INTERNAL CONSTITUTION OF THE GIANT PLANETS

De Marcus: In a discussion of the formation of the solar system's giant planets it seems necessary, even if somewhat tedious, to define our terms. Strictly this would mean some attempt to outline the past physical steps which led these bodies to their present condition. However, it seems possible that some at least of these bodies may not yet be quite through with their formation in the more general and presumably more meaningful sense that changes may still be ahead in which such basic parameters as mean density etc. may be appreciably different than the current values. I do not mean anything here as trivial as improved measures of radii which recently appreciably altered the number we refer to as the mean density of Neptune. Rather as an example of what I mean (but without asserting anything about the probability that the example is true) it could be that Saturn has much more helium than the current models indicate, in which case at some time in future his mean density might become ≈ 2.0 gm/cm^3 or more instead of the present value of 0.7 gm/cm^3. Consequently I first like to address myself to observational evidence which might suggest that such changes could happen.

In the case of the system Saturn plus attendants, I am of the opinion that the unique ring system may very possibly be a transitory phenomenon which mankind has been fortunate to see. That it is transitory on some time scale is almost certain since collisions between the ring particles seem inescapable and as Maxwell showed, such collisions in which statistically speaking energy is lost but angular momentum is not must spread the rings i.e., matter must move both in toward Saturn and away from him. It seems to me that a good argument can be made that the crepe ring has perceptibly increased in matter since the discovery of the ring system; increased enough to change it from a well nigh impossible object to see to one accessible to a modestly good telescope. If so, the repeated observations reported, and usually retracted, of a fourth ring or of filamentary matter outside ring A would be a very likely concommitant to the growth of the crepe ring. In particular also one might even suspect that Janus has not been where he is for a long time astronomically speaking and was formed from material which, at an earlier time, belonged to the rings.

I believe that we must also pay much more attention to Saturn's 'equatorial acceleration', a phenomenon he shares with the Sun and Jupiter and possibly many other rotating bodies. However, Saturn's equatorial current is decidedly different from Jupiter's in detail as well as in magnitude. It seems well established now that the equator rotates in something like 25 min less than the mid latitudes but the Doppler shifts of J. H. Moore (conflicting possibly with recent spot observations) indicates that the differential rotation may increase to high latitudes and the equatorial period could be as much as an hour or so less than that of the polar regions. If this is indeed the case we should try very hard to understand it. It seems possible that a slowly shrinking Saturn could be *one* explanation.

De Jager (ed.), Highlights of Astronomy, 228–238.

In the case of Jupiter, the radio period indicates that the period of the mid latitudes is close to the 'true' period; but for Jupiter the ambiguity is not serious anyway being only 5 parts in 600. The difference in detail between the differential rotations of Jupiter and Saturn as well as in magnitude, warns us however that the appropriate period for Saturn may be different from the midlatitude period by an amount which is serious for theories of planetary interiors. In short Saturn could be slowly collapsing with spin-up feeding primarily into the equatorial zone and his angular momentum may be larger than suspected. On the contrary, Moore's suggestion [seemingly supported by Spinrad and Trafton, *Icarus* **2** (1963), 19] of an over-running wind on Saturn could mean the 'true' period is longer than we think which would make Saturn less centrally condensed than we derive from theory. This would help to explain the difficulty with the ratio K/J^2 experienced by both DeMarcus and Peebles when building models of Saturn.

In short, there is much to be desired in our knowledge of Saturn's rotational structure. Until the situation is clearer, hydrostatic calculations based on the mid latitude spot periods may be nothing more than an '*ignis fatuus*'. The rotational structure of Saturn seems in fact to be potentially (for interior theories at least) a more serious stumbling block than the seemingly much more complicated zonal structure of Jupiter.

Systems showing such phenomena as the 'equatorial accelerations' of the Sun and planets are receiving much attention recently; which they deserve on the grounds of intrinsic interest as well as their importance to astrophysics. In enumerating the properties of several of them one aspect seems to have been ignored which is possibly of great importance (and has to my knowledge been recognized previously by at least two people); namely, the circumstance that observationally the flows are not apparently fully three-dimensional. The fact that two dimensional vorticity does not necessarily show the normal degrading effects – "Big whirls have lesser whirls which feed on their velocity..." even on non-rotating bodies, when they are at negative temperatures thermodynamically speaking was shown on theoretical grounds by Professor L. Onsager [*Nuovo Cimento* Suppl. to volume **6** Series 9 (1949), pp 3ff].

