THE MIXING OF LITHIUM*

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1. Introduction

The observational status of the problem of lithium abundances has been thoroughly detailed in the preceding papers in this symposium, and it is clear why we must consider how matter is mixed from outer convection zones to inner, hotter regions. The need for appropriate mixing mechanisms has also been nicely brought out by Herbig and Wolff (**1**), and Böhm (**2**) has summarized the role of convective mixing. Conventional mixing-length models for the outer convection zones seem to give qualitatively reasonable results for the depletion during pre-main-sequence contraction (**3, 4, 5**) but do not completely account for the observations, and it seems inescapable that main-sequence depletion of lithium must be considered (**1**). I shall therefore simplify the discussion of mixing by concentrating on main-sequence models in the following outline of some possible mixing processes, though most of the remarks to be made should apply generally to other phases. I shall also pretend that there is one principle mechanism (or combination of them) that must be found, though stars in different evolutionary phases, or with different masses, may deplete lithium quite differently. Further, I shall use the Sun as an illustration in general since we know some important details about it that are not always known for other stars.

As to the mixing mechanisms themselves, I shall attempt to organize the discussion by considering four problems. These are:

(1) the depth of outer convection zones,
(2) the penetration of convective motions into radiative cores (overshooting),
(3) the effects of rotation and rotational braking, and
(4) the effect of mass loss.

The first two of these are crucial no matter what the ultimate mixing mechanism is, since they determine the depth to which material is mixed by ordinary convection. In this way they fix that the additional distance material must be carried before lithium is effectively destroyed. The other two are rather uncertain since they depend on incomplete theories or uncertain observational results, but it is certainly worth noting their possible relevance.

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2. The Depths of Convection Zones

The structures of convection zones have been carefully calculated using Mrs. Böhm-Vitense's (**6**) version of mixing-length theory for the relevant spectral types (**7**). In this approach the bottom of the convection zone is the depth at which the star becomes locally stable against convection according to the Schwarzschild criterion (**8**). The familiar uncertainty about the choice of mixing length is normally resolved by adjusting the mixing length so that the stellar radius comes our right when complete stellar models are calculated. This does not necessarily guarantee a correct value for the depth of the convection zone, but it at least makes the models seem reasonable.

Even granting that the mixing length has been chosen correctly, we must allow for some smaller uncertainties in the calculations. The depth of a convection zone is quite sensitive to various parameters, and the work of Weymann and Sears (**9**) indicates that an increase in opacity causes a deepening of the convection zone. Dr. Baker has recently pointed out (private communication) that ionization of heavy elements can also make a small but non-negligible correction to the depth of the convection zone, and this factor is not always carefully taken into account. And finally the use of *mixing-length theory makes our estimates imprecise because it determines the bottom* of a convection zone on the basis of linear stability, whereas the indications from finite amplitude stability theory are that the actual boundary of a convective zone is somewhat displaced when one studies the effects of finite amplitude perturbations (**10**).

Now in the pre-main-sequence mixing these small effects probably are not crucial since the bottom of the convection zone passes well into regions where the lithium can be destroyed (**5**) and no qualitative changes are likely to be brought about by small changes in the thickness of the convection zone. Likewise, in post-main-sequence lithium depletion where the main mechanism seems to be convective dilution of lithium (**11**), the qualitative picture does not depend on such small corrections. But in main-sequence lithium depletion the location of the bottom of the convection zone is crucial, for the following reasons.

First, there is the difficulty that if the temperature at the bottom of the convection zone approaches 3×10^6 °K, lithium is destroyed in a time rather less than the main-sequence lifetime of late stars. A mechanism of this kind is not what is called for by the evidence for slow depletion or the main sequence. On the other hand, if the convection zone is too shallow the (presumably) slow mixing process which takes material below the convection zone will have too long a time-scale since the time for the mixing is probably either linear or quadratic in the geometrical distance between the bottom of the convection zone and depth at which lithium is destroyed. This factor makes the depth of the convection zone an important parameter.

At present, estimates of the depth of the convection zone in the Sun range from $1–3 \times 10^5$ km, if we include rumors about unpublished determinations. Not all of these possibilities have been used in complete solar models so that they may not all give agreement with the observed solar radius. Nevertheless, the spread in estimated depths is large, and we may ask whether solar observations can help to settle the problem.

