My Ref.ContAppE.sam

Appendix E.

A scenario for formation of the solar system

Problems to be faced:
1. The 5° 52' inclination of the solar equator relative to the plane of the solar system (the invariable plane). [The somewhat bigger inclination (about 7°) to the ecliptic, the plane of the Earth's orbit, is more often cited, but is not the dynamically significant quantity in solar system terms.]
2. Most of the mass is in the Sun but most of the angular momentum is in the planets: so the mean specific angular momentum of planetary material is $>10^5$ times that of solar material. This inequality of angular momentum partition has expression in the rather long (26 day) solar rotation period and the very large orbital radii of the Great Planets, Pluto and the six recently-discovered bodies beyond Pluto; and indeed in the Oort Cloud of comets and planetesimals containing upwards of 20 Earth masses at upwards of 20,000 AU from the Sun.
3. Chondritic meteorites are found to incorporate the decay-in-situ products of short-life isotopes e.g. ^{26}Al ($\lambda_{½} < 0.72$ Ma). The isotopes were incorporated into meteorites very close to 4.57Ga. If the planets and meteorites were formed at the same time from the same source extremely rapid planetary growth would seem to be indicated. How was this achieved?
4. What changed the protoplanet accretionary regime to one of non-accretion responsible for the survival of large numbers of satellites around the major planets?
5. Evidence that the Earth's mantle preserves elemental abundance ratios consistent with a chondritic meteorite origin but that some chondrites preserve isotopic ratios representative of red giant stellar evolution, a stage not yet reached by the Sun.
6. Obliquities of planet direct rotation axes that range between 3° and 118° with respect to their orbit planes (except for Sun-controlled Mercury and Venus).
7. Planetary rotation periods, from Earth to Neptune inclusive, vary only between 10 and 25 hours, much slower than would be expected by shrinkage from a solar system nebula, yet only Earth has a satellite system massive enough to have absorbed, by tidal action, a significant amount of the planet's rotational energy. (Pluto, too, has such a satellite and rotates even slower - 153 hours.)

Several of these (1,2 & 5) favour a scenario in which planetary and chondritic matter was derived from another source, not the Sun, a point particularly stressed by Woolfson. That would mean that the age of the Sun may be substantially greater than that (~4.57 Ga) of the solar system. The latter would then no longer be a datum point for the evolutionary timescale for stars of solar mass.

Constraint No.3 apparently denies the possibility, favoured originally by Jeans, that subsequent shrinkage of the Sun could explain the large orbital radii of the planets. It would have taken much too long.

The proposed scenario is that by about 4.65Ga ago the Sun was already in a fairly condensed state, not in the nebular state customarily envisioned in hypotheses of solar system origin. The Sun then entered a cloud of young nova or supernova explosion products. These products had an inherent angular momentum vector whose direction differed considerably from that of the Sun at that time. This material was attracted by the Sun to produce a gaseous, ionized and predominantly axial inflow. [The reason for this expectation is that the several mechanisms (explained later) for producing a quasi-equatorial outward-spiralling of material would negate the inflow from that direction.] This inflow would have a much enhanced angular velocity about an axis whose direction represented a compromise, due to interaction with the solar rotation. That interaction was probably primarily magnetic, between a field already possessed by the Sun and longitudinally flowing ions in the gas cloud, concentrated by its contraction; however, additional interaction between a more intense solar wind of the time and a contractionally intensified magnetic field brought in by the gas cloud could also have been important. Because of that interaction the present direction of the Sun's rotation axis cannot be the original one either. The orbit of Mercury preserves an inclination to that of the rest of the planetary system that is closely similar to that of the Sun's equator, which suggests (as pointed out by Gold) a closer relationship to the Sun, in terms of origin, than for the rest of the planets. Perhaps Mercury condensed from solar material drawn outward by the interaction and may therefore be the only member of the planetary system made wholly from solar materials (and hence, in part, its higher Fe content than the other terrestrial planets), though the others may contain some (see later).

