

EVIDENCE FOR THE DISTRIBUTION OF ANGULAR VELOCITY INSIDE THE SUN AND STARS

INTRODUCTION

L. Mestel We have heard that the solar wind is steadily removing angular momentum from the solar surface via magnetic coupling. We now ask how the internal rotation field of the sun responds to this surface stress. We know that the sun has a deep subphotospheric convection zone, surrounding a radiative core. We shall assume that there are only modest variations of angular velocity within a convective zone, though we should note that there is at least one model of nonisotropic turbulence that, in principle, could allow a marked inward variation [*Biermann*, 1951, 1958; *Kippenhahn*, 1963]. We now ask whether the radiative core also steadily adjusts its angular velocity to stay more or less in step with the outer regions, or whether a steep inward angular velocity gradient is built up, as in *Dicke's* [1970, 1971] model, which has the core rotating some ten times faster than the convective zone.

One feels that a necessary condition for the persistence of the *Dicke* model is the absence of even a modest primeval magnetic field coupling the core and zone, for torsional hydromagnetic waves would iron out nonuniformities in rotation in a time much shorter than the solar lifetime. I personally am doubtful if this is a plausible assumption; however, I shall act as an *advocatus diaboli* and discuss the equilibrium and stability of the *Dicke* model in strictly nonhydromagnetic terms. The complications that arise are a justification for the claim I once made that the magnetic field is one of the great simplifying features of astrophysics.

Howard et al. [1967] and later *Bretherton and Spiegel* [1968] suggested that the *Dicke* model would be destroyed by a process analogous to Ekman pumping that is responsible for the rapid "spindown" in a coffee cup. In an incompressible (or barotropic) fluid the condition of hydrostatic support requires that the centrifugal force be conservative, so that the angular velocity must be a function only of distance $\tilde{\omega}$ from the axis.

Such a law is inconsistent with the no-slip boundary condition at the bottom of the cup, so that a dynamically driven circulation is set up, with viscous force balancing Coriolis force in the thin Ekman boundary layer. Continuity forces the flow to extend through the bulk, yielding a very short spindown time.

The treatment of this problem contrasts markedly with that customary for a nonbarotropic stellar gas, obeying the law $p \propto \rho T$. A nonconservative field of centrifugal force, such as *Dicke's*, can be balanced hydrostatically by suitable variations of ρ and T over isobaric surfaces. The consequent breakdown in radiative equilibrium yields buoyancy forces that drive a slow circulation [*Eddington*, 1929; *Sweet*, 1950; *Baker and Kippenhahn*, 1959; *Mestel*, 1965]. The circulation speeds are normally of the order of the Kelvin-Helmholtz contraction speed times the factor $r|\nabla(\Omega^2 \tilde{\omega})|/g$, where g is the gravitational acceleration. If Ω is slowly varying, this factor is essentially $\Omega^2 \tilde{\omega}$, but in a region of large rotational shear, as in the transition between *Dicke's* core and the convection zone, the circulation speeds will be much faster and will act to reduce the gradient. But before concerning ourselves with processes dependent on heat transport, we want to be sure that there is no analog of Ekman suction, yielding a much shorter spindown time. In fact, if the angular velocity gradient is too large it is impossible to satisfy the condition of hydrostatic support without the density gradient becoming locally positive and so unstable; the thermally driven Eddington-Sweet circulation is replaced by a dynamically driven flow if the scale of variation of Ω is less than

$$d_c \approx r[(\Omega^2 \tilde{\omega}/g)(\lambda/r)]^{1/2} \quad (1)$$

where λ is the local scale height. This is the analog of the layer thickness through which Ekman-pumped currents can travel against the effect of stable stratification

[Holton, 1965]. One is therefore led to consider a model in which a rapidly rotating core and a slowly rotating envelop do coexist, with the transition region between them never smaller than d_c . The evolution of the angular velocity field in the core would be given by the Eddington-Sweet currents, with the sharp Ω gradient and any variations of molecular weight playing an important role. However, a much shorter spindown time could result if the transition layer were to become unstable. One would then arrive at a picture in which the slow but persistent braking of the star would drive a weak turbulence in the radiative core, which would keep the whole sun rotating more-or-less uniformly [Spiegel, 1968].

Let us therefore adopt a Dicke-type model, and study possible instabilities [see Spiegel and Zahn, 1970, for a recent survey]. If the fluid is inviscid and incompressible (with Ω necessarily a pure function of $\tilde{\omega}$), a celebrated criterion due to Rayleigh applies; for stability against axisymmetric disturbances, we require

$$(1/\tilde{\omega}^2)(d/d\tilde{\omega})(\Omega^2 \tilde{\omega}^2) > 0 \quad (2)$$

The angular momentum per unit mass must increase away from the rotation axis. However, there exist some nonaxisymmetric unstable modes even if condition (2) holds [Howard and Gupta, 1962]. Other nonaxisymmetric instabilities occur if

$$(d/d\tilde{\omega})[(1/\tilde{\omega})(d/d\tilde{\omega})(\Omega \tilde{\omega}^2)] = 0 \quad (3)$$

“inflectional instabilities” [Lin, 1955].

The principal modification in astrophysical applications is the stabilizing effect of a density stratification. In a zone that is stable against convection the density gradient is subadiabatic, and energy is required to drive adiabatic motions against gravity. The Richardson criterion for the stability of shear flow [Chandrasekhar, 1961] sets a lower limit to $|\Omega/(d\Omega/d\tilde{\omega})|$, which turns out to be of the order of the Holton thickness (1). However, in the Dicke model Ω is a function of displacement z parallel to the rotation axis as well as of $\tilde{\omega}$; the surfaces of constant angular momentum are not cylinders. Such models are sometimes subject to rapidly growing “baroclinic instabilities,” discussed by Hoiland [Ledoux, 1958] and more recently by James and Kahn [1970] who call them “sliding instabilities” because they involve motion of gas elements along either isobars or isentropes. They occur if the local angular momentum gradient \mathbf{h} lies in the shaded region (fig. 1), where \mathbf{p} is the direction of the pressure gradient and \mathbf{s} the direction of the negative entropy gradient. It appears that some Dicke-type models with the surfaces of constant angular momentum, as in figure 2, would violate the stability criterion, but others, as in figure 3, would not.

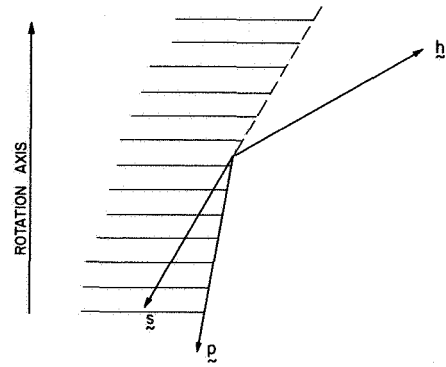


Figure 1. Schematic of angular momentum gradient, \mathbf{h} ; \mathbf{p} is the direction of the pressure gradient, and \mathbf{s} is the direction of the negative entropy gradient in a star for the condition of “sliding instabilities” to not be met. The converse takes place when \mathbf{h} lies in the shaded region.

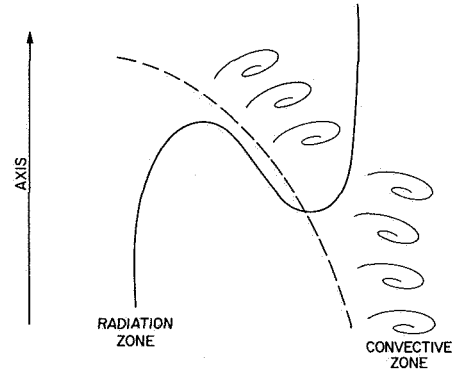


Figure 2. Dicke sun with a surface of constant angular momentum conceptually illustrated by the folded line crossing the radiative-convective transition. (The line is a cut in a surface of rotational symmetry about the axis. This condition might violate stability against the “sliding instability.”)