Here he proved that an ergodic system of a finite number of two dimensional vortices with sufficient energy would be at *bona fide* negative absolute thermodynamic temperatures. Thus the natural trend when energy is abundant and degrees of freedom are in short supply is to use up excess energy at the least cost in degrees of freedom. Hence large vortices of the same sign coming together is the *natural* thermodynamic response, i.e., in accord with the second law. It is almost obvious that the 'negative' viscosity effects are normal in this same sense.

Observational Evidence Bearing on Jupiter's Possible Changeableness (not necessarily directly)

First one should mention the fact that Jupiter – and Saturn too – are emitting more energy than they receive from the Sun. This alone argues that slight changes in both planets' radii will occur but it is easy to show that the amounts observed need not imply a total cumulative change which will be detectable. (It would be very helpful if the radiometry of Jupiter and Saturn were presented in a form more susceptible to independent checks rather than leaving the general scientific community in the position of having to accept these numbers as they are given to us.

Next I would like to mention again the variability of Jupiter's visual magnitude with time which Becker (1933) believed to be periodic (≈ 11.6 yr) and with amplitude of 0.34 mag. Daniel Harris reduced much of the available material to the modern system (G. P. Kuiper and B. M. Middlehurst (eds.) in '*Planets and Satellites*', *Solar System* **3** (1961)), but did not feel he could support or deny Becker's period. However, he accepted the variability as real. I have added one point to Harris' graph reduced from D. Taylor's work on the 1962 opposition. It yields $V(1.0) = -9.23$ for 1962 and adds even more weight to the conclusion of Becker and Harris.

That something 'peculiar' goes on intermittently around Jupiter is strongly indicated by the anomalous eclipse curves of Eropkin. They are so definite and consistent that one can scarcely ignore them. Kuiper and Harris searched for such an effect in 1954 and found no trace of it. However, Harris' statement (in G. P. Kuiper and B. M. Middlehurst, 1961) that the Harvard plates of Sampson did not show it is clearly in error. On page 375 of the famous textbook of Russell, Dugan and Stewart one finds an italicized pragraph indicating that Sampson found unpredictable fluctuations in the eclipse times, as if Jupiter's occulting surface varied by as much as 200 miles. This is not as large as Eropkin's anomalies but nevertheless is significant. It is perhaps only coincidental but Eropkin's observations and the bulk of the eclipse data used by Sampson were taken when Jupiter's visual geometric albedo was *probably* at a minimum. The work of Kuiper and Harris came when the V albedo was at an absolute maximum (as far as the extant data is concerned).

Equations of State of Cold Matter

It is my impression that many astronomers mistrust theoretical work on the interiors of the two largest planets because no theoretical physicst will assert that a rigorous solution to the Schrödinger equation for the many body problem in real condensed matter exists. I would like to take some time to explain why the giant planets, particularly Jupiter, are not subject to as much uncertainty as one might expect.

Suppose one considers the nuclei of the atoms of almost any element fixed on any reasonable lattice. The 'zero-point' motions of the nuclei are usually of minor concern and can be treated as a perturbation. We assume the Schrödinger equation and hence neglect relativistic effects for the moment. Then, as is well known, if any wave function satisfying the Pauli Principle for the electrons is used to calculate an energy, such energy must be (algebraically) greater than or equal to the 'true' energy. If one uses a Slater Determinant of plane waves the energy can be evaluated exactly. The answer is remarkably independent of lattice type if one omits the simple cubic structure and for our purposes can be written without reference to lattice structure as:

$$E_{f.w.} = \frac{15.054\, Z^{5/3}}{V^{2/3}} - \frac{17.0217\, Z^2 (1 + 0.5091/Z^{2/3})}{V^{1/3}} \qquad \text{(Unit } 10^{12} \text{ erg)}$$

where Z is the atomic number and V the molar volume. As a rule for normal solid

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densities the energy is strongly overestimated. However, as volume decreases the 'flat wave' energy should monotonically tend towards the 'true' energies. Hence the calculated pressure of the flat waves is too high or the volume at given pressure of the 'flat wave crystal' is always too small. (This argument is not completely airtight but this lower limit in V for given pressure is never approached by experiments to date in a manner that would indicate it might be trespassed if pressure could be increased sufficiently).

