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Panel Discussion: Does Chemical Evidence Give Diagnostic Tests for the Credibility of Physical Models of the Origin of the Solar System?

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Panel discussion: does chemical evidence give diagnostic tests for the credibility of physical models of the origin of the Solar System?

M. M. Woolfson. Under the conditions of the capture theory, planets were originally formed in highly eccentric orbits which were close to, but not exactly coplanar. A resisting medium rounded off these orbits but, because it produced a non-central gravitational force on the planets, it also caused their orbits to precess. Differential precession gave intersecting orbits from time to time and it is possible to compute characteristic times for major interactions between pairs of planets. It turns out that these are similar to the rounding-off times and it can be concluded that some major event in the early Solar System was more likely than not. In 1977 Dormand and I postulated a planetary collision in the asteroid-belt region. Such a model readily explains the known characteristics of asteroïds and meteorites, especially as a wide range of thermal régimes was present during the collision event. Other Solar System features that could be explained in a very straightforward way included the terrestrial planets, irregular satellites, e.g. the Moon and Triton, and the origin of cometary material.

As an extension of that model one of my students, Miss Michael, and I are considering the way in which an atmosphere of one of the colliding planets might have evolved. The retention or loss of an atmosphere, and the rate of loss if it escapes, is governed to first order by the ratio v_e^2/v_T^2, where v_e is the escape speed from the planet and v_T the r.m.s. thermal speed of an atmospheric molecule. For example, Titan at a temperature of 130 K can retain an N_2 atmosphere with a ratio of 63, whereas Mercury, at a maximum temperature of 700 K, cannot retain a CO_2 atmosphere with a ratio of 37.

We now consider a planet of metal–silicate–ice composition of mean density 3×10^3 kg m^{-3} and a mass three times that of the Earth. If such a planet had a substantial hydrogen atmosphere and a prevailing surface temperature of 400 K, consistent with early planet conditions, then the ratio v_e^2/v_T^2 would be 42 for H_2 but 63 for HD. This suggests that under the given conditions there would have been a high differential loss of hydrogen to deuterium, in fact with negligible deuterium loss, and the ratio D/H would have increased with time from the expected initial ratio of 2×10^{-5}.

Simple modelling of a planetary collision, using known theory of hypervelocity impact, indicates that in the impact region there would have developed a very high density and a temperature of order 10^6 K. This would have given rise to reactions involving D and H on a very short timescale leading to explosive conditions. It is easily shown that if the average particle mass of the totally ionized material was $\frac{3}{4}m_p$ and, locally, deuterium accounted for a fraction 0.005 by mass then with a release of 5.5 MeV per reaction the local temperature would have risen to about 10^8 K. The kinds of reaction which could have taken place in such an environment include

$$^{26}\mathrm{Mg}(p,n)\,^{26}\mathrm{Al} \quad \text{half-life 700000 years}$$

and

$$^{22}\mathrm{Ne}(p,n)\,^{22}\mathrm{Na} \quad \text{half-life 2.6 years.}$$

Much has already been said about ^{26}Al, for the presence of which in the early Solar System there is incontrovertible evidence. The short half-life of this radionuclide presents problems for

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solar nebula theorists and the idea has been advanced that the white, high-temperature inclusions in Allende were of non-Solar-System origin and were introduced in their present form into the mix of material from which the Solar System was being formed.

So far nothing has been said in this meeting about ^{22}Na, the daughter product of which appears as an enrichment of the ^{22}Ne isotope in some meteorites and, in some samples, as pure ^{22}Ne! With a half-life of 2.6 years ^{22}Na presents a far tighter constraint on theories than does ^{26}Al. To produce the radionuclide and to incorporate it into a cold closed system within a few years implies scales of space, density and cooling times which are completely incompatible with a conventional solar nebula or even production in a supernova event.

Other reactions which would take place under the conditions of the proposed planetary collision include those producing ^{16}O and also destroying ^{17}O and ^{18}O which provides an explanation for observed oxygen isotopic anomalies. It seems likely that many other isotopic anomalies can be explained by this model; indeed, locally and for a short time, conditions would have prevailed similar to those in a supernova so any explanation for production of radionuclides calling on a supernova would equally apply to this model.

Temperatures varying from 10^8 K down to the original planetary temperature in regions remote from the collision, together with varying turbulence in different locations after the collision, would have led to the range of physical and chemical characteristics of meteorites which is observed.

