Feautrier, P. 1968, *Ann. D'Astrophys.*, 31, 257.
Frisch, H. 1966,/. *Quant Spectrosc. Radiat. Transfer*, 6, 629.
Gebbie, K.B., Thomas, R.N. 1970, *Astrophys. J.*, **161,** 229.
Gebbie, K.B., Thomas, R.N. 1971, *Astrophys. J.*, 168, 461.
Gingerich, 0., Noyes, R.W., Kalkofen, W., Cuny, Y. 1971, *Solar Phys.*, 18, 347.
Griffin, R.F 1968, *A Photometric Atlas of the Spectrum of Arcturus*, Cambridge Philosophical Society, Cambridge, England.
Jordan, S.D. 1969, *Astrophys. J.*, **157,** 465.
Kurucz, R.L., Peytremann, E., Avrett, E.H. 1972, *Line Blanketed Model Atmospheres for Early Type Stars*, U.S. Govt. Printing Office (in press)
Liller, W. 1968, *Astrophys. J.*, **151,** 589.
Linsky,J.L. 1970, *Solar Phys.*, 11,355.
Linsky, J.L., Avrett, E.H. 1970, *Publ. Astron. Soc. Pacific*, 82, 169.
Noyes, R.W., Kalkofen, W. 1970, *Solar Phys.*, **15,** 120.
Skumanich, A. 1970, *Astrophys. J.*, **159,** 1077
Vernazza, J.E., Avrett, E.H., Loeser, R. 1972, submitted to *Astrophys. J.*
White, O.R., Suemoto, Z. 1968, *Solar Phys.*, 3, 523.
Wilson, O.C., Bappu, M.K.V 1957, *Astrophys. J.*, **125,** 661.

DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY AVRETT

Alter — I should like to ask about the suggested theoretical one-sixth power relationship between calcium emission width and visual luminosity.

Avrett — We find an increased width with decreasing gravity, which in turn is normally associated with an increased luminosity. In the session tomorrow Eric Peytremann will show the results we have to date and how they compare with the Wilson-Bappu relation. To summarize them, the log g = 2 case with a temperature similar to that of the sun turns out with a reasonable mass determination to fit the Wilson-Bappu relation within the error bars. The only other calculation we have done so far is for an effective temperature of 6000° with log g = 4; the error bars again include the Wilson-Bappu relationship but they are very large. At the moment these results are only schematic. Also our choice of a chromosphere in the non-solar cases was completely arbitrary. We have to see whether we just happened to select chromospheres which give the proper optical thickness for the calcium emission.

Jefferies — This is more of a comment than a question. One of the things which bedevils comparison between observation and theory of model atmospheres is, of course, the question of the uniqueness of any derived model. In order to characterize an atmosphere fully one needs to introduce a substantial number of parameters. Because of this, one needs

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to compare computed profiles for *many* lines and obtain good agreement with observation for all of them before one can have confidence in the model. Hence, while an observation of the K line is valuable, its value is greatly enhanced if it is accompanied by simultaneous observations of the other lines of ionized calcium.

Linsky — I should like to second what Jefferies has said concerning the need to observe many lines together. I have found from bitter experience that^ observations of the calcium K line contain insufficient information to define a unique model for a chromosphere or even a chromospheric structure in the Sun. One can always trade off temperatures against densities or broadening parameters at one height, or trade off properties at one height against those at another and obtain the same computed line profile. To surmount this problem we have obtained data, as we will show this afternoon, on the infrared triplet lines as well as the H and K lines of Ca II in a number of solar plages. These lines differ in opacity by about a factor of 200. One surprising thing that we have found is that it is not always true that chromospheric emission appears in the more opaque lines before it appears in the weaker lines. We find that the 8498 A line, the weakest of the infrared triplet lines of Ca II, shows emission before the more opaque 8542 A and 8662 A lines of the triplet.

Underhill — The discussion up to now has necessarily concentrated on solar type objects. These objects can be used as an anchor to confirm the theory, and the development of theory is partly based on explaining solar observations. However, other stars have chromospheres. I should like to ask the theoreticians if there has been any attempt to examine how the theory of the classification of lines into classes which are collision-dominated or into photo-electrically dominated classes must change as the temperature and the total radiation field changes. The density of stellar atmospheres changes from cooler to hotter stars — the atmosphere of a main-sequence B star is essentially the same as that of a G giant — so it seems to me to be possible that a collision dominated line will change to a radiation dominated line as the peak density of the radiation field changes its wavelength range and the density of the atmosphere is reduced. Has anyone any views on this question?

Thomas — The rules for that were set up when the original classification scheme was presented. You can calculate the collision rate and the radiation rate or any of the other indirect rates. Recently there is the work of Auer and Mihalas in which the Ca lines and the Mg lines become photoionization dominated.

Auer — I would like to comment on some of the work by Mihalas and myself on the atmospheres of very hot stars and to clarify the point

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raised by Poland. The primary feature of the non-LTE atmospheres, which we constructed, is a temperature use at the surface caused by radiative heating. The models predict that the Paschen ***a*** line of hydrogen (and presumably the higher ***a*** transitions also) is an emission line. This effect is caused by a combination of the temperature use and the fact that the infrared lines are formed high in the atmosphere. In the region where these lines are formed the collision rates are low and the dominant way out of a state is a cascade to a lower state. The temperature rise aggravates the rate of recombination and, therefore, the rate of emission. The situation is somewhat similar to the planetary nebula case.

This mechanism does not suffice to produce emission in the X4686 line of Hell, which is observed to be in emission in Of stars. We attempted to produce this emission by using the Bowen mechanism. The 2n to 2n transitions of Hell overlap the n to n' transitions of H, and therefore one might expect pumping of the 2n' levels of Hell. If the upper level of a transition is strongly overpopulated, an emission line will result. Such is the theory, but unfortunately the results are not in good agreement with the observations. There is a tendency for emission, but not nearly as much as the observations require and X10124 is predicted to be in absorption while it is observed to be in emission in f Pup.

Cayrel — Is there any observation supporting the calculations of emission in Paschen a?

Auer — Yes. There's another thing I should have mentioned. Helium 5876 and 6678 are also predicted to be weakly in emission at very high temperatures. It would appear that if you are looking for evidence of a temperature rise at the surface of an O star, you should look at the strong lines in the infrared.

Praderie - Would you produce emission in H alpha also and could you say how it would vary as you change the gravity?

Auer — The calculations that we have indicate that at the very highest temperatures the cores are beginning to go into emission, just very slightly. If you had an eclipsing binary and you observed it just at eclipse then you should see it strongly in emission. Unfortunately such binaries are few and far between and often have structures complicated with circumstellar gas. Normally H alpha remains in absorption over the entire range. But to make definitive statements about a strong line like H alpha one really should know more about motions in the upper layers of the stellar atmosphere.

Skumanich — **I** want to raise a word of caution about the broadening velocities Avrett used. There is observational evidence that velocities are

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larger in the giants than in the main sequence stars, in which case the use of the scale of the main sequence amplitudes is incorrect. I'm wondering whether, in fact, using constant energy relations for this broadening, like pv^2 = const, to go from main sequence stars to the giant stars may not be a better approximation. Then you might indeed find that you're not on the damping portion of the absorption coefficient curve but still in the Doppler part and you're not getting this kind of variation then. So it's not the actual thickness of the chromosphere that's changing with g but perhaps the broadening that is still changing.

Cayrel - That's a very fundamental point. Can Dr. Olin Wilson perhaps comment on that?

Wilson — **I** have always liked the velocity broadening but there is nothing sacred about that assumption. I'll wait until all the returns are in.

Peterson — Along that line there is observational evidence that exists for turbulence following the mv^2 relationship.

Pasachoff — May I remind the assemblage that for the Sun we have another way we can look at the surface of a star besides the methods used to produce the very lovely results we heard discussed this morning. We can look at the chromosphere sideways at the limb. Many people here, particularly the HAO, Sac Peak and Hawaii groups, have eclipse results that show the intensities of many lines at the limb very accurately. There are lines of many elements besides calcium. Even outside eclipse we can also study the oxygen infrared triplet, the D_3 line of helium, and with a little more difficulty the 10830 line of helium. There are thousands of rare earth lines. I recently made observations at the Sacramento Peak Observatory of the ionized titanium lines near 3760 A, and the resonance line of ionized strontium at 4077 A. Jacques Beckers, also at Sacramento Peak, has observed a whole sequence of ultraviolet chromospheric lines at the limb, which I am now studying. One can study outside of eclipse much more than the relative intensities of the various lines, which all appear in emission. However, there are calibration and scattered light difficulties, and one can't study the height structure as well as at an eclipse. We have spoken of models of turbulent velocities varying with height; we should study the velocity variation with height by actually following the spectral lines out from the edge of the sun.

Thomas — Could I just make a point of basic principle here. Sometime during this meeting maybe one should have a popularity vote on whether you want the chromosphere to begin where the temperature rises, or where you put the mechanical heating in which guarantees the rise above what you would have from a purely photospheric radiative equilibrium

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model. It's a point one must carefully distinguish, particularly in view of Auer's remarks on the basic characteristics of chromospheres in hot 0 and B stars referring to his and Mihalas' calculations of models with no mechanical heating. I have my own position which is that the mechanical energy input fixes the chromosphere. But that's something everybody has to decide for himself. So maybe we should think about it.

