PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A
MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

General Discussion after Session II

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S. R. Taylor (*Research School of Earth Sciences, Australian National University, Canberra, Australia*). In respect to the paper by Dr Hutchison on chondrules I should remind you that about 100 years ago Professor Sawby described chondrules as molten drops in a fiery rain; it is interesting to see how much progress we have made since then. Dr Kurat described the primitive nature of meteorites, the extraordinary complexity of processes that have to occur effectively at zero temperature and Professor Clayton showed us again the complexities in the isotopic systematics with the implications for heterogeneity in the nebula. I now ask Dr Pellas to lead the discussion on these three papers.

P. Pellas. We have here in our meeting many astronomers and other people who are not directly involved in this very sophisticated study of meteorites. For this reason I shall give an overview of the problems we have to face. The first thing is that when we look at the planets then close to the Sun we have Mercury which is very iron-rich, and that as we go further from the Sun so the composition of planets changes.

For asteroids we observe the same effect; a change with distance to the Sun gives a change in the chemical composition, those closest having the less volatile elements with the carbonaceous asteroids being furthest away from the Sun. This is what we have when we look at the planets and asteroids, and it is probably what we *should* have, although we cannot be sure about that. When the Solar System formed there were meteorites very rich in volatile elements forming, perhaps 8 AU or more from the Sun, and also meteorites forming closer to the Sun. Perhaps information about what was going on is reflected in the redox conditions that exist among meteorites. Now an interesting point is that these redox conditions have, in some way, been corroborated by the oxygen isotopic signature because, as Professor Clayton has not mentioned today, although he has in many of his papers, there is some kind of relation between the chemical state of the material and the oxygen isotopic signature. So the overview we have is interesting because at least it seems to indicate that there is a framework. Of course, when we look in detail we understand very little; as Dr Kurat has mentioned when you are looking at one clast you will never see that same clast in all the meteorites. Just as each one of us is different from the other so it is for the chondrites. But we are reasonably sure that chondrules, even if we don't understand how they were made, were made at the beginning of the Solar System and not later. We do not yet understand what were the processes which formed the chondrules, but at least they require very special conditions, which have never later pertained, to give the chondrule-formation process. Now, when Dr Kurat described his beautiful model, I found two faults, although I agree with most of the points he made. His last point was that the final meteorites to form were the carbonaceous chondrites, those that we call the primitive meteorites. How could *primitive* meteorites be formed at the end? The point, surely, is that they were formed in a location, where fractionation did not occur and that is why we call this meteorite 'primitive'. In other words, they are primitive only in the sense that they have the Solar System abundances. They are not necessarily the most ancient meteorites. The second fault was that in the process which was described by Dr Kurat there was no account taken of different locations with respect to the Sun.

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M. E. Lipschutz (*Purdue University*, *U.S.A.*). It has been implied that post-accretionary heating events occur essentially in a closed system so that the chemical composition of chondrites is essentially that established during nebular condensation and accretion. In fact, shock seems to have caused loss of substantial proportions of certain mobile trace elements (and also ^{4}He, ^{40}Ar and other gases). These mobile trace elements had been thought of as providing cosmothermometric information in a nebula-condensation context. Furthermore, at least some believe that volatile and chalcophile element contents in E4-6 chondrites reflect metamorphic loss in a parent body rather than nebular fractionation: hence, the EH/EL classification scheme is not relevant except where more refractory elements are concerned. Could someone comment on the role of post-accretionary processes in affecting meteorite compositions?

P. Pellas. I was trying to put forward the basic processes that are involved in creating meteoritic material without going into detail, which limitation of time makes impossible. The principal fractionation mechanism, that of the volatile elements, can easily be established by porocity and the size of the aggregates. If chunks with low porocity are created then volatile elements will not easily be able to enter such material. They will enter but they will always have low abundances. Alternatively if there is high porocity then volatiles will easily enter the material and therefore there will be a high content of volatiles. The reference to EL and EH should be taken in a very general way; EH meteorites normally have high volatile-element content while ELs have low contents, but I was not intending to talk about special cases. Professor Lipschutz's special cases actually speak in favour of my model because my model leaves open the possibility of having volatile elements independently of the degree of crystallinity whereas his model needs a correlation between crystallinity and the content of volatile elements because the volatile elements are driven out by recrystallizing a certain piece of rock, whereas I let them in to different extents.