It is interesting to consider the plausibility of this model in the light of the following paraphrased comments made by speakers at this meeting.

(1) Despite qualitative arguments that the interstellar medium must be well mixed we know that the early Solar System contained products recently synthesized and not yet mixed.

(2) Material became differentiated on a short timescale but some was not differentiated.

(3) Chondrules formed early in the Solar System and cooled rapidly.

(4) There must have been rapid heating and cooling on a very short timescale, which argues against a large-scale nebula.

(5) To explain oxygen isotopic anomalies there must have been two reservoirs of oxygen in the early Solar System.

(6) There are required three or four different sources of material to explain the excesses and depletions of some isotopes.

(7) There were spikes of isotopic anomalies injected into the Solar System.

(8) A high-temperature event led to the evaporation of solids. There was metasomatic exchange between solids and vapour.

(9) The best way to produce molten droplets is to have breakup of a molten body.

The planetary collision hypothesis requires special conditions to explain what is observed but it does no violence to any scientific principle and the chain of events leading to the eventual outcome is entirely credible. No other model suggested so far has offered a similarly plausible explanation of observations.

G. J. Wasserburg. I wonder if Professor Woolfson could tell us the timescale of an impact necessary to produce the nuclei, as the timescale for the impact is very short for the high temperatures (10^6–10^8 K) that he talks about.

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M. M. Woolfson. The answer is that some years ago I did some calculations but I think it was with somewhat different conditions than those I am envisaging now and the answers I got at that time were not encouraging in terms of the total production: but I must stress that I think I was calculating with very different conditions.

P. Pellas. I would say that with this scenario you can explain most of the isotopic anomalies that are observed in meteorites, for instance in the iron peak, because you have the energy, the temperatures and the particles. The only things that you cannot produce are actinides, and the problem now is that if, in fact, curium-248 is found in meteorites then you cannot produce it with such a process.

G. Turner. What we are discussing is the link between chemistry and physical models and I suppose what one would like are diagnostic tests to differentiate between physical models. However, what tend to come out, rather than diagnostic tests, are constraints which physical models need to satisfy and one effect of this is that when constraints appear the physical model is simply adjusted to meet those constraints. The nett effect is that one does not actually have a diagnostic test for a lot of the physical models; one simply increases the detail without actually knowing whether the basic assumptions of the model are correct. I wrote down a few things that I thought were the key areas of chemical evidence. The framework within which this has been presented at this meeting is of three major epochs: the presolar epoch, the nebular epoch and the planetary epoch with overlap of the latter two to some extent. Some of the main areas of evidence, are the isotopic uniformity. Although we have heard about isotopic anomalies at least one speaker has stressed that the main observation is isotopic uniformity so that when the Solar System formed there was very efficient fine-scale mixing which either implied material was gaseous or fine dust. Because, by and large, we do not see the anomalies that one might associate with dust then one assumes that that dust was subsequently largely vaporized. Having said that there *are* isotopic anomalies that are preserved to a large extent in refractory phases and these are interpreted as being of presolar origins. There as also some non-refractory phases that preserve these anomalies so there is some evidence for presolar material being incorporated into the nebula. I apologize to Professor Woolfson for phrasing everything in terms of the nebula hypothesis. If I expressed things in terms of his model then I suppose that his model produces a nebula but produces it in a different kind of way rather than from collapse of the Sun. Both these things relate to the presolar period; the isotopic uniformity and the isotopic anomalies gives us information about the presolar period and also about the nebular period when small solids were forming. A third area of evidence that we have heard about involves timescales, which are essentially, expressing it very briefly, the aluminium-26 time-scale. This tells us that nebular processes were occurring on a 1 Ma timescale, the evidence of iodine-129 and initial strontium is telling us that solid objects were forming and being heated on the order of a 10 Ma timescale and then – something we have not heard about at all – there is also evidence of metamorphism of large bodies, the meteorite parent bodies, which was going on within 100 Ma which really tells us something about the size of these bodies that we are dealing with; they are 100 km, i.e. asteroidal-sized objects. So the timescale for the nebular process is basically 1–10 Ma, depending on which processes one is dealing with, but some of the events that occurred in the first million years are frozen into the fragments that we see inside meteorites. A further point relates to chondrules where the main thing, so far as

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physical models of the origin of the Solar System are concerned, is that because the origin of chondrules is not clear then their significance for physical models is not clear. My own preference is that they are produced by collisions between small bodies but because of the uncertainty in their origin I think that there is uncertainty in any information from them. The major observation from chemistry is the importance of volatility in determining the chemistry of these primitive objects and the chemistry is basically telling us that there were high temperatures in the solar nebula, so whatever model one wishes to consider it has to produce in some place appropriate high temperatures. It also has to have chemical differences that account for the oxidation–reduction relations. However, if I can go back to my first point, I think that there is a problem that, at the end of the day, the detail from observations simply adds detail to the particular physical model under consideration rather than saying whether that model is fundamentally true.