Cayrel — Yes. In fact I have noted that nobody has really cared very much about the definition of the chromosphere which was involved in the topic of this morning.

Kandel — I don't want to define a chromosphere, but I think we ought to be more specific about the temperature. We all understand that when we talk about the temperature structure we are talking about the electron temperature, and in some way the energy content of the electron gas. This is not the temperature of the atmosphere as a whole. When we get the source function of H and K, we have a measure of the excitation temperature of the Call gas, and when we talk about energetics we also have in mind some sort of temperature, but of the gas as a whole. We're interested in energetics which perhaps depend on electron temperature which, in principle, we get from continuum measurements and hydrogen ionization and excitation but which, in practice, we seem to have a hard time getting. What we want to do is find out how to determine these things, namely, the specific energy content of the gas in terms of the observables, the calcium populations, and other things. So perhaps we should keep in mind what we mean by a temperature.

Thomas — Electron temperature is always the thing which one has in mind in all these kinds of calculations. I couldn't agree more with your premise and I'd like to know what partition of energy one has over all the energy levels. But so far we have again taken the theological position that there is an electron temperature that defines the velocity distribution of electrons, and that defines all collisional parameters. That's the only reason for doing it.

Kandel — I think when we talk about a given temperature structure we're talking about an *electron* temperature structure. If we talk about an isothermal atmosphere this doesn't mean that the specific energy content does not vary through the atmosphere, and there is really no reason to be surprised at finding absorption lines coming from such an atmosphere.

Pecker: I want to make a simple reply to the question of what is a chromosphere. Jefferies spoke at the beginning and said that the symptoms of a chromosphere are an increase of the electron temperature This is a much too closed definition. You might have heating without having

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heating by mechanical energy, or you might have heating without an increase in temperature outward. I do not think that an outward temperature increase alone determines whether or not there is a chromosphere.

Cayrel — **I** am not sure I have understood your point. If one star with no dissipation of mechanical energy at all has an outward rise in temperature, and if a second star has some dissipation, but not large enough to cause a temperature reversal would you say that the second star has a chromosphere but the first one does not?

Pecker — Yes.

Underhill - I think that the definition of a chromosphere should not consider the question of temperature, however defined. The chromosphere is that outer part of a stellar atmosphere where you have to consider the physical processes in detail.

Thomas — I would really like to comment on this point. Suppose we divide the star into two parts: interior and exterior. Then our aim is to try to make general structural models of stars from the standpoint that the atmosphere is the transition region between the stellar interior and the interstellar medium. As a whole, a star is a non-equilibrium, non-steady-state object. Basically it is a storage pot of energy and mass. The interior is characterized by the fact that the primary focus is on population of energy levels and concentrations of mass particles and you can compute all of these without caring at all about what the fluxes are, using standard LTE distribution functions. You compute the distribution of those TE parameters specifying the distribution functions by always using a diffusion approximation. This is a linear non-equilibrium thermodynamic equilibrium situation. Then consider the other part of the star, the atmosphere. There we are mainly concerned with propagation phenomena. We want to characterize the whole sweep of the atmosphere as a gradual unfolding from a completely degenerate aspect in the interior, which is locally in thermodynamic equilibrium in the broadest sense, to the interstellar medium, an almost completely non-degenerate configuration, not in LTE in any sense. Then we divide the atmosphere into a number of subregions. We characterize each subregion by the unfolding of some aspect of this kind of degeneracy which represents the general thermodynamic equilibrium state. The reason I introduce this now was in answer to Anne UnderhilPs comment. It is not just the chromosphere where we begin to worry about the detailed physical characteristics, it is already in the photosphere. What is the basic point? In the sub-atmospheric regions we have a storage of electromagnetic energy and a storage of mass because we have a kind of diffusion approximation

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characterizing the transfer of process in either case. The photosphere is characterized by an increasing direct escape of photons from the star. So we have in this region the gradual beginning of all those aspects of non-LTE which affect populations of energy levels associated with the fact that the photons can escape directly from the boundary and there is no longer, to a first approximation, an isotropic radiation field. The, chromosphere we characterize as that region where we begin to have a departure from the storage properties of the mass flux. Go back to Eddington's old approximation in his representation of a Cepheid. He had a standing wave as far as the mass transfer and the kinetic energy transfer in the stars were concerned. Where did the model begin to lose energy? Only in the non-adiabatic part where one has a radiation field. The evolution from this thinking applied to a Cepheid atmosphere came in Schwarzschild's work where running waves were introduced in the upper part of the atmosphere. This is analogous to that thing which produces the chromosphere now — forget the details about convection, turbulence, etc. — producing acoustic waves that run out. In the Cepheid we have a system of standing waves in the interior. Suppose, for example, we had a zero minimum temperature at the top of the photosphere (which is the easiest way to look at it), then we'd have all the energy in trapped waves which leak a bit of energy at their top. This leakage is provided by "diffusion" through the[1] system of standing waves in the subatmosphere. It's exactly in analogy with the storage of all the electromagnetic energy in the sub-atmosphere, with leakage from the diffusion approximation, balancing the surface loss, due to direct escape of photons at the boundary in the photosphere. So the photosphere is that part of the atmosphere which represents for electromagnetic energy, a transition from sheer storage with a little bit of leak in the sub-atmosphere to direct escape from the photosphere. The chromosphere is that region where I have a macroscopic escape of energy in the mechanical degrees of freedom, that is, progressive waves going out, as contrasted to the storage properties which hold at the bottom of the chromosphere. So I have then two distinct atmospheric regions: the photosphere and the chromosphere. We think we can do the same kind of thing in the corona in terms of direct mass loss from the star. I would just like you to focus on the physics here: in the photosphere it's the photons, in the chromosphere it's the mechanical energy, in the corona it's the mass. All this should come after what Francoise Praderie is talking about tomorrow; she demonstrates it much more clearly than this. That's why I would buy the chromosphere as the place where we have a mechanical dissipation of energy, because a photospheric temperature rise, as Cayrel and Helene Frisch have very carefully pointed out, has nothing to do with anything except photons and the way in which they are linked to the interaction

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with matter; namely, inelastic collisions are neglible, and we simply have photoionization for the opacity processes considered.

Underhill — I want to make sure that we understand your definition of the chromosphere as the place where mechanical energy is dissipated. Also, we have to consider the end of this conference at the same time as the beginning. You say you are going to talk about a star where we have mass loss. I would like to say that many early type stars are known to have mass loss from direct observations. I don't think we can have mass loss following your types of arguments, which are physically logical to me, without also saying you have a chromosphere. Therefore, I'm going to say qilite happily that I can talk about chromospheres for stars of type A, B and O. Is that logic irrefutable?

Thomas — I'll buy chromospheres for all types of stars.

Linsky - This morning the subject came up of the Ca II H and K lines as indicators of stellar chromospheres. I think that it is relevant, therefore, to present some observations that Richard Shine and I at JILA have obtained in the calcium lines for solar plages and a sunspot. This work will be the basis of his thesis. At present we have reduced the observations and are now in the process of building model chromospheres to explain the data. The data are all photoelectric and were obtained in a double pass at Kitt Peak.

In Figure 1-18 we call your attention to what the calcium lines look like in the average quiet solar chromosphere. Incidently. if the Sun were

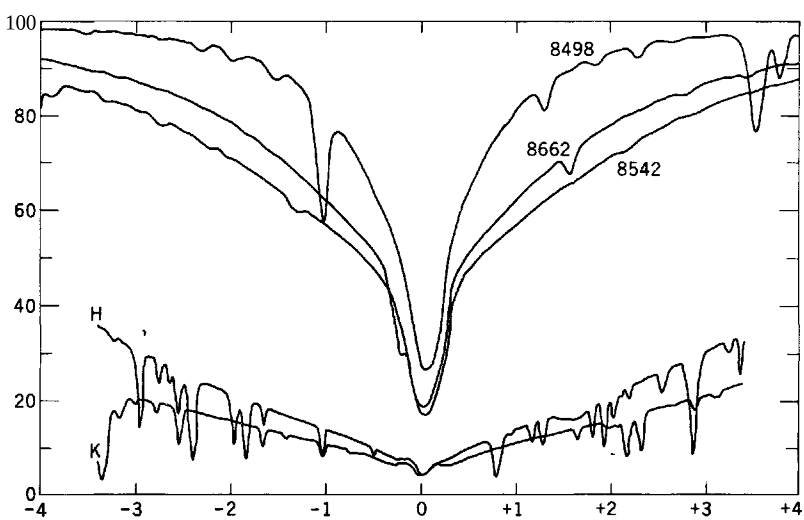

Figure 1-18

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observed as a point source, the profiles would be essentially the same as are seen in the quiet chromosphere. In this and subsequent figures we show the H and K resonance lines as well as the infrared triplet lines (8498, 8542, and 8662 A). As you recall the ratio of gf values and thus of opacity are 1:5:9 for the 8498, 8662, and 8542 A lines respectively. It is important to remember that the 8498 A line, is by far the weakest in the triplet. In these figures we give residual intensities for the lines relative to the interpolated continua at line center as a function of wavelength measured from line center. In the quiet chromosphere the infrared triplet lines show no emission and H and K exhibit weak emission. Also the residual intensities in the cores of H and K are about the same.

