ICARUS 14, 121–125 (1971)

Fourteenth Triennial Meeting, International Astronomical Union, University of Sussex, United Kingdom, August, 1970

CARL SAGAN

Cornell University

At the latest triennial convocation of the approximately 2000 members of the International Astronomical Union, on the tree-lined campus of the University of Sussex near Brighton, there was considerable activity connected with the moon and planets—invited discourses to the entire Union, business and scientific sessions of individual Commissions (particularly Commission 16 on the Moon and Commission 17 on the Planets), and joint discussions among various Commissions. Only some of the highlights are presented here.

The Union, which by tradition bears responsibility for officially naming lunar and planetary features, accepted the report of a working group on lunar nomenclature chaired by Donald Menzel. A large number of features at the lunar limbs and on the lunar farside were named and, in a marked departure from previous custom, six living American astronauts (the Apollo 8 and 11 crews) and six living Soviet cosmonauts had small lunar craters named after them. A similar committee, chaired by G. deVaucouleurs, was appointed to implement solutions to problems of Martian nomenclature in time for the next IAU meeting. The initiative taken by the IAU to establish an interunion lunar coordinating committee was discussed. The new president of Commission 16 on the planets is Gordon Pettengill, and of Commission 17 on the moon, M. G. J. Minnaert. [Since his election, Prof. Minnaert has died, and a saddened IAU will have to select a successor.]

In a session on the Mariner 6 and 7 results, the television team members suggested the possibility that Mars is in an early state of differentiation and surface evolution, and referred to recent calculations on the evolution of the Martian interior by D. Anderson which, for one model, give a lag period of several times 10^9 years before radioactive heating initiates differentiation of the interior. In describing new results from the Mariner 6 and 7 infrared spectrometers, G. Pimental presented confirmatory evidence of the existence of bound water in Martian surface material. His 2.06 μ spectroaltimetry data are in very good agreement with altitude data from the Colorado ultraviolet spectrometer experiment, except where the slant paths to the surface are very long.

In a joint discussion on radio and radar observations of the planets, the new Lincoln Laboratory radar cartography from 0 to 20° N latitude on Mars was presented by C. Counselman; a correlation between low visual albedo and steep slopes seems to be emerging. New Arecibo and Lincoln Laboratory radar interferometric maps of the Venus surface were presented by Gordon Pettengill. It seems clear that problems of Venus surface nomenclature will also be with us shortly. Papers on the millimeter brightness temperatures of Mars by G. Kislyakov, A. D. Kuzmin, and E. Epstein gave somewhat discordant results. Kuzmin also showed that the high microwave temperature of Uranus is understandable if ammonia is present in the atmosphere, in agreement with an earlier argument by J. B. Pollack and C. Sagan. John Dickel stressed his view that the decline in Venus brightness temperature to very long wavelengths is real and represents a major challenge to the theoreticians. C. H. Mayer presented the negative results of his recent search for a Venus phase effect, in contradiction to the apparently unambiguous such effect found by the Naval Research Laboratory group at 3.15 cm in the beginning of the 1960's.

One of the highlights of the meeting was a joint discussion on the Origin of the Earth and Planets, chaired by F. L. Whipple. The format was three half-hour invited discourses, followed by panel discussions and questions from the floor. Panel 1, on the origin of the solar nebula, opened with the comment that H. Alfven and G. Arrhenius (who unfortunately were not present) had recently proposed that there never was such a thing as a solar nebula. The introductory discussion by Fred Hoyle closely followed his presentation on the origin of the solar system in the *Quarterly Journal of the Royal Astronomical Society.* Hoyle adds material to make up solar composition to all of the planets, deriving a nebula with a mass 1% that of the Sun. Since the angular velocity of the Sun, had it contracted from the solar nebula conserving angular momentum, would be much larger than it is today, a transfer of angular momentum from the Sun to the solar nebula by magnetic braking is proposed, thus accounting for the present distribution of angular momentum in the solar system. Hoyle suggested that when iron condensed out of the solar nebula, it was "sticky" so that an iron sphere is formed first and then surmounted by silicates—a departure from the usual view of core formation in terrestrial planets. These calculations assume that the contracting Sun was at a temperature close to 3500°K all the way from 50 solar radii to the present radius. These high temperatures play an important role in the theory and are possible only because of the use of convective models in the Hayashi early evolutionary phase of the Sun.