One possible check is that of the observed motions in the solar convective zone. The granulation and supergranulation are generally believed to be convective motions and their horizontal scales are 2×10^3 and 3×10^4 km respectively (**12, 13, 14**). The granule scale is probably determined by the thickness of the transition layer between the stable photosphere and the adiabatic layers in the deep convection zone, or at least by the scale-height in the upper convection zone, which is closely related. But what determines the scale of the supergranules? One possibility is that it is related to the thickness of the convection zone (**13, 15**). Though the nature of the convection remains unclear, a discrepancy of a factor 2π is not overly surprising. The picture proposed by Simon and Weiss (**16**) of motions extending over several scale-heights seems quite reasonable and may explain the supergranule size. On the other hand, it may be that the supergranulation scale is a manifestation of a boundary layer at the bottom of the convection zone analogous to the upper transition layer. Standard mixing-length theory does not show such a boundary layer, since it does not take into account the slowing down of eddies as they approach the bottom of the convection zone. But Böhm and Stückl (**17**) have attempted to correct this deficiency by setting the mixing length equal to the distance from the edge of the convection zone when this distance is less than the local scale-height. This probably overestimates the effect of the boundary a bit, but it gives a reasonable indication of the existence of a boundary layer at the bottom of the convection zone. (A similar but weaker effect is produced when account is taken of deviation from radiative equilibrium (**18**).) The calculation of Böhm and Stückl leaves the correlation between vertical velocity and temperature fluctuation as a free parameter. When this parameter is $\frac{1}{2}$, the depth of the convection zone is $1{\cdot}55 \times 10^5$ km and the bottom boundary layer has a thickness of 3×10^4 km. This suggests a rough agreement between current estimates of the convection-zone depth and the observed scale of supergranules, if it is true that the lower boundary layer does set the horizontal scale of supergranulation.

Another possible check on the calculation of the thickness of convection zones comes from theoretical and experimental results on the motion of a spherical shell of rotating fluid. When the shell is contained between rigid, concentric spheres rotating at slightly different speeds rather complicated motions develop. These are not completely understood even for the homogeneous fluid in which viscosity is important only at the boundary layers. However, it is known that in this relatively simple case two important regions may be distinguished (**19, 20, 21**) by constructing a cylinder coaxial with the rotation axis and tangent to the inner sphere. The cylinder divides the spherical shell into polar and equational regions, as shown in Figure 1. In the polar regions a circulation as indicated in the figure is found; in the equatorial regions no motions occur in the steady state.

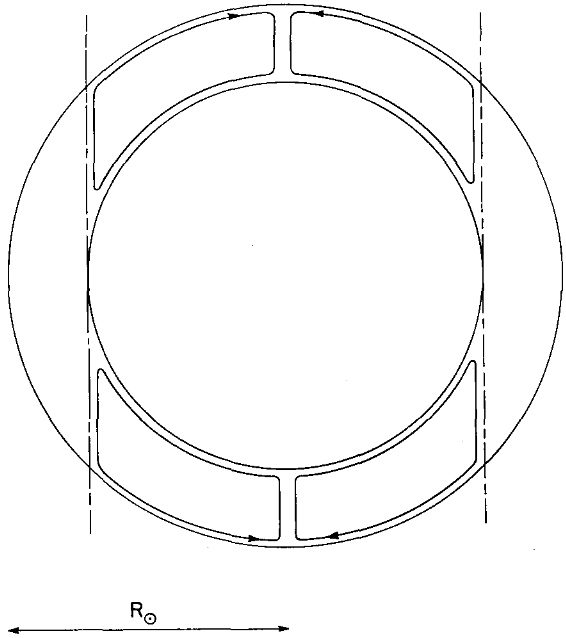

FIG. 1.

Now the solar convection shows a crude analogy to this configuration. The inner stable layers respond to penetrative motions more slowly than the convective zone responds to imposed disturbances such as those on its outer edge resulting from the torque of the solar wind. Moreover, since the bulk of the convection zone is adiabatically stratified, some of the approximations required in the theory of the homogeneous incompressible fluid are applicable. On the other hand, because of turbulent viscosity and the slight degree of instability, we cannot expect the analogy to be really good. Nevertheless, it has been remarked **(22)** that the extreme latitude achieved by sunspots does seem to coincide with the latitude at which the cylinder of Figure 1 cuts the outer sphere if solar parameters are used. That is if d is the thickness of the convection zone, it is related to this critical latitude by

$$d = R_{\odot}(1 - \cos\phi),$$

and with $\phi \simeq 45°$, $d \simeq 2 \times 10^5$ km. This would seem to provide support for the present estimates of the thickness of the convection zones, but until we go farther with the theory of circulation in convection zones, and in particular the effects of stellar wind torques, we can draw only slender comfort from this test.