Figure 1-19 shows the five calcium lines in the weakest plage we observed. As has been known for some time, the cores of H and K show emission

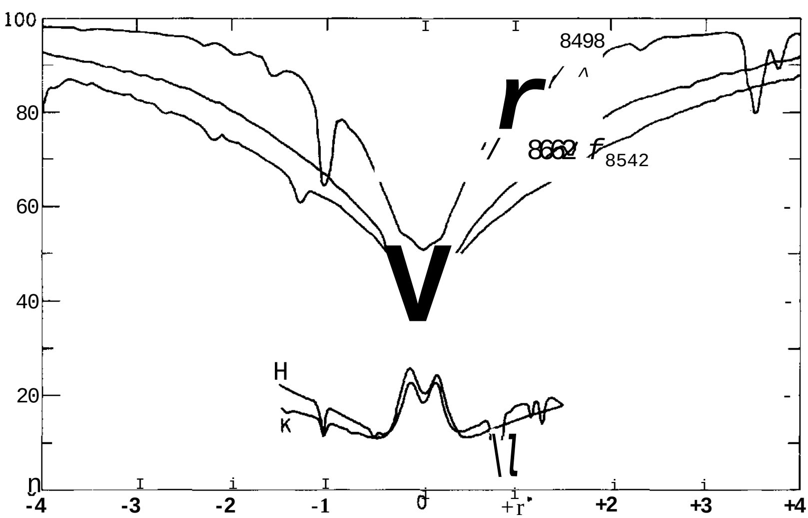

Figure 1-19

and also broaden appreciably. K shows more emission than H with the ratio of residual intensities about 1.1 instead of 1.0. This ratio persists for all plages we observed. Also the residual intensities in the cores of the infrared triplet lines have increased significantly relative to the quiet chromosphere. What was unexpected in a weak plage was that the 8498 A line, the least opaque of the infrared triplet lines, shows a definite double reversal in its core. In a slightly stronger plage, seen in Figure 1-20, there

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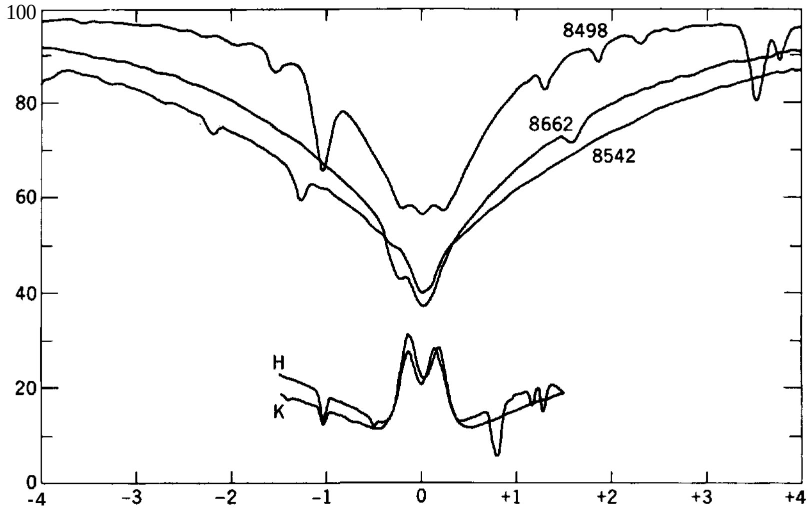

Figure 1-20

is also a definite double reversal in 8498 but not in the other infrared triplet lines. This phenomenon is thus real and may place an important constraint upon acceptable models for weak solar plages. It also says that the 8498 A line may be a very sensitive indicator of stellar chromospheres of stars similar to the Sun.

In the strongest plage we have observed, all five calcium lines, as shown in Figure 1-21, show emission features and K_{2v} is 42% of the continuum. The double reversal in the 8662 A line is exaggerated by an iron line just to the violet of line center. Note that the 8498 A line shows a narrower and stronger emission feature than the other two infrared triplet lines.

In a sunspot, shown in Figure 1-22, an entirely different set of profiles appear. The infrared triplet lines show no emission whereas the resonance lines show narrow emission features in their cores. The emission feature in K is much brighter than that in H with the ratio about 1.6.1 suspect that an explanation for the calcium line profiles in a sunspot will require a much thinner chromosphere as measured in K line center optical depth units and a much steeper temperature gradient for the chromosphere of a spot relative to a plage.

Finally I would like to show an unexpected phenomenon in the wings of the calcium lines. In Figure 1-23 we show the calcium lines for the strongest and weakest plages and for the quiet chromosphere. Note that the wings of the lines for the strong and weak plages are identical and

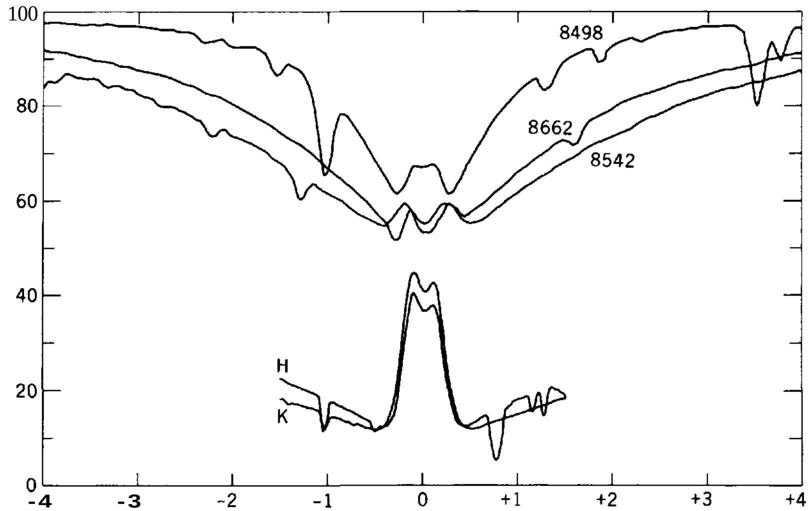

Figure 1-21

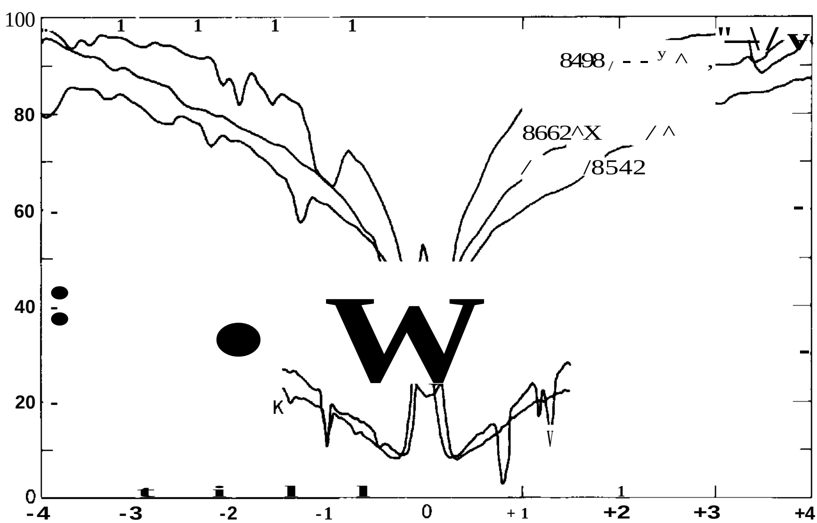

Figure 1-22

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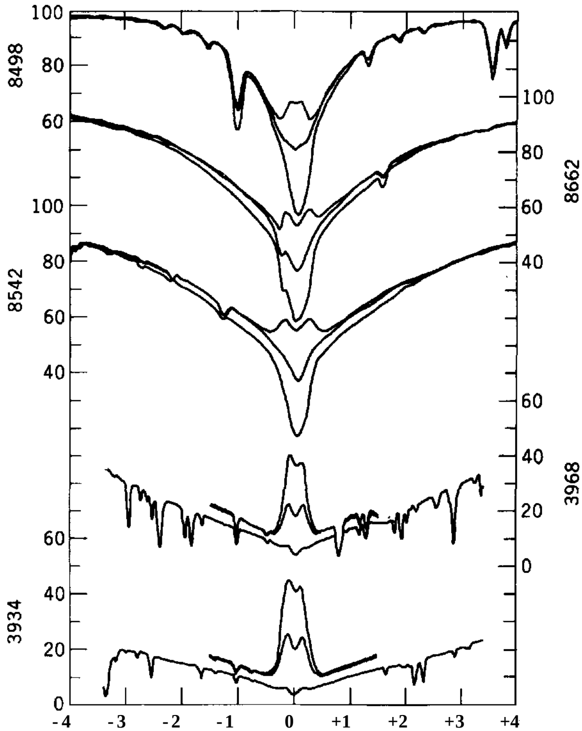

Figure I-23

significantly brighter than for the quiet profiles. This indicates that the plage phenomenon has an aspect which is photospheric and that the perturbation of the photosphere beneath a plage is independent of the chromospheric aspect of the plage. The sun thus exhibits two photospheres in addition to many chromospheres.

The main reason why I show these data before they are analyzed is to emphasize that the Sun has many chromospheres and that the calcium lines are sensitive indicators of these chromospheres. Clearly any acceptable theory for how stellar chromospheres vary with spectral type, luminosity, and age must explain the vast range of chromospheres on the Sun. To my mind this is an important example of why the study of stellar chromospheres and the solar chromosphere must be pursued together.

