DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY KIPPENHAHN

Skumanich — This may be quibbling with numbers, but if you use the revised age of the Hyades that Conti and von de Heuvel suggest then, in fact, I get that the rotation and calcium emission curve decay with an inverse cube root. But then the rotation, the lithium, and the calcium emission very rapidly decay past the Hyades point. Maybe that's due to the appearance of the Goldreich-Schubert strong mixing but in any respect there is this uncertainty about the ages.

Shatten — With regard to the mass loss term. I presume what you mean is the particle mass loss term which mostly affects the angular momentum, to distinguish that, as we said before, from the mass loss term of the star itself, mostly due to the loss of photons.

Kippenhahn — It takes 10^{11} years to get a loss of mass from the Sun due to the mass equivalent of the radiated energy. Correspondingly, the loss of angular momentum is negligible.

Jennings — I'd like to point out that on the pentagon diagram you had the coronal heating directly connected to mass loss. I think Weymann has shown that for late type giants and supergiants, there seem to be rather serious observational problems with that particular mechanism.

Kippenhahn — The arrows in my diagram just indicate possible influences they do not necessarily indicate important effects. The arrow in my diagram which indicates the influence of mass loss from coronae on stellar evolution presumably does not indicate an important effect, either.

Jennings — There is one other point I'd like to make. It seems possible that grains may drive mass loss. If that is indeed the case, and one has a grain field around some stars it would act as a strong sink for heating. One would have inelastic gas-grain collisions and the grains would radiate away a lot of the energy that might normally be deposited in a chromosphere-corona.

Skumanich — This again doesn't change your results. But I might say that Durney's argument follows even without assuming the mass loss, M, to be a constant. In fact one can show that the product of the mass loss times the Alfvenic "gyration" radius squared goes as B^2 So maybe we should look for that other little square root in the moment of inertia; maybe the revision of the Hyades age is correct.

Kippenhahn — One can repeat Dr. Durney's computations with different assumptions. But one always gets something similar to the Skumanich law

Underhill — I'm concerned about the remark you made, that towards their later stages of evolution stars, on their outside, don't really know what their age is inside. This rather worries me, because we stellar spectroscopists look at the outside of stars and say that's part of the star, therefore the star must have such an age.

Kippenhahn — If a star of a given mass comes twice during its lifetime to the same point of the HRD its spectrum should be the same unless chemically more evolved material has been brought from the deep interior to the surface. This is not in controversy with the usual age criteria, which either compare stars with different original metal content or different positions on the HRD. If a star, in its later evolution, happens to cross the main sequence, it normally will have a slightly higher luminosity than it had during its first main sequence stage. But, in principle, it would be difficult to distinguish whether the star is a real main sequence star or just an occasional visitor.

Underhill — That's what worries me, because every time we see a star of a certain type, we go to the first possibility and ignore the second.

Kippenhahn — What I said only holds for simple stellar models, corresponding to the outer ring of my pentagon diagram. But this is insufficient. The boxes of the inner ring are important too. They involve rotation and magnetic fields. The star coming to the main sequence for a second time would differ in its rotational properties. Therefore the Skumanich law should help you to distinguish between a young star and an old star at the same point on the HRD. There is another point which I would like to comment on. In the picture I sketched in my talk, a star like the Sun would slow down its rotation on the main sequence and, after a while, the dynamo would be rather weak and the enhancement of mechanical flux by magnetic fields would be small; the Ca emission would be weak. When the star leaves the main sequence and moves into the red giant area of the HRD, its angular velocity is getting even smaller, due to conservation of angular momentum. But at the same time convection becomes more violent. So we have two effects acting against each other: Rotation which goes down and convection which goes up. Which will win? But it would be possible, also, that even with slow rotation the dynamo becomes more active, since it has not yet been investigated how the effectiveness of the dynamo changes, when convection becomes stronger while rotation becomes slower. We also do not know how the enhancement of convection will affect the differential rotation. We therefore are unable to predict whether the Ca emission of the Sun will come back when the Sun will become a giant star.

Pecker — My comment is related to the question by Anne Underhill. Of course, the question she asked is: Are we right to use a 2 dimensional

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diagram to represent stellar spectra. And the reply is of course "no, we aren't right." To come back to the specific question: "Does the K line emission enable us to distinguish between the pre-main sequence or the post-main sequence stage?", I would like to refer to a computation which has been made in Nice by Nicole Berruyer. She shows (using Larson-Starrfield kind of techniques), that, when you reach the main sequence for stars of high mass (~20 solar masses), then the time of contraction of the envelope is long compared to the time during which the star is staying on the main sequence before leaving the main sequence. Therefore, near the main sequence, you cannot distinquish easily between pre and post main sequence stages; both stars still have very large envelopes. At the opposite, for a lot of stars in the H-R diagram (in the pre-M dwarfs for example, where you have the T, Tauri stars) it is exceptional to find an example which is still in contraction, because the lifetime on the main sequence is 10^{10} yrs. vs 10^{s}-10^{6} yrs. for contraction of the envelope. I think we should certainly look at things in the spectrum that are oriberia of the age of the stars, and others that are oriberia of the age of the envelope.

Aller — We can look at the problem of solar and stellar chromospheres from several different points of view. One point of view, which was emphasized yesterday, is understanding the manner in which chromospheres are created and heated in the neighborhoods of stars. At the outset we assume that chromospheres exist. Furthermore, we have some biased view of what they ought to be like from observations of the solar chromosphere. Now, how can we make use of this information in investigating the radiation of other stars? Here, of course, we are severely limited by the nature of our observational material. Whereas we can make detailed observations of the structure of spicules and other fine points of the solar chromosphere, observations of stars involve only their integrated light. It is true that one can make time resolved studies. These have shown, for example, rapid spectral changes in the emission lines of some stellar envelopes. Whether you call them chromospheres or not depends on your point of view. My favorite star in this respect is HD 45166 whose rapid variations were discovered many years ago by Carol Anger Rieke at Harvard Observatory. This is perhaps an extreme example. The question before the house is to what extent can we make use of chromospheres to evaluate the status of a star with respect to its evolutionary development. This was the point which was raised by Anne Underhill, and it is a matter which concerns many observers. For the most part, we are limited now to a narrow spectral range. Part of the material we urgently need falls in the "vacuum" ultraviolet and, until we get a proper space telescope, we are going to be frustrated in our efforts to get even a rough picture. In the meantime, we have to get by with

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what we have. In addition to conventional spectroscopic observations, we also have some radio data for a few interesting binary systems, though we have not yet begun to understand the physical significance of what we are observing. The rapid rise in the efficiency and sophistication of infrared techniques will undoubtedly give us a great deal of information about this important spectral region. This infrared radiation may not *all* come from dust clouds, as is the favorite hypothesis today, but some of it may come from bona fide chromospheric activity. Therefore, from the observational point of view, there are only a very small number of handles that we can grasp, a very small number of things that we can do. Those of us who are observers would like to have the help of theoreticians who may point out what are the specific observable phenomena that we should seek in different stars in different parts of the Hertzsprung-Russell diagram in order to get clues as to evolutionary development.