In sum, the mixing-length theory seems to give qualitatively reasonable but not sufficiently precise estimates of the depths of convection zones.

3. Penetration of Convective Motions into Radiative Zones

The foregoing discussion of the depths of convection zones assumed that the motions stop where the temperature gradient switches over from super- to sub-adiabatic. But now we must confront the possibility that descending motions originating in the unstable layers do not stop at the bottoms of convection zones but continue an unknown distance farther. In effect this displaces the bottom of the convection, or mixed, zone inward and makes it fuzzy. We have as yet no reliable way to deal with this phenomenon and thus far two main approaches have been considered, one using blobs the other convective cells as the agents of convective transport.

In the blob approach, one proceeds as in mixing-length theory with a blob of fluid starting out in near equilibrium with its surroundings. The blob arrives at the edge of the convection zone with a calculable kinetic energy, and the question can then be asked, What is the distance to which the blob can penetrate into the stable layers before it is brought to rest by the buoyancy forces? In this estimate one temporarily suspends the restriction that an element can be destroyed by turbulence, presumably after it travels a distance comparable to its own diameter. This kind of estimation of the effects of penetration was used by Mrs. Böhm-Vitense (**23**) to discuss granulation in her original mixing-length paper. The same kind of treatment was used by Weymann and Sears (**9**) in relation to the lithium problem. In effect, the results in this way are equivalent to the argument in which the blob is assumed to penetrate to the point where the local entropy is the same as that in the region where the blob originated (**24**). Moore (**25**) has pointed out that the entropy argument is unreliable because it omits entrainment of fluid by the blob and because it neglects the momentum of the blob. The former causes an underestimate of penetration, the latter an overestimate. A phenomenological theory taking account of these effects has been given by Morton *et al.* (**26**) and Scorer (**27**), but only when the diameters of the blobs are much less than the scale-height.

For the present, though, the best available estimate seems to be that of Weymann and Sears. They find that in the solar case only those blobs which form in the upper, highly unstable, convective layers can penetrate deeply enough to cause appreciable mean destruction of lithium. However, if the diameters of the blobs are set equal to their free path for turbulent disruption, and if the blobs have initial diameters comparable to the scale-height in their region of origin, the probability that they can reach the bottom of the convection zone is much too small for appreciable lithium destruction.

It must be stressed, however, that Weymann and Sears used a model in which there is no lower unstable boundary layer such as that found by Böhm and Stückl (**17**). On the other hand, the lower unstable layer of Böhm and Stückl is calculated on the basis that no penetration is possible, and it would not be correct to use their model directly for an estimation of penetration, though one could iterate their model for this purpose as follows.

Böhm and Stückl used the distance from the bottom of the convection zone as a mixing length when this distance is less than a scale-height. If we now use the entropy argument (or an improved version according to Morton *et al.* (**26**)) to estimate a new penetration distance arising in the Böhm-Stückl model, we can define a new effective bottom of the convection zone. We could then recalculate the structure of the convection zone using the distance to this new bottom as a mixing length. In this way, it is possible to iterate till a self-consistent penetrative model is achieved. But until some such calculation is performed, we cannot draw any conclusions from the present blob calculations.

In the other approach to the penetration problem, one applies the equations of motion, treating small perturbations about the average conditions calculated on the basis of mixing-length theory (**28, 29, 30, 15**). The resulting equations can be solved numerically and the relative amplitude of the velocity field at different depths can be found for various modes. The absolute amplitudes can be established in the solar case by adjusting to the observed surface velocities. Böhm (**29**) has found that this approach would lead to the conclusion that lithium must have been wholly destroyed in the Sun. Of course, this is contrary to observation if we assume that lithium is not presently being produced in the outer solar layers, and the implication of the laminar mode approach would be that all observable lithium in stars as late as the Sun has been recently produced. This conclusion seems unacceptable and the fault must lie in the linear theory.