Cayrel — These observations are very challenging, as theoretical predictions are most often compared to average spectra. Yet, these data show

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that we obviously have a wide range of chromospheric activity. Are there comments on this?

Underhill — This observation that the wings of these lines formed in plage regions have more flux in them than the same lines in the photosphere makes one wonder. I would ask Linsky. or any other theoretician, would this heightening of the flux from the deeper layers of the photosphere correspond to a back warming? One comes back to the problem that you cannot logically separate a photosphere and a chromosphere. They overlap. They react back on one another. If you have dense material overlying a radiating region, its going to produce back warming. We've seen a difference of about 2 percent in the energy coming out, and that's a back warming to me. It has more implications that just being one of those oddities you observe on the Sun. You would expect to find this on any star where there is an overlay of dense material. The result might be a totally different combined atmosphere. We may not think of line blocking and back warming in interpreting many ground-based spectra from A stars, B stars, even early F, but when you go to shorter wave lengths, there are a lot more lines, so you are going to get lots of back warming. These are strong resonance lines which are going to produce strong absorption in the outer fringes and which you might not even guess about by observing at 4000 Å. Are any theoreticians able to make these ideas more precise?

Pecker — I would like to comment in a slightly different way. Linsky has given us some beautiful examples of what Jefferies told us this morning, that the source function and the flux in the line are extremely sensitive to such things as density effects. His results illustrate that the effect of very small terms, as shown by Thomas and Jefferies years ago, is sufficient to produce large emission differences in the cores of these lines. From the shape of the source function, you can infer the shape of the line. What is important in the source function, then, are the source terms, even when they are small. For example, consider the difference between the polarization in the case of isotropic scattering and of a small perturbation on the isotropy. The results are significantly different. This is an analogous situation. I'm not sure that I'm replying directly to Anne Underhill's point, but I feel that the source term in the source function equation is the essential one in interpreting the observations Linsky has shown us.

Bonnet — **I** don't understand if you really assume that the differences in observations between the plages and the quiet regions are mainly due to a density effect? Is that correct?

Pecker — More or less. Yes.

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Bonnet — How then do you explain a similar difference in the continuum at 2000 A, where the difference is a temperature effect and not a density effect?

Pecker — I don't want to say it's a temperature effect or a density effect. I just want to point out that the effect of the smaller term on the source function is great, even though it's a small fraction of the source function. It's still sufficient to produce a tremendous difference in the flux in the central part of the line. In the photosphere we might have a different situation, wherein the temperature effect dominates. The density effect there may be absolutely negligible. What counts is source term. That's my main point.

Skumanich — I don't agree that the density effect is great. The source function isN/X(B) at the surface. Now A. is proportional to the density N, and B ex $T^{4 \text{ or } s}$ (for Cak),NfX OC*N/Nwhile B « $T^{4 \text{ or } 5}$ Thus small changes in T are more important than small changes in N in influencing the central intensities.

Peytremann — Let me go back to backwarming effects from the chromosphere down to the photosphere. The backwarming effect cannot be very important because it should be considered as integrated over the entire spectrum, and the chromosphere flux is very small compared to the total photospheric flux.

Underhill — Are your remarks based solely on considering the backwarming from the H and K lines? You must consider all the other lines.

Peytremann — The lines formed in the chromosphere consist of the cores of strong lines, so they don't cover a wide spectral range. What is important to backwarming is the total energy integrated over frequency.

Linsky — I would like to comment on the question of whether the source function increases with density or temperature. One should consider the ratio of the residual intensities of K to H which increases with K emission in the Sun and, as Olin Wilson's work has shown, in other stars as well. In the absence of collisions K would be brighter than H where the temperature gradient is positive since the thermalization length for K is one-half that for H on a common optical depth scale. Fine structure collisions tend to establish equilibrium in the population ratio of the upper states of H and K. Thus the line ratio data on plages could be accounted for by either (1) lower densities or, (2) steeper temperature gradients, or both, in plages relative to quiet regions". The same argument applies to stars with active chromospheres relative to those with quiet chromospheres.

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Cayrel — **I** do not understand how one can exchange density against temperature. How can you change the density without changing the scale height?

Skumanich — I would like to call attention to Domenico's work in which he asks what kind of parameter changes you must have in scale height and in temperature gradient. He found that the major effect which constrains the data (the observed K to H ratio, the observed amplitudes, and the observed half widths of the stellar Ca emission core) is the temperature gradient rather than the scale height. For example, a 33% increase in the temperature excess in the chromosphere of solar type stars will cover the whole range of Olin Wilson's observations.

Thomas — The parameters you have for the CaII H and K lines are the absolute intensity of the peaks, the ratio of K_2 to K_3, the half-width and the position of the peak. If you give the temperature distribution as a function of depth, as we have shown a long time ago, the ratio K_2/K_3,is extremely sensitive to the place where the temperature rises in the chromosphere. The absolute intensity is extremely sensitive to the temperature in various regions. Elske Smith showed long ago that over sunspots, over plages, and over faculae the emission intensity rises up to various fractions of the continuum. What counts is the distribution of temperature as a function of optical depth, to which these things are extremely sensitive functions. And for that very probably the density comes in in a much different way than we are talking about here. Again in the same way, the magnetic field comes in, not because the magnetic field enters directly, but because the magnetic field changes in one way or the other the rate of deposition of mechanical energy that must be balanced against all the rest of things in the energy equation. So, is it sufficient to assume a distribution of temperature and density and ask what will come out of it? Do we not have to ask how the distributions of temperature and density are obtained? If the assumption of a frequency independent source function is wrong, the behavior of the K2 emitting region relative to the low photosphere could be in serious error. And the introduction of the microturbulance parameter to match the width of K2 may be suspicious.

Beckers - I would like to make a comment on the data presented by Linsky on the infrared plage profile. In Iinsky's plage profiles the 8498 lines show self-reversal in the center, while the 8542 lines show a shoulder but no self-reversal in the center except where the plages are very strong. Those two lines have an absorption coefficient, ratio of 1 to 9. This is a very large difference compared to the H and K lines. I assume here that the source functions are equal and that the levels are strongly coupled. The source functions for the three infrared lines are therefore equal.

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I claim that the 8498 profile, because of its shape, must be formed near to the peak of the source function. The 8542 line has a much higher absorption coefficient and the line center therefore originates much higher in the atmosphere. The reversal therefore occurs in the wing of the line. If the source functions are equal and the absorption coefficients occur in the 1 to 9 ratio, then the intensities at the wavelengths where the lines have equal absorption coefficient should exactly correspond; the X8498 profile should be completely reflected in the X8542 profile so that the central reversal in 8498 should occur in the wings of the 8542 line. Why don't we always see that? Perhaps the spectral resolution does not allow one to see such a sharp peak in the steep line wing. Or perhaps, since one is working in the wing of the line, variations in microturbulence with height smooth the contribution function more than in the line center.

Athay — **I** have two comments. First, all of those questions are very easily answered on a computer in a few minutes. Secondly, I don't understand all of the concern about ten percent differences between H and K. We've been talking as though there were infinite coupling between the source functions. You don't get complete source function equality unless the coupling is very strong. It is probably very easy to get ten percent differences in source functions.

Underhill - I wonder if it would be helpful to broaden the discussion to another spectrum with a similar energy level distribution as Call, namely that of Ball. Call has an ionization potential of 11.87 volts and the lowest levels are 4^2S, 3^2D, 4^2P, etc. Ball has an ionization potential of 10.01 volts and there are equivalent 6^2S, 5^2D (metastable) and 6^2P levels. Have the solar people looked at the Ball lines? They are much weaker because Ba is much less abundant than Ca.

Alter — The abundance of Ba is about four powers of ten down from that of Ca.

Underhill — That would certainly make the two cases different.

Jefferies — Has anyone observed the Call infrared triplet lines in stars other than the Sun?

Wilson — Paul Merrill and I did a little of that many years ago but I have no good data on it. I don't remember seeing any reversals in these lines.

Jefferies — Weyman and I made a very few observations of the infrared triplet but we certainly didn't see any reversals.

Cayrel — Of course, the stellar observations would not have sufficient spectral resolution to allow one to see such reversals even if they are there.

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Underhill - Why not observe late type giants with a Fabry-Perot interferometer? That would work nicely at 8500 A.

Linsky — I have some profiles of Procyon and Aldebaran which I will show tomorrow in the session on observations.

Steinitz — I would like to make another comment about the infrared triplet. I don't want to suggest an explanation for the differences between the behavior of 8498 A on the one hand and 8542 and 8662 on the other hand. But just to complicate matters I would like to introduce the problem of the effect of Zeeman splitting on the source function. The 8498 connecting the 3/2 to 3/2 levels has a different Zeeman pattern than the other two lines. 8542 and 8662 have essentially the same Zeeman pattern with only a slight difference in the amount of splitting. The patterns are shown as follows:

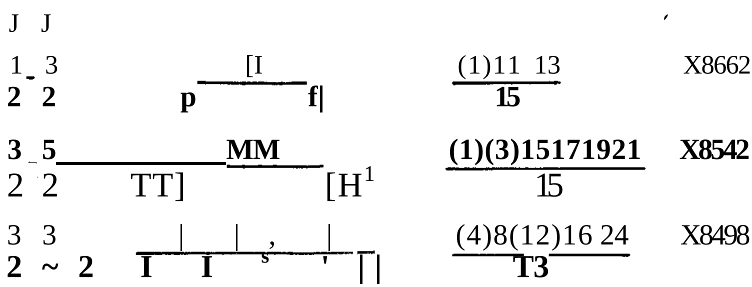

Diagram showing approximate Zeeman patterns for Ca II IR lines.