Durney — I would like to discuss some work I have done recently with John Leibacher of JILA on the location in the HRD of different types of stellar winds. In a recent paper, Roberts and Soward (1972) have determined in the N_o, T_o plane (the density and temperature at the base of the corona) the regions where the stellar winds are A) supersonic for distances larger than the critical point located outside the surface of the star (usual stellar winds), B) always subsonic (stellar breezes), and C) supersonic for all distances larger than the surface of the star. With the help of Kuperus' (1965) calculations of N_o and T_o for a variety of stars, we locate the stellar winds of type A), B), and C) in the Hertzsprung-Russell diagram., The relevance of static envelope models for stars with winds of type C) is discussed.

Figure IV-7 is taken from Roberts and Soward's paper (1972). In the following, we designate by "subsonic" the "Chamberlain" region of Roberts and Soward. This is not quite proper, and we refer to the above paper for a more detailed discussion of this region. If N_o and T_o are located to the right of the dashed curve, then the stellar wind is supersonic from the surface of the star outwards. Since T_o is given in units of $\frac{GM}{\sim}$pp (where G is the gravitational constant, m half the hydrogen mass, M and R_o the mass and radius of the star and k the Boltzmann constant), it is clear that if R_Q is large and the star has a corona with a typical coronal temperature and density, then the stellar wind, according to>Figure IV-1, will be supersonic for all distances larger than RQ. As an example, if we assume $T_o = 2 \times 10^{6\circ}$K and $M = M_o$ the critical radius of the star for which the stellar wind is supersonic from the surface outwards is ~ 13 R_o for $N_o > 2 \times 10^5$ cm"^{3}

We discuss now the physical meaning of stellar winds which are supersonic at the surface of the star. It is well known that the stellar wind

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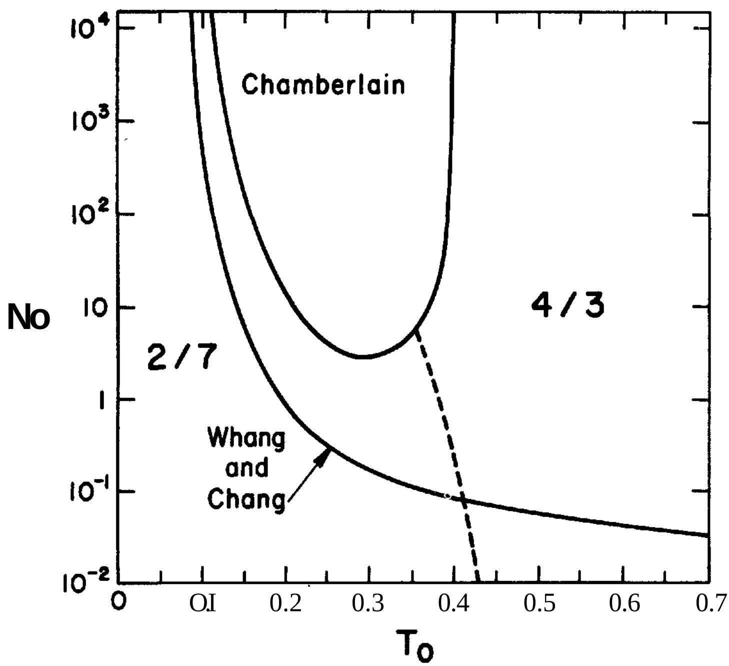

Figure IV-7 The types of acceptable solutions of the stellar wind equations as a function of the temperature, T_Q and density, N_o at the base of the corona (T_Q is measured in units of GMm/kR_Q and N_Q in units of $2K_Q$ **(GMRQ^/k/k** where $K = K_{ft}$ $(T/T_0)5/2$ is the electron conductivity). In the regions denoted by 2/7 and 4/3 the asymptotic behavior of the temperature is $T \sim r^{2}/^{7}$ and $T \sim r^{4}/^{3}$ respectively whereas $T \sim r"^{2}/^{5}$ for the Whang and Chang line (c.f. Durney and Roberts 1971). To the right of the dotted line the flow is supersonic at the surface of the star. (From Roberts and Soward 1972).

equations allow for two degree of freedom: it is possible to give arbitrarily N_o and T_o or alternatively the mass flux, C, and the residual energy per particle at infinity, e«>. The mass flux, C, is introduced in the momentum equation by the use of the continuity equation, and €<» is the arbitrary constant appearing in the integral of the energy equation. These two equations are of first order and the two boundary conditions which determine the flow speed and temperature are (a) T -> 0 as r -* °° and (b) p -*• 0 as r -*• °°, i.e. the solution should cross the critical point. This last boundary condition disappears when the stellar wind is supersonic from the surface of the star and the problem becomes undetermined. The mass flux, for example, could be given, between limits, arbitrarily. In such a star the solution of the stellar wind equations is not as simple as it is for the Sun. The heating of the corona by acoustic waves must be included explicitly and the equations must be started from the chromosphere where the velocities are subsonic. Static envelope models for these stars are probably not meaningful. Ulmschneider (1967) has calculated the structure of the outer atmosphere of cool stars. By virtue of the above, however, we consider that his determination of the initial flow Mach number, M_o, is very approximate; M_o should be determined by requiring only that T -> 0 as r -> 0. The supersonic of subsonic character of the flow cannot be prescribed as a boundary condition.

With the help of Figure IV-7, and values of N_o, T_o and R_o as evaluated by Kuperus for a variety of stars, it is possible to give the approximate location of stellar winds of type A), B), and C) in the Hertzsprung-Russell diagram. This has been done in Figures IV-8 and FV-9 There is no doubt

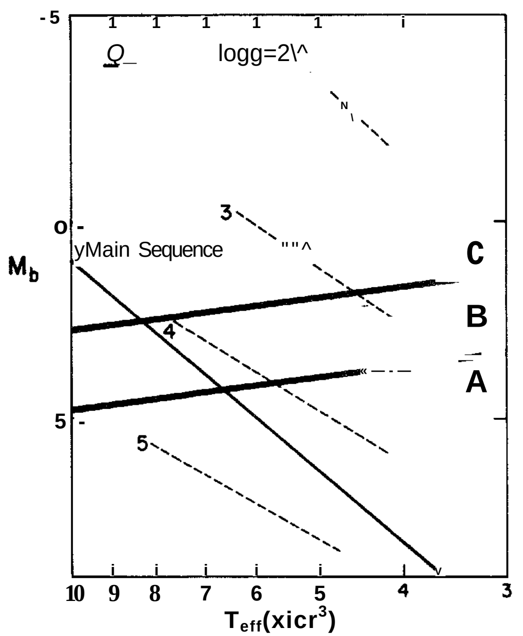

Figure IV-8 The mass of the star follows the mass luminouslty relation. Regions A), B), and C) have been determined from Figure IV-1 and from the values of N_Q T_Q and R_Q as evaluated by Kaperus (c.f. Figure 22 of Kaperus (1965). Region: A) usual stellar winds; B) stellar breezes; C) the flow is supersonic: at the surface of the star.