In considering the conditions preceding the formation of the Sun, Hoyle stressed Wasserburg's result that plutonium-244 must be made within 10^8 years of the formation of the planets and *in situ.* Hoyle proposed two possible sources for the required fast neutrons: (1) a near-by supernova explosion occurring so close in time to the origin of the solar system by chance, or perhaps tied to it by some causal chain; and (2) some large-scale event, perhaps an explosion at the galactic center, which triggered star formation throughout the galaxy.

In discussion, E. Schatzman objected that there was no possibility of angular momentum exchange in the primitive nebula, on the basis of his estimates of the turbulent diffusion coefficients. He believes that initial condensation of the solar nebula proceeded very swiftly. Hoyle agreed that the condensation rate was fast but argued that it was not fast compared with the free fall time to the Sun from the vicinity of the Earth.

George Herbig stressed that if conditions in the Galaxy now are not very different from those 5×10^9 years ago, there should be contemporary solar system formation events elsewhere, and suggested that T Tauri stars today exhibit radial dissipation of solar nebulae. The depletion of calcium, magnesium, and aluminum in the interstellar medium may be due to their burial in grains. The condensation of such grians, he suggested, is connected with star formation.

T. Gold raised the question of the evolution of the solar nebula from the initial gas and dust to the present planets and satellites. There are no planetary collisions today, but this may be, he proposed, because of a kind of natural selection of orbits, eliminating those orbits which are on collision course. He broached the possibility that materials may be formed on objects other than the ones on which they are now found—objects which no longer exist independently in the solar system. Gold suggested that nonconservative forces such as gas drag in the early solar nebula led to orbital commensurabilities, some of which might still be apparent today.

A. G. W. Cameron discribed Hoyle's approach as arguing backward from the present solar system, and contrasted it with his own, in which he argues forward in time from the interstellar medium. He believes the collapse of an interstellar cloud is triggered by pressure fluctuations due to a nearby supernova event or to the turning on of nearby O and B stars, and that magnetic fields play no major dynamical role. In Cameron's view the initial solar nebula has a mass comparable to the present mass of the Sun.

V. S. Safronov calculates that bringing the present composition of Uranus and Neptune up to solar abundance proportions implies a mass of the preplanetary cloud of 5 or 6% of the present mass of the Sun. A similar figure comes from the original Oort cometary cloud. But a solar nebula of approximately one solar mass meets, Safronov believes, serious difficulties in accounting for the loss of this material. He prefers a mass for the initial solar nebula which is a logarithmic average of the values proposed by Hoyle and by Cameron. Hoyle replied that for larger masses of the solar nebula one needs strong dissipation forces, and asked what if the dissipation forces were slightly stronger yet. We are then left with no solar nebula and no planetary system. He believes that unless we were to accept our solar system as very anomalous, we cannot hold that dissipation processes remove a very large fraction of the initial solar nebula. He therefore opts for rather small nebular masses. Whipple commented that if Uranus and Neptune are formed by "cometesimals," then the initial solar temperatures which Hoyle assumed would be too high.

Panel 2, on the internal constitution and thermal histories of terrestrial planets, began with an invited discourse by B. Y. Levin. Levin believes that a high central temperature for the Earth is inevitable, even for a slow accumulation rate, in part because of the finding by Safronov that the impact of asteroidal bodies releases a few percent of their kinetic energy as seismic waves which heat the interior. Levin argued that the only way of avoiding a completely molten mantle is convective circulation within the mantle. He reiterated his early objections to an iron core in the Earth, proposing instead, as Ramsey, Bullen and he earlier contended, that the high density of the core is due to a high-pressure silicate phase transition. In his view the core is composed of metallized silicates. He argues that problems in understanding the silicate phase transition are not more difficult than problems in understanding the origin of the Earth's iron core. Other geophysicists disagree with this view. Elsewhere in the solar system Levin proposed that Mercury was probably never molten, but that the other terrestrial planets have "more or less" molten interiors. He argued that the returned lunar samples from the Apollo mission imply a molten and differentiated moon, although the large age of lunar dust remains a puzzle.