Now we know that plages have a connection with magnetic fields, although I'm not suggesting that this is the ultimate explanation. But it may be necessary in transfer problems of this type to take into account these magnetic effects, especially since we see the nice differences. There is a ratio of about 1:5:9 in the intensities of the lines and these observations may be related to a difference in the slope of the source function as a function of optical depth. Another complication is that it has been generally assumed that the source function over the line is independent of frequency, the frequency dependence coming through the optical depth effect. That has been assumed because in the core of the line it is only fair to assume that there is equality of the emission and absorption profile, but when you take induced emission into account that may not necessarily be true.

Thomas — Steinitz is much too modest. His thinking is what has made me worry about the frequency independence of the source functions. I think he is giving us only a suggestion of the mechanism he is thinking about.

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Peytremann — How strong must your magnetic field be so that the width of the Zeeman pattern competes with the velocity broadening?

Steinitz - I would guess about 1000 2000 gauss.

Sheeley — Assume the Zeeman splitting is 3 x 10~s A/gauss, then 1000 gauss yields 0.03 A. I suspect that those peaks are located well beyond that.

Steinitz — But that is not the relevant point. It is not a question of whether the Zeeman broadening is larger than the velocity broadening. The question is what happens to the source function and how does the line core build up.

Thomas - The point Steinitz is trying to make is the following. Remember, in the source function I have a big radiative term plus a much smaller source term, the eB or the 77B*, and in the denominator, unity plus again a sink term. A complete theory gives still another term in the denominator which results from a difference between the emission profile and an absorption profile. How big does the profile term have to be before it becomes important? It doesn't have to be big at all, because for Call the largest comparable term is e, the collision ratio, which is about 10^{-4} or 10^{-5} So the disparity in the profile term must only be bigger than 10^{-4} or 10^{-5} to have an integrated effect big enough to affect the profile of the Call line. If the emission profile and the absorption profile differ by one part in 10^4 or 10^5 the difference will be important.

Jefferies — I'd like to translate this discussion in case some are getting a bit lost. The problem concerns the preservation of frequency in the scattering process. Consider the absorption of a photon at a certain frequency and its subsequent re-emission. Is there any correlation between the frequency of absorption and the frequency of re-emission? The computed line profile depends very much on this question. The assumption generally made is that the frequencies of these two photons are entirely uncorrelated. Under those circumstances the line source function is not a function of frequency within the line. What concerns me about the arguments given here is the following. One of the infrared triplet lines (8498) is observed to have peculiar properties. When a photon in that line is observed the atom is raised to the $P_{3/2}$ level. What choices are then open to the atom? It can come down in the same transition, in another infrared line (8542), or in the K line. If it re-emits the same 8498 line photon there may possibly be some coherence in frequency between the absorbed and emitted photons. If the atom emits a photon in another line transition, then knowledge of the frequency of the absorbed photon will be lost even if radiative interlocking processes lead to a subsequent

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re-emission of an 8498 photon (a process which could legitimately be called scattering). I agree that the profile of the 8498 line is peculiar and demands some sort of an explanation. I think that this is perhaps the most significant thing that came out of Linsky's observations. But I don't think we can explain this in terms of a partial coherence in frequency because the 8498 line couples so strongly with the other infrared lines and with the H and K lines.

There is one line I know of which may be an important candidate for a departure from the assumption of complete redistribution in scattering: namely, Lyman alpha. In this line most of the scatterings that take place are just direct absorptions and subsequent re-emissions going back and forth between the upper and lower states. It is, thus, not at all like 8498 where you get many sets of possible re-emission paths for an absorbed photon. It is interesting that Lyman alpha is characterized in the solar spectrum as having extremely extended wings which are in fact characteristic of a departure from complete redistribution.

Thomas — You're talking about the J scattering term. What I'm talking about is not the large number of scatterings but the differential effect which comes from a source-sink term. That's very small.

Jefferies — Yes. But you've got the intensities of a lot of different lines mixed in together in the source-sink terms. I don't think you can really argue on the basis of a two level source function for effects that are as sophisticated as this, or even an equivalent two level atom.

Skumanich — **I** want to make a plea. We have been talking about temperature and inferring from the temperature and the temperature gradient what the mechanical heating requirements are. I think one of the very important elements in this whole thing is calibration. As an example, Lemaire and I have compared the magnesium doublet emission with the O I lines at 1300 and we find that they don't compare well at all. (I mean compare by relating the data to some comparable quantity like the temperature distribution which gives you the observed shape as well as the amplitude.) They don't agree to such an extent that the calibration can be different between the two lines by as much as a factor of two, which I think is terrible. If we are after mechanical energy heating, one of the underlying questions we must all have in mind is that we need not only shape information on lines and continua, which is the classical thing astronomers have been doing, but in the new spectroscopy (to quote a colleague of mine) we also need absolute magnitudes, i.e., the absolute flux. So, I want to make a plea for not only careful and sophisticated theory but careful and sophisticated calibrations.

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Ulrich — **I'd** like to ask Jeff Linsky just how firmly he believes in the wing difference of a few percent. I have to agree with Anne Underhill on this. I think that's one of the most significant things in these observations because that indicates a basic change in the thermal equilibrium of the photospheric layers. I feel this is of vital importance. Related to this I wonder if there isn't a similar enhancement of the continuum. If the continuum far from the core of the lines is also affected under a plage I think this would be extremely interesting. As Skumanich has emphasized the results depend critically on the accuracy of the calibration.

Linsky — **I** trust the data on the enhancement of the calcium line wings in plages because spectroheliograms taken in the wings of these lines show bright plages and network out to about 10 A from line center. Whether the continuum is enhanced or not in plages is a more difficult question that Neil Sheeley could better answer. I would not be surprised if there were a 1% enhancement at 4000 A.

Athay — Isn't it true and well known that the continuum is brighter in a plage at least in faculae, that the faculae occur high in the photosphere and that they're more prominent in the active regions than they are elsewhere?

Bonnet — This is obvious in the UV spectrum. When you look at the Mg II lines you have the same mechanism and if you observe the continuum in wavelengths ranging from 2800 angstroms to 2000 angstroms you observe a strong enhancement of the continuum emission.

Sheeley — I'd like to make some comments about plages and continuum at the center of the disc at various wavelengths. We've made simultaneous spectroheliograms in the 3884 Angstrom continuum, which is the only continuum I can find in that range, and the nearby CN bandhead which shows faculae very pronounced. In the 3884 continuum a static photograph does not show brightenings in the continuum. But a time average or a movie of this does. It must be therefore a small effect but it's present. Ed Frazier has made some observations at Kitt Peak using a photoelectric magnetograph looking at the green continuum and finds an effect of about one half of one percent with the plages in the continuum being slightly brighter than average. Then there are some other confusing details such as if you take a spectrogram and look at magnetic field regions sometimes the continuum is brighter than average, but then sometimes the continuum is darker than average in the green. So while there are some details to be ironed out, time averages and high sensitivities do show a small possible effect.

Cayrel — We should now conclude this part of our discussion on line formation. We had a specific question in the program, namely, what lines depend on the local physical parameters in a highly sensitive way. We should try to list those lines that fit the criterion, and then identify those lines that are not too difficult to compute. It seems obvious that the list includes the calcium H and K lines, at least for stars later than **GO.**

Thomas — The answer, categorically, is collision dominated lines. Which lines are collision dominated depends on the star. You can't give specific lines for all stars.

Pecker — This morning John Jefferies started to make a list of lines that are collisionally dominated and those that are photoelectrically dominated but which are classified in this way only for solar type stars. Are we able to make the same list for other stars at the present time?

Athay — **I** want to raise an objection at this point. As far as I know no one has ever found a solar line that is really photoelectrically dominated. The sodium D lines are collision dominated. Even H alpha shows a strong measure of collisional effects. If you compute line profiles it's very easy to get emission cores in H alpha. In the case of every line we've ever computed it's easy to get an emission core if you simply increase the opacity of the chromosphere a bit or raise the temperature a bit. I just don't know of any line that is really photoelectrically dominated in the case of Sun. H alpha is supposed to be the prime example and is found to be a marginal case at best.

Thomas — **I** can't say anything except that I completely disagree.

Athay — A half a dozen people have published results that support the contrary opinion. If you disagree, please publish it.

Thomas — It was published, as you well know, a long time ago.

Athay — And it's been shot down and you haven't replied.

Thomas — No. There isn't a single case of an H alpha profile except in a place like a flare which shows some indication of a temperature gradient.

Athay — The central intensity itself shows it. The only reason that the temperature gradient shows up in the H and K lines is that they are the only lines that have enough opacity to show it.

Cayrel — Yes. That was the second point I was going to raise. It's not enough of course to have a line with a sufficiently large collision rate but you must also have a thermalization length as large as the region where the temperature increases. This double restriction is perhaps why we have so few lines to work with. It is regrettable that we cannot discuss at the same time hot stars and G stars because the conditions are so different.