Details of the interaction will need to be worked out but the Sun's low present angular velocity (for a star condensed from a typical interstellar cloud) requires that either:-

(a) the Sun had already undergone a spin-down process, not currently understood but inferred from the slow rotation of numerous other stars of similar spectral/evolutionary class, in which case the interaction would involve <u>inward</u> transfer of angular momentum from the faster-rotating envelope; or

(b) the interaction involved the <u>outward</u> transfer of solar angular momentum, simultaneously achieving the solar spin-down (or an addition thereto) and by adding to that of the envelope, increasing the envelope's tendency to evolve into a disc (see later).

A requirement for the inward transfer of angular momentum from the cloud material to the Sun would imply the former should have at least as much specific angular momentum as the planets. This is most unlikely, so I shall only pursue (b).

In principle, magnetic coupling at low latitudes between <u>either</u> a radially directed solar magnetic field (e.g. sunspots) and an equatorially directed plasma flow <u>or</u> between a radially-directed plasma flow and a longitudinally directed magnetic field would transfer angular momentum outward.

In the proposed scenario the plasma flow would initially be equatorially directed and would become radially directed near the equatorial plane of the system, so both kinds of coupling would be expected.

In that the scenario involves all the acquired cloud material passing equator-wards very close to the solar surface these couplings would be much stronger and, in that the outward plasma flow would carry the field loops outward `frozen' into the plasma, so they would be effective to a much greater distance from the Sun than in the case of radially infalling nebular material that has been much studied by Alfven and co-workers.

As well as these favourable features for a.m. transfer, there is the additional benefit that the magnetic coupling only applies to ionized material, so condensations that form will cease to experience that form of a.m. transfer. Thus, in principle, condensations could grow by access to a continuing resource of ionized material moving outward past them and would <u>not</u> be limited in ultimate planetary size to the amount of material between a particular range of orbit radius, a problem that has long beset models of planetary growth. The fact that such condensations, although free of magnetic outward propulsion, would experience an aerodynamic outward force from the plasma (and entrained neutral particles) flowing past it, means that a.m. transfer by this mechanism may result in final planetary orbit radii very much greater than the radius at which each individual planetary condensation began to form.

Aerodynamic forces of this kind are going to be much more effective (at increasing the orbit radius of a condensation) for small bodies, whose ratio of aerodynamic cross-section to mass is greater than for large ones. And, in any case, such forces would probably be insignificant for our purpose if the density of the solar system nebula were so extremely low as has hitherto been assumed.

That assumption has, to a major extent, been based on the notion that the planets contain most of the original nebular material (except H and He, in the cases of the terrestrial planets) at the relevant orbit radii. As outlined above, the new scenario escapes this restriction but at the price of finding a way of expelling from the system the now much greater amount of material that wasn't 'grabbed' by the growing planets. The wisdom of moving our thinking in this general direction is further supported by two aspects of early Earth evolution.

One is that the ratios of the amounts of the noble gases in the Earth's atmosphere have been found explicable only if the Earth originally had a vast atmosphere of mainly H and He, the rapid loss of which selectively entrained other gases in inverse measure of their molecular weights.

The other is that I have recently shown (MFO, 1992 IGC; IASPEI94) that a major dense atmosphere, mainly of hydrogen but not excluding higher elements, may be a requirement for providing a satisfactory mechanism for forming the Earth's core, and implicitly that (at least) of Venus too. The point here is that during accretion, and particularly in the presence of a blanketing atmosphere and dense nebula, the protoEarth would inevitably have become so hot that overall rapid convective mixing would have occurred. Not only would the overturn possibly have been too rapid for any settling-out of Fe to occur but such mixing would have brought all the meteoritic metallic iron into intimate contact with the oxidising silicates from which the Earth's mantle was eventually made. It has long been recognised, for a similar reason, that a core of metallic iron could probably not be formed by percolative accumulation of iron towards the Earth's centre - the resulting FeO would be too refractory to migrate and is not (on density grounds) what the core is mostly made of. In addition, the immiscibility of silicate and iron melts would introduce capillary blocking effects which would further hamper percolation. This problem has led H.Wänke to favour a two-stage accretionary model, the iron first, then the silicate. The trouble here is that the problem applies to at least two planets, but it is difficult for both (or more) to have formed at the same critical compositional condensation boundary in the solar system nebula, especially as the condensation temperatures for Fe and magnesium silicate are only some 20K apart.