that Kaperus calculations are very approximate. However, since in classifying stellar winds according to type A), B), and C) the values of N_o and T_o are not too critical we can have some faith in the general location of regions A), B) and C) in the Hertzsprung-Russell diagram. We stated above that the validity of static envelope models of stars in region C) had to be carefully examined. It is tempting to speculate that stars with very large

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radii can suffer appreciable mass loss by this process of "coronal evaporation" (c.f., Weymann (1960), for the cage of red giants). The importance of radiation pressure in the mass loss of hot stars and stars with circumstellar dust shells has been considered by Lucy and Solomon (1969), and Gehrz and Woolf (1971). Further work on this subject is in progress.

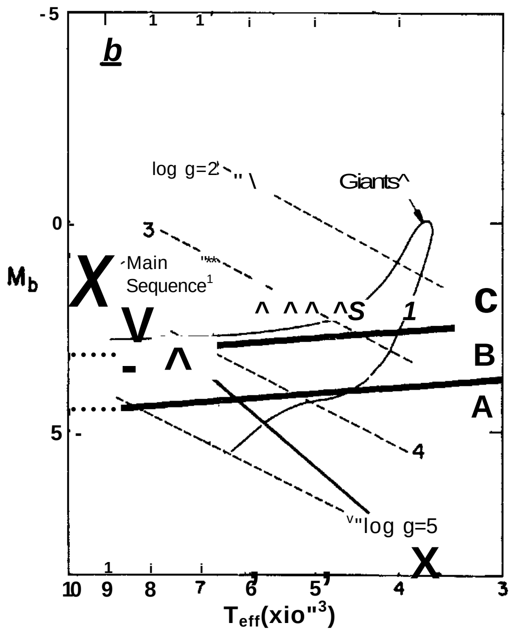

Figure IV-9 The mass of the star is equal to the mass of the Sun. Regions A), B), and C) as in Figure IV-2a).

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DISCUSSION FOLLOWING TALKS BY PRADERIE AND DOHERTY

Heap — We can see where the velocities are perhaps supersonic at the surface of the star. What do you mean by the surface? Is it the stellar evolutionists' surface or the photosphere?

Durney - T_o is measured in units of GmM/kR_0 and N_o in units of 2 $K_0(GMR_0)^{-1/2}/k$; R_o is the surface. In general R_o would be the distance at which energy deposition takes place.

Heap — My next comment is on the stars populating your Region C. A regular 0 star is in Region C, and a planetary nucleus would also be in Region C. Observations of these stars tend to support your suggestion that Region C objects have some sort of chromosphere. As I mentioned earlier, both types of stars show a velocity-broadening that is 75 km/sec or greater. In the case of some Of stars, both young stars and the very old planetary nuclei, the Hell X4686 line is very broad, indicating velocities up to ±1000 km/sec, so these stars have a mechanical flux which could possibly be dissipated in forming a chromosphere. There is one young Of star, Zeta Puppis, showing broad Hell X4686 emission whose chromosphere in fact has been seen. The UV spectrum of this star has been observed from rockets by Morton and Smith, and it shows several high-excitation emission lines. These UV lines would be an indication of a chromosphere, because the excitation of, say the O VI emission line, certainly is greater than that of the photosphere. No planetary nucleus has been observed in the rocket-UV, but there is possible evidence for chromospheric enhancement of radiation in the far-UV, below the Hell limit at 228A. The evidence lies in the discrepancy between the Hell Zanstra temperature and the temperature derived from the visible spectrum of the star. For example, me nucleus of NGC 2392 has a Hell Zanstra temperature of 94,000°, while the visible stellar spectrum indicates a spectral type of 06 or 07. Perhaps the nebula is "seeing" chromospheric radiation from the resonance lines of high-ionization states of C, N, and O rather than photoperic radiation.

Durney — You are right, the temperature range is too large. This is because the figures shown are identical to Figure 22 of Kuperus. The division of the Hertzsprung-Russell diagram into regions A), B), and C) applies only to those stars for which Kuperus did evaluate N_o and T_o. In particular he did not calculate N_o and T_o in the high temperature region of the figures.

Jennings — When you say that this type of flow might affect stellar structure, is that to mean that the quasistatic approximations for calculating models would not be valid?

Durney — For calculating envelopes they probably break down.

Jennings — **I** would argue by continuity that the interior would not know anything about this mass loss.

Durney — In general, I think it would not.

Cassinelli — **I** think it should be pointed out that radiation pressure effects become important in region C. At the higher luminosities the outward acceleration due to the radiation pressure gradient may be greater than, or equal to, the inward acceleration of gravity

Durney — Right. In this calculation the radiation pressure was not included.

Ulrich — How sensitive is the location of the boundaries on the H-R diagram to the mechanism you used to derive those values. You don't know what the coronal temperatures are.

Durney - We have accepted Kaperus results. From Figure IV-7 and the units in which T_o is measured (GmM/kR_0) we expect regions A) and C) not to change much for values of T_o not drastically different from typical coronal temperatures. This is because the range of variations in mass is smaller than variations in radius. Region B) demands a more sensitive balance of N_o, T_o, and R_o, and could, for example, disappear if Kaperus calculations are seriously in error.

Aller — So evidently in these hotter stars the wind starts blowing very close to the stellar surface so that you do not have a conventional chromosphere. I mean you don't have a semi-steady one like the solar chromosphere; the wind is blowing all the time; the material is always flowing outward. Is that your conclusion?

Durney — Yes.

Underhill - But that doesn't mean it's not a chromosphere. We didn't define a chromosphere as having to stay still.

Aller — The physicist's problem is that one must consider differently a mass of gas which is moving violently 'outward from one which is quasisteady. The velocities are already large at the surface of the star.

Thomas — If I were to paraphrase what Delache said yesterday, what you just said is not true. All that happens is, when I look at a sequence of stars, maybe I have to worry more and more about the outward component of the mass flux as I change the spectral type. Sure, I agree in detail it's different, but, in terms of the broad physical picture, it is not.