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Thomas — It seems to me this is the big point. This is a symposium on stellar chromospheres. What we are trying to do is to see physical principles on the basis of which we can proceed.

Cayrel — Yes. Now we should select particular lines for different classes of stars. The Call infrared triplet is somewhat sensitive to a chromosphere but to a lesser extent than the H and K lines. On the contrary the resonance lines of Mg II at 2800 Angstroms are on the whole much more sensitive to a chromosphere. I don't know the order, of magnitude but Bonnet can certainly comment on the comparison between Ca and Mg H and K emission.

Bonnet - The measurements made by Lemaire of-the Mg II doublet emission show that the contrast between the maximum emission in the lines and the adjacent continuum varies from 25% td 40% at the center of the solar disk.

Cayrel — We must also be very careful to indicate what spectral resolution is needed in order to see the central emission in sufficient detail. Could I ask first, what resolution is necessary to distinguish the separate emission peaks with acceptable accuracy and second, what resolution is necessary just to show that there is some central emission — both for Ca and Mg H andK?

Bonnet — For the sun this resolution can be estimated to range between 0.1 A and 0.2 A.

Athay — I would like to make a suggestion that we ought to look at the Fe II resonance lines. We're now talking about an iron abundance that is just as high as that of Mg and just as high as Si. The published f values for the lines are also just as high as for Mg and so, just on that basis, you would predict that the Fe II resonance lines ought to be just as strong as the resonance lines of Mg II. However, it is clear from looking at the rough spectra we have that this is not true at all. The Fe II are very much weaker than the H and K lines of Ca, but if there is as much Fe II as some people say, (and as I believe there is) then there's just no reason why these lines should not also show self-reversals.

Thomas — What about the Boltzman factors for these ionized lines?

Cayrel — And is not the partition function of ionized iron rather large?

Athay — You put all the Boltzman factors in and you still predict lines as strong as those of Mg, even with only a fourth of the ionized atoms in the ground state?

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Thomas — **I** would like to comment on a related matter. Noyes and Kalkofen have produced a model atmosphere of the sun coming from the Lyman continuum analysis. If you remember, this model is strikingly similar to the one we had in that book of ours a long time ago. There we made the same kind of a model on the basis of an analysis of the free-bound and the H-emission in the solar atmosphere. All that depended very carefully on being able to determine b_t and b_2 of hydrogen. The basis of that determination was that the n_2 and the n_3 levels were fully ionization controlled; so that there is a large population of the n_2 state throughout the atmosphere, and also that H alpha was photoionization controlled, so that one could make **a** correction to the ionization equilibrium coming through the presence of H alpha. The Noyes and Kalkofen model essentially agrees with ours. So now if you believe this current model of the solar atmosphere you have to believe that H alpha is photoelectrically dominated.

Athay — All that says is that we were approximately right.

Thomas — Kalkofen, in your ionization equilibrium calculations, don't you find that the ionization terms are the dominant ones?

Kalkofen — It is true that the most important transition upwards from the second level is by photoionization.

Thomas — OK. That's the thing that controls the population.

Cayrel — I presume that this discussion is still related to iron, in which the interlocking terms may be more important than in hydrogen or calcium because of the greater complexity of the atom, hence, many more possibilities beyond the 1-*2—*-l process.

Underfill — There are some interesting peculiarities because some of the Fe II lines go into emission before you cross the limb. Somebody mentioned these lines earlier in the day. There are quite a few such lines in the solar spectrum, for example, Ce II and other rare earths. However, the Fe I lines apparently do not have this behavior.

Cayrel — **I** would add to this list the lines of the type suggested by George Wallerstein, forbidden lines in which C_2i is much larger than A_{21}. The point was raised that the C_{21} should then be also larger than other competitive transition probabilities, so that we are sure that the source function is really the Planck function. One point is that these lines are never as strong as the permitted lines, and that they do not allow you to reach very high in the chromosphere.

Pasachoff — **I** have some Sacramento Peak spectra that show the resonance line of Sr II going into emission slightly inside the limb. Nearby

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are various rare earth lines including mostly Ce II. They are also in emission inside the limb, which is well known since Menzel's work and they are in emission further inside the limb than the Sr II seems to be.

Cayrel — The problem of Zeeman splitting has been raised which may make the whole theory described this morning by Jefferies more complicated, if one wants to take into account redistribution due to changes between Zeeman components. It should be pointed out that the Zeeman splitting is much less of a problem for H and K than for the infrared triplet lines. This should be true for Mg as well as Ca.

Johnson — May I add Na D to this list? Since its source function is collisionally dominated (an exception to the rule mentioned), it may be sensitive to a temperature reversal. Also, whereas these other lines may be weak in cooler stars, Na lines are extremely strong, and are sometimes used as luminosity indicators. Does anyone know of observations showing emission reversals in the cores of these lines in cool stars or the Sun?

Underhill — They appear in emission in a few peculiar hot stars.

Sheeley — I think that this may be a matter of height of formation more than what the particular energy level scheme is. Spectroheliograms in many lines such as the core of the Na D lines, Sr II, Ba II, Sc II, Fe II . (all strong lies) the core of Mg I b lines, the Ti II resonance lines at 3349 and so forth all look similar. They fall into a special class of their own. This business of classifying isn't too unreasonable since you can get the same sort of classes that Jefferies got this morning for example, from the same approach. So, I think it's a matter of where the lines are formed. The classes that Jefferies indicated are formed high in the atmosphere. All these other lines (Fe II, Sc II, Ti II, etc.) are formed in the intermediate chromosphere. And in the lower chromosphere or the upper photosphere, whatever you want to call it, there is another class of lines and molecules — neutral iron lines, neutral metals in general, and so forth — which also show very bright plages as for example CN shows. The CN bandhead at 3883A shows faculae that are brighter at that height in the atmosphere than even the K line. The K line has a contrast of say 50% in the lower chromospheric faculae (AX^3A) whereas the CN bandhead has a contrast of 100%. So perhaps CN is worth looking at in stars.

Pecker — By all means we should look very carefully at molecular lines, but primarily for very cold stars.

Underhill — No, the molecular lines, in particular CN, are very important in moderately hot atmospheres. Consider the flash spectrum of the Sun. You can look back to the 1930 list of lines in the flash spectrum by

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Menzel and some of the most prominent are due to CN. They're low chromosphere lines even though they are molecules and they are formed where the temperature may be 8000 degrees. When you say cool stars and molecules, you may be thinking 3000 degrees or less. CN arises at twice such a temperature and I think CN is a very important intermediate temperature indicator. The reason I say that is because of the well-documented presence of CN in the flash spectrum, which is definitely chromospheric.

Pecker — **I** completely agree with you. I just wanted to stress the fact that so far this is the first time a molecular line has been mentioned today And that we shouldn't forbid the molecular lines to enter into our analysis.

Boesgaard — **I** want to add to the list of lines the Fe II lines discovered by Herzberg in M stars and found in an MS star and in Carbon stars. There are 17 lines in the region 3150-3300 A from multiplets 1, 6, and 7

Cayrel — Can you observe these from Mauna Kea?

Boesgaard — Mauna Kea is one of the best observing sites because of the high UV transparency at 14,000 feet. However, these cool stars are not emitting very much in the continuous background in that wavelength range so the exposure times are long.

Cayrel — I am surprised that nobody has mentioned the He 10830 line.

Beckers — The helium lines are very strongly radiation dominated. If there is any line that is not collisionally dominated, it is this line.

Sheeley — At Kitt Peak, Giovanelli, Harvey and Hall have taken some very nice spectroheliograms in 10830 with high spatial resolution. They look very similar to, although not exactly the same as, H alpha. 10830 would fall in the same category as H alpha, H beta, gamma and so forth.

Cayrel — But it is an absorption.

Sheeley — Yes.

Cayrel — We don't worry too much about what kind of source function we get in this line as long as we detect it is •absorption. The attractive thing is that you can observe it in hotter stars if it exists, without having a bright continuum masking a weak emission line.

Linsky — Another helium line that appears prominently in absorption in strong plages is the D_3 line at 5876 A. This line certainly indicates a chromosphere and should be looked for in solar-and later-type stars. I would like to point out that the CN bandhead at 3883 A is a very interesting spectral feature to study. A detailed non-LTE analysis of the

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violet system of CN will not be easy, but the bandhead should be sensitive to temperature at the temperature minimum and above for stars like the sun and somewhat later. Since the CN bandhead consists of about five overlapping lines, it is essentially a piece of continuum and thus insensitive to broadening, velocity fields, and magnetic fields. Spectroheliograms taken by Neil Sheeley in the CN bandhead show great contrast between bright and dark regions and appear to show fine structure in the chromospheric network quite well. George Mount, a graduate student, and I are presently studying CN spectroheliograms and center-to-limb photoelectric data in an effort to understand what the spectroheliograms are telling us.