In the above-cited references I have shown that subduction-like processes were in operation very early in the Earth's history. [Subduction, as currently understood, is the upper 600km, or so, of the descending limbs of the Earth's internal convection system.] In the presence of a hot, dense reducing atmosphere the FeO (present in all lavas) in the hot lavas at upwelling centres could have been reduced to pools of metallic iron or FeS. These would be conveyed to the growing core by the subduction/convection system, largely without a need for percolation.

The model clearly favours S, rather than O, as the principal Fe-dilutant in the core, demanded by the density. H_2S would certainly have been a constituent of the dense atmosphere. The solidus-lowering property of S (O raises it) has caused S to be favoured by various workers considering the need to achieve a melting point at the top of the outer core that fell sufficiently below the silicate solidus at the base of the mantle, but S was hitherto rather more difficult to get into the core in the first place.

This model of core formation also supports a proposal by Ringwood, as early as 1960, that the U/Pb systematics of the mantle, which define a Pb-loss date of about 4.56Ga are the result of that Pb being carried into the core at the time of its formation. I speculate that the model might also enable enough U and Th to have been carried into the core to drive the magnetically obvious convection there. Hitherto, the best bet has seemed to be the proposal by Jacobs that ongoing solidification of the inner core provides the energy that drives the flow.

An intriguing possible outcome of this model of core formation is that, because reduced iron is thereby lying about on planetary surfaces, planetesimal impacts will be liable to scatter some of this material back into interplanetary space. Is this the origin of iron meteorites, or at least of some of them? This would get away from the hitherto apparent need to invoke giant impacts to break open bodies in which an iron core had already formed.

One further aspect of the dense-atmosphere model deserves mention here. Many of the more volatile elements, notably including K, Cs and Rb, are markedly depleted in the observable Earth, relative to chondritic values. The very high planetary surface temperature due to this atmosphere, plus the further blanketing effect of increasing H_2O/H_2SO_4 content resulting from the magma reactions, could very possibly result in an outward diffusive loss of these light elements through the atmosphere, and their subsequent loss with that atmosphere.

The much more extreme depletion of K relative to U on the Moon may fit this scenario in view of the Moon's much smaller gravitational field. The evident lack, in Venus, of a mantle convective overturn vigorous enough to disrupt the surface in the way seen on Earth as plate tectonics suggests that Venus may have a much lower radioactive content and therefore less heat to dispose of. Being closer to the Sun, and therefore at a higher temperature during the primordial atmosphere stage, Venus may have lost a much larger part of its K content by volatilisation. Whether this would be a sufficient explanation will need to be worked out.

Retention of such an atmosphere by the Earth (and even more so by Mars if its core was formed in the same way) would only be possible in the presence of a dense pervading nebula in the interplanetary space.

The idea of a dense nebula has another potentially attractive feature, which I am still exploring. It is this. The convergence of isotopic dating on 4.57Ga as "the age of the solar system" may in fact be dating the departure of the dense nebula and sudden cooling of planetary surfaces and of the remaining meteoritic material. In that case the present tight time constraints on how long it took the planets to grow would disappear, with 4.57Ga marking the effective <u>completion</u> of (terrestrial, anyhow) planetary growth, and not its <u>inception</u>.

In that case at least some of the present chondritic meteorite population may represent condensations that formed during the final rapid cooling and attenuating stage of the nebula. Consequently their incorporation of young radionucleides at that time would not necessarily bear directly on what, somewhat earlier, was incorporated into the planets. The presence of the daughter nucleides (e.g. ^{26}Mg) within the Earth does not, I think, prove that the corresponding decay was a substantial player during the later stages of planetary build-up.