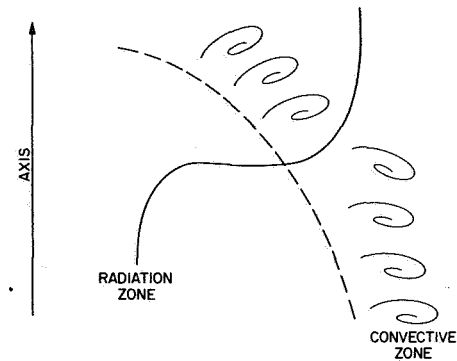


Figure 3. Dicke sun under the condition where stability is satisfied. The definitions of the figure elements are the same as those in figure 2.

So far we have assumed adiabatic motions. As soon as we allow for finite transport processes the whole situation changes. In stellar interiors the ratio of viscosity to thermal conductivity is very low ($\approx 10^{-6}$), so that viscous effects can often (though not always) be ignored compared with heat flow. *Townsend* [1958] and *Yih* [1961] showed how radiative transfer can remove the stabilizing effect of stratification, so that for a completely stable state conditions (2) and (3) replace the Richardson criterion. The reason is that when temperature perturbations are smoothed out, the stabilizing effect of buoyancy is simultaneously removed [*Moore and Spiegel*, 1964]. More recently, *Goldreich and Schubert* [1967] and *Fricke* [1968], again ignoring viscosity, have shown that another necessary condition for the absence of "secular" (dissipation-dependent) instabilities is $\partial\Omega/\partial z = 0$, or angular momentum constant on cylinders. We have noted that this is a condition for the equilibrium of an incompressible star. *Fricke* [1969a] summarizes these results by the prescription: to determine which states of a real star are *secularly* stable, solve the problem of the *equilibrium* and *dynamical* stability of the corresponding inviscid, incompressible system.

It is then clear that even Dicke models that are not subject to baroclinic instabilities are certainly secularly unstable on the Goldreich-Schubert-Fricke criterion. However, it is still not generally agreed what asymptotic state the star reaches, and in what time scale. *Colgate* [1968] and *Kippenhahn* [1969] argue that the developed weak turbulence that follows from secular instabilities takes in general at least a Kelvin-Helmholtz time to alter substantially the overall angular momentum distribution. More recently, *James and Kahn* [1970] have proposed that an arbitrary initial rotation law rapidly approaches a state with the surfaces of constant angular momentum either cylinders or isentropes. The secular instabilities to which such a model is subject are suppressed by the much more rapid baroclinic instabilities which they themselves generate. *James and Kahn* [1971] also have studied the evolution of the junctions between the isentropes and cylinders, where the breakdown in radiative equilibrium leads to locally large Eddington-Sweet velocities; they conclude that the time for overall redistribution of angular momentum is the average Eddington-Sweet time, and this would be just about comparable with the solar lifetime if Dicke's internal rotation is correct. However, the subject remains controversial.

I have assumed that there are no inward gradients of mean molecular weight μ . It has been known for many years that a very modest μ gradient will suppress the Eddington-Sweet circulation [*Mestel*, 1953, 1957;

Kippenhahn, 1967], and that the growth of μ in the center of a star is normally able to prevent the circulation from homogenizing the star. Similarly a μ gradient will kill secular instabilities [*Goldreich and Schubert*, 1968]. The μ gradient that can be built up during the early solar lifetime clearly depends on the rate at which instabilities develop and mix matter and angular momentum (see Dicke's discussion of the lithium problem, p. 290). *Fricke* [1969b] finds that the maximum oblateness due to internal rotation that can be obtained from a rotation field satisfying the secular stability requirements (including the effect of μ gradients) is a factor 4 less than Dicke's value. But if we can tolerate secularly unstable rotation laws, because we have grounds for believing that the consequent angular momentum diffusion time is at least as long as the Kelvin-Helmholtz time, then a μ gradient can be built up that will stabilize the Dicke model for the much longer nuclear lifetime of the sun. To return to a point made earlier, those who accept the arguments but do not like the Dicke model might very well claim that the conclusion is an argument in favor of magnetic coupling between core and envelope.

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COMMENTS

R. Kraft I would like to go back to the issue of the Li and Be abundances in the sun, to remind you of what Mestel said, that in comparison with young stars the solar Li abundance is very low. The solar Be abundance, however, is appropriate in making these comparisons. One knows that Li can be destroyed at a temperature somewhat higher than the base of the external convection zone, but that to destroy Be requires still higher temperature. So one imagines now there must be some way to mix the subadiabatic sub-convection zone material. And the issue is whether the turbulence that can be set up by the spindown process may be sufficient.

R. H. Dicke Mestel raised the question of stability that is a source of worry in connection with a rapidly rotating core in the sun. The instabilities in question are thermally driven: the Eddington-Sweet thermally driven currents and Goldreich-Schubert, and Fricke types of mild turbulence also driven by thermal effects. I will take the following viewpoint: Assume that the Goldreich-Schubert-Fricke instability holds and then calculate the turbulent transfer of angular momentum out of a star when despinning. This amounts to a turbulent diffusion of angular momentum. The same turbulent diffusion of angular momentum out of the star implies a turbulent diffusion of Li and Be downward into the interior, where these elements are burned. The two effects are boot strapped together: observe the rotation, and you should be able to say what is happening to the abundances of Li and Be. By observing rotations and abundances of Li and Be we decide whether or not this instability exists. This is program 1. I have another program after that, which is to use the "observed" depletion of Li in the sun as a basis for some conclusions about the present solar wind torque.

I assume that the thermally driven turbulence at the condition of marginal instability, after averaging $\Omega \sin^2 \theta$ over spherical surfaces, leads to $\Omega \sim r^{-n}$, where $0 < n \leq 2$. The Goldreich-Schubert instability results in a function $\Omega(r)$ in reasonable accord with the above equation with $n \simeq \Omega_0/\Omega$ where Ω_0 refers to the present surface rotation of the sun. For isotropic diffusion the transport of angular momentum is controlled by the diffusion equation

$$(\partial/\partial r)[D\rho r^4(\partial\Omega/\partial r)] = \rho r^4(\partial\Omega/\partial t)$$

where D is the diffusivity of the turbulent diffusion. Assuming marginal instability $\Omega(r, t)$ is known everywhere in the interior if the surface rotation $\Omega_s(t)$ is known. Integration gives $D(r, t)$.

The next step is to ask what controls the diffusion of lithium. Let's designate the abundance of Li, Be, or whatever the isotope is by the symbol F . The corresponding diffusion equation is

$$(\partial/\partial r)[D\rho r^2 (\partial F/\partial r)] = \rho r^2 (\partial F/\partial t)$$

The diffusivity D is the same as before. To emphasize the point, we know, or at least we assume that we know, the time dependence of surface rotation. As the rotation rate of the surface of the star decreases the variation of the angular velocity of the stellar interior is known as a function of time and position from the condition of marginal instability. Instead of the usual interpretation of the diffusion equation, it is interpreted as a first-order differential equation for D . We solve that differential equation, substitute the resulting diffusivity D in the diffusion equation for Li (or Be), and solve the differential equation to obtain the depletion of Li (or Be) at the surface. The stellar rotation and the depletion of the isotope (Li^6 , Li^7 , or Be^9) are boot strapped together. The relation is $F/F^* = (\Omega_s/\Omega_s^*)^\Lambda$ where the asterisks (F^* and Ω^*) refer to original values on the main sequence, and Λ is an eigenvalue derived from the solution of the differential equation for F as an eigenvalue problem. Figure 1 shows Ω for marginal instability calculated from the

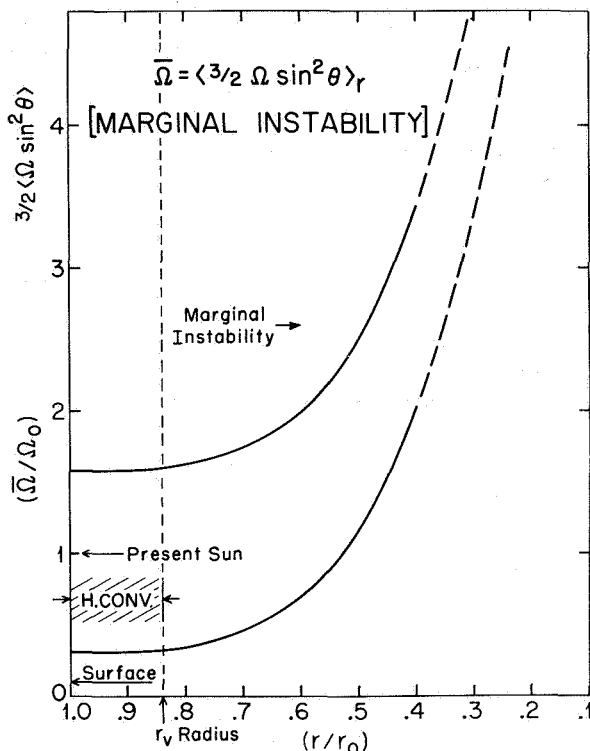


Figure 1. $\Omega(r)$, after averaging over spherical surfaces.

