ON THE STELLAR ROTATION-ACTIVITY CONNECTION

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ABSTRACT

I discuss the connection between the rotation rate of late-type dwarf stars and their level of surface "activity" (as deduced from visible, UV, and X-ray observations) from both theoretical and observational perspectives.

1. INTRODUCTION

Understanding the connection between stellar activity and intrinsic stellar parameters (such as stellar mass, age, and rotation rate) is, strictly speaking, a rather formidable problem because it presupposes that one has a full understanding of the various theoretical components that are thought to be involved: stellar interior and evolution theory, stellar dynamo theory and, finally, the theory of chromospheric and coronal formation. On a qualitative level, much is of course known: that is, after all, part of the point of this symposium. Nowadays, it is common wisdom that the production of stellar magnetic fields in stellar interiors largely determines the level of stellar surface "activity" (see, for example, discussions in Bonnet & Dupree 1980). It is on the detailed, quantitative, level that the difficulty manifests itself: given a star with known mass, age (or radius), and surface rotation rate, can we predict the expected level of surface activity? In trying to address this question, three major stumbling blocks arise:

(i) It is not obvious that the given parameters are sufficient to uniquely determine stellar surface emission levels. In addition to the (trivial) fact that activity levels vary substantially during the course of the solar cycle (during which the Sun's age and mass hardly can be thought to vary), one must keep in mind the much more profound problem that stellar surface rotation rates may be a very poor guide to the actual rotation profile in the stellar interior (which is presumably a major determinant of activity levels). I will not concern myself with this basic difficulty here.

(ii) It is not obvious in what sense current theories of stellar magnetic field production and coronal activity are predictive; one must carefully examine not only the limitations of the theoretical tools used, but also scrutinize the limitations of the physics which enters current calculations. I will address this problem first.

J. O. Stenflo (ed.), Solar and Magnetic Fields: Origins and Coronal Effects, 279–300.

(iii) It is not obvious what theory is to account for. Before theorists invest much effort in detailed modeling of specific "rotation-activity" correlations which are claimed by observers, it is well to ask for the basis of these correlations; as I will try to show, although the sought-for connection between rotation and activity (for example) can be made very precise, it is another matter whether observations are presently able to establish this connection.

Unsurprisingly, there are thus both observational and theoretical conundrums to puzzle over; in the following, I will try to discuss these, keeping in mind that much of the basic details of both the observations and theory are summarized here by others (see especially Gilman 1983; Golub 1983; Schussler 1983; Vaiana 1983).

2. ON THEORY

Can one predict the variation of stellar surface "activity" with the stellar rotation rate Ω and other intrisic stellar parameters, such as stellar mass and age? To begin the discussion, it is worthwhile to distinguish the several possible theoretical approaches[1], and the roles that these play in answering the above question (see Schussler 1982 for further details):

(i) *Linear (kinematic) dynamo theory* is exploratory, but not quantitatively predictive. Linear kinematic dynamo theory is based on the supposition that Lorentz forces are negligible, so that in order to determine the evolution of (mean) stellar magnetic fields, it is sufficient to consider the field as kinematically transported by a given fluid flow (which is determined by some other theory, such as a model for interior stellar convection). Such theories (which by far dominate what one usually refers to as "dynamo theory") cannot provide information concerning the amplitude of the magnetic fields built up during the course of dynamo action; this is so because the equations are linear in the very variable one would like to have information about, the magnetic field. However, such theories do provide a physical picture for many aspects of the dynamo process, motivate the construction of more sophisticated models, and allow the calculation of growth rates, for example, for unstable dynamo waves (cf. Moffatt 1978, Parker 1979). It therefore makes little sense to attempt to link the *amplitude of stellar activity* with the measures of dynamo activity derived from linear kinematic theory (such as the growth rate or dynamo number). Naively, one might of course hope that such a connection does exist; indeed, the literature abounds with such hopes. But it remains a disheartening fact that linear theory has little, if any, predictive power regarding the behavior of solutions to the full non-linear equations (which include, for example, the back-reaction of the magnetic fields on the fluid flow). An instructive example to convince the skeptics in this regard are the recent calculations of simple non-linear extensions of standard dynamo equations by N. O. Weiss and collaborators (Weiss 1983; see also Robbins 1977, Jones 1981, and Ruzmaikin 1981); it is very evident from their work that many of the essential features of the non-linear solutions (such as quasi-periodic and chaotic behavior) are entirely absent in the linear solutions. This is of course not to say that linear theory ought to be forgotten: the point is only that the essence of the present problem -- understanding the effects of magnetic field production in the non-linear regime -- is virtually by definition outside the domain of linear dynamo theory (whose more modest aims are more in the nature of demonstrating the possibility of dynamo action, rather than in providing detailed models of observed dynamos). I suspect that it is the success

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of linear theory in successfully "modeling" the gross features of the solar cycle, such as the sunspot butterfly diagram and the polarity laws, by parameter fitting that has obscured the fact that such modeling actually says little regarding the governing physics (viz., Gilman 1982, 1983).

(ii) *Model non-linear dynamos* fall in a very distinct class of dynamo calculations in which the equations used are approximations of the full non-linear equations of motion (through, for example, some truncation process and/or simplifications of geometry or of the dimensionality of the system); these approximations are generally made in the spirit of, first, preserving what are believed to be the essential non-linear interactions thought to occur in the physical system and, second, making the problem mathematically tractable. The emphasis here is on capturing the basic properties of the physical processes likely to be occuring in a computationally feasible way; because these calculations are not full simulations (see below), they are not likely to provide a quantitatively-accurate account of the competition between various non-linear effects in the actual physical system (i.e., the solar convection zone). Recent calculations of magnetoconvection (see Weiss 1983 in this volume) fall into this category of dynamo modeling.

(iii) *Dynamo simulations*, that is, "full" numerical simulations of the magnetohydrodynamic equations, seek to model the dynamo behavior of the (thermally-driven) convective flows which are ultimately responsible for dynamo action; the calculations of Gilman (1981, 1982a, b), reported in this volume, are exemplary of this kind of approach. In this case, a serious attempt is made to solve the full set of equations in a realistic geometry, so that one would think that limitations of the simplified non-linear models to have been overcome. As Gilman (1983) has noted, there remain, however, several major limitations: present calculations are not fully compressible, do not take magnetic buoyancy into account, and are limited in spatial resolution. The latter difficulty manifests itself in the necessity to impose artificial (turbulent) diffusivities (because the diffusive range is not simulated), and in the fact that the formation of localized flux ("flux ropes"), as in the classic calculations of Weiss (1966), cannot be taken into account. An interesting further limitation of present calculations (which is likely to be relatively easily disposed of) is the fact that the effects of fluid helicity (i.e., $\langle v \cdot \mathrm{curl}\, v \rangle$) are derived only from fluid flow on the resolved scales (unlike the eddy diffusivities, which do take into account the "macroscopic" effects of turbulent motions on unresolved scales); thus, the turbulent diffusivities and the "α-effect" due to unresolved turbulence are not taken equivalent account of. Finally, it is argued by some (see Frisch 1982 in this volume) that the very introduction of eddy diffusivities into a simulation negates the purpose of the simulation; although, strictly speaking, this point of view has merit, I believe that for the purposes of modeling stellar dyanamo behavior, the introduction of eddy diffusivities is benign (the limitations imposed by the neglect of compressibility being much more serious).

[1] It is useful to note at this point that there do exist dynamo calculations which fall into none of the above categories; specifically, these are models in which account is taken of possible non-linearities, but without providing a systematic basis for deriving these effects (so that the non-linearities are imposed *ad hoc*). At times, such models are very enlightening (the best example perhaps being the original dynamo model of Leighton 1964, in which an artificial flux eruption and loss term was inserted into the induction equation); but, in general, I do not see such models as useful contributions because, in a profound sense, these models are not testable.