H. Wänke. The fractionation of volatile elements is a very important observation and for this we need high temperatures somewhere, but not in the nebula itself, which necessarily is to be hot. Your preference is to make chondrules by collision of small objects an idea for which I have argued because two objects, colliding with molten interiors, would make a lot of droplets. The question really is whether the high temperature for chondrule formation was related to the fractionation of the moderately volatile elements. I think that a more important issue is just the formation of chondrules themselves.

J. T. Wasson. It should be understood that most chondrules were never completely molten. Most of them were incompletely melted so if you started off with a molten body it seems to me very difficult to prevent the formation of chondrules that themselves are completely molten.

S. R. Taylor. Just to comment on the chondrule formation problem. If you disintegrate large bodies then you are likely to produce a wide variety of material – chunks of rock, bits of glass – in addition to the chondrules which from our knowledge of impact processes are produced rather inefficiently. I prefer nebular models because what we see in the meteorites is a super abundance of chondrules and none of the evidence for the other collisional fragments which one would expect from the disintegration of bodies.

R. Hutchison. Something like 20 or 25% of chondrules form as completely molten droplets but of the others many are called clast chondrules; they are in fact fragments of pre-existing rock and some have the major element chemistry at least of planetary differentiates.

G. W. Wetherhill. What I should like to do is to talk a little about the present Solar System. It is generally accepted among meteoriticists, I think correctly, that meteorites are fragments of asteroids and asteroids are real things which are in the Solar System today. If we had the fare we could go there and bring back samples of them but we are actually fortunate in that we have a very detailed array of samples already which have been studied in great detail. One might just ask how to make progress in trying to put together this story, and a very important part of that story is to try and understand asteroids. It is commonly thought that if you put together what you learn from a group of people from different disciplines – astronomers, chemists, physicists – then the combination of the firm constraints of what they really know will

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eliminate all impossible ideas and lead to progress. My experience is that it really turns out to be the opposite, that horrible problems remain, but that this is not all that bad because it makes you think and after a while you finally learn something. Today all meteoriticists know that meteorites come from asteroids and yet 20 years ago, the man who had thought most about this problem, Ernst Öpik, would have said that it was impossible to bring meteorites from the asteroid belt to the earth and therefore ordinary chondrites must come from comets. As a result of that challenge it is now possible to understand something about resonance mechanisms by which meteorites can be brought from asteroids into Earth-crossing orbits. There is another problem of similar magnitude and that is the composition of the asteroids. The best spectra from photometric data tell that there are not any ordinary chondrites out there. I firmly believe that a large fraction, probably the majority, of the asteroids are ordinary chondrites. Nevertheless, I cannot dispel the detailed data that Gaffey and other people have assembled, which argue very strongly that my favourite candidate regions of the asteroid belt, from the dynamic point of view, are not sources of ordinary chondrites. They will also tell me that the most difficult place in the Solar System to get anything from, from my point of view – namely the region in which Vesta is found – is the source of the 10 % or 8 % or so of meteorites, namely the basaltic achondrites. So we have many problems in the present Solar System; by confronting these problems some progress may be made because, after all, if meteorites come from asteroids then the planetary geology of asteroids and the subject of meteoritics are just simply two names for the same thing and we see in the meteoritic record the evidence for events that took place during the past few hundred million years. Professor Turner himself has demonstrated this with argon and other people have in other ways. During the formation of the Solar System it is quite likely that the asteroid belt was populated with a much larger density of objects. These would collide with each other and the meteoritic record of all this fragmentation, this brecciation, perhaps involving the formation of chondrules or perhaps not involving the formation of chondrules but rather metamorphism of chondrules, all form part of the story. Thus I think that the asteroids are really quite special and although we would like to understand the origins of Jupiter, the Moon and the Earth, we have a unique opportunity here in that we have planetary bodies that are not only observable but we also have a detailed record of them in our collections today.