Goldreich-Schubert dispersion relation. Table 1 gives Λ for three different values of n , $n = 1/2, 1$, and 2 . The values of Λ given in table 1 are all so great that there should be no lithium or beryllium in the Hyades for which Kraft has measured a rotational slowing by a factor 2; that is, $\Omega/\Omega^* = 1/2$. On the contrary, we do see Li^7 and Be^9 , from which I conclude that this turbulence does not extend down deep in the star and *the Goldreich-Schubert instability does not occur deep in the star.*

Table 1. Λ eigenvalue for deep lying mild turbulence

	Li ⁶	Li ⁷	Be ⁹
n	$r_b = 0.63$	0.58	0.47
1/2	237	140	46
1	174	100	31.3
2	222	120	33
2 (2nd mode)	1460	780	212

$\Lambda = \gamma\tau$

The next question is whether the instability exists at all or whether it exists only part way down. Since we don't at the moment have any other explanation for the depletion of lithium I'm going to try on for size the idea that the lithium is depleted as a result of angular momentum being transported by means of this turbulence – angular momentum flowing out of the star into a stellar wind – but that the turbulence is terminated at a certain radius (which we will call r_c) because of a slight jump in the mean molecular weight ($\Delta\mu \sim 2 \times 10^{-3}$). As was noted by *Goldreich and Schubert* [1967], such a molecular weight jump provides a means for turning off the turbulence. Incidentally, when you turn off the turbulence you also turn off the circulation currents at that point; they both terminate.

Figure 2 shows a hypothetical way of obtaining the molecular weight jump. In the process of stellar contraction in the core, density goes quite high. But increased density in the core ought to result in increased angular velocity in the core. The curve marked “after core contraction” shows the high angular velocity of the interior leading to an angular velocity gradient that may exceed the instability limit. Goldreich-Schubert turbulence and circulation currents may occur inside the core, and if there is any extra helium as a result of hydrogen burning while this mixing is occurring, extra helium may be mixed throughout the core while the core's angular velocity tends to become uniform. As noted before, the jump in molecular weight required to stabilize is only $\sim 2 \times 10^{-3}$, which is very small. There is a possibility that one ends up with a stabilized boundary at $r \cong 0.55$ with the region $0.55 < r < 0.84$ being the thermally driven turbulent region. Outside is the hydrogen convective zone where angular momentum is moved convectively. These are the assumptions we make.

Table 2 shows the eigenvalue Λ discussed earlier, but now the turbulence is assumed to be cut off at r_c . The tabulated values are for various assumptions about cutoff radius and the index n . These have been chosen in such a way as to give reasonable values for the depletion rate for Li⁷. It is found that the cutoff can never go deep enough to deplete Be⁹. For a reasonable depletion of Li⁷, Li⁶ should be essentially completely burned. If you reduce Li⁷ by a factor of 5 the Li⁶ ought to be out of sight.

In attempting to apply this situation to the sun or stars of precisely $1 M_\odot$, one runs into the problem indicated before. We don't have observations giving the slowing of rotations of $1 M_\odot$ stars. But we do have the stars that Kraft has observed at $1.2 M_\odot$, and we see that their rotations have decreased with time; we also have the lithium abundances decreasing with time. When you boot strap these two together you find a best fit; you get the best explanation for the depletion of Li⁷ if you take $n \sim 2$. For $\Omega \gg \Omega_\odot$, this is much too large a value of n to be associated with the Goldreich-Schubert threshold, and some modification of the Goldreich-Schubert effect is required, perhaps by nonrotational motion of the fluid such as a slight oscillation of the core.

For the sun we are stopped for lack of observations and don't know what to do. So we

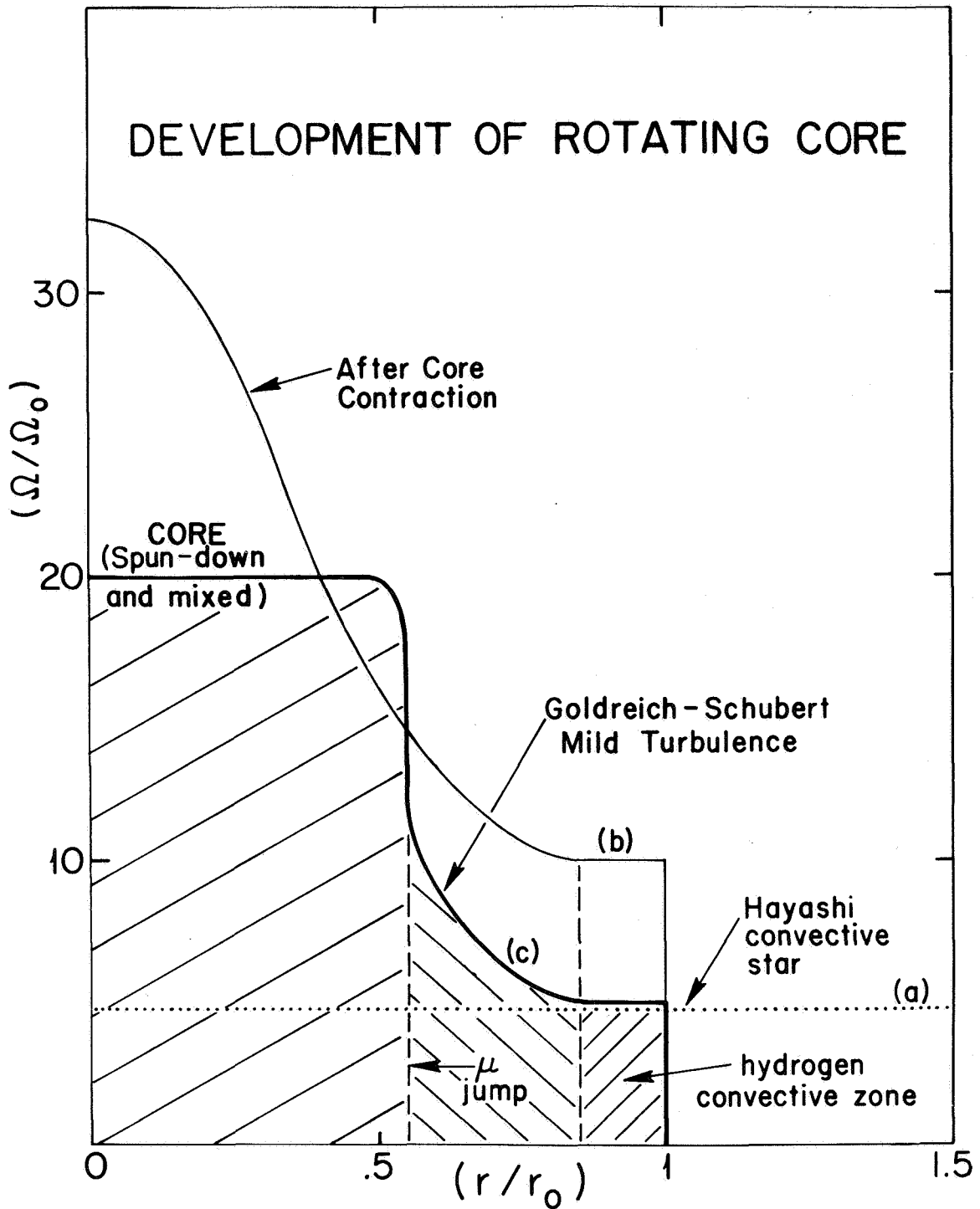


Figure 2. Hypothetical rotational history for the sun.