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It is an interesting fact that magnetic buoyancy plays a very subsidiary role in most of the above approaches to dynamo theory: it is either altogether neglected, or enters only by defining a critical time scale (i.e., the typical rise, or crossing, time for magnetic flux generated in the deep convection zone to reach the solar surface; cf. Robinson & Durney 1982 and Schussler 1980). This neglect is very likely seriously in error. Acheson (1978) showed that, if stellar rotation is taken into account, magnetic buoyancy changes from a problem of lack of steady equilibrium to a classic instability problem; hence, a new time scale enters the problem, namely the inverse growth rate of the relevant instability (see Schmitt & Rosner 1982 for a full discussion of the various possible instabilities and associated growth rates). This insight led a number of theoreticians to re-consider the possible location of the magnetic flux generating region of the dynamo (Rosner & Vaiana 1979; Spiegel & Weiss 1980, 1981; Golub *et al.* 1980; Knobloch *et al.* 1981); and, in particular, to consider the possibility that this generating region actually lies below the convection zone proper, i.e., in the "overshoot" boundary layer that must separate the fully-convective zone from the stably-stratified radiative core, leading to the so-called "shell dynamo" (Rosner 1980).

It must be noted that this new approach to the flux generation problem is conceptually distinct from Parker's (1975) reanalysis of the rate of rise of magnetic flux due to magnetic buoyancy. In this calculation, Parker showed that reasonable estimates of the rise rate of buoyant flux tubes lead to the difficulty that, unless flux tubes are produced in the lower region of the solar convection zone, the rise time to the surface is too short to allow the "α-effect" (resulting from the action of the Coriolis force on rising flux "bubbles") to function effectively; hence, Parker argued, it must be the case that dynamo action does not occur everywhere within the solar convection zone, but rather occurs preferentially in its lower depths. In this case (as in the case of Schussler 1979, 1980, Spruit 1981 and, more recently, van Ballegooijen 1982), the focus is on the motion of flux tubes; and, in the case of Parker (1975, 1979) and others, the assumption is that the relevant dynamics occurs within the convection zone proper.

This second approach of course does not attempt to answer the question of how these flux ropes came to be, regards this problem as distinct from the buoyancy problem and, typically, appeals to (for example) the calculations of N. O. Weiss (1966) and collaborators in order to account for magnetic flux intermittency (cf. Schussler 1983). In the alternative approach which J. Schmitt and I have been following, the initial focus is on the stability of *diffuse* magnetic flux lying just below the base of the convection zone; i.e., we assume that in the region of toroidal magnetic flux amplification (which is assumed to coincide with the boundary layer separating the solar convection zone proper from the radiative core), the magnetic field has not as yet filamented into flux ropes, and is therefore "diffuse". One then investigates the following formal problem: consider the MHD stability of an electrically conducting and differentially rotating gas in the presence of a toroidal magnetic field **B**, an external, constant gravitational field, and radiation pressure, with the effects of viscosity (ν), magnetic field diffusion (η), and heat diffusion (κ) to be included. Thus posed, the problem can be addressed by application of classic linear (and local) analysis, and was first studied in depth analytically (without inclusion of radiation pressure) by Acheson (1978, 1979); the full linear analysis, covering the plausible range of available parameters (the most important of which are the (thermal) Prandtl number ν/κ and the (magnetic) Prandtl, or Schmidt, number η/κ), and presenting growth rates for both low and high frequency modes, has been

recently given by Schmitt & Rosner (1982). In the following, I will briefly summarize the principal conclusions from these studies that directly impinge on the subject at hand.

To orient ourselves, we chose cylindrical coordinates (r, ϕ, z) such that the local Ω-vector points in the z-direction, the local magnetic field in the ϕ-direction and local gravity g^* is a vector in the meridional plane; Ω and B are functions of the meridional coordinates; viscous forces, radiation pressure and, of course, gravity are regarded as external forces. The basic "equilibrium" state is assumed to be axisymmetric and stationary; no motions other than the prescribed rotation rate $\Omega(r,z)$ are assumed to be present (thus one ignores flows whose time scales are of order the Eddington-Sweet circulation). Each of the diffusive processes (of heat, magnetic field, and momentum) defines a characteristic time scale: we have ν/λ^2, η/λ^2, and κ/λ^2, where λ is some scale length. These time scales may be vastly different; this possibility is of considerable importance because it is this disparity in diffusive time scales that allows for the presence of so-called doubly-diffusive instabilities (Stommel, Arons & Blanchard 1956; see review of Huppert & Turner 1981).

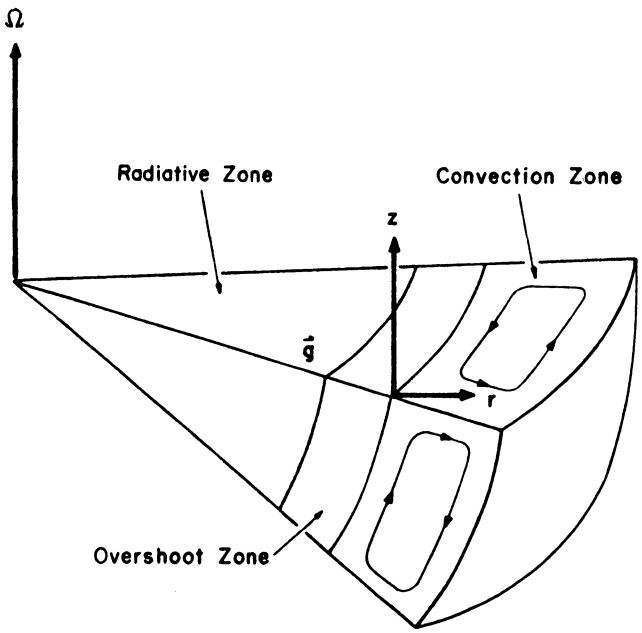

Figure 1: Sketch of the magnetic field geometry envisaged for the convectively-stable boundary layer separating the radiative core from the outer convection zone (from Schmitt & Rosner 1982).

Because the phenomenon of double-diffusive instability may not be universally familiar, I digress for a moment. Consider the classic case of cold, fresh water overlying warm salty water (Stern 1960) such that the density (as well as the salinity and temperature) decreases with height. Naively, one would think that because the density profile is statically stratified, the system ought to be also

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stably stratified; this is incorrect. Focus on the upward displacement of a "bubble" of water from its equilibrium position: the dense, hot, and salty bubble will try to equilibrate with the ambient less dense, cooler, and fresher water by means of molecular diffusion. Suppose that the heat diffusivity is larger than the salt diffusivity (as is indeed the case for water); the bubble's temperature will then equilibrate with its surroundings more rapidly than its salinity does, so that the bubble, upon its return to its equilibrium position, will be relatively more dense than its surroundings at this position. It therefore sinks past its initial equilibrium, and the above process repeats; hence the bubble will execute overstable oscillations which are driven by the unstable temperature gradient (i.e., in the absence of a salinity gradient, the fluid would be unstably stratified as denser cold water would overlies less dense warm water). Note, however, that the key to the instability is the inequality of the two governing diffusivities (of heat and salinity), with the third diffusivity in the problem, the viscosity, simply acting to damp the bubble's motion. Of course, more complex cases can be considered, the best-known of these being warm, salty water overlying cold, fresh (and denser) water; in this case (which is commonly encountered in oceans), the water develops highly horizontally-structured and vertically-enlongated "salt fingers", again as a result of the higher diffusion rate of heat than salinity (so that a descending, initially hot and salty "bubble" becomes yet denser than its surroundings, and continuous to descend). Where does the connection to the magnetic buoyancy problem lie? Here the appropriate analogies are between the density, temperature, and salt distributions in water and the temperature, entropy, and magnetic field in the magnetofluid, respectively; analogous to the saline water case, the magnetic diffusivity is substantially smaller than the heat diffusivity, so that overstable oscillations and, presumably, analogues of salt fingers become possible. It is the latter possibility that led J. Schmitt and me to consider the possibility that the magnetic buoyancy problem and the magnetic flux tube problem in the deep convection zone may have a common solution.