Yet another benefit of a dense nebula is that it would enable tidal and atmospheric drag processes to play a part in planetesimal capture during planetary growth. I return to this later.

The departure of the nebular and atmospheric blanket would, of course, terminate core formation, halt the further loss of volatile elements and limit the Fe (and associated element) depletion of the mantle.

The foregoing discussion provides strong motivation for resolving the one outstanding requirement that the new scenario, with a dense solar system nebula, introduces, namely the means of expelling the dense nebula from the system, leaving the planets already formed and at approximately their `final' orbit radii.

This is where standard physics seems to fail and a strong need to appeal to continumn theory arises.

If the throughput of nebular material was large, coming in near the solar axis and moving outward in and near the solar system plane, there is no need to invoke a final qualitative or quantitative change in the expulsion mechanism; mere exhaustion of the incoming material (Sun passes out of the source cloud) would probably do. On the other hand, the mechanism does need to be capable not just of moving material outward but of expelling material and lowering nebular density to a point where other mechanisms, such as infall to the Sun due to the Poynting-Robertson effect, could do the rest.

Although, with a dense nebula, it might be arguable that ionization, and therefore the magnetic coupling necessary for a.m. transfer, could be maintained out to (say) the orbit of Pluto it seems impossible that enough ionization could be maintained as the density and temperature dropped during the terminal phase. Further, although it is obviously too early to attempt the sums on this, it seems quite probable that the optical opacity of the dense nebula would so restrict the penetration of solar radiation that ionization might decrease more rapidly, with distance in the solar system plane, than for a tenuous nebula, despite the radial outflow of material.

If I were restricted to standard physics I would have to conclude that the whole scenario fails and that another one must be sought.

Fortunately, continuum theory appears to offer a way out. Under my proposal (Section 2) that gravitation is a communicated, stimulated response of one body to the presence of another, the velocity of communication being c (or a simple function of c), there results a phenomenon which I have called the Orbital Stability Criterion (OSC) (see Section 4). The OSC states (in simplified form) that the orbit of a planet or protoplanet immersed in a substantially co-orbiting nebula will not be stable in radius but will spiral outwards continuously due to alteration of the gravity vector: by the same token, the nebula material spirals outwards too.

Conversely, when the density of the interplanetary medium is low and its motion is influenced by forces (e.g. solar wind) that are stronger than the gravitational ones, the planet's orbit radius may become essentially stable.

Thus continuum theory appears to provide, qualitatively at least, exactly what the new scenario requires; it provides a primary mechanism for the generation of an outward-spiralling disc of nebular material, assisted by any magnetic propulsion of its ionized components; it provides a mechanism for the attainment of large planetary orbits; and it provides for the final dispersal of an orginally dense nebular disc.

I shall now show that the resulting overall regime, in which the cloud material falling continuously towards the solar poles was then diverted to form a dense disc of outward-spiralling material, provides a new basis for tackling another of our problems (No.3 in those listed above), namely "how did the planets grow?"

The problem has had two aspects. Firstly, how to achieve an adequate capture rate, of one small condensation by another, if all capture was by collision. Secondly, how to disperse from the Solar System the rest of the much greater total mass (solar composition assumed) from which the terrestrial planets and the inner cores of the Great Planets represent the tiny high-condensation-temperature fraction.

A mechanism for dispersal has been outlined above but here I divert briefly into the subject of compositions.

The ex-nova/supernova cloud material responsible for genesis of the disc possibly included far less hydrogen than "typical solar composition". On the other hand it is very likely that the interaction between the Sun and the infallen cloud material would intermix solar hydrogen (etc.) with the infallen material, particularly in the quasi-equatorial belt from which disc growth was occurring. Consequently the mass to be dispersed from the system is now uncertain on compositional grounds, in addition to the uncertainties introduced by invoking a much higher nebula density and by the probable inefficiency of the mopping-up process by the growing planets.