Table 2. Λ eigenvalue with turbulence quenched at r_c

n	r_c	Li ⁶	Li ⁷	
		$r_b = 0.63$	0.58	
2	0.578	5.265	0.901	
	.57	6.01	1.633	
	.56	5.97	2.372	
	2nd mode	↓	52.3	20.5
	3rd mode	↓	145.0	55.8
1	.55	8.0	3.013	
	.53	10.33	4.376	
	.50	14.57	6.772	
	0.578	9.64	1.761	
	2nd mode		70.3	20.98
1/2	.57	10.9	3.30	
	.56	12.5	4.48	
	.55	14.2	5.61	
	2nd mode		100.5	43.5
	0.578	18.6	3.48	
2nd mode		134.9	40.36	
	.57	20.75	6.45	
	.56	23.63	8.70	

$\Lambda = \gamma\tau$

take a new approach. After all, we have, or at least we think that we have, a rough value for the solar wind torque. We can insert that to give the flow of angular momentum inside the sun, from which we can calculate a present rate of decrease of Li⁷ at the surface of the sun.

Figure 3 shows lithium abundance in meteorites in the Pleiades, coma cluster, the Hyades, and the sun. There's some argument concerning the abundance in the sun, but I would guess the best value is about [Li⁷] ~ 0.8. There is a problem if you take as the solar wind torque density 6×10^{29} dyne cm/sr, which for an isotropic solar wind is $\sim 5 \times 10^{30}$ dyne cm total, and attempt to calculate the rate at which Li⁷ should be decreasing. One must decide whether angular momentum is coming from deep inside the star or only from an outer shell with inner radius r_c as a result of the slowing of the shell. If it comes from slowing of the outer shell alone, the rate of decrease of Li⁷ with time is given by the dashed line (1). If angular momentum arises in the deep solar interior, the rate of decrease of Li⁷ is given by (2).

But I forgot an important point, that if you do have a rapidly rotating core for which there may be viscous diffusion of angular momentum from the core, you can't say exactly how much it is. You can give an upper bound because the initial steepness of the angular velocity gradient in the young sun cannot exceed a certain amount without also exceeding the Richardson criterion for instability to ordinary dynamically driven turbulence. The assumption of an initially steep angular velocity gradient provides a takeoff point for the solution of the viscous diffusion problem to obtain the diffusion of angular momentum. Adding viscous diffusion as a source of angular momentum gives lines lying between (1) and (2). You can calculate a value for the flux of angular momentum from the core, hence a *lower bound* on the angular rotation of the core, by adding the right amount of core angular momentum flux to obtain the correct present values for the

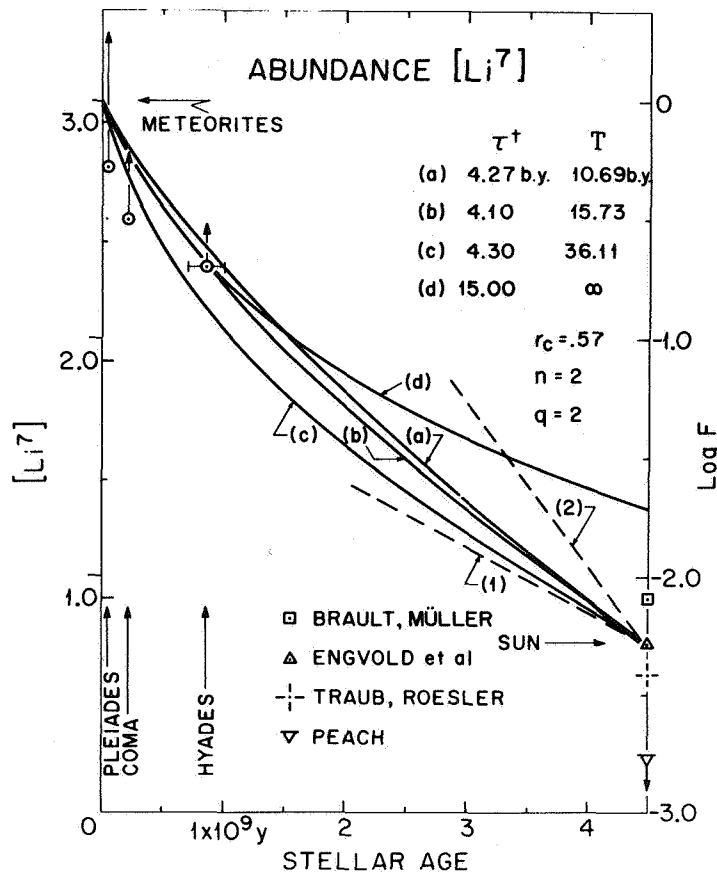


Figure 3. $[Li^7]$ and $\log F$, logarithmic depletion of Li^7 . The curves (a), (b), and (c) are integrations applicable to the sun. Curve (d) is an interpretation applicable to Kraft's stars of mass 1.2. The associated angular velocity curves (surface angular velocity as function of time) are given in figure 4.

abundance of lithium and the angular velocity at the sun's surface. Curves a, b, and c of figures 3 and 4 give integrations for $\log F$ and Ω calculated in this way. Corresponding lower bounds for the angular velocity of the core are included in figure 5.

Now let me turn the problem around another way. Ask yourself the following: suppose we know nothing whatever about the solar wind torque, know nothing whatever about the location of the radius r_c except to say that it is somewhere in the zone of Li^7 burning. It is found that to obtain the correct values for the present abundance of Li^7 and surface angular velocity the present solar-wind torque is $\sim 4 \times 10^{30}$ dyne cm if the torque is proportional to the square of the solar angular velocity. For a torque proportional to the solar angular velocity, the calculated solar-wind torque is roughly a factor of 2 greater. These are surprising results. The present value of the solar-wind torque implied by the loss of lithium in the sun is quite insensitive to detailed assumptions and is quite close to the "observed" solar-wind torque. Another interesting result is that the maximum value for the angular momentum flux (by viscous diffusion) from a core rotating rapidly enough to account for the solar oblateness ($20 \Omega_0$) is 3.5×10^{30} dyne cm. The close correspondence with the calculated torque (from lithium depletion), 4×10^{30} dyne cm, and the "observed" torque, 5×10^{30} dyne cm, suggests that the present source of angular momentum for the solar wind may be viscous diffusion from a rapidly rotating core.

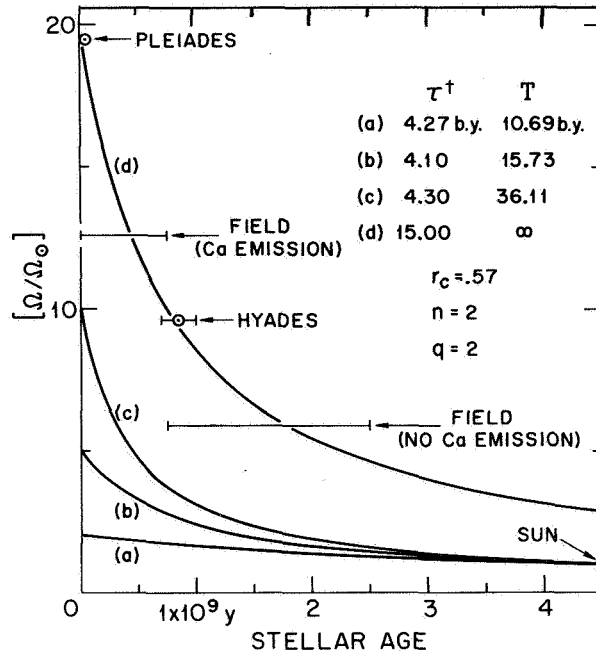


Figure 4. With various assumptions concerning the initial main-sequence value of the surface angular velocity, the time dependence is calculated. The curves of figures 3 and 4 assume that the solar-wind torque is proportional to the square of the angular momentum. (a), (b), and (c) are applicable to the sun and (d) refers to Kraft's stars.