As an example of the possible behavior of the magnetofluid, consider the physically-simplest case, namely that corresponding to the axi-symmetric solutions of the full dispersion relation (studied first by Acheson and Gibbons; see Acheson 1978) for a stably-stratified region. Three different inertial frequencies must be considered: the rotation rate Ω, the Brunt-Vaisala frequency N (i.e., the frequency of adiabatic buoyancy oscillations), and a "magnetic" Brunt-Vaisala frequency M (defined analogously to N, but based on the magnetic field, rather than the entropy, gradient), which we assume to be much smaller than $\max\{N, \Omega\}$; we hence require for stability that the frequency of buoyancy oscillations far exceeds the rotation and magnetic Brunt-Vaisala frequencies. We then adopt the (astrophysically interesting) assumption that the entropy gradient is stable, but that the magnetic field gradient is unstable -- as could occur in the overshoot zone lying below the solar convection zone. The diffusivity (η) corresponding to the destabilising ingredient (the magnetic field) then obeys the ordering $\nu < \eta < \kappa$, and is thus bracketed by the diffusivities corresponding to the stabilising ingredients (rotation and entropy). In that case, buoyancy modes will be damped by efficient heat conduction on shorter length scales; and one obtains doubly-diffusive instability as long as the modulus of the ratio of the normal and magnetic Brunt-Vaisala frequencies does not significantly exceed η/κ. What actually happens? For these (axisymmetric) modes, the fluid motions must lie in the r-z plane; because of incompressibility, modes whose wave vector lies in the r-direction correspond to motions along z, and vice versa. Consider first motions along z (which are not subject to Coriolis forces, but only to buoyancy effects); because there is no effective restoring force, these modes will grow exponentially. Next, consider what happens as we rotate the

wave vector in the r-z plane: as we cross the direction in which wave motions lie in the plane perpendicular to the buoyancy forces (i.e., such that $g \times k = 0$), we obtain stability (because there are no driving forces for these motions). When $k \times z = 0$, the corresponding fluid motions along the r-direction will be subject to both Coriolis and buoyancy forces; hence, Coriolis forces may stabilise buoyancy effects (by "pushing" in the azimuthal direction). However, if instability occurs, it will be oscillatory, with a frequency determined by the rotation rate Ω if the magnetic Brunt-Vaisala frequency is less than Ω. In that case, doubly-diffusive effects will enter again: the influence of Coriolis forces is now damped by diffusion of momentum (controlled by ν), while the influence of buoyancy is damped by diffusion of heat (controlled by κ) and/or magnetic fields (controlled by η).

A basic feature of these doubly-diffusive instabilities is that they occur only on "intermediate" length scales, that is, on scales lying between the short wavelengths which are heavily damped, and are thus stabilised, and the long wavelengths which are assumed to be stable; one can thus calculate cut-off wavenumbers, such that damping dominates at larger wavenumbers, by invoking the condition of marginal stability; we find that, to leading order (and for the ordering $\nu \ll \eta \ll \kappa$), the cutoff wave number s depends very strongly upon κ

$$s^4 \sim -[\eta\kappa]^{-1} \cdot N^2[M^2 + \Omega^2 + (\eta/\kappa)N^2] / [M^2 + \gamma(\eta/\kappa)\Omega^2 + (\eta/\kappa)N^2] \quad (2.1)$$

(where γ is the ratio of specific heats), in contrast to Acheson's (1979) result obtained for an unstratified atmosphere.

It is evident from the above that the actual behavior of the magnetic layer depends crucially upon the actual values of the various diffusivities, as well as on the actual value of the superadiabatic gradient and mean (diffuse) field strength in the unstable layer. Consider first the diffusivities. In the radiative interior, typical values for the Prandtl (ν/κ) and Schmidt (η/κ) numbers are

$$\nu/\kappa \sim 2 \times 10^{-6}, \qquad \eta/\kappa \sim 7 \times 10^{-4}, \quad (2.2)$$

and thus we are most certainly in the parameter regime $\nu \ll \eta \ll \kappa$ assumed by Acheson (1978): application of this theory to the radiative interior of the Sun thus seems to be on safe grounds (but is not particularly interesting). The situation is however more problematical in the solar convection zone: this region is turbulently convective (the Reynolds number is very large), so that if one regards the physical variables in the MHD equations of motion as averages (i.e., as variables defined on spatial and temporal scales large when compared to the integral scales of the turbulent flow), then the *eddy* (rather than the molecular) diffusivities are appropriate; these we take to be isotropic. This latter assumption is probably reasonable because, as the scales modeled by us are much smaller than the local pressure scale height, and as the scales of turbulent motions which provide the eddy diffusivities occur on yet smaller scales, the relevant Rossby number is likely to be large; hence rotation is unimportant for the scales of the turbulence responsible for the eddy diffusivities.

As alluded to above, there are a myrad of other obstacles to (numerically) evaluating the growth rates for the unstable modes: we do not know the value of the superadiabatic gradient in the boundary layer separating the convection zone from the radiative interior (this is related to the classic problem of calculating the

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extent of the overshoot region which must underlie the level at which the Schwarzschild criterion is met); and we do not know with any certainty exactly where the azimuthal (toroidal) magnetic flux is really built up (the earlier arguments not withstanding). But let us suppose that magnetic flux storage *does* occur below the fully-developed convection zone (cf. Arter 1983). In order to scale our results, we adopt the parameter values

$$B_{mean} \sim 10{,}000 \text{ gauss}; \tag{2.3a}$$

$$|B|/|\nabla B| = 0.2\rho/|\nabla\rho|; \tag{2.3b}$$

$$\Omega = 10 \times \Omega_{\odot}; \tag{2.3c}$$

$$\nabla - \nabla_{adiabatic} \sim -10^{-4}. \tag{2.3d}$$

The values for the density, the density and pressure scale heights, the sound speed, the curvature radius, the gravitational acceleration, and the adiabatic index γ are relatively well established, and are kept constant throughout our calculations. The radiation pressure is neglected, and the surface angular velocity is adopted as the rotation rate (although the rotation rate very likely varies radially); we thus neglect local effects of differential rotation.