G. Kurat. In my discussion I was mainly concerned with the 'metamorphism' model of chondrites, which implies a planetary heating event, and which I consider not to be necessary. Of course, there are several examples of meteorites that experienced partial degassing which could be mainly due to heating via impact. There is also plenty of petrological evidence for such events, which, however, cannot explain the general (normal) features of common meteorites.

Regarding the E chondrites and their volatile and chalcophile element fractionation patterns I am very well aware of the general belief in a metamorphic event loss mechanism. In my model I try to offer an alternative mechanism: the isolation of matter from the vapour by fast aggregation and closure of pore space. This model has the advantage to 'explain' both, the different degrees of volatile element depletions in different chondrites *and* the omnipresent inhomogeneous distribution of these elements within a given chondrite.

Postaccretionary processes and their possible influence on meteorite compositions will naturally depend on the mass of the accreted body and the intensity of external disturbances (impacts, heating by radiation and others). Because most 'high-grade metamorphic' chondrites preserved delicate primitive features (e.g. non-equilibrium trace element distribution between minerals, relictic high-temperature crystal structures, non-equilibrated textures) it appears not to be likely that they experienced a metamorphic event. However, such an event should alter the chemical composition of the rock as Professor Lipschutz has shown in many experiments. These changes naturally grossly mimic the abundances expected in my model. However, there should be some volatile elements which, once present, could be retained by the rock via solution into a stable phase. A search for such element-mineral pairs and their abundances could eventually resolve the question of whether the distributions of volatile elements are primary or secondary.

M. E. Lipschutz. The intent of the comment is merely to indicate that the definition of primitiveness is an important question and the astronomer should realize that when we talk about primitive meteorites there is still a considerable argument as to what is meant by primitive.

G. Kurat. Obviously I was not too successful in explaining the meaning of primitivity; what I meant to say is that we should really consider almost all meteorites to be primitive. The differences between them indicate the succession of events they have experienced in the Solar System and nothing else. The only exceptions are the SNC and lunar meteorites.

P. Pellas. But that assumes that the primitivity of the meteorite can be different if it is at one given distance from the Sun compared with another meteorite which was formed at another distance from the Sun. In this sense the primitivity has no real meaning, only a location meaning.

G. Kurat. Not really, it does not depend on the location actually.

P. Pellas. Mercury is primitive; it is a primitive planet and everybody agrees on this. Jupiter is also a primitive planet but the composition is different. We cannot claim, for instance, that the eucrites are primitive meteorites because they are fractionated. These consist of fractionated material so in this sense they are not primitive but primitiveness depends on the different distances from the Sun. I suppose that we can imagine that at 1 AU primitiveness will be different to that at 8 AU. I think we can agree at least on this point.

S. R. Taylor. I think it is fair to say there is some dissent on the question of Mercury as a primitive object. Does anyone wish to address that point? I believe that the iron to silicate ratio on Mercury is about twice that in the Earth, for example, and that the general concensus at the moment is that it is as the result of a large collision, and doesn't depend on heliocentric distance. At least, that is my prejudice.

P. Pellas. If you can show that the iron silicate fractionation can be accounted for by a nebula or accretionary process then Mercury is of primitive composition but if it is due to some other mechanisms, as we know that there are several theories on this, then it will not be primitive, of course.

R. J. Tayler. It has been suggested that chondrites may have formed over a period of some 10 Ma. It is unfortunate that we cannot say what the Sun was doing at that time but we do know that it took about 10 Ma to reach the main sequence and that it may have been much more luminous and much more active before reaching the main sequence. What in relation to the solar history determines when chondrites form?