The flat wave pressures admit a scaling law with atomic number. Put

$$\pi = \frac{P}{Z^{10/3}\,(1 + 0.5091/Z^{2/3})^5}$$

$$v = ZV\,(1 + 0.5091/Z^{2/3})^3$$

and π is a 'universal' function of V. Neglecting relativity the true and flat wave equations of state should converge asymptotically for small v with double contact.

If we neglect relativity (as we did in the flat-wave case) and nuclear stability rules, there is no reason we could not have an atom with arbitrarily large Z. For very large Z the Thomas-Fermi atom model would be accurate. Such a hypothetical atom would, in a solid crystal, be the softest material known – the more so the larger its Z. Hence for any pressure, the Thomas-Fermi atoms not attainable in practice due principally to relativity (but Francium in solid form would probably conform startlingly close to the Thomas Fermi case if someone would be brave enough to do the experiments) give in the reduced (π, v) space, an upper limit on v for given π. Moreover the Thomas-Fermi case and the flat-wave case agree for the limit of small v through the two leading terms.

$$\pi_{f.w.} = 10.036/v^{5/3} - 5.6739/v^{4/3} \quad \text{(exact)}$$

$$\pi_{T.F.} = 10.036/v^{5/3} - 5.6739/v^{4/3} + c/v \quad \text{etc.} \quad \text{(an infinite series)}$$

At $\pi = 10^{11}$ dyn/cm^2 on this basis for any element for which relativistic effects would not be significant we should know that $4 < v < 7.2$ cm^3/mole. These limits should be quite safe. In fact it is quite probable that $v = 5.7$ ($\pm 10\%$) cm^3/mole (raising the median to adjust for zero point motion). Some pressures and volumes for $\pi = 10^{11}$, $v = 5.7$, are:

$Z=1$ $p = 0.78 \times 10^{12}$ dyn/cm^2 $V = 1.63$ cm^3/mole
$Z=2$ $p = 4.05 \times 10^{12}$ dyn/cm^2 $V = 1.215$ cm^3/mole
$Z=8$ $p = 186.0 \times 10^{12}$ dyn/cm^2 $V = 0.489$ cm^3/mole

Thus one sees that these broad general limits are not of use for the terrestrial planets where $Z \gg 1$ and typical pressures are 10^{12} dyn/cm^2 and/or less. For the giant planets, however, these limits are relevant indeed for the two elements hydrogen and helium. For these elements we have experimental data for

$$\pi < 10^{9.5} = 3 \times 10^4 \text{ dyn/cm}^2 \text{ (hydrogen)}$$

$$\pi < 10^{8.7} = 5 \times 10^8 \text{ dyn/cm}^2 \text{ (helium)}$$

We only need to interpolate the gaps for these two elements, and for hydrogen, good theory is available for the metallic phase.The usual criterion for the influence of relativity on cold planetary equations of state do not apply. The question is at what density does pressure calculated relativistically differ by, say, $x\%$ from pressure calculated non-relativistically. In the planetary case p is substantially a competition between the degenerate electron gas pressure and Coulomb attraction on nearly an equal footing. Hence a 1% change in the degenerate electron gas pressure due to considering relativity (this is the major effect) may be much more than a 1% change in total pressure. However for the Jovian planets the influence of relativity is negligible for the light elements H and He. For transition elements with $Z \sim 25$ or so, if we could calculate $p(v)$, relativity effects could be fairly important.

For the flat-wave crystal the dominant correction for relativistic effects can be easily included but no reduced form applies. It is only necessary to multiply the one term $10.036 \times 10^{12}\, Z^{5/3}/V^{5/3}$ by

$$f(x) = \frac{1}{1+x^2}\left[1 + \frac{9}{14}\left(\frac{x^2}{1+x^2}\right) + \frac{83}{168}\left(\frac{x^2}{1+x^2}\right)^2 + \frac{169}{348}\left(\frac{x^2}{1+x^2}\right)^3 + \cdots\right]$$

with

$$x = 0.010082\,(Z/V)^{1/3}\,.$$

The other terms are not affected. This relation converges for $x < \infty$ and is the Euler Transform of the power series for the perfect relativistic Fermi gas which otherwise only converges for $x < 1$. For the giant planets a maximum x of 0.02 would appear extreme for which $f(x) = 1 - 0.0001428$. At the volumes for which such values occur, the cancellation with the Coulomb term would not be severe enough to cause the total fractional error to exceed 2 or 3 times 0.0001428. These remarks apply only for H and He. However, heavier elements present do not need more than a semi-quantitative treatment in any event.