G. J. Wasserburg. I would like to make a few comments with regard to Professor Turner's talk. The problem is that we have to draw distinctions between the pre-solar state, the early stages of the Solar System and post-planetary-formation processes. From all the meteoritical observations and observations of the planets we may conclude that, chemically, heterogeneity is the law of the Solar System so that, manifestly, there are processes of chemical differentiation and fractionation. Further, there is clear evidence that everything could not have been hot and everything could not have been cold and that most things were mixtures of material, some of which was hot and some of which was cold. Discussions which attribute things to purely hot processes, or purely cold processes, absolutely confound the issue because it makes it impossible to have a rational discussion. Identification of nuclear processes is very important but the site of these processes is in no way ascertained; as Professor Woolfson pointed out, many of us are still frustrated by the neon-22 problem. It is perfectly clear that, from the nucleosynthetic point of view, there are no clear *a priori* calculations that produce any specific cluster of nuclei which we see, and everything is made by rather *ad hoc* blends. Although I concur with Professor

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McCrea about the importance of chemistry in these issues, I think that the problems we are facing are those of chemistry mixed with physics. The issue that we have to face is the conversion from a thousand atoms per cubic centimetre in the nebular state to around 10^{14} atoms per cubic centimetre, which is a pretty high compression ratio, from a molecular cloud to the denser matter of the Solar System before condensation. The questions which need to be resolved concern the materials that ought to be in the interstellar medium and which will be preserved in that high compression and elevation of temperature. We should not attribute chemistry which we now see to pre-solar processes unless we have some reason to believe that they will be preserved during the compression stage. Then we should consider what other processes might take place inside the solar nebula in the approach to equilibrium, taking cognizance of the fact that the Sun might have been very active; and there is no reason to believe that the Sun did not go through an X-ray phase, if not T-Tauri, from which there is reasonable expectation that there might even have been some local nuclear activity. Last of all, we should note that the planetary processes which have already been discussed. In the presentations of this meeting we have heard, for the most part, about the end results and morphological classification of what we see. The physical and chemical processes and actual state of the medium have not really been well described. It seems to me that the issues that I have mentioned are those that will determine whether we will make progress in the future.

R. J. Tayler. I should just like to stress that there are still very considerable uncertainties in pre-main-sequence stellar evolution and about whether or not the Sun had a T-Tauri phase but it must be recognized that before the Sun reached the main sequence it may have been much more luminous and much more active than it is today and the formation of the meteorites and the suggested 10 Ma formation period for chondrules must be considered in relation to that.

Sir William McCrea. Was there a phase when the Sun was less luminous?

R. J. Tayler. When it first reached the main sequence it would have been less luminous than it is now; not by a very large factor, but it could well have been much more luminous in the pre-main sequence phase. But as I say, ideas of star formation, pre-main sequence stellar evolution and whether all stars behave in the same way are still very very uncertain so I think all we can say is 'might'. I do not think we can say 'must'.

G. H. A. Cole. Is there any way of distinguishing between stars that have gone through one of these early special stages and those that have not. Do they end up more luminous or do they end up in some way different? Is there any way of even guessing?

R. J. Tayler. Unfortunately this is very difficult. The gross properties of a star when it reaches the main sequence are independent of its previous history. That is why it has been possible to make progress in understanding stellar evolution. Some of its secondary properties relating to rotation and magnetic fields might be dependent on the T-Tauri phase but we do not know enough at present to be able to make the required deduction.

G. W. Wetherhill. What about rotational spin-down as evidence?

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R. J. TAYLER. Again, I do not think that one could say by the rate at which it is rotating today that once it was more rapid or not. That is still another too-subtle untangling of history.

G. J. WASSERBURG. I would like to just pursue that. It is satisfying when, for example we, find ^{26}Al and show something very clear; when you find ^{107}Pd that is also satisfying but the nuclear connection is absolutely obtuse. If results on ^{53}Mn are correct then the problem is that the way you produce ^{26}Al does not produce the ^{53}Mn and so you begin to end up with an *ad hoc* series of results. What you really know is that something happened and it was nuclear. The question is where, and some of the mechanisms may well involve simple local events with chemically differentiated materials in the early Solar System. We have to be careful not to invoke the full scenario of mysticism in explaining the results.