The reason that linear theory gives an overestimate of the penetration is that it takes full advantage of the upper unstable layer without suffering the turbulent losses inherent in the blob picture. Laminar modes can extend throughout the convection zone and below the zone their amplitude drops roughly exponentially in depth with a penetration distance comparable to their horizontal scale. Thus they are continually driven by the upper unstable layer. It is possible to remedy this failing by means of eddy coefficients, and Professor Böhm is attempting to do this. For the moment though, we have no conclusive results from linear theory which relate to lithium depletion.

In summary, it is not at all clear whether penetrative convection can be the main mechanism involved in lithium mixing. The estimates that do exist make it either too effective or negligible while what is needed is a slow but definite process. Before we can decide finally on the prospects for penetrative mixing we need to know not only the depth of penetration but also the sharpness of the cut-off at the lower limit reached by penetrative motions. It would seem that only if there is a spread in the depths to which motions from a given convection zone can penetrate can we attribute lithium destruction to penetrative mixing; otherwise the penetrative mechanism is an all or nothing proposition and cannot explain the observations.

It may also be that penetrative convection plays an important part in lithium depletion even though it is not the ultimate motion that carries lithium to its destruction. Thus, it is important to know at each spectral type the lowest depth to which penetrative motions extend.

4. Rotational Processes

If we allow for the effects of rotation in the lithium-mixing process, we can find a variety of mechanisms which may be relevant. The most natural of these to consider is probably the Eddington-Sweet current (**31**), which results from the stellar deformation due to rotation. Crudely speaking, this deformation produces a pole to equator-temperature variation, hence a horizontal pressure gradient. Motions are thereby established and these permeate the star, if no gradient of molecular weight intervenes. The circulation time for the Eddington-Sweet current is

$$t_{\mathrm{E.S.}} \cong S t_{\mathrm{K.H.}},$$

where $t_{\mathrm{K.H.}}$ is the thermal (Kelvin-Helmholtz) time of the star and

$$S = \frac{G\bar{\rho}}{\Omega^2},$$

where $\bar{\rho}$ is the mean density, Ω is angular velocity and G is the constant of gravity. In the Sun, $\rho \approx 1$ gm/cm^3, $\Omega = 3 \times 10^{-6}$ sec so that $S \approx 10^4$ and $t_{\mathrm{E.S.}} \approx 10^{11}$ years. Thus it would appear that the Eddington-Sweet currents play no role in lithium depletion in the Sun at present.

However, it is now reasonably well established that the angular velocity of the Sun is at present decreasing with a half-life of about 5×10^9 years as a result of the solar-wind torque (**32, 33, 34**), though this value may have once been lower (see Section 5) and may at present be higher (**35**). In any case, it would seem that the Sun arrived on the main sequence with a somewhat higher angular velocity than at present. If, moreover, the mass-loss rate were initially higher than at present, Ω may have been higher by as much as a factor of 10 and $t_{\mathrm{E.S.}}$ could then have been as low as $\sim 10^9$ years, which means that Eddington-Sweet currents may have played a role in lithium depletion in G stars, at any rate. Unfortunately, we cannot specify precisely what the rate of rotational mixing is without knowing the history of Ω in time, the location of the bottom of the convection zone, and the interaction of the Eddington-Sweet currents with the convection zone. Indeed, these uncertainties are present in the other mechanisms discussed in this section, so that only an indication of the possibly important mechanisms can be given.

In the Eddington-Sweet theory, rotation itself causes a thermal imbalance which gives rise to a slow circulation. But in rotating stars the loss of angular momentum through stellar winds sets up surface stresses which can also give rise to internal circulations. The problem is quite complicated and a number of points of view have been put forward lately. However, there is general agreement that the surface stresses resulting from stellar winds are rapidly transmitted through the outer convection zones, as a result of eddy viscosity. The time-scales on which the radiative cores respond to these transmitted stresses are thus presumed to be longer than those of the convection zone so that the interesting circulations produced in the convection zones need not be considered in detail here.

One suggestion about the response of radiative cores to the external stresses is that the time-scale is so long that the entire radiative core is in relatively rapid rotation compared to the surface layers, in the case of the sun (**36, 37, 38**). For example, Dicke (**36**) proposes a rotation rate in the core which is ten times that in the surface layers. In that case the Eddington-Sweet time is $\sim 10^9$ years, and we would expect appreciable mixing during the Sun's main-sequence lifetime.