Pasachoff — I should say that I am now working on a continuing program of- observing D_3 lines in late type stars to look for stellar chromospheres. I think that a report is better fitted for the discussion tomorrow morning. It is a tricky line to detect and there are some atmospheric lines in the region so it is not just a matter of looking for it and finding it. The original work done on the D_3 line was by Wilson and Aly, published in the PASP in 1956 (68, 149) in which they reported finding a line near the D_3 wavelength in several stars. The M star spectra are too complicated to tell whether a line that falls at that wavelength is the D_3 line or not. Since that time Vaughan and Zirin (Ap. J., **152,** 123, 1968) have published results of their extensive observing program and Zirin is continuing a program on 10830 with the 200-inch telescope. They published many equivalent widths of 10830 lines both in emission and in absorption in late type stars, finding some that even seem to vary in intensity. In my search I had the benefit of knowing which stars, such as X Andromeda, have a lot of 10830 in them. One way we can tell the origin of lines that we see at the D_3 wavelength is whether the intensities correlate with 10830. I should point out to people here who are calculating models that it would be of great interest to have more detailed models for the He lines, in particular the expected intensities and ratios of equivalent widths of 10830 and D_3 for various kinds of stars of type F, G, and **K.**

Fosbury- M.W. Feast (M.N.R.A.S. 1970, **148,** No. 4, 489) reported, in a paper on Lithium Isotope Abundances in F and G dwarfs, seeing X5876 in absorption in an F8 dwarf. The star is Zeta Doradus and is slightly pecular in several respects. It lies slightly above the main sequence (AM=0.6) and Feast measured a higher Li^6/Li^7 ratio than in any of the other stars in his program. It also shows unusually strong H and K emission for its spectral type. Wesson and I have looked for the X5876 line in some later type giants; we have also had, discussions with Griffin and looked at some of his very fine high dispersion tracings. We could not

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be certain of an identification in any of our samples. Figure 1-24 shows the Hel X5876 line in three spectra of Zeta Doradus. (Original inverse dispersion 13.7 A/mm. M.W Feast)

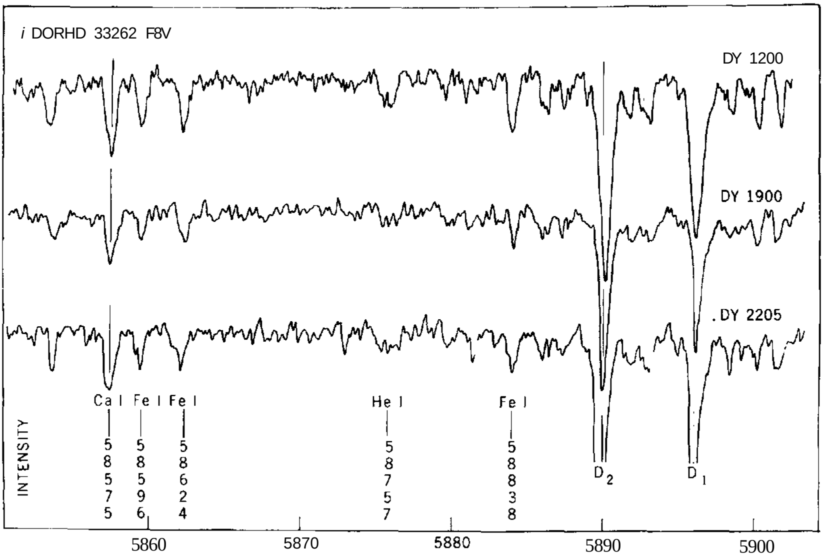

Figure 1-24

Cayrel — Can we now give the name of a line in a hot star (a B star) which is the best case for detecting a chromosphere if B stars have chromospheres. Is anyone ready to answer this question?

Pecker - I'm not ready to answer this question, but this goes back to the discussion that Jefferies made about the geometrical emission properties and the real, true emission properties of a line.

Cayrel — If you are in a geometrically thin layer in which you have a temperature that is significantly higher than the boundary temperature that you predicted from a model in LTE, how will you detect that? I think that the distinction into two classes by Thomas is not the real point, because the collisional rate is certainly large for most lines, because the electron density is high when hydrogen is ionized. I refer here to hot stars.

Thomas — **I** disagree. I really think what you want to do is look at the very recent calculations Mihalas has been doing on this distinction between the photoionization dominated lines and collision dominated lines. He's imposed the conditions of radiative equilibrium but it's easily

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generalized to the case where you have a chromosphere and lots of Mg lines, lots of Ca lines, although not Paschen alpha. He has very specific results on this.

Cayrel - I don't doubt that you can find lines that are collisionally dominated in hot stars, but I doubt whether there are lines strong enough in the visible spectrum, so that you could detect a chromosphere if it is not geometrically thicker than in the Sun. That's the problem.

Jennings — I would like to comment on the shell properties. I think if you make a distinction between stars with shells and stars with chromospheres, you're going to run into trouble among the late type stars. I would cite as an example alpha Orionis, which is certainly a late-type star with a chromosphere, since it has Call H and K, as well as Fe II, in emission. On the other hand, from the work of Deutsch and Weymann, there is certainly evidence for a very extended atmosphere involving mass loss. So here we obviously have a chromosphere co-existing with a very massive shell; and so I would argue that one would have to be careful in dividing stars into those having only a shell or only a chromosphere.

Underhill — They're not mutually exclusive; the shell is never accurately defined for B stars. To add to the list of lines, I would guess that for the middle B stars the Si II lines are important. It is well known observationally that 4128, 4130 change their intensity relative to the red Si II lines 6347, 6371 which are from simple levels, are well behaved and are associated with the other multiplet at 3856A. Now this has never been explained, though it has been observed. You never know whether the 4128 and 4130 lines are going to be strong or weak. The f values have been calculated by detailed configuration-interaction calculations. They've been observed and we know pretty well what they are with respect to other lines. Anyway you can't count changing one multiplet very much in one star and blaming it on f values. So the only thing that is left is the effect of chromospheric conditions. You have to compare the 4128 and 4130 lines with the red multiplet and the violet one.

Cayrel - But they are very weak.

Underhill — No. They're quite strong. The other Lines will vary in intensity as 4138 and 4130 vary. They come from a 3^2D level and 3^2D levels always cause you trouble.

Thomas — There's one more thing. We've been concentrating here as though what you need to do is take a line such as H or K whose profile somehow tells you the existence of a chromosphere. But just as the 10830 line in the Sun indicates for you that there's a chromosphere simply because you see it, so does any line in a hot star which should not

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be produced under conditions of radiative equilibrium; for example, lines of O VI in the Wolf Rayet stars, tell you that there is either a chromosphere or a corona. Since listening to Kuhi this summer I am convinced that Wolf Rayet stars have coronas rather than chromospheres, but I think the thing one should put here as an indicator of the presence of chromospheres and coronas are ionization levels. Simply the presence of any lines, no matter how they are formed, which you would not observe under radiative equilibrium in that star indicates a chromosphere. For that reason it is absolutely essential that we have good ideas of upper level limits of temperature such as Auer and Mihalas have been calculating. We need to know the highest temperature levels you would have under radiative equilibrium.

Cayrel — I think it is time to end the discussion on lines. At least we know how to raise interesting problems for theoreticians. For example, someone should determine what happens with Si II in hot stars and see if these lines are really coUisionally dominated and if the optical thickness is large enough to indicate a chromospheric temperature rise. I would now like to turn the discussion to continua and ask what are the good continua that indicate a temperature rise in the surface layers of stars.

Underhill — I would like to stress the importance of continua as chromospheric indicators. If you think about the long wavelength region around 8000 A where H" comes to a maximum you have one sort of opacity pattern. If you heat the atmosphere up to a temperature of 12000° or so instead of 7000° the opacity pattern in this spectral range changes its shape considerably, and free-free becomes one of the more important sources of opacity.

It has a different shape than H" That means your lines are going to fight against a different opacity, and it will change your relative intensities in that region. Therefore, there is the possibility of the continuous source changing, whether the star has an extended atmosphere with a temperature that goes down or goes up. Continuous opacity is an important indicator in regions where there can be differently shaped continua corresponding to a change in temperature.

Pecker — **I** agree with Anne Underhill; the Paschen discontinuity is important in hot stars, and there is a strong relation between it and the H~ opacity

Jefferies — Perhaps the source function is not always the Planck function. If the absorption coefficient is decreasing toward longer wavelengths and if the radiation temperature is decreasing toward longer wavelengths, then you probably have a case for saying that the temperature is increasing upwards — this is the sort of thing Mme Gros will talk about in the

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session tomorrow. However, when you get into regions where the hydrogen continua dominate you might have good reason to question whether LTE is the correct description for the source functions.

Underhill — When you get into the hot stars you may have a hot chromosphere starting at 50000K, then a high radiation field from 300 to 500A. If you have radiation from such continua, this is going to affect the rest of the atmosphere. What sort of criteria could we suggest to look for? Lines in these spectra might serve as criteria for the presence of **a** chromosphere.

Pecker — Jacqueline Bergeron has computed several early type star models with a corona to explain the IR spectra, and the heating of the HI region which is outside the HII region surrounding the star.

Cayrel — Can anyone propose continua or lines in the visible as a diagnostic for hot stars?

Peterson — Hot stars have strong metal continua, particularly carbon continua primarily in the **UV**.

Peytremann — **I** would object to the continua since they are hidden by lines. UV spectra show that you never see a nice absorption edge. They are washed out by the high density of lines.

Underhill — Continua with no lines are the only ones that can be used. There are too many lines from 912A to 6000A from average stellar spectra to do much with the continua.