D. CLAYTON. It is possible that there may have been a disc in the Solar System before the Sun became very luminous and we should not always think of having the Sun in place. There is a great deal still to be understood about the hydrodynamics of collapse and the growth of the central body that is presently our Sun and it could well be that, if there was a T-Tauri phase in the Sun, as I think there was, then it might have shone on planets and meteorites and other objects that were already there. Although I have been accused of having countless *ad hoc* explanations of everything, I would like to say that my intent has always been quite different. There is room in science for trying to get away from explaining everything as it is discovered and rather to try to build a self-consistent picture of the Universe, as best one can picture it, from beginning to end. This is a valid way of doing research; trying to build a model of, say, our Galaxy from its beginning and modelling its interstellar medium and the kind of memories that that medium carries and matching it on to how stars are formed and see what it predicts is also a useful way to proceed.

R. J. TAYLER. You could have things happening to the planets and the smaller bodies before the T-Tauri phase but if it is true that the ages of the chondrules vary by 10 Ma then you could not have all that happening before the T-Tauri stage. If meteoriticists say that interesting things were happening to the components of the meteorites over a period of 10 Ma then that is either after the T-Tauri phase or it bridges it but it cannot all be before.

S. R. TAYLOR. My comments will deal principally with the evidence that meteorites provide for conditions in the early solar nebula, and their relation to the compositions of the inner planets, as discussed by several of the speakers at this meeting. There is a general consensus that the composition of the original nebula is given by that of the C1 class of carbonaceous chondrites. The rationale for this is the match for the non-gaseous elements between the composition of the C1 meteorites and that of the solar photosphere, although it is recognized that these meteorites have undergone some secondary processing, including hydration. Nevertheless, as discussed by Dr Kurat, many meteorites are indeed very primitive and do not seem to have undergone extensive processing in parent bodies.

However, elemental fractionation has affected the inner portions of the solar nebula, for the terrestrial planets are not of C1 composition. They seem to have formed from material which was already depleted in elements volatile at about 1000 K. The evidence is shown clearly by the relations between volatile elements such as potassium and refractory elements such as uranium or lanthanum.

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What caused the volatile depletion, which presumably extended out to a 'snow line' at about 4–5 AU? This depletion, which must have occurred close to T_0 (4.56 Ga), was due probably to the following two causes.

(1) Early intense solar activity as the Sun settled on to the main sequence.

(2) Heating during dust infall into the median plane of the nebula, possibly responsible for chondrule formation and separation of siderophile elements.

A significant point which emerged from the discussion session on the terrestrial planets is that their compositions are all distinct. This is shown by oxygen isotopes, K/U ratios, and densities, among other parameters. Thus there does not appear to be much evidence for lateral mixing during planetary accretion and it seems likely that the planets were assembled from rather narrow feeding zones, to account for the variations in oxygen isotopes, chemistry and density. In this context, it is interesting that the asteroid belt is zoned. Thus the zonal structure of the asteroid belt might be a primary feature of the solar nebula. At the least, it indicates that lateral mixing of planetesimals, which formed very early, and from which the planets most likely were accreted, was limited.

There was considerable discussion about the temperature reached in the nebula. There appears to be a consensus that the nebula was cool, not hot, with astrophysical models predicting temperatures of a few hundred kelvin at the present location of the asteroid belt. Among other evidence, the carbonaceous chondrites, which match the S type asteroids, contain hydrated silicates, which would decompose to anhydrous silicate plus H_2O at above 700 K. Water ice which is stable beyond 5 AU condenses at 160 K at nebula pressures (10^2 Pa). The oxygen isotope evidence and the general differences among the planets also suggest low nebula temperatures. This apparently conflicts with the petrographic evidence from meteorites for high temperatures in the asteroid belt, as noted particularly by Professor Wasson.

Is there a resolution to this paradox? The high-temperature mineralogy (which is not ubiquitous: most meteorites are complex mixtures of low and high temperature phases) probably results from transient high-temperature processes. In this context, the best evidence comes from the chondrules, which record heating, element fractionation and cooling on very short timescales (minutes to hours). Thus transient localized high-temperature events seem necessary to explain the meteoritic evidence. Nebula-wide heating is not consistent, for example, with the rapid cooling (quenching) observed in chondrules.