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Aller — Precisely these details may be very important in an interpretation of the data and analyzing the obtainable observations which may admit several, equally plausible, but different pictures.

Thomas — The details are always important, but unless I know the structure first, I become a number juggler. You must first have a structure; then of course, if *all* you have is a structure, you're going to miss the details. I must have the detailed computation of the numbers to be able to convert the observations. But if I just compare the numbers without having the structure, then it looks as though each different star is a problem by itself; and that is a viewpoint I disagree with.

Aller — I would disagree with that too.

Kippenhahn — Am **I** to understand that you want to redo the work and replace the normal static by kinetic boundary conditions? One would then expect that this would remove the difficulty that near the surface you get supersonic velocities. Then everything would look rather decent. The difficulty that you encounter is that you use stellar models with static atmospheres and fit to these dynamic atmospheres.

Durney — This proves that one cannot construct static models. One needs to construct consistent kinetic models.

Pasachoff — Again here we have to be careful that the definition that we get for a chromosphere does not exclude the solar chromosphere, which is not at all static. The solar chromosphere is probably composed entirely of spicules, which have velocities of approximately 20 km/sec.

Durney — But the outward flow velocity is small in the solar chromosphere. It is only randomly non-static.

Underhill — I think we're just hung up on the fact that an observation of a line profile gives you an average over the stellar surface. It can frequently be **a** net general outward velocity. What you're saying about spicules in the Sun is that you're looking at individual features and you can see there are large changes. It is a question of statistical averages.

Aller — That is correct. The fine structure doesn't change things very greatly. I think there is a rather important qualitative difference between **a** chromosphere like that of the Sun and the atmosphere of a P Cygni star. Aside from the greatly different temperatures involved, the inherent differences in the velocity fields give them a very different character. I'm not saying they shouldn't be called chromospheres, but I think we have to be aware that the definition may embrace envejopes that are almost, but not quite, in hydrostatic equilibrium, on the one hand, and also evanescent structures, where you have a violent wind blowing, on the other.

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Stein — In looking at your first figure, one might turn that around and say that the boundary of the region where the flow is supersonic from the surface, gives an indication of the maximum temperature which is possible in a corona, since, for higher temperatures, such a large mass loss (and therefore energy loss) occurs, that the temperature is reduced to approximately the critical value. This might provide a limit to coronal temperatures.

Durney — Yes, but there is some arbitrariness in this. We think that one needs to solve the problem consistently.

Stein — It's true that it's arbitrary, but because the thermal velocity is approaching the escape velocity, the maximum possible corona temperature must be of the order of your T_o.

Durney — **I** agree.

Skumanich — I'm disappointed that Dean Petersen hasn't said anything. I received recently a preprint from him about the problem of a radiation driven flow in which the sonic point is inside the envelope. He uses a plane parallel approximation to the flow, because he's dealing with a fairly large radius, but I think the physics is really the same. You only get aC^2/rterm difference in the driving forces what he finds is that the flow is decelerating after it goes through the sonic point. It reaches a maximum and then becomes a decelerating flow. But what bothers me about the work is that there is a finite pressure, a wall as Delache said the other day I don't know where it comes from and what its consequences are on the actual detailed dynamical flow. Is this wall the back pressure of the outward traveling shock that the wind has ultimately produced in the interaction with the interstellar medium? There must be some time dependent phenomenon at the leading edge of this wind and at the tail where the rarefaction is eating into the envelope of the star. So these steady flows are quasisteady flows in the sense that they settle down to a constant form in space, but they have time dependent leading edges. I don't know what that does to this whole problem, the time variations and so on.

Pasachoff — Though we haven't agreed on what a chromosphere is in the general case, I thought that we should at least show the meeting a picture of one, so that we know what it really tooks like. Figure IV-10 is a photograph taken of the solar chromosphere in Ha at the Big Bear Solar Observatory on May 22, 1970. It represents the current state of the observer's art. Resolution is better than 1 second of arc.

Underhill — I'm not at all sure about the revisions of T_{eff} vs B-V around type AO V The revision you were talking about brought Vega down from 10,000 to 9750°K.

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Figure IV-10

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Conti - No. It was 10800 to 9750°K.

Underhill - So it brought T_{eff} down 1000°K. Consider the problem of the large ultraviolet blanketing which I discussed a couple of days ago. If you enter that into our standard model calculations, you get the usual effect of backwarning. The net result is, as Deane Peterson mentioned, you can probably make a fully line blanketed model for Vega that fits all of the visible region with an effective temperature of the order of 9300°K. This is one of the difficult things that you have to remember about model atmospheres: they are only models, and every time we put some more factors in them, this parameter, T_{eff}, which is essential for stellar interiors comes out a bit different. The real problem is that this parameter is not at all essential for stellar atmospheres. We're trying to tie this B-V to a non-essential parameter. In fact stars can have exactly the same spectrum and be of entirely different ages. You have to realize that this pillar of stellar structure is no pillar whatsoever for the stellar atmosphere. You have to look for another pillar.

Conti — There was some discussion by Andy Skumanich on the revised age of the Hyades from a paper by van den Heuvel and the applicability of his number. I should say you better believe it. If you look at his paper, which is in the P.A.S.P. of about 2 years ago, you'll see that when you draw a theoretical H-R diagram with the turn-off age between 8 and $9 x 10^9$ years, for the Hyades, it matches extremely well all of the members of the cluster. That's where the age determination comes from. The reason that the age was revised by about a factor of 2 was that when you go from a theoretical H-R diagram to an observed H-R diagram, you've got to make some connection between a B-V color and an effective temperature. What had happened was that the T_{eff} vs. B-V relationship for A type stars had been altered, primarily because of the continually changing temperature calibration of Vega. It has now pretty much settled down, and what van den Heuvel realized was that this would effect the turn-off diagrams and, therefore, the evolutionary times of clusters that have turn-off points somewhere in the A stars. For example, this does not affect the Pleiades nor M67 but it does affect the Hyades. This somewhat more elderly age for the Hyades does now have implications for the solar system, the lithium depletion, and the H and K emission, and so on, as Andy mentioned. That's the first comment. My other comment has to do with massive stars. We've heard that the star forms and that the star starts burning nuclear fuel, and that the envelope still doesn't have enough time to fully contract. This is the work of Larsen and Starrfield. I think this has direct application to the star I discussed on Tuesday, 0^1 Orionis C, where we see material accreting. I just wanted to make that connection.

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Pecker — I want to reply to Peter Conti. Consider the H-R diagram. Now we've just discussed the calibration of T_{eff}. I want to draw attention to one thing which is extremely relevant to the problem. In those stars with strong emission lines, very often infrared excesses are strong, but not always known. Then bolometric corrections are absolutely wrong. For example HD 45677, a Be star, has a bolometric correction a little bit more than one magnitude in error, compared to the value given by the classical B models. That is my first comment.

