could trap things inside them. You can certainly trap things inside C_{60}. We have evidence that there are metal atoms trapped inside a calcium lanthanum which actually like to get entwined between the layers of carbon.

With nitrogen yes, I think that there is the possibility of nitrogen getting stuck into the plane or in the coiling shell itself. Almost certainly you can trap, say, helium inside this thing and once helium is inside it it can possibly never get out; at least not until you actually get to a temperature which will fragment the shell. As an example I think that the Chernobyl dust is stuffed with radioactive species trapped inside these types of shell species.

R. J. Tayler. Do Professor Kroto's ideas about molecules such as C_{60} have any effect on the general view that in the atmospheres of cool stars the most important compound is usually CO?

H. W. Kroto. Any star that is surrounded by carbonaceous grains must have experienced conditions in which C—C bond formation has been preferred relative to C—O bond formation. Such situations can occur in some flames and in some hydrocarbon oxidation processes. C_{60} is produced by the same mechanism that leads to large carbon particles and thus has given us an insight into the types of physicochemical conditions needed to produce them. The detection of grains thus indicates that some significant quantities of carbon may have ended up in particles even in stars with large quantities of oxygen.

S. Chang. I would like to make a comment in response to Dr Pillinger's question about trapping of gases. In 1979 it was shown that by producing soots in the laboratory you could trap noble gases in the atmosphere with fairly high fractionation into the solid particle material. In addition there was elemental fractionation which tended to favour the incorporation of the heavy gases.

Now a question for Professor Kroto. The processes that he talked about are essentially high-temperature processes in which one would expect not to have fractionation of deuterium with respect to hydrogen preserved in the material or accompanying the material that is produced. Would he agree with that, and predict as I would that if one made the measurement of D/H ratios in those molecules they would be comparable to galactic D/H?

H. W. Kroto. That is very much outside my field. All I would say is that I agree with you that we are talking about dust particles produced in circumstellar shells. I do not think that we can get nucleation very easily in the general interstellar medium and I think Dr Chang is probably right on that, but again it is outside my field, very much so.

G. W. Wetherhill (*Carnegie Institution of Washington, U.S.A.*). I understand from reading review articles that there is also a small amount of gas associated with the β Pictoris dust disc and the question is whether or not this is consistent with the gas that one might expect from the comet model.

R. D. Wolstencroft. Is Dr Wetherhill referring to the narrow line observations?

G. W. Wetherhill. I have read each article by Boston and Gass and I believe also by Lars Chandler and in which they say only β Pictoris has gas associated with it.

R. D. Wolstencroft. All I can say is that the evidence, such as it is, suggests that β Pictoris is the youngest of the system so it may be the system which will have the gas blown out at the latest. It is the only one that is likely to have any gas left in it but with four objects only, statistics are very uncertain.

G. W. Wetherhill. One should be able to make a theoretical calculation of the dust to gas ratio associated with the comet model that you Dr Wolstencroft spoke of and ask whether this would be the gas that would be emitted from active comets at the present climate at that distance.

R. D. Wolstencroft. Assuming that that dust has come from comets, that hasn't been done but it is certainly an interesting line of investigation.

M. M. Woolfson, F.R.S. (*Department of Physics, University of York, U.K.*). The capture theory of the origin of planetary systems considers interactions between pairs of stars, one condensed and the other still a diffuse protostar, in the environment of a young galactic cluster. For a range of conditions a filament is drawn out of the protostar of sufficient density for planets to be formed directly from condensations within it. However, under a much wider range of conditions, material pulled out of the protostar and captured would be in the form of a cloud, or nebula, surrounding the condensed star. Dust settling into the mean plane would then give what is being seen in the IRAS observations.

Because both theory and observation suggest that lesser-mass stars are the last to form in a galactic cluster, and that F and G stars form first, it seems reasonable that late type stars are not observed with associated dusty discs.

G. Turner, F.R.S. (*Department of Physics, University of Shefield, U.K.*). I wonder whether Professor Kroto would like to comment on the possibility of formation of diamond.

H. W. Kroto. Not really! I do not think we have any evidence that those structures form under our conditions. The whole point of this is that we are obtaining structures by gas-phase nucleation, not on a surface, and therefore the control is due to an energy-driven process in which a sheet in the gas phase is actually energetically more stable if its curved. If you have a surface then the would-be graphite sheet would follow that surface and therefore you would get a graphitic substance. The other question is why are they not all soccer balls? It is only in the small range up to 100 to 200 or 300, maybe 500 atoms, where the curved structures and the round structures are more stable. For a large macroscopic object, such as a particle, then what will happen is that the sheets are actually very large or almost flat, and then flat, where the edges will become more important, and then a flat graphitic sort of crystal results. Perhaps diamond may occur when surface control is important but I have seen recent data on meteorites and they are very exciting. There is also some evidence, from Jack New's work, on the growth of diamond-type structures on a surface, I think from methane. However, under our conditions we have only seen a certain kind of observation and we have interpreted it in a certain way: as a gas-phase nucleation process which I think is important for circumstellar and interstellar grains.

M. M. Woolfson. I am not a mineralogist but I have always understood that to produce high-density modifications of minerals you need the pressures of deep down in the Earth or other planets and it surprises me that there is a suggestion that diamond, which is a high-density form of carbon, can be produced almost *in vacuo*.

P. Pellas (*Museum National d'Histoire Naturelle, Paris, France.*). I would like to ask Professor Kroto if he has made any experiments to destroy the C_{60} organization. What are the energies of the γ-rays or the ultraviolet rays or the dissociation temperatures which can destroy this molecule?

H. W. Kroto. I did show some data in which as the power is raised from, say, 1 mW to 100 mW, photofragmentation of large particles results, but C_{60} remains. This is a joint project with the Rice group and they have actually been able to photofragment C_{60} recently with extremely high power; much higher than that required to photofragment soot but I do not have those data at hand. So there are data on C_{60} photofragmentation but it takes something like 24 or 25 eV, depending on how you put the power in, but it is certainly a multiphoton process. These are very early days for this type of experiment. Many experiments have been done but it is very difficult to photofragment C_{60}; it seems to be more difficult to photofragment than carbon monoxide.