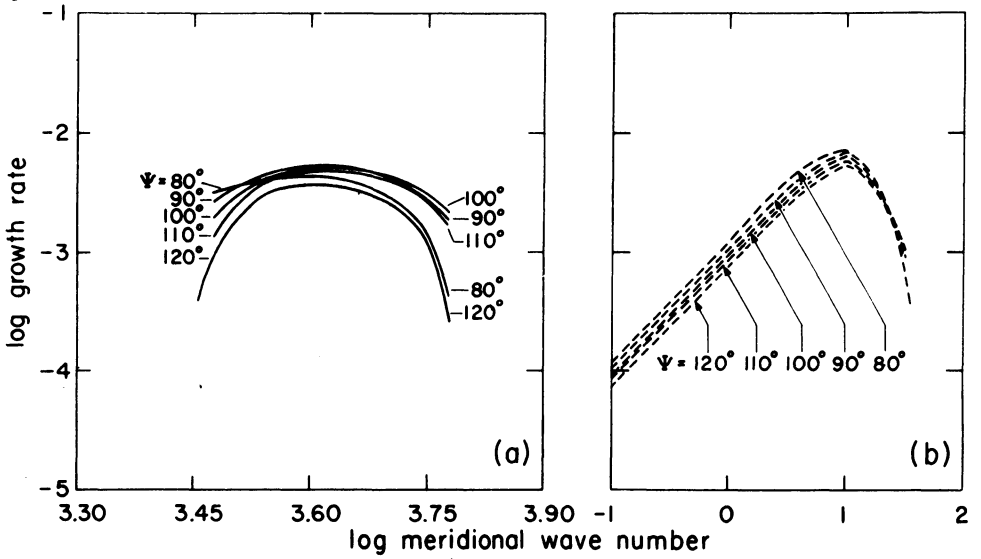

Figure 2: Growth rate versus wave number in the nonaxisymmetric case, with azimuthal wavenumber m = 3, rotation rate $\Omega = 10 \times \Omega_{sun}$, for (typical) values of the superadiabatic gradient $\nabla - \nabla_{ad} = -10^{-4}$, field strength $B = 10^4$G, and magnetic field gradient scale $H_B = H_\rho/5$; ψ is the angle between the direction of wave propagation and the radial direction at colatitude $\theta = 60^\circ$. Except for the singular case $\theta = 0^\circ$, the results are insensitive to the choice of colatitude. We assume that there is no magnetic field gradient along the z-axis, and plot the growth rate for various values of ψ. There are two principal branches, corresponding to (a) inertial ("fast") waves, whose growth rates peak at small spatial scales (large wave numbers); and (b) magnetostrophic ("slow") modes, whose growth rates peak at large spatial scales (small wave numbers) (from Schmitt & Rosner 1982).

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Typical results for the growth rate of the unstable modes are shown in Figure 2 for the simplest non-axisymmetric case, in which there are no magnetic field gradients along Ω (so that there is no buoyancy driving force in the direction in which fluid motions are unaffected by the Coriolis force); we arbitrarily adopt an azimuthal wave number m = 3. Under these assumptions, motions along Ω will be stable, and motions along the magnetic field gradient will be subject to the full stabilisation of rotation. The most unstable meridional wavevectors lie in the radial direction, as shown in Figure 2; in particular, the "slow" (magnetostrophic mode) instability occurs on the longer scales (smaller wavenumbers), and the "fast" (inertial mode) instability occurs on shorter scales (it turns out that axisymmetric modes, with m = 0, have no "slow" mode instability). Because the maximum growth rates are almost independent of the assumed Brunt-Vaisala frequency if $|M^2/N^2| < \eta/\kappa$, we conclude that, qualitatively, the assumed magnetic field configuration does not seem to be critical for the appearance of instability.

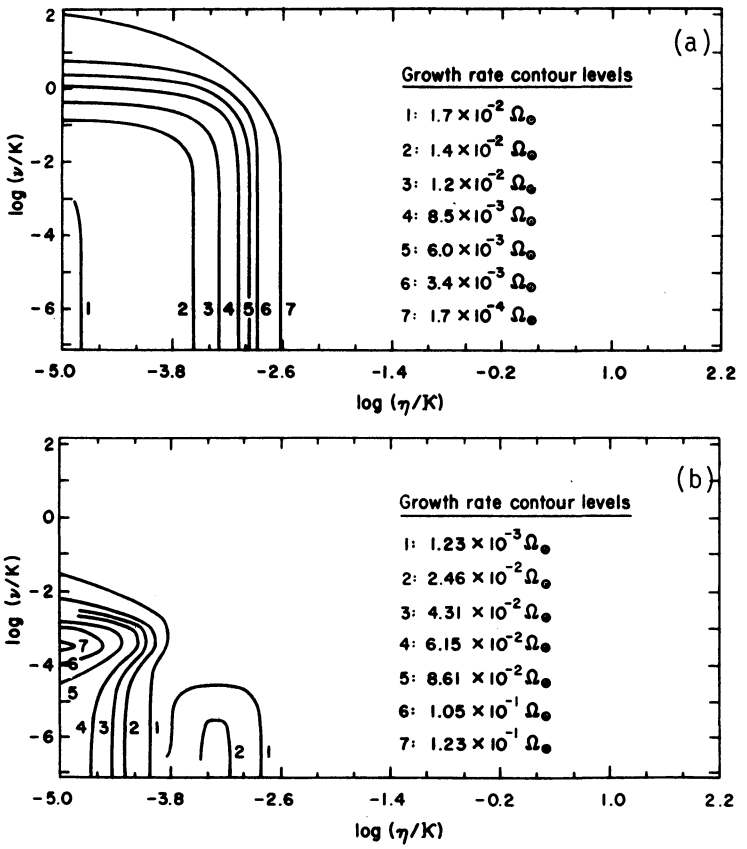

Figure 3: Variation of the maximal growth rate versus the Prandtl and Schmidt numbers, for the parameter values used in Figure 2; we fix $\psi = 160^0$, and show results for two different wave number regimes, $\kappa s^2/\Omega$ = (a) 10 ("slow" mode regime); (b) 10^3 ("fast" mode regime). Note that if turbulent eddy diffusivities apply (such that both the Prandtl and Schmidt numbers are of order unity), the magnetic field configuration is stable (from Schmitt & Rosner 1982).

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Now, suppose that the diffusivities were given instead by their eddy values; does the behavior of the instabilities change? On the scales of interest, the turbulent viscosity will be given by $\nu_{turb} \sim u(\lambda)\lambda$, where u is a typical velocity on the scale λ. If we assume u to be given by typical velocities in the lower part of the convection zone, we have $u \leq 10^3$ cm sec^{-1}; setting $\lambda \leq 10^5$ cm, we then obtain $\nu_{turb} < 10^8$ $cm^2 sec^{-1}$. We assume that the energy flux is largely carried by radiation, so that $(\nu/\kappa)_{eff} \sim O(1)$; and, because the magnetic Prandtl number and Schmidt numbers are not well established, we shall explore the range of values $10^{-4} << \eta/\kappa << 10^{+2}$.

In order to carry out this exploration of the parameter space of the (thermal) Prandtl and Schmidt numbers, we fix the wave numbers s and m, and calculate growth rates as a function of η/κ and ν/κ. Figure 3a shows the result of such a calculation for the case $\kappa s^2/\Omega = 10$, m = 3, assuming again a spherically symmetric mean magnetic field configuration. The "slow" mode is evidently unstable for $\nu/\kappa \leq 10$, $\eta/\kappa \leq 10^{-2}$. Similarily, we obtain the growth rates for the most unstable mode for the case $\kappa s^2/\Omega = 10^3$ (so that one expects to obtain the "fast" mode instability), shown in Figure 3b. These figures clearly show that the region of instability in the η/κ - ν/κ plane is a function of both parameters, although the growth rate, once it exceeds zero, depends only weakly on η/κ and ν/κ.

What are some of the implications of the above formal results? Here we tread on far less certain grounds. Doubly-diffusive processes characteristically give rise to intrinsically preferred length scales; thus, in our case, maximum growth rates typically occur on (dimensionless) scales $\kappa s^2/\Omega \sim 1$ - 1000 for the range of subadiabatic gradient values considered by us, corresponding to meridional wavelengths $\lambda_{mer} \sim 10$ - 400 km for $\kappa \sim 10^8$ $cm^2 sec^{-1}$. One interesting possibility is that this instability (in the non-linear regime) gives rise to *flux tubes* of diameter $\sim \lambda_{mer}$ (cf. Spiegel & Weiss 1980; Hughes 1982); simple-minded scaling arguments for the change in flux tube diameter during the course of its rise to the surface then yield flux rope scale sizes of ~ 10,000 km or more, not an outrageous number. Thus, it may be that flux tube formation may be a natural (nonlinear) consequence of doubly-diffusive instability.