The second comment is linked with what Peter Conti said about the reversed P Cygni profile observed in a Trapzium 0 star. Such P Cygni profiles are associated with contracting (or pre-main sequence) objects. There is another case which no one has mentioned yet at this meeting, and that is FU Orionis. I would like to draw your attention to a series of papers which has been published in Russian by Ambartsumian (and so far translated for me by an Armenian astronomer). This comment is quite relevant to the origin of the heating. Ambartsumian is regarding the Hayashi like theories for contraction before pre-main sequence stars as unsuitable for objects such as FU Orionis. He is assuming that there is a new class of objects that he calls "FUORs", the first one of them being P Cygni itself. I might recall the fact that P Cygni is now of a magnitude which is visible, while at the time of Tycho Brahe it wasn't. This is the reason which makes Ambartsumian think it is a star of this type. P. Cygni is number 1, FU Ori is number 2, in this series of objects. Number 3 is Lick Ha 190. According to Ambartsumian, a FUOR is a super dense star, a member of a binary, and from time to time the super dense star is throwing away high energy particles, which are heating the outer part of the other star. This is what creates the chromosphere and its abnormal heating. I just wanted to draw this to your attention because I don't think we've been exploring all the possibilities of heating. We have so far been trying to concentrate only on the heating from inside. My question is, are there any possibilities of heating *from outside?*

Then I come to my third point, which is a question for Dr. Kappenhahn. (Now let's forget about this reference to Ambartsumian; I don't know whether or not I can believe it. I think I am myself more in favor of the classical contraction theory of Larson, or Penston, especially for the interpretation of FU Orionis.) My question is: when you have a pre-main sequence star, in pre-main sequence evolution, then you have something which contracts. To avoid confusion, let's not take a hot star where there is the extension of the HII region which mixes up the problem. Let's take a cold star. There is some energy which is released by contraction of the mass. Now where is this energy liberated, what is the quantity of energy which is liberated, and can it contribute to the formation of a chromospheric heating and of a chromosphere? I ask this question specifically for the T tauri stars.

Kippenhahn — Do you have in mind that the star is contracting and probably there are some outer layers which follow more slowly? We come back to the old problem of meteorites falling on top of a star and heating the outer layers. I would say this is still possible for the T Tauri stars, although what we observe is that there is mass loss from these stars and no infalling material. But, on the other hand, if you look at the Larsen solutions of the problem of star formation, you find dust clouds raining on top of stars for long periods. Material is falling on stars which have just been formed. They might already be close to the main sequence. What will happen with the kinetic energy of the infalling material? Certainly -this is a problem which should be looked into.

Kuhi - **I** think bringing these stars into the discussion is going to throw the field wide open for drastic speculation. It is interesting to me that the calculations by Larsen and others for contracting stars always show material falling in during the contraction phase, and also a large amount of dust surrounding the star which presumably then reradiates in the infrared. Aside from about six or seven stars in Orion, there is no evidence for any infalling material in any contracting star that I am aware of. It is ironic indeed that Peter Conti should mention a star which is a very high temperature object, with which we normally associate a large HII region, a large radiation pressure, and from which we would normally think material is being driven off the surface. On the other hand, he finds material falling in. It seems to me that somewhere our theory is in drastic error. The point about the dust clouds surrounding these young stars is a very good one. I should mention the observations of Gary Grasdalen (a graduate student at Berkeley) of stars like Lk Ha 190, which is also known as V1057 Cygni. This object was an extreme case of a T Tauri star before it blew up (or whatever else it did), having a very rich emission line spectrum which some of us would call a chromosphere. Anyway, if we accept Larsen's picture, then we must also accept a large infrared contribution to the flux for this object in its pre-outburst phase. After its outburst it was indeed a bright infrared object, so we might say everything is fine. However, Grasdalen has looked at a number of T Tauri stars in the same part of the sky which have virtually identical spectra to the pre-outburst Lk Ha 190 spectrum, and'he finds no infrared excess whatsoever. So I think that our theoreticians have much further to go than they would be willing to admit.

There is one other point that I would like to add about bolometric corrections. The infrared observations have cast considerable doubt on our old ideas concerning even the hot stars. Many of the hot stars, especially

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Ae and Be stars, have shown large infrared excesses, and when one adds these to the total fluxes emitted by the stars to get the total bolometric luminosity, I think we find again serious discrepancies with previously held ideas. The same thing applies to the pre-main sequence contracting stars. You often find that the luminosities in the infrared are many times that in the visual and coming back to the T Tauri stars, you find that you need masses much larger than the previously assumed one or two solar masses to explain the total luminosity. Just to make one concluding remark, Lick Ha 190, a typical T Tauri which we all thought was one solar mass popped up and is now an A supergiant. Explain that.

Aller — You're giving the theoreticians a pretty rough boundary condition.

Böhm — I would like to ask Peterson: how can you calculate mass loss in a plane parallel approximation? Isn't it true that in a plane parallel approximation you have to do an infinite amount of work to push matter to infinity? I don't see how one can ever get mass loss, but maybe I misunderstood something.

Peterson — That's right. Because it is an artificial geometry, you have to impose a sink at the top of the atmosphere. Basically, it shows up in the equations as a finite boundary pressure. Fortunately the equations do not leave that boundary pressure a free parameter.

Böhn — So this pressure which was mentioned is somewhat artificial.

Peterson — It's artificial, yes, and it goes away in the spherical case.

Lesh — I'd just like to add something to Anne's comment that stars can have the same spectrum and still have widely or slightly different ages. We have been looking at a class of B type variable stars, the j3 Cephei stars. As a star evolves away from the main sequence it turns around at a certain point and describes a loop in the H-R diagram, as you well know. Near the turnaround point, a star can actually be doing quite a number of things. It can be evolving away from the main sequence; it can be contracting back; it can be burning hydrogen in a shell source; and, in addition of course, it might be contracting towards the main sequence. In a particular small region of the HRD, there are a large number of normal non-variable B stars, but there are also about 20 of these odd creatures called j3 Cephei variables. It seems very likely that they (variable and non-variable stars) occupy the same region of the H-R diagram, because they are'in different stages of evolution, in other words, because they have slightly different ages. However, the work I have done on these stars with Morris Aizenman at Montreal has shown that there doesn't seem to be any spectroscopic distinction between the variable stars and the

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non-variable stars. So it would appear that here we have a case of stars which do, in fact, have slightly different ages, if we assume that contraction towards the main sequence is ruled out, but which do not differ in any observable spectroscopic fashion.

Aller — This would appear to be another incidence where the surface of the star doesn't pay any attention to what the interior is doing.