Although the cold predominantly hydrogen models of Jupiter have been very successful in predicting, as it were, the seemingly asymptotic values toward which spectroscopic data seem to be settling, one should bear in mind that the spectroscopic abundances are not quantitatively reducible by a reliable theory of planetary line strengths. One should also bear in mind that theoretical models of Saturn of the 'cold' type have always had one fairly disquieting feature common to the work of Ramsey (Jeffreys, 1954), DeMarcus (1958), and – not quite to the same extent but nevertheless still present – Peebles (1964). Namely, models giving J_2 correctly give values of J_4 higher than observed. The imminent fly-by's and the uncertainties present therefore suggest that it is high time to take a more careful look at alternative models.

By a cold body one means that the stress tensor throughout most of the volume of the body is but slightly affected by temperature. Perhaps a better way to describe a cold body is to characterize it as one for which the Third Law of Thermodynamics acts as a severe constraint. Antithetical to cold bodies are hot bodies, bodies in which stress is strongly dependent on temperature throughout most of their volumes. All stars are hot bodies save the white dwarfs, which are cold in this sense and possibly stars of very late spectral type. The intermediate category is of course obvious: a warm body is one in which the stress tensor is significantly but not dominantly determined by temperature. For an explicit example fluids near the critical region would seem to be very aptly characterized as warm. One can safely rule out the possibility that Jupiter and Saturn are hot bodies. There are so many arguments – mostly cogent – one can bring to bear against this that it seems safe to proceed directly to the important point. In principle, can Jupiter and/or Saturn be warm bodies? To answer this question let me first say that the answer is no if appreciable amounts of elements beyond hydrogen and helium are desired. I must at this point, to be technically accurate, file a 'caveat'. At 0 K and the modest pressure of 20000 atm., the density of normal isotopic lithium falls below that of helium possibly to stay below until pressures exceed the planetary range.

A natural first step in investigating how much helium might be present in Jupiter and/or Saturn is to consider pure helium models for if one can build a warm pure helium model one might reasonably expect that he could build a continuum of models with abundances anywhere between the cold models and 100% helium. I do not believe such warm pure helium models can be made for I have recently tried for the *n*th time. However, this is predicated on some shaky assumptions. The equation of state of helium at 0 K is fairly close to being correct and the Debye theory yields an adequate treatment for thermal effects for densities greater than 1 for temperatures up to 10 or more times the Debye temperature. But for the case of Saturn assuming the usual rotational rate etc. and the above, the situation is uncomfortably close. If one chose $T=100$ K at $p=1$ atm, an adiabatic model of pure helium has a radius only 12% or so too small. Raising the entropy further would remove this discrepancy but we then run afoul of the external potential, i.e. the model planets have too much central condensation.

For the case of Jupiter the discrepancies are more marked and one feels somewhat more secure in ruling out warm helium models.

In view of all the uncertainties about Saturn – rotation, near miss of warm helium models, etc. – I feel that one can only admit that he offers neither concrete help nor hindrance to the cosmogonist who might wish to tamper with the cold abundances.

Jupiter seems to be rather different. The spectroscopic data is seemingly monotonically tending toward the cold model abundances. The failure for Jupiter of pure helium models (if the failure is genuine and, I repeat, the disagreement there is quite sharp and strains at the bounds of credibility since the cold equation of state necessary covers a larger density range and at the high density end is squeezed closely by the Thomas-Fermi and flat-wave boundaries and at higher densities the Debye theory becomes safer also) then leads me to the opinion that one cannot have as much helium as he may wish in Jupiter. Nor does it follow that, if pure helium will not work, say 60% helium or some such number will, for when extra helium is added one must ask for temperatures to lower the densities. But hydrogen is an effective heat sop on a per gram basis. What I am trying to say is: The situation is complicated (and in fact may depend on the mechanism of Jupiter's formation) but one cannot simply assert that Y (the helium proportion) for Jupiter is anywhere you wish between say

$0.22 < Y < 0.70$ for an example only. Unfortunately, I cannot assure you that this may not be the case, for the caloric equation of state for elemental hydrogen or elemental helium in pure forms is difficult enough. The caloric equation of state for mixtures of the two (and a little Z!) dictates prudence. However, to underscore my remarks that mixtures may not work, I refer to Peebles who considered not a warm, but at least a lukewarm, Saturn and ended up with slightly more hydrogen than the cold one.