That brings me back to the first aspect mentioned above - the provision of an adequate protoplanetary growth rate. The new scenario appears to relax the most rapid growth rate requirements but as growth times lengthen there is a risk of the new scenario becoming invalid because of inadequate heat input by accretionary impacts. Accretion by collision in a strictly Keplerian environment leads to the eventual result that a protoplanet cleans up all the material in a band of limited radial width depending upon the magnitude of the random radial motions superposed upon the Keplerian motion. In fact, the planets have such a wide radial spacing that it has proved difficult to see how they could have cleaned out the material between them.

In the new scenario, as outlined below, the protoplanets in effect sit on the sidelines while disc material spirals out past them continuously, enabling the protoplanets to accrete those few planetesimals among the population that present themselves favourably as they pass outward. Hence the total mass of the planets may represent only a tiny fraction of the total throughput of compositionally appropriate disc material.

The recently determined rather high density (~2.0) of Pluto has shown that it must have a high proportion of rocky material, much higher than many people had expected so far out in the solar system. Whilst the large eccentricity and inclination of Pluto's orbit have appeared to favour the explanation that Pluto originated much nearer the Sun and that its present orbit it is the result of a sling-shot interaction with another planet, the presence of its relatively large satellite, Charon, of apparently similar density, sets limits on the violence of such an interaction such that the twin bodies were not flung apart in the process, although the fact that Charon is now in a near-polar orbit around Pluto could well be the consequence of that interaction. The scenario proposed here bears on this question in that it does not lead to the expectation of nearly such a strong radial gradient of planetary composition as current models do because it is here accepted that the vast majority of the original nebular material, including its high molecular weight components, passed completely out of the system. Thus the sling-shot argument may be appropriate but the need for it to have caused a substantial increase in Pluto's orbit radius disappears.

This argument is consistent with the view, initiated by Stevenson more than a decade ago, that the Great Planets incorporate massive (in Earth terms) inner cores of terrestrial materials. The proposed scenario then explains their atmospheres as having been retained by the gravitational fields of those terrestrial cores when the rest of the nebula departed. Thus Jupiter, with the largest terrestrial core, has retained the largest proportion of hydrogen - contrary to the compositional gradient idea.

By the same token, similarities between the compositions of Earth and Moon, often used to argue that the Moon was derived from the Earth as the product of a giant impact, lose much of their force. I do not favour a giant impact origin for the Moon; I believe its differences in composition (e.g. its much lower K/U, mentioned earlier) may be more satisfactorily explained if the Moon was formed as a separate small planet with low gravity. Its capture by the Earth might have occurred as an end product of a major solar system event that produced the climactic impact cratering of the Moon at ~3.9Ga (and perhaps the asteroid belt too?). The smallness of continental masses in the Archaean would have produced far less tidal reaction on the Moon than is now the case so a timescale as long as this for the distancing of the Moon may prove acceptable. Further discussion is not appropriate here.

The presence of a dense nebula greatly increases the strength of orbital capture processes in several ways. Presumably the protoplanet and the planetesimals will be orbiting the Sun together with the nebular material. Gas drag, which operates more effectively on the velocities of bodies of small mass (e.g. planetesimals) will tend to reduce velocity differences between protoplanet and planetesimal, thus reducing the energy loss requirement for capture.

As already noted, a dense ambient nebula makes possible a denser atmosphere around a protoplanet, so planetesimal energy loss during closest approach is increased.

Finally, in such conditions, temperatures of the discrete bodies, especially the larger ones (protoplanets), will be increased by blanketing effects inhibiting loss of the heat associated with capture impacts. This may imply a quasi-molten state (data from the Earth's mantle do not tolerate total fusion at and time in the past but permit a mush stiff enough to inhibit the selective settling of mineral species) for protoplanets, giving large tidal energy losses for any capture-induced tides.