However, it has been suggested that the Dicke model is not valid because it is unstable (**39**) and because the surface stresses drive a circulation which would wipe out sharp variations in angular velocity (**40**). Even the Eddington-Sweet currents resulting from the Dicke model would seem to prevent the model from being self-consistent, as Professor Mestel has privately remarked. Hence, what seems to be important here is that the motions involved in the instability and the circulations induced by the solar-wind torque may be important for the mixing problem, though only suggestions of their importance have been published as yet.

The instability of differential rotation as the mixing mechanism has been favored by Goldreich and Schubert (**39**). The instability is essentially that first discussed by Lord Rayleigh (**41, 42**), who showed that a differential rotation is unstable if the angular velocity as a function of distance from the axis, r, drops off more rapidly than r^{-2}. Taylor (**43**) later showed how viscosity could impede the instability. A stabilizing density gradient can also suppress the instability, but as Lieber and Rintel (**44, 45**) noted, conductivity can reduce the effect of the density gradient so that for large conductivity, and small viscosity, the Rayleigh instability, in a modified form, can occur in the presence of stabilizing density gradients. Similar reductions of the effect of stabilizing density gradients by radiative transfer apply in the case of shear flow instability (**46, 47**), Goldreich and Schubert (**39**) have discussed this instability more fully with the solar problem in mind and conclude that the criterion of Rayleigh is not strongly altered in the solar conditions.* They also appear to be the first to attempt the problem in spherical coordinates, which leads them to an additional instability criterion.

Let Ω be the angular velocity and z be parallel to the axis of rotation. Then Goldreich and Schubert find that instability occurs whenever $\mathrm{d}\Omega/\mathrm{d}z \neq 0$. (Actually, this is their criterion in the limit of small viscosity.) This result is rather important since it implies that a very slight variation in Ω can produce instability, whereas the Rayleigh criterion demands a great Ω variation for instability. However, I have some misgivings about the interpretation of this result since it is not based on a precise solution of the steady state of motion whose stability is being tested. Recently Barcilon and Pedlosky

* A related stability study has been recently carried out by Fricke (**65**).

(**48, 49**) have studied the steady state of a stratified fluid to which a $d\Omega/dz$ is being applied at boundaries. A complicated circulation is set up by the applied $d\Omega/dz$ and it seems clear that, especially for large conductivity and small viscosity, the fluid tries to achieve $d\Omega/dz \approx 0$. The proper stability question would seem to be whether the kind of flow found by Barcilon and Pedlosky is unstable. I am not clear whether the instability found by Goldreich and Schubert is in part simply a manifestation of the fact that there is no solution with $d\Omega/dz = 0$, which does not try to have a circulation, so that if the circulation is left out of the steady state, the perturbations are bound to grow. But in any case, it seems clear that when $d\Omega/dz$ is far enough from zero, motions must be set up, and one possibility to consider is the instability discussed by Goldreich and Schubert.

Presumably, the applied stress requires a slight excess of $d\Omega/dz$ over its value for the onset of instability. This excess should be just enough to permit the resulting weak turbulence to transport angular momentum out of the star at a rate demanded by the stellar wind. Thus, in principle we should be able to estimate an eddy diffusivity for angular momentum transport if we know the rotation and mass loss. The diffusivity can then be used to estimate the turbulent diffusion of lithium. This has not yet been done, partly because the whole picture is still controversial (**50**).

I have already mentioned that the applied stresses on the boundary of a fluid can set up an internal circulation. Howard, Moore and I (**40**) have proposed that this circulation is an important feature in the problem of lithium depletion. We discussed the problem in terms of the transient process called 'spin down'. In this process, when a rigidly contained rotating fluid is suddenly subjected to a change in the angular velocity of its container, a boundary layer forms and in turn drives a circulation through the body of the fluid. For a homogeneous fluid, this motion was already discussed qualitatively by Einstein (**51**), and it has been shown that the effects at the boundary alter the internal angular velocity in a spin-down time which is the geometric mean of the viscous time and the rotation period (**52, 53, 54**). If the boundary layer, or Ekman layer, is turbulent the spin-down time for a homogeneous fluid is approximately 160 rotation periods (**55**).