Wildt: De Marcus has essentially dealt with the equation of state applicable to the interior. We would like to know more about the equation of state applicable to the outer $\frac{1}{10}$ of the radius and here theory is no good. We would have to rely on experimental data which are unfortunately not available. We may be better off a year from now.

Wildey: Wendel De Marcus has a tough act to follow. He has a very bad habit of telling it like it is, or certainly at least like it should be. We have all seen the chemical abundances within Jupiter alternately shine through and then disappear in a mist of confusion. Some rather embarrassing situations with the abundances and temperatures associated with spectroscopic measurements have arisen in recent years. Some hope now appears of reconciling this situation by invoking a more sophisticated cloud structure. What one would really like, of course, is a theory with a deduced cloud structure rather than an *ad hoc* one.

Some major questions with which I have been concerned, primarily on an observational basis – in the wavelength region of 8 to 14μ – have been the issues of whether theories which neglect partial derivatives with respect to time, and matter-currents, may validly represent (1) the interior of Jupiter; (2) the extent of an internal energy source, and (3) the ratio of helium to hydrogen.

Dr Trafton of the University of Texas and I have been working together on these problems and achieved a result immediately prior to this General Assembly. Of course a dynamical atmospheric model might overly a static interior model, and vice versa, so that such variations as I have found in the past may be quite irrelevant to the question of whether Jupiter is a thermodynamic engine or not. We have reached the conclusion, however, that if he is, his radiative (thermal infrared) envelope is rather flexible in adjusting to his temporal and ensemble variations, which may be treated as linear perturbations of a quasistatic atmosphere, because the average of many approximately equatorial infrared scans agrees well with the predictions of a quasistatic model of the radiative envelope in which the continuous opacities of the rotational-translational interactions of H_2-He mixture is considered, together with the contribution of ammonia not only to atmospheric brightness extinction but to temperature structure as well. On the negative side, the He/H_2 ratio cannot be pinned down, but the observed limb darkening seems to be bracketed by all the 4 possible combinations of a He/H_2 ratio of 0 and 1 and an effective temperature of 135 K and 140 K. The afternoon limb darkening definitely fits the theory very much better than the morning limb darkening, and by choosing to honour the former our previous T_e is revised downward. It thus agrees very well with the absolute average bolometric brightness-temperature measurement of Low and his colleagues, which is a very, very difficult measurement to make and invites such tests as this.

Thus we agree that Jupiter is radiating about $2\frac{1}{2}$ times as much energy as it is receiving from the Sun; and you may make of this whatever you will!

Smoluchowski: I should like to make three comments:

1. *Heat emission of Jupiter and Saturn*

It is known that Jupiter's internal heat emission can be accounted for by a progressive change of solid hydrogen from its molecular to metallic form at a rate of about 1 mm per year. (Nature, **215**, 691, 1967). It turns out that the same rate of phase change agrees with the observed rate of heat emission from Saturn (2.7 and 2.4 times greater than the incident solar radiation respectively).

2. *Convection and heat flow on Jupiter*

The physical properties (density, viscosity, thermal conductivity, etc.) of solid and liquid H_2 layers on Jupiter and a cellular convection mode lead to rates of heat transport which are in reasonable accord with observation (*Science* **168**, 1340, 1970 and *Physical Review Letters* **25**, 693, 1970). The convective velocities thus obtained are in agreement with those deduced by Golitsyn from his similarity theory of planetary atmospheric circulation (*Icarus*, **13**, 1, 1970).