Another interesting question is whether planets are assembled from a hierachy of planetesimals, or from fine dust. Several observations point toward the former alternative.

(1) The evidence from planetary surfaces of innumerable impacts of large bodies, a prime example being Mare Orientale.

(2) The obliquities of the planets are consistent with collisions of large bodies (e.g. Uranus).

(3) Accretion from a dusty nebula might be expected to result in uniform planetary compositions.

(4) Individual meteorites are mixtures of several components and do not consist of single components such as mineral grains, chondrules, metal grains.

(5) The retrograde motion of Venus and the absence of a venusian moon, are both consistent with impact of large bodies.

(6) The anomalous compositions of Mercury and the Moon are most reasonably explained by the effects of large collisions. Thus a large collision is more likely to be responsible for the

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high density of Mercury than the alternative of evaporation in a high-temperature nebula, while the most reasonable hypothesis for lunar origin is the collision of a Mars-sized body with the Earth, with the Moon being derived from the silicate mantle of the impactor.

In discussing accretion theories for the origin of the Earth, the current view is that planets accrete from a hierarchy of planetesimals. Such bodies of course may be differentiated into cores and mantles, so that much diversity of composition among the accreting bodies can be expected. Accordingly, I find some difficulty in identifying the two components in Wänke's model of planetary formation, particularly because they would come from separate parts of the nebula, one oxidized and the other highly reduced. The common identification of the oxidized component with C1 chondrites (from the asteroid belt?) on the one hand, and the reduced component with the enstatite chondrites (originally from within 1 AU?) on the other, does not accord with the oxygen isotope evidence. If the inner planets all accrete from only two such components, one might also expect more uniformity, for example in oxygen isotopes. The overall message seems to be one of diversity, not of uniformity in the Solar System. Attempts to fit Mercury and the Moon into a grand design do not work, because these bodies result from stochastic processes. The oxygen isotopes tell us that there is little evidence of lateral mixing, so that the zonation in the asteroid belt may be an analogue for the whole nebula.

Even the giant planets are notably different among themselves. Whereas Jupiter and Saturn may be predominantly composed of H and He, Uranus and Neptune are composed mainly of ices (H_2O, NH_3 and CH_4). The regular satellites of the giant planets form most probably from subnebulae spun out or knocked out by collisions, while the irregular satellites are probably captured.

In summary, the evidence seems mostly against simple condensation models related to heliocentric distance for the formation of the inner planets, and in favour of accretion in narrow feeding zones from differentiated planetesimals in a system in which stochastic processes play a leading role.

A final comment on the paper by Dr Chang on planetary environments and conditions for life concerns the nature of crusts of the inner planets. These seem to be of three types: *primary*, formed during accretional melting (e.g. lunar highland crust, and possibly the mercurian crust); *secondary*, formed much later by partial melting of planetary mantles (e.g. lunar maria, terrestrial ocean floor basalts, and probably also the martian and venusian crusts); and *tertiary*, produced by reprocessing of secondary basaltic crust. The only example of this latter type appears to be the terrestrial continental crust. This emphasises the unique nature of the terrestrial environment for the origin and evolution of life.

Sir William McCrea. Where will Venus come in terms of K/U ratio?

S. R. Taylor. We have four or five observations of K/U ratios in the Russian data for Venus. They indicate about 5000 or 6000 for the ratio. For volatiles there is a sequence; Mars seems to be more volatile rich, the Earth less so and Venus somewhat less than that.

G. W. Wetherhill. I think one needs to be very cautious about using volatile elements to argue for heterogeneities in the solar nebula or heterogeneities of accretion. Volatiles are just such poor preservers of records and one can conceive of so many ways to lose them both in planetary situations as well as in nebula situations that I think one should not spend too much time

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discussing these as indicators. One example is that concentration of indium in L group chondrites varies from chondrite to chondrite over a range of a factor of a thousand; those that are the most metamorphosed have very low indium content, those that are still relatively unaltered have very high indium content. Almost certainly that is not some record of interstellar accretion that is being preserved there.

S. R. TAYLOR. I feel that there is rather widespread evidence even for the moderately volatile elements, such as potassium, being depleted and it seems to me that we are looking at a very widespread depletion of the volatiles. I think we have a differing point of view in this. I know that Professor Wetherhill is interested in refractory elements, and I think I am more impressed by the volatile-element evidence.