Our growth rate calculations also show that in a not too strongly stabilised region, *both* "slow" hydromagnetic waves *and* "fast" inertial waves may become unstable; these instabilities occur with comparable growth rates, but rather different characteristic spatial scales. Radiation is sufficiently efficient to cause these waves to lose their thermal buoyancy, but magnetic field diffusion does not occur rapidly enough to remove their magnetic buoyancy; the energy driving the instability therefore must come from the magnetic field free energy. It is not evident which modes are likely to be excited; however, experience with other systems exhibiting convectively unstable and stable zones (viz., thermosolutal convection) suggests that gravity modes will indeed be efficiently excited (see Press 1981).

Perhaps the most intruiging consequence of these doubly-diffusive instabilities is the implication for the so-called "shell" dynamo (Rosner & Vaiana 1979). As argued above, the overshoot convection boundary layer is thought to be the site for toroidal magnetic flux generation by the ω-dynamo. Because of the presence of turbulent flows, the Schmidt (or magnetic Prandtl) number will be large [i.e., O(1)]. Hence, the ratio of the magnetic and non-magnetic Brunt-Vaisala frequencies will be smaller than the Schmidt number, and the doubly-diffusive instabilities discussed above will be inhibited. Thus, toroidal magnetic flux accumulation is possible in this

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boundary layer. However, once the toroidal magnetic field exceeds some critical threshold, turbulent motions will be suppressed; the eddy diffusivities will hence decrease (and so the Schmidt number will become small), and the system will become unstable. Because the instability growth rates are large, toroidal magnetic flux ejection in this model would then be episodic on a recurrence time scale fixed by the rate of toroidal flux amplification by the ω-dynamo. We conjecture that the formation of active region complexes is a consequence of this episodic eruption process, and that the spatial scale of the erupting magnetic flux is determined by the doubly-diffusive mechanism we have discussed above.

Where does this theoretical work leave us? It is not hard to notice the many assumptions adopted in the above discussion; although these are necessary in order to progress in the present instability calculations, one would really prefer to know the actual parameter regime applicable to the Sun. The overshoot problem is just now being seriously addressed (cf. Hulbert, Toomre, & Massaguer 1981; van Ballegooijen 1982; Marcus, Press & Teukolsky 1982; Schmitt & Rosner 1983), so that there is some hope of understanding the stratification of the deep convecton zone and overshoot boundary; and the crucial problem of the non-linear development of the doubly-diffusive modes is now also beginning to be attacked. But whether a definitive statement regarding the quantitative connection between stellar rotation rate and "activity" can be made is somewhat doubtful.

3. ON OBSERVATIONS.

I now turn to a far more observationally-oriented aspect of the stellar "activity-rotation connection"; and ask to what extent we have observational knowledge of the existence of such a "connection" and, more specifically, to what extent one should -- as a theoretician -- pay heed to the precise nature of the correlations found by observers to connect stellar rotation rates and the various measures of stellar surface activity.

The most basic question to be addressed is, quite clearly, why one ought to expect difficulties in the first place. Certainly, it must be that, as the volume of data has grown (virtually exponentially), application of standard data reduction and error analysis techniques, together with careful attention to the possible existence of sample biases, will avoid gross error; this ought certainly hold for correlation analyses of *complete* samples (that is, samples containing every star meeting some *a priori* selection criteria or, somewhat more weakly, containing a truly random selection of such stars). In the following, I will show that such *standard* analyses, when applied to *currently-available* stellar data, will very likely lead to error.

To be more precise, consider a volume-limited sample of stars, for which we have measurements of stellar parameters such as rotation rates, mass, effective temperature, x-ray luminosity, Ca II index, etc. Let us assume for the moment that for every star in the sample (of N stars), *all* data are available; that is, every star in the sample is characterized by M distinct parameter measurements $F \equiv \{f_1, f_2, \ldots, f_M\}$, so that the full sample is completely characterized by the N x M matrix of measured parameters. Classically, one can describe the sample by specifying the joint probability distribution function $\psi(f_1, f_2, \ldots, f_M)$, such that

$$\psi(\cdots)\, df_1 \cdots df_M \qquad (3.1)$$

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is the probability that a given star has its characteristic parameters lying in the range (f_1, $f_1 + df_1$; ... ; f_M, $f_M + df_M$). Now, it is always possible to write ψ in the form (with $f_1 \equiv L$, L some passband luminosity, and $f_2 \equiv \Omega$, Ω the stellar rotation frequency)

$$\psi(L, \Omega, \cdots) = \theta(\Omega) \cdot \Phi(L|\Omega, \cdots), \tag{3.2}$$

so that the first moment of the marginal (or conditional) distribution function Φ yields the *regression curve*

$$\langle L(\Omega, \cdots) \rangle = \int dL\, L\, \Phi(L|\Omega, \cdots). \tag{3.3}$$

The aim of standard correlation analysis (in its various guises) is in fact to construct this regression curve, which functionally relates the *mean* luminosity to the remaining stellar parameters. Least-squares, or minimum-variance, fitting and common factor analyses all fall into this generic class; in the most sophisticated versions, one attempts to construct a minimal set of stellar parameters (with $M' < M$) that fully describe the sample, and so "factor out" those parameters which are not truly independent (the functional dependence of these "factored-out" parameters on the remaining stellar parameters is one of the principal goals of such analyses). All of this is subject to two major restrictions:

(i) In general, we do not know $\Phi(L \mid \Omega, \ldots)$. The common remedy is to *assume* some *a priori* functional form for $\langle L \rangle$ which contains several adjustable parameters θ_j, and then to minimize the variance of the fit of $\langle L \rangle$ to the data by variation of these parameters. This is the aim of classical regression analysis, and has been used to describe, for example, the correlation between stellar rotation rate and stellar x-ray emission (cf. Pallavicini 1980, 1982; Walter & Bowyer 1981; Walter 1981, 1982); in this case, the adjustable parameters θ_j are the coefficient and exponent of the power law usually assumed to define the *mean* variation of L_x, $\langle L_x \rangle$, with Ω.

(ii) In general, one may not have measurements of *all* M of the parameters characterizing a given star; more specifically, some of the stars in the sample may have only upper bounds on their (X-ray) luminosity or rotation rate. This lack of information is only troubling insofar as standard regression analysis cannot deal with upper bounds. For example, classical least squares fitting can only be performed if one has a measurement to compare theory with. It is unfortunately common to deal with this difficulty by simply ignoring it; typically, one finds that stars whose parametric description is incomplete are simply removed from the sample under study. Naively, one might think that removal of incomplete information is benign, but in fact upper bounds do provide information concerning the distribution of a given parameter within the test sample.

To see how both detections and upper bounds contribute information about the sample population, consider the luminosity function $\phi(L)$,

$$\phi(L) = \int d\Omega \cdots \Phi(L|\Omega, \cdots), \tag{3.4}$$

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obtained by integrating Φ from Eq. (3.2) over all the remaining stellar parameters; $\phi(L)$ has the property that the probability for a given star drawn from the sample population to have a luminosity L in the range [L, L + dL] is just

$$\phi(L)\ dL$$

The basic idea, first discussed by Avni *et al.* (1980; also Avni 1981) in the context of quasar luminosity functions and quasar evolution, is that upper bounds impose integral constraints on the luminosity function which provide information rather analogous to that provided by actual detections. More specifically, consider the following simplified heuristic argument. Suppose we bin our detections and upper bounds as shown in Figure 4; and let us suppose that we really know the true distribution of L, that is, we know the bin probabilities ϕ_i, i = 1, ... , M (where M is the total number of luminosity bins), defined by

$$\phi_i \equiv \int_{L_i}^{L_i + \delta L} dL\, \phi(L)\,. \tag{3.5}$$

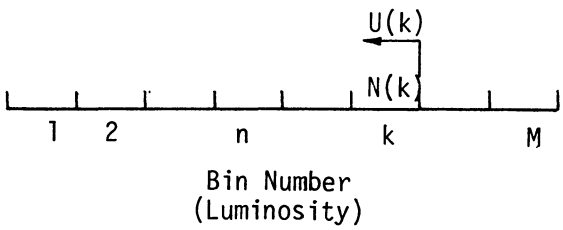

Figure 4: Sketch of luminosity binning arrangement; the horizontal luminosity axis is consists of discrete bins, each characterized by an as yet unknown bin probability that a given observed star will have a luminosity in the luminosity range spanned by the bin. See text for further details.