3. *Source and Location of the Magnetic Fields on Jupiter and Saturn*

It has been suggested that the huge magnetic field of Jupiter is generated either in its outer liquid H_2 layer in the deep liquid metallic interior (*Nature* **215**, 691, 1967). The latter proposal was evaluated more quantitatively using Hubbard's (*Astrophys. J.* **152**, 745, 1968) radial dependence of temperature on Jupiter and Trubitsyn and Ulinich's theoretical results on metallic helium (*Dokl. Akad. Nauk SSSR* **142**, 578, 1962; translation 7, 45, 1962) in conjunction with the earlier conclusion that helium is insoluble in metallic hydrogen at pressures below about 12×10^{12} dyn cm^{-2}. In order to estimate the melting point of a 10–20% H-He alloy a comparison was made with all known similar metallic systems. Among nine such systems all except one show a rapid drop of the melting point with increasing content of the divalent metal. The results for Jupiter and for Saturn are shown in the table below in which R_0 is the radius of the planet and the approximate helium concentrations are those given by De Marcus, Peebles, and myself (for references see *Nature*, quoted above). Melting points for Jupiter are those given by Hubbard for hydrogen while the corresponding temperatures for Saturn were obtained in the same manner as those for Jupiter. The last column shows the melting points of the appropriate H-He alloy. It follows that on Saturn, the pressure is too low to permit the formation of a H-He alloy which would be liquid at the existing temperatures and thus Saturn's metallic interior is solid. On the other hand, the inner part of Jupiter would be liquid for R/R_0 between 0.1 and 0.5 which happens to be quite similar to the conditions existing on Earth. If this result is correct, it explains the presence of a high magnetic field on Jupiter and the probable absence of a similarly high field on Saturn. This difference could not be easily accounted for if the fields were generated in the outer layers of the planets which according to Peebles (*Astrophys. J.* **140**, 328, 1964) are rather similar. This is not to say that on both planets there may be additional weaker magnetic fields generated in these layers.

TABLE I

Source and location of magnetic fields

	R/R_0	Helium concentr. DeM.	Peeb.	Smol.	Press. Megb.	Temp. Hubbard	Melt. pt.	He-H alloy
Jupiter	0.8	0	0.2	0	2	3600		
	0.55	0.2	0.2	0.0–0.4	12	5260	7000	→ 5100 liq.
	0.4	0.2	0.2	0.4	20	6350	7700	→ 5700 liq.
	0.3	0.2	0.2	0.4	26	6900	8000	→ 6200 liq.
Saturn	0.55	0	0.3	0	2	(3500)	5200	solid
	0.4	0.1	0.3	0?	4.2	(4400)	6000	solid
	0.3	0.9	0.3	0.9	6.9	(5500)	6500	solid

Öpik: All problems of origin and structure are extremely complicated where the number of factors usually exceeds that of those which come to our mind and the omission of *one* of them may upset the theory or the interpretations.

First, Jupiter cannot be a hot structure; Saturn, which is less dense, could be. An extreme case of a hypothesis of differing structure was proposed by Professor Alfvén some 10 yr ago. From certain considerations of origin he thought that the giant planet Jupiter would not be made of H or He but of the CNO group of elements and... the impossibility of this model also applies to a hot planet Saturn with some helium in it, because besides the atomic volume, and the equation of state we have the question of the radiation of the planet to space. I thought because N atoms are more massive, there is enough space in the volume of Jupiter so that one gets a non-degenerate gas which has its central temperature proportional to the potential energy. At the same time, we have definite limits to the surface temperature and density of a certain layer in the planet. If one calculates the average ensuing gradient for this gaseoos planet or applies the polytropic equation one finds this small mass cannot be in hydrostatic equilibrium. The gradient always considerably exceeds the adiabatic gradient. In that case, the turbulent transport of heat is so powerful that the surface temperature will be forcibly installed as the adiabatic value, in Jupiter's case, 2000°–3000 K. Helium for Jupiter is excluded by the equation of state. For Saturn, it will be the same.

Even if Alfvén could have his nitrogen planet, it would shine (as long as the energy lasted, about two or three hundred thousand years) as a mini-sun. This would be followed by collapse to a smaller volume, and this is not the case. A hot structure for the giant planets is not confirmed by the observations and it could not survive for a long time.

For the internal composition, H = He is a very convenient mixture, but there are other ingredients. The original nucleus of meteoritic material must begin the condensation; only when it gets to some Earth-mass size, does it begin to accrete gases. The excess central density for Jupiter, found by DeMarcus, could be this meteoritic material and there could be a liquid core.