In the stellar case, the fluid is not homogeneous and is not rigidly contained, so that the problem is quite complicated. However, it appears that the replacement of the rigid container by a convection zone only enhances the effects of spin-down. In the first place the exchange of fluid between the convection zone and the interior at the bottom of the convection zone gives a strong coupling between the two regions (**40**). But more important, it appears that because of turbulent viscosity the whole convection zone can play the role of the Ekman layer in pumping the fluid through the radiative core (**56**). The effect of stable stratification in the core, on the other hand, works against the spin-down process.

In a simple model, with a stably stratified adiabatic fluid, Holton (**57**) showed that the effect of spin-down is confined to a layer of thickness $R/S^{1/2}$, where for our

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purposes R is the radius of the radiative core. Since $S \sim 10^4$ for the Sun, the Holton layer is probably too shallow to directly influence the mixing of lithium. But the Holton layer sets up an imbalance which in turn drives a slow circulation in the interior if the effects of radiative conduction or viscosity are admitted. The spin-down time for this process has been studied by Pedlosky (**58**) in the case of a cylindrically contained fluid. When the side walls are thermally insulating, Pedlosky's results indicate that the time-scale for stellar conditions would be just the Eddington-Sweet time. The problem with conducting side walls is more difficult to treat, but Pedlosky found a simple special case which, curiously, gives the same spin-down time as for a homogeneous fluid.

In the spherically contained fluid, the Ekman layer encompasses the whole interior and sidewalls do not really enter, while in the Sun, where the convection zone seems to close the interior circulation, this is even more forcefully the case. Hence the actual stellar spin-down time remains in doubt. Moore and I have been looking at simple models for this process and with Newton's law of cooling have found that the Eddington-Sweet time applies for spin-down. However, Newton's law does not take proper account of the small scales set up in the temperature perturbation in the Holton layer, so that the result with a proper diffusive law may be different. At present we are attempting to study this more difficult case.

The possibility that the Holton layer is unstable must also be considered, since this would seem to give a spin-down time close to that for the homogeneous fluid (**40**). I have already mentioned the difficulty of this stability problem and though we have discussed the problem with experts in this field (especially W.H. Reid) we have not yet been able to reach a definite conclusion. The rather sharp shear layer or Holton layer that is central to the problem may well become unstable, but it may be some time before we can be sure. If the instability does occur we can expect a weak turbulence in late stars and once the instability condition is established, a mixing-length theory can be constructed, as I mentioned earlier. We have already made a crude version of such a theory and it seems clear that the turbulent diffusion of lithium is, on this basis, very dependent on the angular velocity of the star.

From all this discussion we can abstract very little that is definite, but the main point is that the rotational mixing mechanisms certainly give a slow overall circulation and possibly a weak turbulence. Very likely, when late stars first arrive on the main sequence, they have somewhat higher rotational velocities than at the end of their lifetime on the main sequence. It is quite possible that even the Eddington-Sweet time, which characterizes the usual types of circulation and seems to be an upper bound to the time scale for spin-down circulation, is sufficiently short to be of interest in the mixing problem. Much remains to be done on the theory and we are in need of an accurate knowledge of the mass-loss rate in order to evaluate the surface stresses which can drive circulation.

In spite of these uncertainties, the rotational mechanisms seem attractive. In the first place, they suggest a correlation between rotation and lithium abundance and thus suggest an origin for the large scatter in lithium abundance for stars of the same spectral type. In this connection it is interesting that for stars earlier than G2 the dispersion in lithium abundance is much larger than for later stars (**1**). It is also likely that these stars have a greater dispersion in angular velocities than the later ones since they have weaker convection zones, and less rotational braking. By the same token they will suffer weaker surface stresses and their internal circulations will be more weakly driven, hence the observation that their lithium abundances are higher than in later stars also seems reasonable.

Secondly, Conti (**59**) has just reported observational evidence of a correlation between lithium abundance and angular velocity in G stars. Though this does not demonstrate the relevance of rotational mixing, it does suggest that we inquire further into this possibility.

And finally, the spin-down mechanism itself permits a qualitative resolution of the following difficulty. If we require a mechanism that brings material from the bottom of the convection zone to deeper layers, we must be aware that the same motions may drag magnetic-field lines into deeper layers too. Indeed, there is no indication of how deep the mixing will penetrate and we demand only that it go deep enough. On the other hand, it now seems probable that the mean solar magnetic field varies with the solar cycle, and Babcock (**60**) has, on this basis, concluded that the mean solar magnetic field must be confined to shallow layers, since the time-scale for magnetic variation would otherwise be too long (**61**). But with spin-down, another possibility arises.