Now denote the number of detections in bin k by N(k), and the number of upper bounds in bin k by U(k). Then the probability that a given upper bound in bin k (say) "really" belongs in bin n (that is, the star really has a luminosity that would place it in bin n, if we had sufficient sensitivity to observe it), is

$$\phi_n \Big/ \sum_{k'=1}^{k} \phi_{k'}. \tag{3.6}$$

Thus, the expected number of stars in our sample that have upper bounds on L which placed them in bin k, but which really "belong" in bin n, is just

$$U(k) \cdot \phi_n \Big/ \sum_{k'=1}^{k} \phi_{k'}. \tag{3.7}$$

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Thus, the "effective" number of stars which belong in bin n is just the sum of the stars with measured luminosities which fell into bin n, plus the number obtained from the upper bounds, Eq. (3.7):

$$N_{effective}(n) = N(n) + \sum_{k=n}^{M} U(k) \cdot \phi_n \Big/ \sum_{k'=1}^{k} \phi_{k'}. \tag{3.8}$$

Now, in the limit of large samples, we expect that the ratio $N_{effective}(n)/N_t$ (N_t the total number of stars in the sample) will approach the bin probability ϕ_n; hence, with a bit of algebra, we can solve for ϕ_n in Eq. (3.8):

$$\phi_n = N(n) \Big/ \left[N_t - \sum_{k=n}^{M} U(k) \Big/ \left[1 - \sum_{k'=k+1}^{M} \phi_{k'} \right] \right]. \tag{3.9}$$

This result gives, in closed iterative form, the differential luminosity function for the sample, and takes the upper bounds into account; given the set of values {N(k), U(k)}, one starts with n = M (the upper-most luminosity bin) and work downwards to smaller n. *Note that this result is significantly different from what would be obtained if upper bounds were not taken into account (i.e., if U(k) = 0 for all k in Eq. 3.9).* For some purposes, it is more convenient to work with the integral luminosity function F(L), defined simply as

$$F(L) \equiv \int_{L}^{\infty} dL\ \phi(L); \tag{3.10}$$

an example of a result of such a calculation is shown in Figure 5, the integral x-ray luminosity function for dwarf M stars (Rosner *et al.* 1981).

Now let us return to the original problem of determining the functional form of the marginal distribution $\Phi(L \mid \Omega, \ldots)$, which contains the crucial information linking activity and the classical stellar parameters, such as rotation rate. Following the approach just described for deriving the stellar luminosity function, we will now outline the method for constructing Φ; the parametric form presented here is due originally to Y. Avni (Avni & Tananbaum 1982; Tananbaum *et al.* 1982).

To begin, we shall adopt three major assumptions:

1. We suppose that the problem can be described parametrically, that is, we shall assume that the mean variation of the stellar (X-ray) luminosity $\langle L \rangle$ (defined in Eq. 3.3) can be expressed as an analytic function of the remaining stellar parameters,

$$\langle L \rangle = f(\Omega, \cdots; \theta_1, \theta_2, \cdots \theta_m), \tag{3.11}$$

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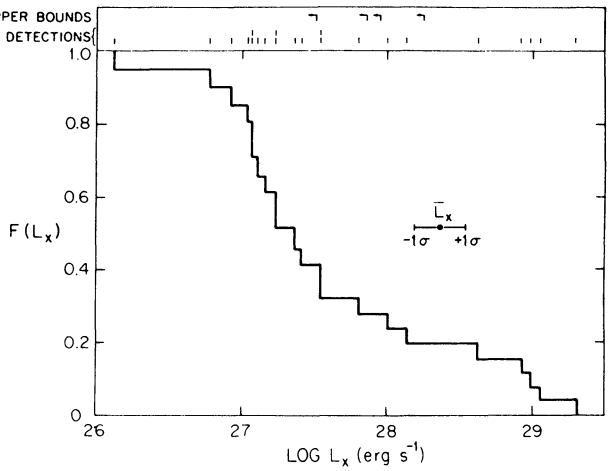

Figure 5: Integral x-ray luminosity function for a volume-limited sample of M dwarfs, as derived from *Einstein Observatory* data; F(>L) indicates the fraction of the entire population of dwarf M stars whose luminosity exceeds L (from Rosner *et al.* 1981).

where $\theta_1, \ldots, \theta_m$ are the m regression parameters to be determined. For example, it is commonly assumed that one can connect $\langle L \rangle$ with Ω using the relation

$$\log \langle L \rangle = \theta_1 + \theta_2 \log \Omega, \tag{3.12}$$

where θ_1 and θ_2 are the fitting (or regression) parameters. It is not essential to assume a specific functional form for the connection between $\langle L \rangle$ and, say, Ω; indeed, it is straightforward to apply this method non-parametrically (so that no assumptions regarding the functional form of $\langle L \rangle$ must be made), as a student at Harvard, J. Schmitt (1982), has recently done.

2. We assume that all stellar parameters have been determined, with the sole exception of L, for which we have measurements as well as upper bounds; we regard each value of L (or upper bound on L) as an independent random variable whose distribution we seek to determine. Again, it is straightforward to extend this method to the case in which upper bounds for some of the other stellar parameters also exist (most crucially, for the rotation rate Ω); J. Schmitt has carried this extension out as well.

3. We assume that the residuals $\{(L^o - f^{best})_i\}$, where L^o is the observed luminosity and $f^{best} = \langle L \rangle$ is the best-fit mean relation, have a Gaussian distribution with zero mean and variance σ. This assumption is essential, but may be invalid: whether this assumption is applicable can only be determined *a posteriori*.

The method is now quite straightforward: we first construct the likelihood function Λ (Kendall & Stuart 1976)

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$$\Lambda(X \mid \theta) = \Phi(L_1 \mid \theta)\, \Phi(L_2 \mid \theta) \cdots \Phi(L_n \mid \theta) \tag{3.13}$$

where $X = (L_1, \ldots, L_n)$ is the set of detections and upper bounds for L, and $\theta = (\theta_1, \ldots, \theta_m)$ is the set of m independently varied parameters in the fitting distribution function (in our case, the distribution function is assumed to be a Gaussian, and only one parameter enters, namely the variance σ). Note that the likelihood function is nothing but the joint probability density function for the independent random variables L_i. It is customary (and algebraically helpful in the following) to work with the negative natural logarithm of Λ, $S \equiv -2 \ln \Lambda$; for the case at hand, we find that S is given by

$$S = 2N_d \ln \sigma + \sigma^{-2} \sum_{i=1}^{N_d} (L^d_i - f)^2 - 2 \sum_{i=1}^{N_u} \ln G[\, (L^u_i - f)/\sigma \,], \tag{3.14}$$

where

$$G(t) \equiv \int_t^\infty dt' \, (2\pi)^{-1/2} \exp(-t'^2/2); \tag{3.15}$$

As an example of this method, I have applied the above formalism in order to obtain the conditional x-ray luminosity function

$$\Phi \equiv \Phi(L_X \mid \Omega) \tag{3.16}$$

for single late-type main sequence stars (using the data of Pallavicini *et al.* 1982); this distribution function basically measures the variation in dispersion of the stellar x-ray luminosity about the mean $\langle L_X \rangle$ as a function of the stellar rotation rate Ω. The outcome of our calculation, cast in terms of the cumulative distribution F (scaled to unit variance)

$$F(L_X) = \int_0^{L_X} dL_X' \, \Phi(L_X' \mid \Omega), \tag{3.17}$$

is shown in Figure 6. The most striking result is that the residuals of the best-fit relation connecting $\langle L_X \rangle$ with Ω do seem to follow a Gaussian distribution of zero mean and unit variance, as we *assumed* in the beginning; that is, the distribution of L_X about its mean $\langle L_X \rangle$ for fixed rotation rate is consistent with a random distribution; this suggests that:

(i) the variation of $\langle L_X \rangle$ with Ω is the dominant systematic contribution to the observed range in x-ray luminosities;

(ii) the remaining scatter in luminosity (associated with the residuals $L^d_i - \langle L_X \rangle$) may be due to intrinsically stochastic processes; the obvious possibilites are a superposition of contributions from short-term stellar variability (for example, from flares) and from stellar activity cycle variations (which are randomly sampled in phase).