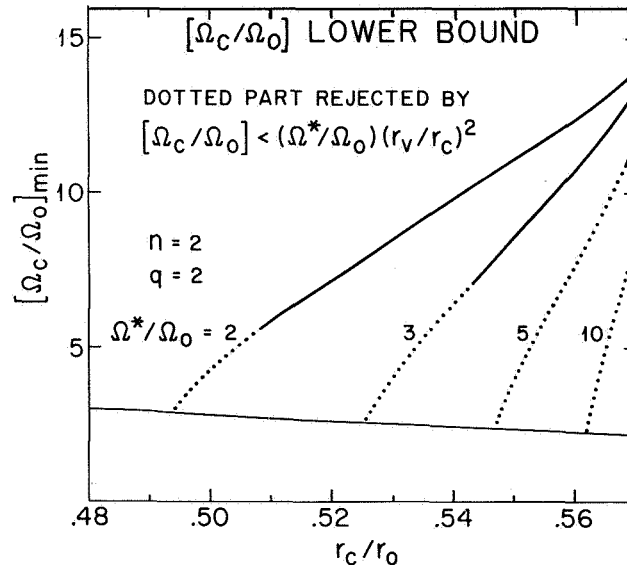


Figure 5. Lower bounds for the angular velocity of the solar core obtained assuming viscous diffusion of angular momentum from the core. The corresponding angular momentum flux is that required by integrations such as (a), (b), and (c) of figures 3 and 4. The interpretation of the solar oblateness of $\Delta r/r \sim 5 \times 10^{-5}$ as the effect of a rapidly rotating core requires an angular velocity of $\sim 20 \Omega_0$ for $r_c \sim (1/2)r_0$.

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COMMENTS

E. Schatzman There are a number of questions related to this discussion concerning the transport of matter or momentum, including the question of whether we can apply a diffusion equation. I would like to give a number of ideas concerning the possibility of an observational test of this transport inside the sun and possibly in stars, by detailed analyses of the abundances of certain isotopes at the surface of the sun or in the solar wind. If we consider the different nuclear reactions that can take place inside the sun, first there is H^2 burning which can be neglected because it takes place in the very outer layers.

Next there are Li^6 , Li^7 , Be^9 , B^{10} , and very deep inside the sun He^3 formation by the reaction $D^2 + p \rightarrow He^3$ together with C^{13} burning which takes place at the very core of the sun. Now, what we have to do irrespective of whether turbulent transport from the inside has taken place, is to compare some initial abundances to the observed one. We don't know the initial abundances of the sun and can only make guesses, the validity of which I am not certain. I shall discuss this briefly.

In units of $\log N_H = 12$, where N_H is the abundance of hydrogen, the initial value of $\log N_{Li^7} = 3$. Assuming earth abundances, then the initial value for Li^6 in these units ~ 1.9 . Now, if we consider the spallation ratio, if produced by spallation of carbon or nitrogen, the value would be about 2.7, that is to say, about one-half the abundance of Li^7 .

In regard to Be^9 there are some difficulties. Again using earth abundance for Be, a value of ~ 1 is obtained whereas using the spallation ratio yields ~ 1.7 . For B^{10} the spallation ratio should be 3.3. These numbers are to be compared to what observations?

For lithium, we can take three for the initial value and the observed value is depleted by a factor of a hundred; this can be explained by turbulent transport from the lower boundary of the convective zone to the place where Li is being burned. Li^6 is not observed and probably has an abundance less than one-twentieth of Li^7 , that is, $\log N_{Li^6} \sim 0.3$. Using the values 1 and 0.3, we have compatibility with the turbulent process in which the time scale is proportional to the square of the distance over which the transport takes place. For Be^9 with an observed value of 0.7-1, depending on interpretation of the profile of the spectral lines of Be, based upon the earth abundance, no burning exists, in which case we would have the case raised by Professor Dicke. On the other hand, using the spallation value for the initial concentration of Be we note depletion by an appreciable factor, which could also be explained by a turbulent transport. For Be^{10} we know that $\log N < 2.7$, given by the limit of visibility of the spectral lines. This is a depletion by a small factor, if any, perhaps 4, and this is also compatible with transport. But the real clue concerning this problem rests with He^3 and the C^{13} . He^3 has not been observed spectroscopically, but we have solar wind observations and I want to refer here to Professor Geiss' measurements on the surface of the moon for which he reports a value of $He^4/He^3 \sim 2 \times 10^3$. Now, what is the initial value? Perhaps it corresponds to the very lowest value which has been obtained in meteorites, which is ~ 4 or 5×10^3 . So there is a possibility that the present He^3 concentration is larger than the He^3 concentration in the solar wind say a few million years ago. This can be interpreted also as due to turbulent transport and in fact we have two ways of estimating the rate at which the turbulent transport takes place. One is by considering the rate at which the He^3 concentration increases with time at the surface of the sun, and the other one is the absolute value of the present abundance of He^3 at the surface of the sun, if it is assumed that He^3

is being produced at the center by thermonuclear reactions. Now, this represents one of the possibilities for testing the turbulent transport from the center to the surface. And just from orders of magnitude we also obtain a turbulent diffusion coefficient $d \sim 10^3$.

C^{13} is also interesting because if we take the earth abundance ratio $C^{12}/C^{13} \sim 80$, do we observe in the sun the same or possibly a smaller ratio? This cannot be considered as settled. Suppose $C^{12}/C^{13} > 80$ can be explained by C^{13} burning at the center of the sun because the C^{12}/C^{13} ratio in the carbon cycle is about 4. This is an increase and seems to go the other way around, but we have to remember that the carbon is essentially turned into nitrogen during the carbon cycle, which means finally the destruction of carbon in favor of nitrogen and consequently a greater destruction of C^{13} than C^{12} . If the ratio is larger than 80, this could possibly give an indication of the presence of turbulent transport from the center to the surface of the sun. I don't mean at all that this is a demonstration which has taken place because as you can judge, there are a number of difficulties concerning the initial abundances which are present.

COMMENTS

A. Ingersoll I want to discuss the question of whether the oblateness measurements that *Dicke and Goldenberg* [1967] made do indicate that the core of the sun is rotating rapidly, or whether there is an equally attractive alternate possibility. Dicke and Goldenberg looked at the shape of the sun in visible light, and there are really three ways that the sun might look oblate in visible light. The first possibility is that the equipotentials, gravitational plus centrifugal, are oblate, which would be the case if the interior of the sun were rotating rapidly. The second and third are variations of the possibility that the solar equator is somehow hotter than the poles. If the equator were hotter, it would also be brighter, and this might be confused with an oblateness because of the limitations of seeing in the earth's atmosphere.

I divide this hotter-equator possibility into two categories because the first of these, the one considered and rejected by Dicke and Goldenberg, is that the equator of the sun is hotter at all depths by a certain amount of ΔT . This would be like saying that the equivalent temperature of the sun is greater at the equator than it is at the poles, or that the radiant flux is greater at the equator than it is at the poles. Their measurements suggest that this is an unlikely possibility, although I do not feel that it can be conclusively ruled out.

The second possibility, which Spiegel and I have proposed [*Ingersoll and Spiegel*, 1971], is that the equator of the sun is hotter only in the chromosphere but not in the photosphere. This possibility is much easier to confuse with a real oblateness. To show why this is so, I must digress to define certain aspects of the Dicke-Goldenberg experiment. They took an image of the sun and projected it onto a perfectly circular occulting disk, slightly smaller than the solar image. The radial angular distance from the edge of the disk to the mean solar limb is δ , and they did their experiments for $\delta \approx 6.5''$, $12.8''$, and $19.1''$. In each case, they scanned around the edge of the disk, measuring all the light that was coming from beyond the occulting disk, and looked for an increase in flux at the equator relative to that at the poles. This difference in flux is the signal they used to infer the solar oblateness. The important thing about this quantity δ is that for each of the three possibilities that I mentioned earlier, there is a different relationship between signal amplitude and δ .

First, if the sun is truly oblate, then the signal is approximately independent of how much sun is in the field of view, and therefore, the signal amplitude is proportional to δ^0 — that is, independent of δ . In this case the signal simply depends on the difference between the equatorial and polar radii of the sun, and not on how much sun is occulted. Next, if the equivalent temperature of the sun is greater at the equator than at the poles, then the signal amplitude is proportional to the fraction of the solar disk in the field of view — that is, to δ^1 . From the data taken at the three values of δ , Dicke and Goldenberg

concluded that this was very unlikely. What Spiegel and I pointed out is that if the equator is hotter than the poles, but only in an optically thin part of the sun's atmosphere, then the dependence on δ is intermediate between these two and is proportional to $\delta^{1/2}$. Here we postulate an equatorial temperature excess in parts of the sun's atmosphere that can be seen even on the extreme limb – that is, in the very top of the photosphere and in the chromosphere. In this case, each emitter in the field of view contributes as much to the signal as any other, and the number of emitters in the field of view is simply proportional to the solar surface area exposed from the edge of the occulting disk to the limb, and this is proportional to $\delta^{1/2}$.