There is some evidence that the atmosphere of Jupiter could contain more helium. An occultation of a star in Aries by Jupiter determined the scale height, and the molecular weight depends upon the assumed temperature. Kuiper got a molecular weight of 2.8, assuming a very low blackbody equilibrium temperature; the minimum temperature is 112 K and the corresponding molecular weight is 4.1 just the molecular weight of helium.

Further evidence is in the intensity of a certain forbidden line of molecular hydrogen, and the estimates ranged over two orders of magnitude.

Hide: Attempts to understand, in terms of basic hydrodynamic and magnetohydrodynamic processes (a) the Great Red Spot and other less persistent and generally smaller markings on Jupiter's visible surface, (b) the banded appearance and complicated and striking variation of rotating rate with latitude, including the pronounced equatorial jets, of Jupiter and Saturn, and (c) the origin of the magnetic field of Jupiter, have advanced our knowledge of the major planets (references [1–4]), but this knowledge has not yet been fully assimilated into theories of their internal structure. I was particularly interested to learn from previous speakers that confidence in the traditional theoretical models of five to ten years ago is now much less than it used to be and that some of the recent models investigated contain non-fluid regions. Not many years ago it was possible for one leading theoretician (see reference [5]) to assert that Jupiter must be fluid throughout, summarily (and not without sarcasm) dismissing work (my own) that presumed otherwise.

The dynamical influence of Coriolis forces on relative motions in the atmosphere of the major planets is much more pronounced than in the case of the Earth's atmosphere, though effects due to vertical density stratification are probably much less-important. Possible complications arise because (1) the major planets rotate hypersonically with respect to the speed of sound in their atmospheres, and (2) the electrical conductivity of the lower reaches of Jupiter's atmosphere might be sufficiently large for magnetohydrodynamic processes to occur there. If, as has been suggested, these processes produce, or at least modify, Jupiter's magnetic field, then future research of the major planets should include attempts to detect the magnetic field of Saturn, Uranus and Neptune, and to determine the configuration of the magnetic field in the vicinity of the visible surface of Jupiter, carried out in conjunction with attempts to measure the electrical properties of the outer layers of the planets and systematic theoretical studies of the hydrodynamics and magnetohydrodynamics of hypersonically-rotating fluids.

Horizontal and vertical transfer of angular momentum within the planet Jupiter are implied by the existence of the equatorial jet, the motion of the Great Red Spot and various characteristics of Jovian decimetre and decametre radio emission. The (nearly) fixed latitude but variable rotation period of the Great Red Spot have been interpreted as evidence of a gross hydromagnetic torsional oscillation of Jupiter's internal layers involving an internal toroidal magnetic field of over 1000 G. This field, and the comparatively weak poloidal field of 50 G (at the visible surface) whose lines of force are not confined to the interior of the planet, are probably produced in the lower atmosphere or in a metallic liquid core (if Jupiter has one) by hydromagnetic dynamo action, maintained by convection driven by gravitational energy release within the planet. As in the case of the Earth, rotation probably enhances the efficiency of the dynamo process through its influence on the pattern of core motions. It will be surprising if any large, rapidly rotating and partially fluid planet, such as Saturn and the other major planets, is found to be non-magnetic.

Dr De Marcus has emphasised on this and on previous occasions his view that (notwithstanding substantial errors in observations) thermal radiation from Jupiter undergoes significant fluctuations. It is not inconceivable that such fluctuations could be due to variations in ohmic dissipation associated with the electric currents responsible for Jupiter's magnetic field (1). It should be possible to test the validity of this suggestion when better magnetic and thermal data are available.

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De Marcus: Professor Öpik made me aware that I had omitted two important statements:

(1) When I ruled out warm models, I built them up to the adiabatic gradient and no further. I could perhaps have made a model, had I been willing to go super adiabatic.

(2) I usually try to work with cosmogonic arguments or cosmochemical data but if you want to build a warm lithium model of Saturn or Jupiter, you probably can.

Wildt: I think a brief historical comment is appropriate.

This morning we have listened to Sir Harold Jeffreys speaking with the authority based on 50 yr work on the internal constitution of the Earth. I wonder how many people in this audience are aware that he could speak with equal authority on the interiors of the giant planets. We have heard a great deal about hydrogen and helium in the interior of Jupiter and Saturn. As far as I am aware, this idea was first enunciated by Sir Harold in 2 papers in 1923 and 1924 and it is appropriate to recognize this fact on this occasion. Thank you.