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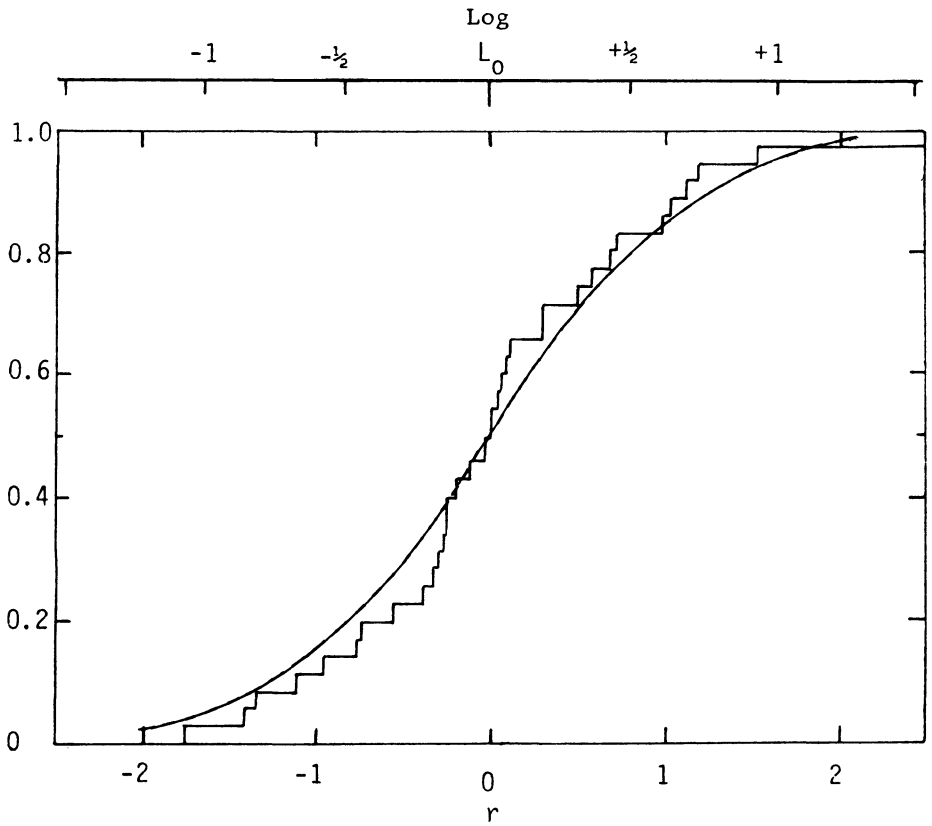

Figure 6: Cumulative x-ray luminosity distribution function $F(L \mid \Omega)$, obtained from a sample of single, late-type dwarf stars for which x-ray luminosities (or upper bounds) and rotation rates were available (from Pallavicini *et al.* 1982). Φ gives the distribution of stellar X-ray luminosity about the regression curve $\log \langle L_x \rangle = \theta_1 + \theta_2 \log \Omega$. Also shown is the predicted distribution function (solid curve) for a Gaussian random variable of zero mean and variance 1; note the close correspondence between the observational result and the theoretical curve.

It is interesting to note that, if the residuals are dominated by stellar cycle variability, then the observed variance of the residuals for fixed Ω (given by Figure 6) immediately gives the amplitude of stellar activity as a function of rotation rate; hence, long-term monitoring may not be required to study this aspect of activity cycles (long-term monitoring is of course crucial to studies of, for example, cycle periods).

4. SUMMARY.

I have outlined some of the limitations of present attempts to "model" the connection between stellar activity and intrinsic stellar parameters, such as the rotation rate; and have discussed some remedies. It appears that at present, it may

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be premature to ask theory to account for reported correlations between, for example, x-ray luminosity and stellar rotation rate, on a quantitative level; but the time seems ripe to apply more rigorous analysis techniques in order to establish the observational *quantitative* connection between intrinsic stellar parameters and "activity" characteristics. Rather than "reinventing the wheel", we have taken advantage of recent work on quasar luminosity functions (in which many of the observational problems encountered in stellar research – upper bounds on detections, incomplete samples, etc., come up as well), and have applied this methodology (with some modification) to analysis of stellar x-ray and rotation data; these methods could be easily extended to similar work with other "activity" indicators, such as Ca II data.

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DISCUSSION

GILMAN: Parker's argument that magnetic buoyancy requires the solar field to be retained mostly in the deep layers of the convection zone for the dynamo to work must assume a magnitude for the α effect consistent with the observed magnetic cycle length. But in the global MHD dynamo models, the problem is that α is much too big to give the right period. Perhaps the presence of magnetic buoyancy would not eliminate the dynamo from the bulk of the convection zone, but rather would simply reduce the effective α to the point where a more nearly correct period comes out. What do you think about this?

ROSNER: I think that in the context of your global model, in which the dynamo processes occur throughout the convection layer, your suggestion is very plausible, and in fact is yet another good argument for including magnetic buoyancy in the model.

SPICER: If you look at the question of magnetic buoyancy as an MHD stability problem, you find that magnetic buoyancy is just a magnetized form of the Raleigh-Taylor instability called the Kruskal-Schwarzschild instability. Now it is possible to stabilize the K-S instability by shearing the magnetic field by adding a parallel current, albeit a weak parallel current. Hence, shearing the field may help keep the field from rising through the convection zone too quickly allowing amplification of the field.

ROSNER: The relation between magnetic buoyancy and the Kruskal-Schwarzschild instability is well-known. Indeed, there is a poster presentation by D. Hughes at this symposium on the interchange instability (which is what you are addressing), which discusses the geat richness of the possible instabilities. Now, if one knows the particular instability to be suppressed, you are quite right that one can in principle stabilize the system by imposing suitable current flows (as is done in the laboratory). However, in the case of the Sun, it is very hard for me to see how the Sun manages to arrange for stabilizing current flows for the entire zoo of MHD instabilities which follow from the full dispersion relation. Perhaps some modes are stabilized by the kind of effect you propose, but at most this will just change the dominant mode of instability.

SPRUIT: In general by adding complications to the magnetic configuration, like shear, one increases the free energy. Though certain instabilities may be reduced, the overall instability tends to get worse.

SPICER: In response to the comment that adding an additional parallel current may suppress the K-S instability but causes other instabilities, I must state that it is a question of the magnitude of the magnetic field associated with the parallel current relative to the ambient field. Only a weak shearing field is necessary to stabilize the K-S instability, while large fields, associated with the parallel currents, give rise to the instabilities Spruit refers to — generically referred to as kinks. Perhaps fields are kept at the base of the convection zone by shearing but brought up only after the current density gets large enough to cause a kink.