The essence of tidal action is as follows. The tidal bulge is effectively a viscosity-controlled response to the pull of the orbiting body. When the angular velocity of the protoplanet rotation (Ω_p) equals the orbital angular velocity (ω_c) of the captured planetesimal (synchronous orbit) the bulge is static on the surface of the protoplanet and the entire gravitational pull on the captive is directly along the line joining their centres. Protoplanet rotation, however, carries the bulge forward or backward from that line, according as $\Omega_p > \omega_c$ or $\Omega_p < \omega_c$. In the former case the resultant pull on the captive has a direction which causes the captive to spiral outward. In the latter case (which also applies to all retrograde values of ω_c) the captive winds inward to coalescence, imparting its angular momentum to the protoplanet.

Initially, let the protoplanet be spinning quite fast. Direct (i.e. prograde) captures are low in probability because both tidal and gas drag effects during the initial pass may be insufficient. Any capture that does occur will spiral outward and may get lost by other encounters. Conversely, retrograde captures will be strongly favoured. They will spiral inward and their coalescence with the protoplanet will reduce Ω_p. This process will continue until Ω_p is so low that it is less than ω_c for typical prograde encounters. Such encounters will then also lead to capture and coalescence.

It follows that the maximum capture->coalescence rate will occur when Ω_p is low, and the process will ensure that it stays that way.

I regard this as the origin of slow planetary rotations (No. 7 on E1). Only the Earth has a prograde satellite massive (Moon = M_E/81.3) enough and now far enough away to have retarded this planet's rotation significantly by tidal effects, uniquely enhanced by the presence of oceans throughout its history. Pluto's Charon (at M_p/13.7) is certainly massive enough but is in a much too tight orbit to have absorbed enough a.m. to explain Pluto's slow rotation, even if its orbit was formerly prograde.

The loss of the dense solar nebula would have major effects. I have already mentioned the possibility that 4.57 Ga dates are recording this event. Also I have argued that the `moment' of loss would terminate metallic core formation in planets (and the related H_20 production at the surface). But there would be an important dynamical consequence too: the rapid cooling of planetary surfaces would quickly reduce body tidal effects and the supply of planetesimals for capture will largely cease. Existing retrograde captures will always, for given tidal and atmospheric drag conditions, wind in to coalescence faster than existing prograde captures will wind outward. So, as tidal influences decay, the prograde captures will remain stranded in prograde orbit. That could explain why the great majority of satellites now present are prograde with respect to their planets. Apart from the four outermost ones of Jupiter and one outermost of Saturn, all of small size and probably too remote for tidal influence, the only significant exceptions are Neptune's Triton, twice the Moon's mass, and Pluto's Charon. Triton's closeness to Neptune, and the large empty region outside it, between it and prograde little Nereid, probably implies that Triton has wound inward since 4.57Ga, either slowly throughout this period or, more likely, more rapidly during the first 1Ga or so, while the outer levels of the planet were still relatively hot and prone to tidal distortion and energy loss. Charon's near-polar slightly retrograde present orbit (inclination 98.8° relative to Pluto's equator) seems likely to be the result of the sling-shot interaction with another planet, as mentioned earlier.

The obliquities of planetary rotation axes have long been a puzzle. To alter the rotation axis of an already-formed planet by a significant amount requires the hypothesis of a very late impact by a body of a least 10% of the mass of the planet. In the proposed new scenario for planetary growth, outlined here, the process of growth is controlled by the rotation of the growing protoplanet. So, from an early stage of growth, the axis that the protoplanet happened to have (and the direction of rotation) tends to be retained. The requirement for relatively large impacts to produce the observed range of obliquities is thus relegated to a very early stage of growth and then is no longer a problem.

Finally, one may enquire why most planet rotation is prograde with respect to the solar system plane. It seems to me that early planetesimal growth by gravitational condensation from a dense nebula will achieve this, whereas growth by impact clearly will not (except as an extreme and correspondingly improbable member of a balanced probability distribution). Thus, here too, the concept of a dense nebula has an important part to play.

In the foregoing I have concentrated on those matters which appear to become more tractable in a scenario involving a high nebular density which departed from the solar system at ~4.57Ga. The need to perform that departure emphasises the need for a dynamic framework that can probably, and perhaps only, be provided by continuum theory.

* * * * *

Revised 21 June 1994. 4894 words. 63KB. MFO.