In the discussion, Sir Harold Jeffreys began by saying that he had originated four hypothesis on the origin of the solar system and believes none of them. But, he suggested, the Earth must at some time have been fluid (1) because of the existence of land and water hemispheres, implying some transfer of materials in a medium neither wholly fluid nor wholly solid, and (2) because of the concentration of radioactive materials to the top 30 km of the Earth's crust. This has been understood since Goldschmidt's time as fractional crystallization—material of high ionic radii sweated out of the condensing silicate magma. If the radioactivity was originally distributed uniformly, Sir Harold believes, decaying radionuclides, especially K^{40} and U^{235}, would melt an initially cold Earth in some 10^9 years. He inquired whether tectonic activity might have started only after enough concentration of radioactive materials occurred some 10^9 years after the origin of the Earth.

S. K. Runcorn suggested that the Apollo findings of an internal magnetic field on the Moon 3×10^9 years ago imply a contemporary iron lunar core. The mechanical properties of solids, he suggested, depend critically on temperature, and therefore many questions of the evolution of planetary interiors cannot be approached in detail until the thermal histories of the planets are well understood.

Z. Kopal expressed satisfaction that the Soviet school considers the Moon an elastic solid down to some hundreds of kilometers. He believes the Moon was not completely molten and that local melting due to impacts is responsible for the Apollo results. Kopal suggested that a contemporary stellar system in which some of the events which led to the origin of our planetary system may today be occurring is ϵ Aurigae, some 1500 parsecs distant. It has a 10^6-year-old secondary component which Kopal now believes is a semi-transparent, highly flattened disc, 40 a.u. across, weighing 20 solar masses, and at a temperature of about 500°K. Its low temperature is deduced from its infrared excess. In considering its gravitational stability Kopal concludes that the principal constituent of the secondary component of ϵ Aur is not a gas; that the mean free path is large; that the constituent particles are large; and, therefore, that planetesimals are present.

Edward Anders expressed his disagreement with Levin on the question of the composition of the terrestrial planets. He believes there is clear evidence of chemical fractionation in the early solar nebula. One of the strengths of the silicate phase transition hypothesis of the Earth's core is that it can explain the properties of all the terrestrial planets (with the possible exception of Mercury) on the assumption of a constant chemical composition. But in the meteorites, the refractory elements, such as calcium, the siderophiles, such as iron, and the volatiles each show a constant fractionation. This is impossible to understand in meteorite parent bodies, but could be understood in the solar nebula. There is also evidence that the Earth and the Moon have been fractionated. If the silicate phase change hypothesis were to be accepted, Anders asked how we can understand the planets having escaped the metal/silicate fractionation we see in the meteorites. He believes that, instead, the density differences of the planets are due to differences in the nickel–iron content.

In discussion from the floor, John O'Keefe mentioned that Dunscombe's new mass of Pluto implies a mean density approximately that of iron. This high density, O'Keefe argued, is hardly attributable to fractionation and loss of silicates due to Pluto's proximity to the Sun!