G. Wetherhill. Actually I would suggest that the principal distinction regarding the Sun is between the time in which the Sun became a compact object as opposed to being a collapsing molecular cloud. I am not an expert on this but, based on what I read, I gather the timescale for this would be of the order of a few times 100 ka up to 1 Ma years or so. Subsequently, if it indeed went through the kind of phenomenon that FU Orioni stars do and T-Tauri stars do, then this is the time when it is most likely that the gas was removed from the solar nebula and therefore the formation of Jupiter, which largely consists of hydrogen, would have to be compressed into this very short timescale of a few million years. I think that a good case could be made that the timescale we are talking about with regard to the formation of meteorites was also down to this 1 Ma or so during which the gas was removed from the solar nebula and during which perhaps Jupiter formed and began to stir up the asteroid belt. And during all this time I think it's important that we recognize that the Sun was not a normal star that there were perhaps great outflows of high-velocity particles and very different luminosities.

S. R. Taylor. We do need the early solar activity to get rid of the volatile elements in the inner Solar System. The inner planets are depleted not only in the gases but also in the volatile elements.

R. Hutchison. I think that the iodine–xenon systematics in fact show that chondrule formation must have taken place over something like 10 or 15 Ma. There are some crystalline chondrites which certainly appear to be 10–15 Ma older than individual chondrules in unequiliberated chondrites. I do not believe that chondrule formation was a single process at a single time but rather it was something that recurred over perhaps a 10 or 15 Ma timescale.

H. Wänke. I think we heard this afternoon from Dr Hutchison and then from Dr Kurat, two types of scenario which seem to oppose each other but I want to ask the question, can one not bring both types of observation which were put forward into agreement? The planetary processes, emphasized by Dr Hutchison must have taken place it seems to me. We have fractionated differentiated meteorites, eucrites, but also the iron meteorites and stony-irons and so on, and they are old. The Paris group has lead ages for the eucrite material of 4.58 Ga so they are within the range of age of those chondrules in chondrites. For some time it has seemed to me and to some others that to make liquid droplets, the best way is to disrupt objects which have a molten or partly molten, interior, and we know that we have eucrites that apparently have to be molten, or partly molten at least, at that time. If you disrupt such an object you will get most of the things Dr Hutchison has described. On the other hand, even if the solar nebula was not hot over a wide range it was certainly hot closest to the centre where the Sun formed and as astrophysicists are very fond of turbulence so you can get material moving out and in. I think if you look closely enough with a scanning electron microscope it is actually no surprise that you see features of almost any kind of process within a chondrite. So I ask whether we should not expect that both types of processes, solar nebula type processes as well as planetary type processes, went on at the same time?

P. Pellas. I think that we can agree with what Dr Wänke has said because we are absolutely sure that planetary processes are observed in some meteorites, differentiated meteorites, and ages measured in chondrites, agree within a few million years. This is true for meteorites and

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it is true for Angra dos Reis which is a totally differentiated object which has an age of 4.556 Ga plus or minus 2 Ma which indicates that very early the material has differentiated. Now we do not know exactly what has allowed one fraction of material to differentiate and another fraction not to differentiate. Perhaps this is because the ^{26}Al had a different distribution in the primitive nebula where the meteorite formed. Perhaps Professor Clayton can tell us his ideas about the subject. But we can be sure that there existed chondrites which we call primitive objects and chondrites which were highly differentiated within a very short timescale. Now concerning the question by Dr Taylor on the T-Tauri phase I can only say that we are looking for some T-Tauri indication in meteorites and, up to now, we have seen nothing.

S. R. Taylor. There will be an opportunity following Dr Wasson's paper to continue the discussion on chondritic meteorites. My impression from the chondrules is that they seem to me to provide evidence of extremely rapid processes, probably localized, with both melting and cooling on very short timescales, which would argue against a very extensive hot nebula but we may reach some resolution of these problems later in the meeting.

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