GARCIA DE LA ROSA: According to the current ideas on magnetic buoyancy, the velocity of ascension for a fluxtube is roughly proportional to the magnetic flux content of the tube. If you consider a unique layer of departure for magnetic fluxtubes travelling towards the surface, how can you fit the obvious observation of the simultaneous coexistence on the solar surface of large and small active regions corresponding to fluxtubes with a difference of flux content of several orders of magnitude.

ROSNER: Your question is quite apt, and in fact similar arguments led L. Golub, G. Vaiana, N.O. Weiss, and myself to a phenomenological picture of flux emergence, in which the small-scale fluxtubes indeed do come from higher layers, i.e., they are produced as large flux ropes rise through the convection zone and are "shredded" by turbulence (1981, *Astrophys. J.* **243**, p. 309).

BASRI: I just wanted to point out that the RS CVn stars are the best current sample of stars which satisfy the theoretical properties Dr. Rosner has indicated are desirable. Observation of a volume limited sample has the best chance of having all detections in various diagnostics (no upper limits) coupled with well determined values of Ω . One must then just be careful to know whether results from this sample are generally applicable to a larger class of stars.

ROSNER: I agree. In fact, as I pointed out, because the sample of stars considered by Walter and Bowyer contained only detections (i.e., every star looked at was detected), their "rectified" L_X/L_{bol} distribution is in fact identical to the one derived by maximum likelihood techniques. I would however caution that in those cases in which periods of RS CVn stars are established purely by considering the orbital period of the system (i.e., arguing that because of tidal coupling, the rotation period is identical to the orbital period), there may be a difficulty: as discussed by J.-L. Tassoul (*Theory of Rotating Stars*, p. 36ff), synchronism does *not* always obtain. For this reason, one should try to obtain the photometric period (as has in fact been done for many of the stars in this sample).

GIOVANELLI: (1) Observations tell us a great deal about the origin of the small-scale flux elements. These *have* their origins in sunspots, and their disintegration has been studied in great detail. This is no mere possibility — it is a fact. (2) We have been able also to follow the transport of flux elements to polar regions. Since the tubes of force do not break, we have long subsurface tubes joining the polar flux elements back to their origins in the sunspot flux ropes. There must be a deeper flow back to the equator, for continuity, and these meridional circulations carry tubes of force downwards, where they are concentrated greatly *unless* gas can cross *into* the tube. I don't know whether this is a vital problem. But the main result is that the fluxtubes are *always* strong deep in the convection zone. I believe that the theoreticians have been concentrating on the wrong thing: they have been concerned too much with building up strong fields deep in the convection zone, whereas the problem is rather in dealing with strong fields, and explaining how these strong fields can survive without floating.

ROSNER: (1) My comments were directed not at the problem of forming small magnetic features *at* the surface, but rather at the problem of explaining the emergence of magnetic features (such as fields associated with X-ray bright points) which are already small upon emergence. In this case, Golub et al. (1980) argued that their origin might well be in the "shredding" of larger flux elements. I thus do not believe we are in disagreement. (2) I believe that we are not discussing the same issue. I agree that the ultimate fate of fluxtubes is an important problem, but it is not the problem I discussed. I addressed the question: How might fluxtubes be formed in the first place? Thereby, J. Schmitt and I considered the stability of an equilibrium field configuration at the base of the convection zone to doubly-diffusive modes.

IONSON: What is the rotation-dependent force that competes with the buoyancy force? If one builds this effect into, for example, an α-ω dynamo, is the cycle time modified?

ROSNER: The Coriolis force is the competing element. Although it is of course hard to specify without doing the full calculation, qualitatively the answer is yes, the cycle time *is* modified. E. Knobloch, N.O. Weiss, and myself in fact recently looked at this very question (1981, *Monthly Not. Royal Astron. Soc.* **197**, p. 45P), and suggested that it is the buoyant loss process from the flux-producing layer that controls the time scale of field eruption.

FRISCH: You have rightly stressed the basic difference between ad hoc non-linear theories, which have little predictive power, and full non-linear calculations. Among the limitations of presently achievable calculations you mention problems relating to *turbulent diffusivities*. This, however, is a concept that belongs to ad hoc modelling (like dimensional analysis). The only known case where a systematic justification can be given for the use of turbulent diffusivities is when there is a clear-cut separation of scales. Otherwise, if we were to take seriously the concept of turbulent viscosity we would predict that high and moderate Reynolds number flows have essentially the same large scale features and differ only in the small scales. This however contradicts experimental evidence (e.g. from Taylor-Couette

flows) that by increasing Reynolds numbers we may have a transition from chaotic large scales to *more* organized (coherent) large scales. Such *relaminarization* phenomena are likely to play a role both in dynamo theories and in stellar variability theory.

ROSNER: I agree with your basic point, but I note that, as a practical matter, one requires some artificial dissipation at small scales in current simulations, which are very strongly limited in spatial resolution. In the absence of "turbulent" transport coefficients, energy cascading to large wavenumbers could not be dissipated at the smallest calculated scale length (where classical diffusion is still negligible).

VAN BALLEGOOIJEN: It was pointed out by Acheson (1978) that a small radial gradient of angular velocity brings about a new instability. This instability is more important than the usually considered buoyant instability for $v_A \ll 2\Omega H$ (this corresponds to $B \ll 6 \times 10^4$ G in the deep layers of the solar convective zone). An outward decrease of angular velocity makes the field unstable, while an outward increase stabilizes the non-axisymmetric modes of the tube.

ROSNER: Quite right. In our work, we explicitly avoided looking at these modes, except for the $\mathbf{B} = 0$ case, in which case we again showed (cf. Acheson, 1978) that one can retrieve the well-known Goldreich and Schubert result. As far as "importance" is concerned, I note that we use $B \approx 10^4$ G (which is, in light of our total ignorance, as reasonable a number as any), so that the inequality is barely satisfied. Furthermore, my comments regarding the inertial mode apply here as well: we really ought to do the non-linear calculation before deciding which modes are really important.

GALLOWAY: In your linearized model you took a prescribed magnetic field. In practice the field must be generated by a dynamo, and that needs motions. The convective overshoot motions themselves could do it, but they would presumably also bring the flux into the convection zone, where it would be removed very quickly. Do you have any other ideas about how to generate the field?

ROSNER: Your question aptly contrasts our idealized model with reality. Of course, we would like the penetrative flow into the boundary layer to carry flux down (to be amplified within the boundary by the ω-effect), but these very motions can also carry flux outwards. The point is that the actual situation is likely to be very far from a simple stationary equilibrium, so that there may never be a time during which linear analysis applies. That is yet another reason to look at the non-linear regime, as we are now doing.

SCHÜSSLER: We should distinguish between buoyancy-related instabilities, which depend on an influence of the magnetic field on the overall stratification holding up more material than would be there without a field, and genuine buoyancy of an isolated fluxtube, which simply is a non-equilibrium phenomenon. Furthermore, we must not forget convection: small fluxtubes are dominated by the drag force due to convective flows, while big flux ropes are influenced by buoyancy. Thus the two types of fluxtubes may behave quite differently.

ROSNER: You are quite right on both counts. In our paper (J. Schmitt and R. Rosner: 1982, *Astrophys. J.*, in press) we in fact go to great lengths to distinguish between the instability problem we deal with, and the buoyancy of "isolated" fluxtubes. As far as convection is concerned, I am a bit worried about available theories for the rise of buoyant fluxtubes. The problem may be very much complicated by processes such as recently discussed by Parker (1982), in which he shows that fluxtubes lying parallel to the axis of convective rolls can be "trapped" within regions of downflow, executing a stable motion with no net upward component. This seems to say that the actual rise rate of fluxtubes in a convection zone may be very sensitive to the details of the convective flow, and hence be a very difficult problem to solve generally.