Panel 3, on the internal constitution of the giant planets was led off by W. deMarcus. He believes that the Saturn ring system may be transitory, because of collisions in which energy is lost but angular momentum is not. The ring particles move both in and out, and the rings dissipate. He believes that the crepe ring is especially labile. Moving to the question of the equatorial acceleration of Saturn, he mentioned the work of Moore implying that the poles rotate one hour slower than the equator and raised questions about the definition of the true period of rotation. This is not a problem for Jupiter where the discrepancy is only on the order of a percent. But in the case of Saturn the effect is sufficiently important to affect models of the interior strongly. In the case of Saturn the ratio J_4/J_2 is satisfied by none of the contemporary models. deMarcus said he was prepared to believe in the infrared heat excesses of Jupiter and Saturn, but raised questions on the calibration of the infrared instruments used for these measurements. He stressed that the visual albedo of Jupiter is variable, having experienced, for example, a decline of 0.3 magnitude between 1951 and 1962. He believes that there may be some indication from early observations of the eclipses of Jupiter's satellites of a discoid debris ring around the planet.

In the discussion of the interiors of the Jovian planets deMarcus drew attention to two equations of state, each connecting a Z-normalized pressure with a Z-normalized specific volume. One is a "flat wave" crystal which for a given p has a v less than that of any known substance; the other is represented by the Thomas–Fermi atom, which has the opposite p–v limit. At high pressures the two state functions approach each other closely. He criticized the use of Lennard-Jones potentials for the Jovian planets and stressed that relativity corrections were unlikely to be important. In contrast to the cold interior models which are usual, deMarcus is attempting to construct warm models (although hot models—for example using the ideal gas law—do not work at all, because of too great a central condensation). He finds, for Jupiter, that he can reproduce the external potentials satisfactorily with warm models, but his calculated planet is 15% too large.

deMarcus concluded by stressing that a warm helium-rich Saturn is a bare possibility, but not a warm helium-rich Jupiter.

R. Wildt drew attention to the fact that equations of state for the upper few percent of the radius of the Jovian planets remain clouded in substantial mystery and that all present theories are expected to break down in this pressure regime. R. Wildey presented his and L. Trafton's results on 8 to 14 μ Jupiter limb darkening, in which they provide independent support for an infrared excess by a factor of 2.5 over the absorbed solar flux. No information can be derived from the limb-darkening on the H/He ratio.

R. Smoluchowski began his comments with an aside on Levin's discussion, to the effect that only a small fraction of lunar fines are 4.6×10^9 years old, and that the remainder are 3.5×10^9 years old. Smoluchowski proposed two different possible origins for the Jovian magnetic field: (1) currents in an external conducting layer, and (2) currents in a metallic core. The critical pressure for metallic hydrogen is $\sim$2 Mbar. When helium is added, the melting point is lowered. Smoluchowski pointed out that the liquid core of Jupiter is at about half its radius, so that the Elsasser self-sustaining dynamo explanation of the geomagnetic field might work as well for Jupiter. However there is no liquid core expected on Saturn, and this, he proposed, may be why there is not synchrotron emission observed from Saturn.

[As alternative possibilities one might suggest that there is a magnetic field arising from external currents on Saturn, but that the ring particles efficiently sweep out trapped charged particles in a Saturn radiation belt—the charged particles must intercept the ring particles once in each bounce between mirror points. Alternatively there is the possibility that the heliopause occurs as close to the Sun as the semimajor axis of Saturn's orbit, so that the solar wind does not reach Saturn at all.]

E. J. Öpik mentioned that high-Z meteoritic material is an alternative to helium as the nonhydrogenic component of the composition of Jupiter. Some early Jupiter model atmospheres had 150 km-atm of hydrogen. Öpik pointed out that the Rayleigh scattering in such an atmosphere is so large that one would see no cloud details at all.

R. Hide discussed the possibility of a high dynamo in the Jupiter atmosphere, on the grounds that it is unsatisfactory to have too good a conductor to generate the Jovian magnetic field. He suggested that there may be some correlation of changes of visible surface features with changes in the magnetic field over the disc, as might be detectable from a Jupiter orbiter.

The joint discussion on the origin of the Earth and Planets was closed with a remark by R. Wildt that the high hydrogen content in the interiors of Jupiter and Saturn, which are today accepted without question, was first suggested in 1923 and in 1927 by one of the members of the panel, Sir Harold Jeffreys.