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

General Discussion after Session IV

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G. Hunt (*PACTEL, London, U.K.*). It is an incredibly important problem to try to understand the Solar System; where we are now, where it has come from. We are looking today at things that happened in the past; the Sun has changed during its lifetime and that upsets some of the chemistry that we are looking at. Professor Gautier's presentation does raise a number of very important questions of interpretation. The error bars on some of his critical ratios are very large. How can we reduce those error bars? Can this be done as a result of doing remote measurements or must we make *in situ* observations? Are there more things that we can be doing in the laboratory to improve our spectroscopy, for example? Theories develop more rapidly than observations, that is obviously one of the problems that we are always facing. Something that has been given some attention is the question of the colour of some of the objects we have been looking at. Colour was not mentioned this morning; is it something we should be taking into account. When we make these observations, from *Voyager* particularly, we are looking right at the very top of the atmosphere, we are looking at the dirt on the skin of the orange type of scenario, yet we are talking about what is happening all the way through. Just how well do we understand those interiors? The weather systems that we think we understand can be explained either by a deep model or a very shallow model. Is that important? Does it affect the way we interpret these results? These are some of the things that are running through the minds of people as we discuss these factors today, coupled with the fact that when we move away from talking about hydrogen and helium and get involved with other components of the Solar System, things like oxygen, then we really are in difficulties because they have their own chemistry at some depth, and affect the dynamics and the chemistry at these particular levels. Let us just ask ourselves whether are we asking the basic questions, the real questions; have we really set up the ways in which these things can be answered in the next ten years.

D. Clayton. I would like to ask about the possibility of deuterium enrichment in Jupiter even with homogeneous accretion. Can it been discounted if one takes into account a much more luminous and much more active early Jupiter before it settled down, with fractionation in a wind leaving Jupiter, so enriching deuterium in its surface?

D. Gautier. According to evolutionary models of Jupiter that is not possible because it requires Jupiter to be a much larger size than it is.

G. Wetherhill. I understand by homogeneous accretion Professor Gautier means that Jupiter is formed without prior formation of a core, along the lines that Cameron used to speak of as a giant gaseous proto-planet? Stevenson has argued against this, on the grounds that Jupiter apparently does have a rocky core and that if it were formed homogeneously that this core would be dissolved into the rest of the planet. Does Professor Gautier agree with that?

D. Gautier. I am sorry to have been so unclear. I believe that I said that the nucleation scenario is much more plausible for many reasons; all our requirements seem to impell us towards the nucleation scenario. The only problem is that it takes a long time to form Jupiter in this way.

G. Wetherhill. I think that the reasons given for the long timescale for the formation of Jupiter by nucleation are by no means necessarily valid. Fundamentally they are simply that the orbital period falls off as $a^{\frac{3}{2}}$ and also together with the assumption that the original nebula density fell off something like $a^{\frac{3}{2}}$ gives a factor of a^3, semimajor-axis cubed, timescales. So if the Earth took 10 Ma to form then Jupiter took about 10 Ga to form; you can play with the numbers. There may be other factors which are just as important which can offset this, for example, whether the growth occurred by runaway or whether a growth occurred in the absence of runaway. The two classical approaches to this problem, by Safranov and Hayashi, ignored factors which promote runaway. For example, Safronov's work ignored gas drag and Hayashi's work ignored collisional dissipation of energy. Both these are very important in lowering the relative velocities for planetesimals. Even more important they *both* failed to include in their calculations equipartition of energy, whereby large bodies have very low velocities and smaller bodies are pumped up to high velocities. This factor is very important in promoting a runaway; it turns out that in the formation of the planetary embryo by runaway, a greater heliocentric distance favours the formation of large objects by a factor proportional to a^3 rather than inversely proportional a^3. This is as a consequence of the fact that a body can accumulate planetesimals out to something like three or four of its helisphere radii; the distance to the nearest Lagrange point. The traditionally assumed long timescales for formation of the cores of the giant planets are more an historical accident of the way in which problems were initially treated too simply before more complex treatments were applied.

M. K. Wallis. We know that photolytic and ionic reactions are dominant in effecting escape to space of the Mars and Venus atmospheres at present, and may have been so through most of the planets' histories. Such chemical processes produce highly suprathermal atoms around 1–3 eV generally more numerous than any thermal 'maxwellian' tail.

For oxygen escape (McElroy 1972), the dissociative recombination of O_2^+ produces atoms with 3.5 eV kinetic energy if in the ground state, 2.5 eV if one atom is in the ^1D metastable state, or a litle more if the ion is vibrationally excited. On Mars, 2.5 eV atoms escape directly if upward moving and avoiding collisions (originating at more than about 210 km altitude). On Venus, I predicted they populate an atomic O corona penetrated by the solar wind, which sweeps away most of the O^+ created subsequently via photoionization and proton charge exchange (Wallis 1978, 1982) as confirmed by the *Pioneer Venus Orbiter* spacecraft.

For effecting hydrogen escape from Venus, the energetic O colliding elastically with atomic H in the current exosphere (more than about 170 km altitude) is important, while energetic H^+ colliding with charge exchange was probably dominant in the past (Kumar *et al.* 1979). Both these processes operate less efficiently for deuterium atoms, so allowing us to explain the observed 100-fold enrichment of deuterium in the Venus atmosphere (Wallis 1984). Indeed, the loss of venusian oceans comparable to terrestrial, via such chemical escape processes, is compatible with the deuterium enrichment.

References

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G. H. A. Cole. The deuterium enrichment data for Venus are highly interesting in supporting an outgassing of materials, including water, common to all the terrestrial planets. However, it does require a 'suprathermal Maxwell tail' of the form Dr Wallis quotes as a continuing feature. If the deuterium enrichment is interpreted this way, the proportions of the principal initial components for Venus could well have been as listed in table 4 of the text. The material outgassed by Mercury has, of course, all been lost by now. A reliable assessment of the amount of frozen crustal water on Mars incidentally becomes even more urgently needed. The link between the compositions of the early atmospheres of the terrestrial planets and of the mantles of the icy satellites would seem to become stronger. If only one satellite can be investigated, a knowledge of the composition and structure of Mimas is a central issue in exploring such a link.

Sir Bernard Lovell, F.R.S. (*Jodrell Bank, Macclesfield, U.K.*). May I ask Professor Cole if he can account for the relatively small concentration of the inert gases in the atmospheres of the terrestrial planets compared with that which must have existed in the solar nebula? For example, argon and neon should be retained in the terrestrial atmosphere for an immense time (10^{200} years) but the proportions of these gases in the atmosphere are far below those to be expected from their relative cosmic abundance compared with other heavy elements on Earth.

G. H. A. Cole. Yes, this is a very important question. The anomoly that Sir Bernard quotes is consistent with the terrestrial planets (though possibly not the major planets which are fundamentally different) not condensing directly from a solar nebula. The inert gases, in common with other atmosphere components, would have outgassed from the condensed planet. If this were so, the amount present will depend on the efficiency of the outgassing process in any particular case. This must depend on the history of the planet and such things as the details of the internal convection and how long it operated. I believe a detailed inventory of the inert gas components of atmospheres will have a lot to tell us about planetary formation in the future, when we have learned to interpret it.

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