Boesgaard — I'd like to discuss *α* Centauri in connection with differences in the chromosphere with stellar age. *α* Centauri is a triple system. The first component A is exactly the Sun observed at stellar distances; it's a G2 V star. Component B is a Kl dwarf, and component C, Proxima, is a dMe flare star. The fact that component C is a flare star would indicate that it, at least, and presumably all three stars, are probably young. However, the intensity of the chromospheric calcium emission in *α* Cen A and B, gives, as far as I can see, no indication that those two stars are young, *α* Centauri A has very weak calcium emission. It looks similar to the Sun. At 3.3 A/mm on a long exposure one can just see weak K2 features. I have two long exposures of this taken at Mauna Kea. (We can get down to v declinations of -60.) The two spectrograms look slightly different in the K2 structure. In one case it looks like the red peak is stronger than the blue; in the other case it looks like the 2 peaks are of equal intensity, but I'm not willing to say that this represents a solar cycle type of variation, or the kind of local variation you see in the Sun, because the emission is so weak.

The Kl star shows a calcium intensity of 2 on the Wilson scale. This was also observed by Warner. That's about the relative intensity you'd expect, for the relative temperatures of the two main-sequence stars.

Aller — Do we really know enough about emission processes in dMe stars to apply this rule? I was under the impression that *α* Centauri C was a fairly "late" M dwarf, that is to say, advanced in the sense of spectral type, in other words, a very cool object. I wonder how well the calibration works down in that spectral region.

Mullan — There is unfortunately no simple relationship between the age of a flare star and its level of flare activity. Haro and Chavira (Vistas in Astronomy 8, 89, 1965) observed flare stars"in seven clusters ranging in age from the Orion group to the Hyades. They found that, as a flare star evolves towards the main sequence, it flares more frequently. This was directly opposite to a prediction of Poveda who believed that the youngest flare stars high above the main sequence should have retained fossil magnetic fields, and should be more active than older flare stars near the main sequence. However, observational selection could account for the effect discovered by Haro and Chavira if the absolute luminosity function of flares is [illegible]

On the subject of fossil magnetic fields, I would like to supplement what Professor Kippenhahn said about dynamo fields by pointing out that fossil fields may also be important in understanding stellar chromospheres. Unsöld showed that the decay of emission in the H and K lines of Ca II can be understood in terms of the decay of fossil fields by Joule dissipation. This is not to say that dynamo fields are never important. For example, although flare stars, in all likelihood, require strong surface magnetic fields, it may not be important whether the fields are fossik or have been generated by dynamo action. A flare star might conceivably go through two phases of flare activity, one in which its field is fossil, the second in which the field is dynamo generated. This would help to interpret the lack of a unique relationship between the age of a flare star and its level of activity.

Boesgaard — Isn't there information on the statistics of the galactic orbits, to get an age indicator for the flare stars?

Mullan — Galactic orbits provide information about ages of field stars. The results of Haro and Chavira are confined strictly to cluster stars. In the case of the field stars, the most significant feature of the galactic orbits of M stars is Delhaye's discovery that the dispersion of peculiar velocities of dMe stars is significantly smaller than that of dM stars without emission. As a subgroup of the dMe stars, flare stars are then expected to be, on the whole, a young group. But within a group so young, age discriminators are not really available.

Boesgaard — Any connection between that and the amount of flare activity?

Mullan - I don't know.

O. Wilson — About a year ago, Woolley and I had a paper in the Monthly Notices in which we compared the results that I got on about 400 (Vissotsky) stars, on which I made very careful eye estimates of the intensity, with the predictions of galactic dynamics, which are that the older the group of stars, the greater should be the eccentricities of the galactic orbits and the greater the inclinations. This correlation was extremely good. There were no flare stars in the group, or there were so few that they didn't matter. But just looking at the spectra, I would say that the flare stars form a continuation at the end of the sequence where the calcium emission is very strong and where you see Balmer emission; they lie just a little bit farther along. But of course they're relatively rare.

Aller — And you would conclude that these are relatively young stars.

O. Wilson — **I** think there's no question about it.

Underhill — But the question is: does young mean a fraction of the total evolution track, or does it mean literally counted off in seconds as determined by atoms on the earth?

Alier — I presume that it means young in the sense that ***a*** Centauri A and ***a*** Centauri B would not be as old as the Sun, according to this reckoning.

Boesgaard — That's my impression, because C is a flare star. But I don't have any estimate in years.

Kippenhahn — I would like to comment on the question about the fossil fields. In that area of stars where we deal with calcium emission, we have no evidence of fossil magnetic fields. In the case of the Sun we have a dynamo generated field. With the dynamo fields you can expect an age dependence of the chromospheric activity, as it is observed, while for the fossil fields you would have a time independent chromosphere.

Jennings — **I** have a clarification question. At what dispersion were those observed?

Boesgaards— 3.3 A/mm.

Jennings — Have you actually traced them to see what the emission percentage is, and is it about 4% of the continuum like the sun?

Boesgaard — Approximately. I don't have an exact number.

Kandel — I must say that the pictures occasionally have been puzzling. Several years ago I looked at 61 Cygni B at lOA/mm. 61 Cygni is generally said to be old, associated with a group which has an H-R diagram like M67. Yet, it has awfully big H and K emissions. I couldn't resolve whether there was a central reversal there, but the emissions themselves were rather big. I think Dr. Wilson has observed variations there. Perhaps he would comment on that.

O. Wilson — I will talk a little bit about this subject this afternoon and, while 61 Cyg B has certainly a well marked emission, I can find you other stars of similar type that have 2 or 3 times as much. So it's a relative matter.

Aller — **I** would like to ask some of the stellar evolution people if they have tried to determine an age for the ***a*** Centauri system by seeing how well it fits the general main sequence. My impression is that it has not evolved off the main sequence by any distance sufficient to allow us to draw any conclusions. That's why it will be difficult to get its age by evolutionary arguments, even though it is certainly a star whose mass, luminosity, and perhaps even radius, are very well known.

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Kippenhahn — I would guess that it is not possible to do this. Just the uncertainties we have in the opacities may spoil the whole picture.

Steinitz — It has been mentioned a few times that there is a connection between the age and the characteristics of the spectrum in the atmosphere. One thing that has been mentioned is the chromospheric activity. It has been claimed that the atmosphere doesn't know about the age of the star below it; as an example, the B Cephei stars have been taken. I would like to mention the mere fact that we have classified them; that they look like other stars in the same region; yet, they oscillate while the others don't. So obviously the atmosphere knows that something else is going on.

Lesh — The fact that some of the stars oscillate while the others do not does not mean that the atmosphere of the stars knows how old they are; the interior does. It is very likely that the oscillation arises in the interior and not in the atmosphere.

Steinitz — It is an age effect.