Figure 1 is our reworking of the Dicke and Goldenberg data. We have plotted signal

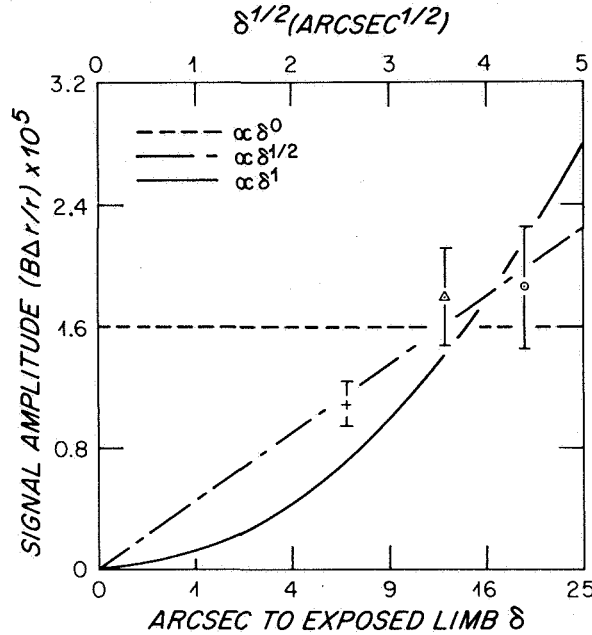


Figure 1 Signal amplitude versus $\delta^{1/2}$ after correction for surface rotation. Units are $B\Delta r/r$, where B is relative brightness at the occulting disk, and $\Delta r/r$ is measured oblateness [Dicke and Goldenberg, 1967]. Error bars give the square root of variance for each δ . Curves illustrate three possible dependences on δ .

amplitude versus $\delta^{1/2}$, for $\delta = 6.5''$, $12.8''$, and $19.1''$, which are the three values of δ used in the experiments. The three lines drawn represent the three possibilities: signal amplitude $\propto \delta^0$, $\delta^{1/2}$, δ^1 . Actually, the signal due to a true oblateness would not be exactly $\propto \delta^0$, but would depend on the brightness at the edge of the occulting disk, and this brightness increases slightly with δ . So a true oblateness is consistent with these data. Dicke and Goldenberg ruled out the parabola, signal $\propto \delta^1$. The curve shown corresponds to $\Delta T_e \approx 5^\circ \text{ K}$ – that is, to a 5° excess in the equivalent temperature of the sun at the equator relative to that at the poles. Obviously, it would be very interesting to measure that somehow – I suppose by sending a satellite over the poles. The line on the graph labeled $\delta^{1/2}$ corresponds to what Spiegel and I suggested, with

$$\tau_0 \Delta T \approx 0.3^\circ \text{ K}, \quad \tau_0 \ll 0.1$$

Here ΔT is the required temperature difference between equator and poles, which is restricted, we assume, to an optically thin layer. And τ_0 is the value of the optical depth at the level below which this temperature difference is assumed to vanish. The restriction $\tau_0 \ll 0.1$ simply ensures that this layer is optically thin. Examination of figure 1 shows that this possibility fits the Dicke and Goldenberg data quite well.

Now if Spiegel and I are correct in our interpretation, and if the chromosphere really is hotter at the equator than it is at the poles, the heat source for the equatorial chromosphere must be greater than the heat source for the polar chromosphere by a specific amount. This excess mechanical flux upward at the equator must be whatever is necessary to supply the excess emission implied by the relation $\tau_0 \Delta T \approx 0.3^\circ \text{ K}$. The required excess flux is $\Delta F \approx 2.5 \times 10^7 \text{ ergs/cm}^2/\text{sec}$, which is comparable to what many people believe is the total mechanical and hydromagnetic energy flux into the chromosphere. So if our interpretation is correct, then we have to be prepared either for a mechanical heating of the chromosphere, which is larger than what most people believe, or a variation in this heating from equator to pole, which is comparable in magnitude to the heating itself.

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DISCUSSION *R. H. Dicke* There are three points I would make. First, the question was raised as to whether a general temperature difference of the photosphere between the equator and the pole could account for the observations. The measurements were made with three different amounts of limbs exposed, which lead to a light flux ratio of approximately 1.0 to 2.5 between the smallest and the greatest amount. Under an oblate sun hypothesis these two signals have a ratio of about 1.0 to 1.2 and when we renormalize (correct the signal of the biggest exposure by a factor of 1.2 downward), the observations are satisfactory. I can't believe that they would be satisfactory if we had reduced the signal by a factor 2.5. There would then be a sizable discrepancy in those three curves. I don't think that's possible.

On the question of a hot layer, I think one must go far above an optical depth of 0.1 to make the scheme work. For levels above 0.01 you need at least a 40° temperature difference between the equator and the pole. For this case, I think that the signal could be sufficiently close to what we observed that this might be a satisfactory way of accounting for the signals. On the other hand, one has to make a physically reasonable statement. There are two requirements to be satisfied. One is the requirement of energy balance for the necessary steady state – the problem of getting excess energy at the equator into the particular layer, the upper photosphere, to heat it up enough to give the excess radiation. And the other requirement is one of dynamic balance for the necessary steady state. There may be several ways this can be done; the one that's been suggested by the authors, which is to require that the angular velocity increase outward in the upper photosphere with a scale height of about 1,500 km, may well be in difficulty with what is known observationally about the rotation of the sun at various levels. So I would say that insofar as the observations are concerned it is possible that one could account for them in this way, but I haven't seen a coherent physical statement of how such a physical state would be maintained or dynamically balanced.

A. Ingersoll The first point Dicke raised was that he didn't feel that the data could be consistent with a temperature difference between equator and poles that extended

deep into the atmosphere of the sun. Now, that really hinges on whether you feel that the parabola can be made to fit the three data points, the parabola being the solid line in the graph I showed earlier.

R. Dicke I don't know how you got these points. The paper didn't list them — the paper didn't even give the normalization ratios that you would have had to know to compute these points; the ratios weren't in the paper.

A. Ingersoll We assumed that the values of δ and the values of the photospheric brightness at the edge of the occulting disk were those which you gave in your paper. We used the limb darkening curve you gave in your paper —

R. Dicke We didn't give a limb darkening curve.

A. Ingersoll Well, not in *Dicke and Goldenberg* [*Phys. Rev. Letters*, 18, 31, 1967], but in *Dicke* [*Ap. J.*, 159, 1, 1970] from which we took these values.

R. Dicke But those were not observations, but a theoretical limb darkening curve from a theoretical paper.

A. Ingersoll Let me put it this way: All the data we got for making this graph came from various papers you have written; we consulted no others for this.

Now, the second point, I guess, was the question of the dynamical balance. If we are to accept the fact that the parabola does not fit the data, then the temperature difference between the equator and pole is concentrated only in the chromosphere, and it is true that you need to balance the forces implied by this horizontal temperature difference. The most likely way is that angular velocity should be increasing with height. We calculate that if angular velocity increases by ~ 5 percent in 100 km over some 100-km region near the temperature minimum, that would be enough. So there's another observation that should be made in order to test this observation.

E. Schatzman There is a very well-known solar oblateness in the meter wavelength that corresponds to a structure of the corona, but very high in the corona. The oblateness is considerable. So might there be a relation between your assumption concerning the chromosphere and what has been observed at meter wavelength?

R. H. Dicke It seems to me that the postulate of the increasing angular velocity does fit observations; that is, one sees angular velocity increase with height in the chromosphere. The sign is correct for the chromosphere and consequently may be correct for the upper photosphere where the balance is actually needed if the upper photosphere is to be extended on the equator with a higher temperature. So it's not a question of whether the idea is qualitatively wrong but whether in fact it is quantitatively right. (Ed. note: See comment by Livingston, p. 304).