In the spin-down of a homogeneous fluid, the time for a complete circulation of the material is generally very much longer than the spin-down time, i.e. than the time required to alter the internal angular velocity of the fluid appreciably. Let us suppose that this discrepancy in the two times can also occur in a stratified fluid. Moreover, the alteration of a magnetic field proceeds by a process similar to the vortex stretching, which alters the angular velocity. Hence, even if the solar magnetic field were to extend deep into the Sun, if the solar spin-down time were 11 years, there would be no dilemma, so long as the circulation time which is relevant to lithium depletion is $\sim 10^9$ years. A theory of this whole process is very difficult, as Moore and I have been finding, since the internal motions can enhance the general field, which in turn alters the co-rotation distance of the solar wind and thus alters the circulation in the interior. Whether a feed back oscillation can thus result is not at all clear, but at least there does seem to be the hint of a resolution of the 'lithium-magnetic field paradox' along these lines.

5. Mass Loss

The last possible process I want to discuss is not strictly a mixing process, though it is the most elegant of all. It is the possibility that late stars lose enough mass on the main sequence so that, after a time, the material we see in the surface convection zones has an appreciable admixture of matter that was once well below the convection zone. The mechanism was considered by Weymann and Sears (**9**) following a suggestion of Woolf and by Herbig and Wolff (**1**).

If the structure of the star is not noticeably affected by the mass loss and if the abundance of lithium is sensibly constant down to the depth at which it burns quickly, then the efficacy of this process is governed by the rate of loss of mass and, in effect, the thickness of the convection zone.

Let M_c be the mass in the convection zone and M_1 be the mass in the shell between the bottom of the convection zone (in the sense of Sections 2 and 3) and the depth at which lithium burns. If the lithium abundance is approximately constant down to the depth at which lithium burns, then the mass-loss mechanism does not alter the observations of the lithium abundance until a mass M_1 has been lost, that is until a time t_1 defined by

$$\int_0^{t_1} |\dot{M}|\, \mathrm{d}t = M_1 .$$

Here $\dot{M}$ is the rate of mass loss and I have assumed that the process begins when the star arrives on the main sequence ($t=0$). (Though pre-main-sequence effects could clearly be important, not enough appears to be known to estimate them.) If $\dot{M}$ is constant in time, and we consider the solar case where $\dot{M} \sim -10^{-14}\, M_{\odot}/\mathrm{yr}$ and $M_1 \sim 10^{-2}\, M_{\odot}$, then $t_1 \sim 10^{12}$ yr. Thus, the process would not even have gotten started in the Sun. However, M_1 is highly uncertain and perhaps it is now being overestimated. But it is more likely that, as has been considered (**1, 9**), $\dot{M}$ is variable and was larger when the Sun first arrived on the main sequence.

This latter effect can be crudely estimated if we make use of Kraft's (**62**) recent study of rotation in main-sequence stars. At the time of this writing I have available only his results for the average rotational velocities in the Pleiades and Hyades G stars, namely $V=19$ and 7·9 km/sec. In the Sun, however, $V=2$ km/sec. The ages for these three samples of G stars are $\sim 5\times10^7$, 5×10^8, and 6×10^9 yr respectively. As Conti has remarked (**59**), if we treat these data as representative of the time dependence of G-star rotation, we see the half-life of the rotational velocity is time-dependent. Nor is this surprising.

Solar wind theory indicates that the rotational velocity (assuming a fixed structure) varies according to

$$\frac{\mathrm{d}V}{\mathrm{d}t} = \frac{\dot{M} R_{\mathrm{A}}^2}{M R^2} V ,$$

where R_{A} is the radius to the Alfvén point, M is the stellar mass, and R is the radius of the star. The Alfvén point occurs essentially where the magnetic-field strength has decreased so that it no longer forces the escaping gas to co-rotate with the star. In this formula, I have omitted a factor of order unity which depends on the stellar model, and another factor discussed by Mestel (**35**). Mestel points out that though an increase in magnetic field increases R_A, it likewise tends to inhibit escape from the equatorial regions, and that the braking is not indefinitely increased as the magnetic field increases. But there is a further effect to consider and that is the role of magnetic fields in the generation and propagation of the waves heating the corona. If the magnetic field causes enhanced heating, then the increase in mass loss from this effect may compensate for Mestel's effect and we could still expect magnetic braking to increase with increasing field.