Lesh — Yes, it is an age effect in the interior and not an age effect in the atmosphere.

Aller — I don't know to what extent we want to discuss the spectra of oscillating stars. That is a fascinating field in itself, but perhaps we'd better settle this question first.

Hack — The line contours are rather different in the spectra of normal B type stars and in the spectra of *(5* Conis Majoris, which sometimes show one, two, or three components variable with time and having different radial velocities. So I don't agree that they are equal to the normal main sequence stars.

Aller — Well, certainly with high dispersion, the spectrum of *a* Scorpii, for example, doesn't look just like that of a normal B star. There are important differences. Please tell us What dispension you are using. We are talking about utterly different problems here in the sense that the K line effects mentioned by Mrs. Boesgaard can be detected only by going to very high dispersions, of the order of 3A/mm. They are very small effects, whilst the effects that you see in some of these oscillating stars like *a* Scorpi, which belongs to the j3 Cephei class, are fairly obvious at relatively low dispersion. The changes are probably photospheric effects rather than chromospheric effects or strictly upper atmospheric effects of some kind.

Conti — I'd like to return again to these 0 and Of stars, and point out that what we think is the mechanism for the emission forming region and the extended envelope has something to do with the radiation pressure. I think Cassenelli has already mentioned this. Another thing which has been

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mentioned a little in the literature, but which is now receiving more attention, is the variation in the emission lines that you see in these stars. For example let's say you observe that emission lines vary on time scales somewhere between time scales of several minutes to several hours, I mean they're really drastically varying. So in addition to the extended envelope which we certainly do have, we have very good evidence of changes going on in this envelope which are very reminiscent of other kinds of stars. In fact, if I may return to my Zeta Puppians looking at the atmosphere of their star in the light of X4686, they might not be too surprised to see something looking a little like a solar spectroheliogram in Ha.

Aller - It would probably look even more striking than that.

Lesh — If **I** may just answer the comments of Dr. Hack. I'm talking about mean properties of these stars which are, in fact, observed at rather low dispersion, on the order of 60 to 100 A/mm. It is true that the line profiles in the /3 Cephei stars vary, but the mean profile — unless I'm very much mistaken — is not distinct from the mean profile in a non-variable star. Likewise the colors of the |3 Cephei stars vary. But if you take the mean color, which is actually what you use to locate stars in the H-R diagram, it is not statistically different among the variable stars than among the non-variable stars.

Aller — That's an interesting point. I don't think it's a statement that can be made for Cepheids. Maybe Mrs. Gaposhkin could answer that. Does the mean spectrum of a Cepheid look like any other star, or can you tell it immediately from the appearance of the spectrum.

C. Payne-Gaposhkin — You certainly can tell.

Heap — What happens to the Call emission of say, a G star and a B star when they enter the red-giant branch? What are the time-scales involved in the development of their chromospheres? If the magnetic field and calcium emission of G stars decrease with time, why do red-giants of one solar mass have strong chromospheres?

Kandel — Nobody knows, but, in principle, the calcium emission should be detectable.

Kippenhahn — The effect of rotation on the Ca lines, via magnetic fields, during the evolution, will become less and less important while convection will become more effective when the star becomes a red giant.

Thomas - We're presumably worrying about chromospheres, and I read this very ambitious statement: "what properties of stellar chromospheres vary with stellar mass and age" So long as one talks about chromospheres

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associated only with the convection zone, then you're limiting your sights very much. I agree, from the standpoint of rotation, that in the example which you have given, you've tried very hard to tie in with something else, which one knows can produce a mechanical flux. Still, from my own standpoint, as one who believes that *all* stars have chromospheres, so long as you restrict yourself to only those two viewpoints, then you're still restricting your sights very much. I prefer Len Kuhi's comment of just a little while ago, that maybe the theoreticians should be more ambitious than they are. He's trying very hard to understand what is meant by a chromosphere from the standpoint of understanding. Is it indeed something which is a property of all the stars? So I think one of the things we have overlooked badly in this conference is to ask all those kinds of physical processes which can produce, in any way, any kind of mechanical flux of energy. That's why I personally like to associate the definition of a chromosphere with a mechanical flux of energy. But let's not argue. Let's take whatever definition we want, but realize that we are talking about *general* structures of stellar atmospheres.

Cayrel — I have a question related to Dr. Kippenhahn's talk. Dr. Kippenhahn pointed out that rotation, age, and magnetic fields are three related things. I remember that at the time it was said that microturbulence could be also correlated with these three things. The problem is that I don't really see the mechanism by which microturbulence could be related to these things. Would you comment?

Kippenhahn — I am not prepared to say anything at the moment to your question, but since I am already standing I would like to make a comment. I agree with what Dr. Thomas said. I think we should ask what are the observational facts, or how can we find out whether the chromospheres are related to convection or not. Before the meeting, when I still was very naive, I thought that the calcium emission we see in G stars indicated chromospheres. Now, I learn that if we do not see Ca emission, this does not tell us anything. We have to determine whether the lines are collision dominated or photoelectrically dominated, and — as far as I have understood the complicated story — we then still do not know whether there is a chromosphere or not. On the other hand, we learned from Dr. Praderie that the border line in the HRD between stars with Ca emission and those without is a straight line which coincides roughly with the Cepheid strip and its extention to the lower left. It happens that this line is close and parallel to the line which separates the stars with pronounced outer convective zones from those without. *Is this accidental?* Can we learn from the experts of line formation whether, from this fact, we can conclude that chromospheric activity is driven by convection? Or must we say Praderie's border line of Ca emission is just a border line for the significance of the Ca emission as an indicator for chromospheric activity?

Thomas — My comment was not that calcium emission may not be a strong indicator of chromospheres, where it occurs, but that there are also many other kinds of indicators of chromospheres in regions where we don't find convection zones. I don't disagree with what you say. I say only: please expand it.

Böhm-Vitense — I would like to ask a question. Is the magnetic field proportional to the velocity Omega independently of the efficiency of convection? The convection is important isn't it?

Durney — Yes. We used Leighton's model for the solar cycle. This model has some arbitrary parameters which are chosen so as to reproduce the Sun's magnetic cycle. For stars with different convection zones these parameters would be different. There is every reason to believe that again B anand £2 would be proportional.

Böhm-Vitense — This means that the proportionality constant depends on the spectral type, doesn't it?

Durney — Yes. The proportionality factor between B and fi may depend on spectral type.

Ulrich — I'd like to make a connection between today's and yesterday's discussions. I think the connection of the magnetic field to these motions is really a most intriguing aspect of the heating problem. I think in order to properly understand the heating problem, we must put in the magnetic fields. This is a real challenge to the people trying to solve the heating problem. You must be able to reproduce the hot plages over a magnetically active area. Another comment refers to the fossil magnetic fields. In some solar models which I've calculated, the decay time for the fundamental mode is 25 billion years, so the field is quite constant. However, this doesn't rule out the higher modes which have decay times of some three to five billion years, so these could give time variations in times comparable to main sequence lifetimes; however, you would have a constant term in addition. You'd have to add *a* variable to the constant, so it might not give you the correct behavior.