COMMENTS

C. P. Sonett We have carried out extensive calculations regarding a mechanism for early electrical heating of meteorite parent bodies with the view to obtaining clues about the early solar system especially the question of the pristine solar spin rate and evolving conditions in the solar nebula just after condensation of the primary objects. The proposed mechanism and the calculations which have been carried out are based upon the following observational evidence. Certain classes of meteorites, particularly the iron-nickels and achondrites, has been exhaustively studied for evidence of cooling from elevated temperatures [*Wood*, 1964; *Goldstein and Short*, 1967]. The iron-nickels show evidence for cooling rates which range approximately from $1-10^\circ$ /million years indicating that at the time of the cooling cycle these objects were at depths within parent bodies to several hundred km radius. Some error might accrue in these estimates on the basis that for the nickel-irons the diffusion of Ni across grain boundaries between kamacite and taenite, both of which are Ni-Fe phases, varies from the values used because of "doping" of the matrix by trace elements which can adversely affect diffusion coefficients. However,

the basic phenomenon cannot be avoided by this argument; only the rates can be modified, which means that the parent body sizes would have to be adjusted. On the other hand, it has been argued that because Si grains are found within a metallic matrix, that primordial condensation is required to form the meteorite bodies. Here we assume that the parent body heating mechanism is correct. There are compelling reasons for believing that, for example, the Widmanstätten patterns in the irons could only be produced by a well-behaved cooling from an elevated temperature.

The time setting for the cooling cycle is early in the chronology of the solar system. This is established, at least for Weekeroo Station, by *Wasserburg et al.* [1965], who dated Si inclusions as about 4.6 billion years old. Thus, at least on this basis, the heating and cooling episodes are very early. To explain a heating episode for parent bodies of the restricted sizes postulated, since the event appears to have taken place very early, requires either fossil radionuclides or some exotic form of heating. Long-lived radioactives are ruled out because their energy-deposition rates are too low for the short time scales proposed. Similarly, accretional heating released by the potential through which objects fall in accreting would be ruled out because of the small size of the bodies and the small gravitational energy *Sonett* [1969].

The classical means of heating of parent bodies has been based on a class of extinct isotopes thought to have been present during the formative period of the solar system. That such isotopes were present is clear from both the presence of Xe^{129} from the decay of I^{129} , Xe components from Pu^{244} fission decay and the appearance of fission tracks in meteoritic matter. Although the existence is verified for these cases, the speculated level of activity assignable to these isotopes is far below that required for the heating cycle. Other nuclides have been popular candidates in the past. Perhaps the most prevalent has been Al^{26} hypothesized to have arisen in spallations associated with the early sun. However, the most recent tests show no evidence for this isotope [*Schramm et al.*, 1970], and thus the hypothesis is not well supported.

In view of the lack of strong evidence for radioisotopic heating, the study of the fossil residues remains a fundamental requisite for understanding of the cosmochemical formative processes leading to the condensation into material bodies, but the source of the heating cycle appears to require a separate explanation.

It seems likely that the early sun was spinning rapidly and that it was endowed with at least a modest magnetic field. These conditions arise quite naturally from the trapping of field in the Hayashi contraction and the spinup due to condensing angular momentum from the primordial cloud. If we associate the contractive period with the precursor phase of an early star prior to a *T* Tauri efflux of mass, then conditions are quite naturally established for the establishment of strong electric fields in the expanding cloud, a result of the combination of high spin, magnetic field, and plasma outflow [*Sonett et al.*, 1970].

The conditions just described can lead to strong electrical currents flowing through planetary objects, the circuit being completed through the surrounding "solar wind." Electromagnetically the interaction is classified as transverse magnetic (TM) and has been discussed extensively in the literature [*Sonett and Colburn*, 1967; *Schubert and Schwartz*, 1969]. Its application to the present cases, forming in effect a linear unipolar generator, requires that the electrical impedance along the current streamlines through the body be sufficiently small so that strong currents can flow. On the other hand, too low an impedance will result in the formation of a steady-state magnetohydrodynamic bow shock wave ahead of the body facing into the direction from which the flow of plasma comes.

To provide an appropriate impedance, we invoke the well-known exponential dependence of the bulk electrical conductivity of rocky matter on the reciprocal temperature.

Extensive calculations have been made involving parameterization of the problem. Significant heating due to Joule losses from the current system are found. It is clear that because the currents close through the surface of the body that the crustal temperature is a crucial aspect of the heating, and that a sufficiently elevated temperature is required. To provide this it is only necessary to consider further the general properties of *T* Tauri objects, which are often endowed with an infrared excess attributed to dust-induced opacity. We term the enclosure a *hohlraum* and invoke an interior surface temperature to this enclosing matter; thus, the planetary object "sees" a background temperature sufficient to maintain an adequate bulk electrical conductivity [Sonett *et al.*, 1970].

Although this all may appear as unduly complicated, the effects required appear to be commonly hypothesized or observed in what are thought to be early stars. Their parametric association, numerically adjusted to provide significant heating, has shown that only quite modest requirements must be placed on the system to provide the heating cycle.

We now turn the problem around to discuss the spindown issue. *It is clear that a rapidly spinning sun must eventually be braked so that the present epoch spin rate is achieved* [Durney, Chap. 4, p. 282]. Although the calculations referenced use an exponentially decreasing field and magnetic braking, some other shaping of the field decay is equally appropriate and angular momentum can also be shed by the outflowing gas. Thus, in the present calculations, the field and spin damping are represented in an integral sense only, and the instantaneous rates cannot easily be determined. However, the evidence is strong that some form of heating other than fossil nuclides is required if the heating cycle continues to be maintained as a viable requirement.

The electrical problem is complicated by the additional presence of a TE (transverse electric) mode of interaction, which simply stated is due to eddy current generated from the action of \dot{B} , the time rate of change of the interplanetary magnetic field seen in the frame comoving with the planet [Schubert and Schwartz, 1969]. The tendency would be to associate this mode more with turbulence in the outflowing gas which in turn is reflected in magnetic field disturbances. This mode also has the simplification that the *hohlraum* is not required as the current system is toroidal closing wholly within the planet. Calculations are in progress to determine the efficiency of the TE mode and the coupled action of both the TE and TM modes together with a modest addition of radioactives, which are known to inhibit the later stages of the heating by the TM mode.

Figure 1 shows the heating of small bodies as a function of their radius. The peak temperatures are achieved in times of the order of 0.5 million years for the larger cases,

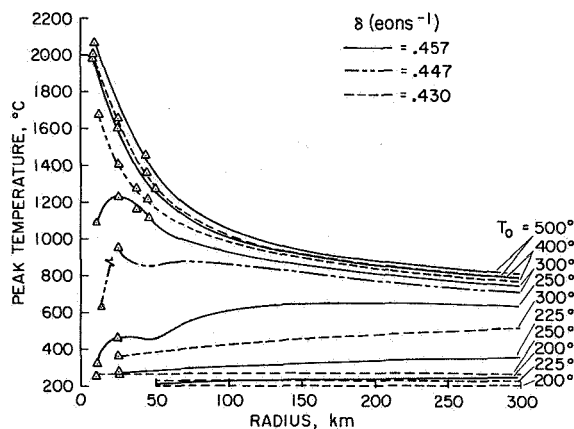


Figure 1. Peak core temperature versus parent body radius.

while for the very small objects of 10- to 25-km radius the peak heating of the core is achieved in a much shorter time, so that a relaxation begins to take place and the interior cools long before the overall induction process is completed.

The clue to the high solar spin rate is carried in the requirement for the large electric field at the site of the parent body which then leads to the large induction. The very substantial electric field in turn is strongly dependent upon the winding up of the interplanetary field into a tight Parker spiral because of the large spin. Thus, the evidence for early heating of parent bodies leads quite naturally to the condition where the sun was endowed with a high spin rate during its early history. Although the specific spin rate at a given time cannot be presently foretold, it is clear that the elements of the overall theoretical treatment leading to better understanding of this are intrinsically carried in the thermal history of these small bodies, provided that electrical heating proves to withstand the test of further examination.