G. H. A. COLE. In listening to the fine detail one must not forget that there are broader pieces of information that would be very useful and again I would like to mention one or two. For example, it would be enormously valuable to know whether the icy satellites really are differentiated or not, particularly the small ones. There is general agreement that the larger ones are differentiated; one would like to know about smaller ones, for example Mimas and one sees that Miranda has had a very peculiar history and does show signs of having at some point been differentiated. Once a space vehicle is available then it might not be too difficult to get at least a tentative 'yes' or 'no' as whether it is completely or partly differentiated. Another interesting thing would be to know about the water content in martian crust. One would also like to know something about the crust of Venus, whether there is or has been a plate tectonic system or anything that would tell us something about the dynamical structure in the past. Again a planet that is really quite interesting, and of which we know really very little, is Mercury. So really I am stressing the need for fairly elementary pieces of information although they may be difficult to acquire.

K. BURKE. The topography of Venus, and the impact density on its surface, suggests that it is not an old surface and that there has been dynamic activity within the past few hundred million years. Some people have even claimed to have seen evidence for plate tectonics.

S. R. TAYLOR. Concerning the surface composition of Venus, although there are rather large high-standing areas there is no real evidence of anything on the surface except basaltic rocks and the early evidence of high potassium–uranium–thorium concentrations, which has certainly been substantiated, is due to the presence of alkali basaltic rocks, which have 3 or 4% potassium, not granites which have about the same concentrations. Of the five sites from which we have data, all the evidence is of basaltic rock, although the sites are scattered. Insofar as we can extrapolate from all this, it looks as though the surface of Venus is basaltic and hence Dr Burke had the problem of supporting these high standing areas by some kind of dynamic tectonic processes. You cannot do it by appealing to their low density.

S. K. RUNCORN, F.R.S. (*School of Physics, University of Newcastle upon Tyne, U.K.*). It is very interesting to think about the difference between the chemical evidence which has, of course, been the main topic of this discussion meeting, and the physical evidence. Obviously the chemical and the isotope evidence enables one to see very far back into the Solar System but

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as we know some of the interpretations are not uncontroversial. Regarding the physical evidence, I have always been attracted, as I suppose most in this room have been, to the solar nebula hypothesis because we know that there are dust and gas clouds in space of dimensions hundreds of thousands of astronomical units. We can see very clearly the dynamics of the contraction process and their flattening by conservation of angular momentum. Recently, in the IRAS data, we have seen that these dust discs do exist around certain stars and it is very natural to begin constructing models of the Solar System from these facts. When one looks at the mechanics of such a dust cloud it is seen that there is going to be quite a lot of heating by collisions but the existence of gas gives one the hope of explaining the chondrules; I must say that although there are many suggestions made I do think that their shape suggests that their final formation occurred in a gas. When one looks at the meteorite evidence one sees that most of the meteorites must have formed very early in the Solar System and one must, I think, ask again the question, which has hardly surfaced in this meeting, as to the nature of the heat sources which melted the parent bodies of meteorites. We see in the evidence of the meteorites, we see in the asteroid belt evidence of early heating and the source is still a matter of great uncertainty. In the last stages of the formation of the Solar System as we know it today, there is evidence of the ubiquitous bombardment by small bodies and Dr Wetherhill, in his computer calculations has shown how the Solar System could be reduced from some hundreds of bodies to the present number, particularly of course in the terrestrial planet belt. But it is important for us to look for evidence of what the Solar System was like a little before the final events of which we talk so much, particularly about the formation of the Moon. I have argued in recent years that the palaeomagnetic studies of the lunar surface strata show that the Moon has undergone many re-orientations in its early history evidently brought about by the great impacts that have produced the large multi-ring basins and changed the moment of inertia tensor. Moreover I have shown from the palaeomagnetic data that the palaeo-equators of at least three different epochs, 4.2, 4 and 3.8 million years, lie close to the multi-ring basins which have been dated by people like Don Wilhelms: the equators and the multi-ring basins of corresponding age are close together and I have concluded that the Moon had a satellite system, the satellite orbits decaying and hitting the Moon and contributing very greatly to the regolith. There are many geochemists and cosmochemists such as Professor Wänke and Professor Englehart who think that further studies of the regolith of the Moon may give one more clues about the composition of these small bodies which were evidently in the early Earth–Moon system. In addition one ought to be looking for evidence that gives insight into the Solar System just before the present time.

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