Kippenhahn — If the star is rotating rapidly, we must really include the effect of turbulence and use the total pressure. If it is only slowly rotating you can use hydrostatic equilibrium as a good approximation.

Böhm — May **I** just add one minor point to Kippenhahn's talk. When we talk about the Lighthill output of the convection zone we must remember that this depends strongly on the helium abundance. For

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example, the numbers mentioned for white dwarfs sounded a little surprising. These white dwarfs are surely helium white dwarfs. The point is that the helium convection zone persists to very high temperatures and, if you have a very dense star, a large fraction of the energy must be carried by convection. For these high temperature objects with highly developed convection zones, you get high convective velocities, which means, in turn, a high acoustic output.

Aller — Would you say that some of the non-white dwarf helium stars might have such strong convection zones that they would be good places to look for chromospheric activity?

Böhm - It certainly is true that we expect higher acoustic outputs from helium stars than from stars of normal composition.

Böhm-Vitense — I think that for a gravity of $g = 10^4$[Cgs] the convection extends to about 13000 degrees for helium stars rather than to about 8000 degrees as for hydrogen stars.

Evans - The decline of the RCB star, RY Sgr, in 1967 and its return to maximum in 1968-70 was studied spectroscopically by a group at the Radcliffe Observatory, Pretoria. A strong emission line spectrum (originally studied by Cecilia Payne — Gaposchkin in R Cr B and attributed by her to a chromosphere), comprising mainly lines of singly ionized metals having upper excitation potential less than 6 eV, was present early on the decline. This decayed on a time scale of ~22 days, compared to a time scale of ~5 days for the initial rate of decline in photospheric radiation. The level of excitation and the effects of self-absorption declined with time. A strong continuum short of X4000 was attributed to CN" At minimum light only emission lines of very low upper EP., mainly of Ti II, were present. The lines H and K of Ca II appeared broader than the rest. Broad emission with a central absorption appeared in H and K of Ca II at times during the rise and near maximum light. These observations indicate strong chromospheric activity in a helium star.

Aller — That's somewhat cruder than the solar-model theory, but the level of excitation you describe is comparable with that observed in the Sun. So in giants and even supergiants you see that we can have densities and so-called excitation temperatures not significantly different from what we have in the Sun. This brings out a point Thomas mentioned earlier about using spectra for diagnostic purposes.

Kippenhahn — I must repeat my question: Can I conclude that when there is no calcium emission there is no chromosphere either? Or would the stellar atmospheres people say that at some point in the HR-diagram the calcium emission goes away even though the star still has a chromosphere?

Pecker — Isn't it just a matter of the pressure sensitivity of the calcium emission?

Thomas — **In a** Wolf-Rayet star, I certainly don't observe calcium emission. However, that doesn't mean the Wolf-Rayet star can't have a chromosphere.

Kippenhahn - I am not dealing with special objects like Wolf-Rayet stars. I am interested here in normal main sequence stars earlier than F. What is the significance of no calcium emission in these objects?

Linsky - One can do a very simple experiment to answer this question. Take a simple model to represent the quiet Sun. This model will show a slight emission core for calcium. If you decrease the opacity by a factor of two or three, the emission is gone. You'll never see it. The emission is very sensitive to the optical depth in the line. As you go up the main sequence, the chromospheric temperatures are most likely hotter, because the temperature minima will be hotter, and the calcium will be more nearly completely ionized, thus decreasing the chromospheric optical thickness in the calcium line. You'll very soon reach a point on the main sequence where the emission will not be seen at all, even though you may have a very pronounced chromosphere.

O. **Wilson** — I'll say something about this in my talk, but it is noteworthy that the cutoff for calcium emission is amazingly sharp. The corresponding variation in mass, radius and effective temperature across this boundary is negligible. I don't know what causes this cutoff, but I think it must be something very fundamental. This whole transition takes place in a range of b-y of a couple of hundredths. It's just like you'd cut it with a knife.

Jefferies — **I** think the answer to Kippenhahn's question is that we have really not explored the matter enough yet. Along the lines of Linsky's comments, let me draw a line on the board and say that the gas below it represents the photosphere where the continuum is formed, and then say that the chromosphere is up here above the line with the temperature increasing outwards. Now I have a certain optical thickness in the K line as I look down through this chromosphere. If I have a temperature increase and the optical thickness is greater than about three, then I should see some K line reversal. The size of the reversal depends on the size of the temperature increase outwards, and the value of the optical thickness. If for some reason, the base of the chromosphere moves in the Sun's atmosphere, we ultimately reach a situation where we have no optical thickness left — we have run out of chromosphere, and no reversal will be seen. This is a possible situation as we go from the Sun to earlier stars on the H-R diagram. It is important to search for other sensitive diagnostic tools for chromospheres there. One such indicator would be the very strong resonance doublet of Mg II, which shows such strong emission probably just because of the greater abundance of magnesium. You get some idea of their greater strength just by comparing them in solar spectra with the weak solar K line. The emission cores of the Mg II lines are enormous by comparison. So that's one additional chromospheric indicator, which has a certain disadvantage in that it must be observed from above the earth's atmosphere. We should also search for other indicators, and among those, I have suggested that emission lines might be very valuable. In order to determine whether an emission line is intrinsic, and so a good indicator of a chromosphere, we have first to solve the problem of what an emission line means. In particular, is it intrinsic or geometric in origin? Does this offer a partial answer to your question?

Kippenhahn — I think so. Would you suggest, then, that when we move up the main sequence, we better get observations of the Mg II lines from a balloon in order to check for chromospheres.

Underhill — Don't forget satellite observations here.

Jefferies — Yes, and if the magnesium doesn't show us an effect similar to the calcium, then I think that we've run out of chromospheres.

Underhill — You have run out of magnesium emission after the middle B's.

Thomas — Of course it's all a question of how we define chromospheres too.

Heap — Hasn't Kippenhahn's question already been answered by some of the observations discussed here earlier? For example, Kondo's observations of Mg II emission, suggests that chromospheres may be found in stars having spectral types much earlier than F4.

Kondo — I just want to mention that our balloon program was initiated in the philosophy, similar to that articulated by John Jefferies, of searching for evidence of chromospheres and of enhancing our understanding of chromospheres through investigation of the magnesium resonance doublet. I also want to add that, in future flights, we hope to address ourselves to the point raised by Anne Underhill regarding where in spectral type the magnesium emission is unobservable.

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