As to the field itself, it seems very likely that, at least in the solar case it originates in a dynamo mechanism (**63**). In that case, the field strength will depend on rotation and probably like Ω^2, as Cowling (**64**) has pointed out. Thus, for a given spectral type both $\dot{M}$ and R_A^2 should depend on rotational velocity. Let us try to parameterize this dependence with the formula

$$\frac{\dot{M}R_A^2}{MR^2} = -\alpha\left(\frac{V}{V_0}\right)^n$$

where V_0 is the rotational velocity when the star arrives on the main sequence and α and n are constants. Then, we find,

$$V^n = \frac{V_0^n}{1 + \alpha nt}.$$

Current estimates give $MR^2/(\dot{M}R_A^2) \sim 5 \times 10^9$ yrs, as the present half-life of solar angular velocity (**34**) – remarkably coincident with the present age. The above formulae give an instantaneous half-life $(1 + \alpha nt)/\alpha$, which for $\alpha nt \gg 1$ is nt. Hence, if $\alpha^{-1} \ll$ age of the Sun and $n \approx 1$, we recover this coincidence. In fact with $n = 1$, $\alpha^{-1} \approx 5 \times 10^9$ yrs, $V_0 \approx 21$ km/sec, we obtain a passable representation of the data for G stars.

We are now in a position to reconsider the mass-loss mechanism for lithium destruction. To get an upper bound let us assume that all the variation in $\dot{M}R_A^2$ is due to variation in $\dot{M}$. With this assumption we readily find

$$t_1 = \frac{1}{\alpha}\left[\exp\left(\frac{M_1 R_A^2}{MR^2}\right) - 1\right].$$

With $R_A \sim 10R$, and $M_1 \sim 10^{-2}M$, $t_1 = 10^9$ yrs. Thus solar lithium depletion by mass loss could be detectable with the extreme assumption that $\dot{M}$ depends on magnetic fields and thus rotation, while R_A does not. But I think that for G stars we must conclude, that as far as one can tell, the possibility that the mass loss by itself depletes lithium is marginal. On the other hand, the indications from Kraft's observations are that in G stars newly arrived on the main sequence, rotational mixing can be quite important and this, combined with mass loss, may give a reasonable depletion rate. In any case, as Dr. Woolf has pointed out to me, the detection of lithium in certain stars may permit us to place upper bounds on the mass loss. In this connection it would be useful to know how α varies with spectral type.

If t_1 is as low as the extreme estimate suggests, then during the main-sequence lifetime of a G star the lithium abundance will diminish. That is, for $t > t_1$, the mass coming into the convection zone to replace that lost in the wind will be lithium-poor. The lithium abundance, A, will then be governed by

$$\frac{\mathrm{d}A}{\mathrm{d}t} = \frac{\dot{M}}{M} A .$$

Since we used the empirical extreme for $\dot{M}$ to get t_1 above, let us continue the illustration by using it again, though I would not wish to put great weight on this kind of treatment. We then obtain

$$A = A_0 \left(\frac{1 + \alpha t_1}{1 + \alpha t} \right)^{MR^2/M_c R_A{}^2} \quad \text{for} \quad t \geqslant t_1 ,$$

where A_0 is the initial abundance. With $MR^2/(M_c R_A^2) \sim 1$ (probably an overestimate) we find that at $t \sim 5 \times 10^9$ yrs, $A \sim \frac{1}{3} A_0$, which is not sufficient to explain the solar lithium abundance. Nevertheless, there is clearly interest in the mass-loss mechanism and as relevant observations such as those of rotation are increased, the picture should be clarified. For example, if the magnetic field does vary like V^2 we would expect $H = H_0 (1 + \alpha t)^{-2}$, and thus G stars in the Pleiades should have mean magnetic fields 10^2 greater than the Sun. The determination of such data will certainly be very helpful in refining these estimates. And if the mass-loss mechanism does work, the correlation between rotation and lithium abundance is to be expected while the conflict with the magnetic problem at the end of Section 4 does not arise.

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