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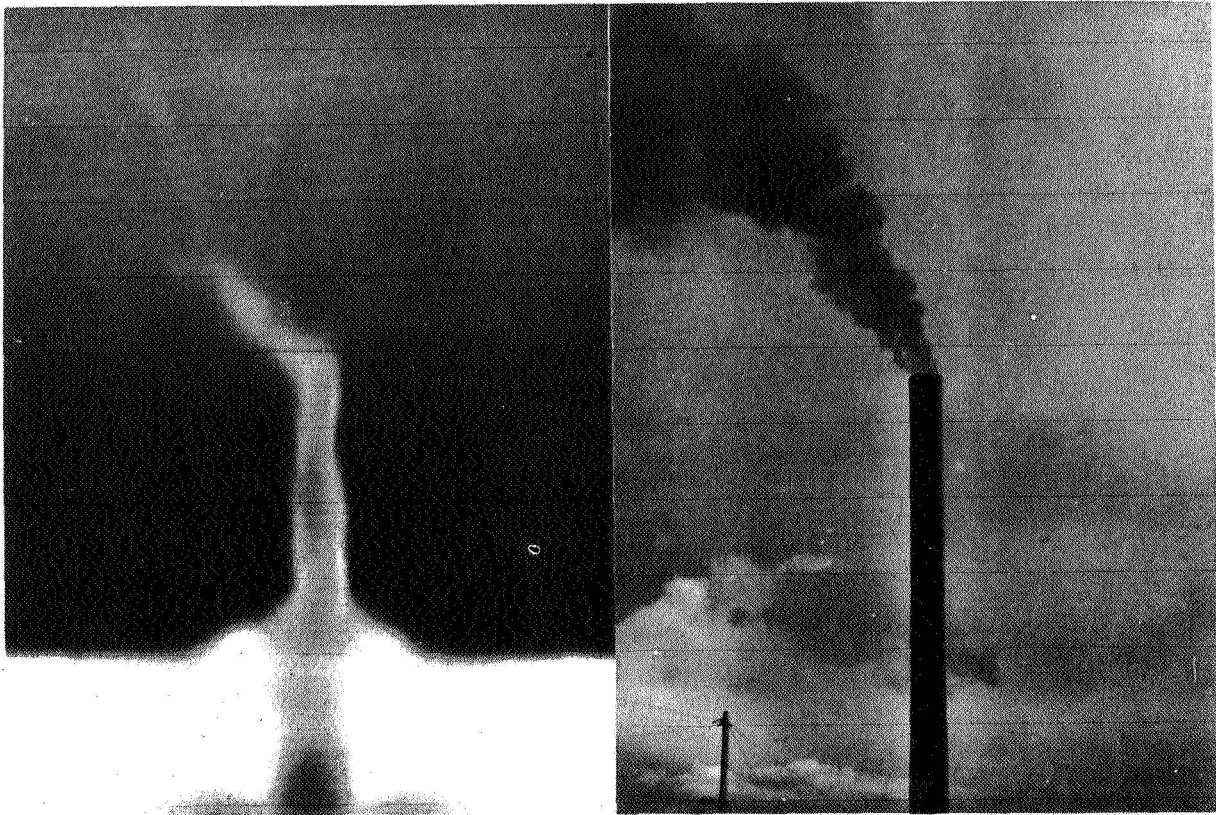
COMMENTS

W. C. Livingston Spectroscopic observations made on the solar disk near the equatorial limbs consistently indicate an increase of angular velocity as we pass outward through the sun's atmosphere. The chromosphere, as revealed by $\text{H}\alpha$, appears to rotate 5 to 8 percent faster than the underlying photosphere as represented by the metallic lines [*Livingston*, 1969]. Because manifestations of magnetism such as sunspots, filaments (or prominences), and plages corotate with the spectroscopic photosphere, it has been suggested that we are observing in $\text{H}\alpha$ the "superrotation" of neutral matter that can flow independent of magnetic constraints. (The term *superrotation* is borrowed from aerodynamics where it is used to describe an analogous condition in the atmosphere of the earth and Venus [*King-Hele*, 1970; *Gierasch*, 1970].) As a working hypothesis we propose the existence of an east to west wind whose lower boundary is the photosphere and whose upper extent is unknown.

Seeking additional evidence of this superrotating wind, in 1968 we began to obtain

prominence spectra. The structure of prominences undoubtedly is dominated by magnetic forces, so one would not expect to find any degree of superrotation in these objects. However, some early work by *Evershed* [1935] suggested some peculiarities in their spectroscopic rotation rates.

Our spectra are taken with the slit placed normal to the limb and generally at a position angle such that the height of the $H\alpha$ emission is at a maximum. Records are taken in both $H\alpha$ and Ca^+K . Figure 1 illustrates a phenomenon often found on our spectra. At the top of the line, corresponding to the upper edge of the prominence, the weakened emission typically becomes diffuse and exhibits an abrupt displacement in wavelength, indicating line-of-sight motions differing from the main body below. By analogy with smoke escaping from the confines of a stack, we picture gas escaping from the magnetic confines of a prominence to be picked up and accelerated by a wind. Indeed, in the majority of cases within our limited sample this displacement is in agreement with an east to west superrotating wind [*Livingston*, 1971]. Further examples are



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Figure 1. Spectrum of solar prominence in Ca^+K 3933, slit perpendicular to the limb. Magnetic constraints of the main body of the prominence is analogous to the smoke stack with gas escaping at the top and caught up in the prevailing wind.

given in figure 2. The magnitude of the displacement ranges from a few km/sec to as much as 50 km/sec.

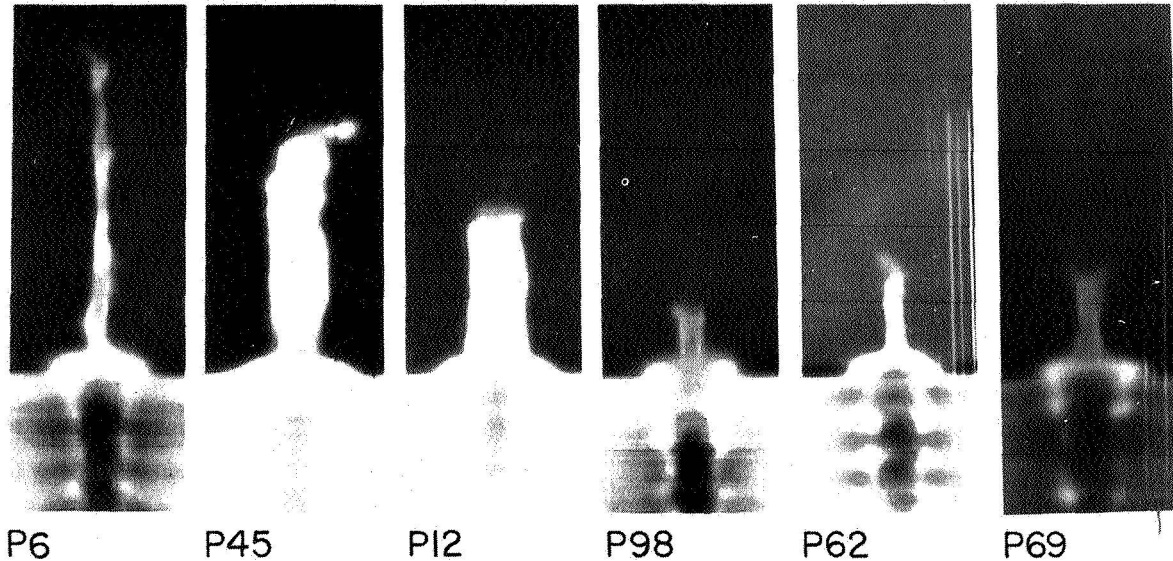


Figure 2. Examples similar to figure 1 taken in both $H\alpha$ and Ca^+K . In all cases (except the ambiguous last) the Doppler displacement of the upper emission is compatible with a hypothetical east-west wind.

In summary, both disk and prominence spectra suggest the existence of systematic east to west flow patterns at the chromospheric level. Whether or not such a superrotating surface wind of neutral gas would interact with the solar wind plasma remains to be studied. We can note that any interaction would be in the forward direction and counter to the backward drag of the Archimedes spiral.

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