Sheeley — Where no energy is put into the spectra, it doesn't really matter, I would think the lines would have a negligible effect.

Underhill — Look between 3000 and 4000A. There are so many lines no one knows what to do. In a paper by Houtgast and Namba a couple of years ago, In BAN they found between 40 and 50% line blocking, which is quite a bit. Line blocking can alter the spectra in these regions considerably.

Cayrel — From the viewpoint of models, is the continuum brightness temperature sensitive to the chromospheric temperature?

Cuny — Yes, it is sensitive.

Kalkofen — You couldn't use the Lyman continuum as a chromosphere indicator for stars earlier than B.

Thomas — From the HII region I can observe whether or not I have a chromosphere-corona. The HII region is a big part of the stellar atmosphere.

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Underhill — Don't forget that we use the planetary nebula to tell us what the nuclei are producing in the way of flux. One of the best photon counters is a planetary.

Aller — Are you sure it is strictly a photon counter and that the emitted radiation cannot sometimes be enhanced by energy imparted by a stellar wind?

Underhill — The gas is moving, and there is mass motion, but it's still a photon counter, a gas flow counter. Now, for cooler stars, is there anything else we can use for a photon counter?

Pecker — I just want to object to what Anne Underhill just said. Is a planetary nebula a real good photon counter, or is it a counter of only detected photons? The Zanstra mechanism shows that Te in a PN is sensitive to the *quality* of the radiation, not to its *quantity*. The state of ionization, to the contrary, in an HII region, is a function of quantity of UV photons. So the sentence of Anne's is ambiguous, and should be used with a great deal of caution!

Linsky — One potential indicator of chromospheres in very late type stars, which has not been mentioned, is the pure rotation band of water vapor in the region of 20/z and longer wavelengths. Many very late type stars exhibit infrared excesses at 20ju, which have been interpreted as circumstellar emission. An alternative explanation is that the pure rotation band is sufficiently opaque that the region of formation of the band is in the lower chromospheres of these stars.

Jennings — I would like to comment on the H_2O. Even though water may have bands at 20JU, it is difficult to explain the strong features at IOJU, and it should be pointed out that various people have suggested silicates which have 10 and 20/z peaks. A number of investigators have discussed the shape of these peaks, and find that molecules cannot reproduce it while grains like Mg and Fe silicates can.

Johnson — Besides the spectral feature already discussed, there is another class of observations that might indicate stellar chromospheres. Spectral lines in late type stars often appear to be broadened by very large turbulent velocities (sometimes supersonic), and there are displaced lines in other stars that show outflowing material. In these stars we thus see evidence of energy dissipation or matter flowing from the photosphere, both of which phenomena we might call chromospheres.

Vernazza — We determined an empirical solar chromosphere model by assuming a temperature as a function of height and solving the hydrostatic equilibrium, statistical equilibrium and the radiative transfer equations for a 4-level H atom, an 8-level Si I atom, an 8-level C atom, a

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5-level Ca II atom and H-, to obtain the continuum emergent intensity at any wavelength. T_e vs. height is adjusted until agreement with the observations is reached. As a result we are able to match the observed solar continuous spectrum from 500A to centimeter wavelengths, as well as several lines such as Lya Lyj3 and Ha. From the model, which also includes a microturbulence structure, we can determine approximately the radiative energy losses at every height and every continuum frequency, as well as the losses in some of the hydrogen lines. I will give a brief summary of how the temperature model shown in Figure 1-25 is adjusted. Essentially, the T_e vs. height model begins in the upper photosphere, extends through the temperature minimum at 500 km above r 5000 = 1, through a quasi plateau in the chromosphere and finally through a high temperature plateau between 2000 km and 2200 km in the transition region. The temperature minimum is put at 4100 K. The first quasi - plateau is around 6000 K and the second at roughly 20000 K. In the photospheric region between the temperature minimum and 5000 K the temperature structure coincides with the H.S.R.A. Below 5000 K our model has a lower temperature because we solve the non-L.T.E. problem for H. The departure coefficients from L.T.E. for H are less than one, which gives a higher electron density than in L.T.E. As a result we have a

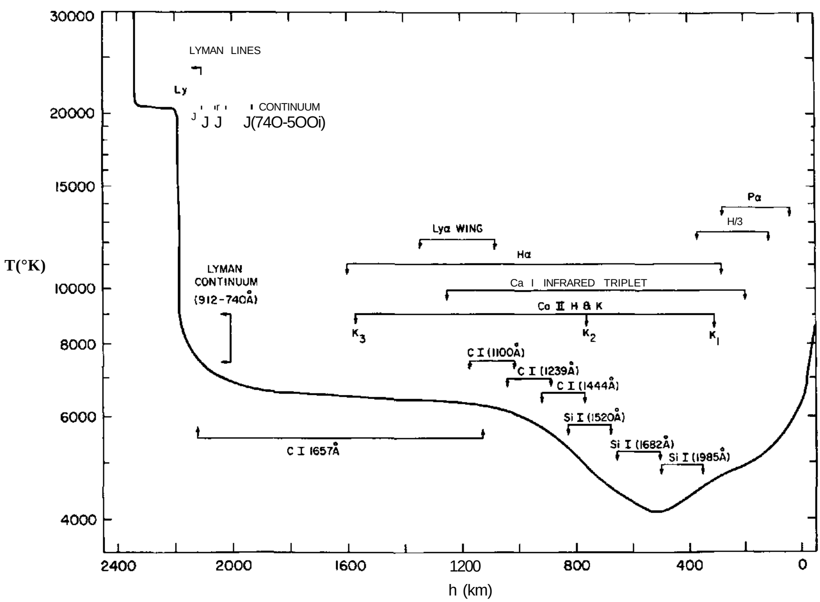

Figure 1-25

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lower T_e, but we nevertheless compute the emergent infrared intensities as they are observed. In the region of the temperature minimum the Si I ^3P, ^1D and IS continua are formed. These continua serve to give us a good hold on the temperature structure at the temperature minimum. Until recently all realistic solar models have obtained the Si I continuum intensity in L.T.E., (except for some preliminary work by Y Cuny) and required a higher T_e to explain the U.V observations. Since we have a non-L.T.E. Si I solution the temperature can be lower because the Si I ground state source function is larger than the Planck function. This is due in part to the interaction between the Si I ^3P continuum and the Lya line. Since the Lya line has a higher source function than in L.T.E., it controls the Si I continuum source function. The C I ^3P, *D and*S continua are formed above the temperature minimum. These continua provide information about the temperature distribution at around 6000 K. The observed continuum intensities between 1440A to 912 A are reproduced by the present temperature model. In addition Ha Hj3 and Pa which are formed over an extended chromospheric region are also reproduced. At around 8400K the Lyman continuum is formed. Above, in the 20000K plateau the Lyman lines are formed. There are several reasons for the existence of this small plateau at 20000K. One of the best observations we have is the ratio of the Lya, Lyj3, Ly7, Ly5, and Lyy to Ly5 integrated intensities.

In order to satisfy these observations we need to have the 20000K temperature plateau. We know the Lyman continuum is formed at approximately 8400K. So above the Lyman continuum formation region, we are forced to have a very sharp temperature rise. Otherwise the optical depth in the Lyman continuum will be too large, and will be formed at a much higher temperature. Then somewhere at 20000K the temperature gradiant must flatten to the point of producing a plateau to reproduce the Lyman line integrated intensity ratios and their absolute intensities. At the same time the plateau is necessary to obtain the central reversal in Lyj3 that, otherwise is impossible to obtain. Unfortunately there is only one observation of Ly|3. The only way we have to reproduce the Lyj3 profile is by having a Lyj3 source function which decreases toward the surface. And the only way to obtain this decreasing source function is by means of a plateau. In addition we have center-to-limb observations of the integrated intensity of Lya, Lyj3, Ly7, Ly5, and at six wavelengths in the Lyman continuum.

The limb darkening observations are not good because inhomogeneities, namely spicules or dark mottles could introduce additional darkening, and by how much we do not know. That is the reason we do not rely too much on limb brightening or darkening observations. Lyman *a* has strong

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limb darkening, about 75% of the Sun's center. Most of this XUV data comes from the Harvard OSO IV and VI experiments as well as from some unpublished rocket data from H.C.O. With this temperature structure we can compute the energy losses in the chromosphere. We have to keep in mind that these are still provisional results. In Figure 1-26 the solid line represents the radiative energy loss as a function of depth for the present temperature distribution. The Lyman *a* contribution is shown by a short dashed line, Ly/3 by a long dashed line, Ha by a dotted line, and the Lyman continuum by a dashed-dot line. In the upper chromosphere Lya is the main cooling agent, while in the low chromosphere Ha is responsible for most of the cooling. There is a diffusion of Lya photons from the upper chromosphere into the low chromosphere. This produces some heating in the low chromosphere. The continuum losses are negligible except by some CI continuum cooling around 5500K.

Delache - **I** would like to ask if this 20000° Lyman plateau exists because of mechanical energy deposition in this region, or because the radiative losses have to occur in Lyman *a*

Vemazza — We have calculated these loss curves from a temperature model which has been chosen empirically in such a way that the predicted spectrum agrees with the observed one. Then we have deduced the radiative gains and losses in order to determine the mechanical energy input necessary to maintain the temperature model.

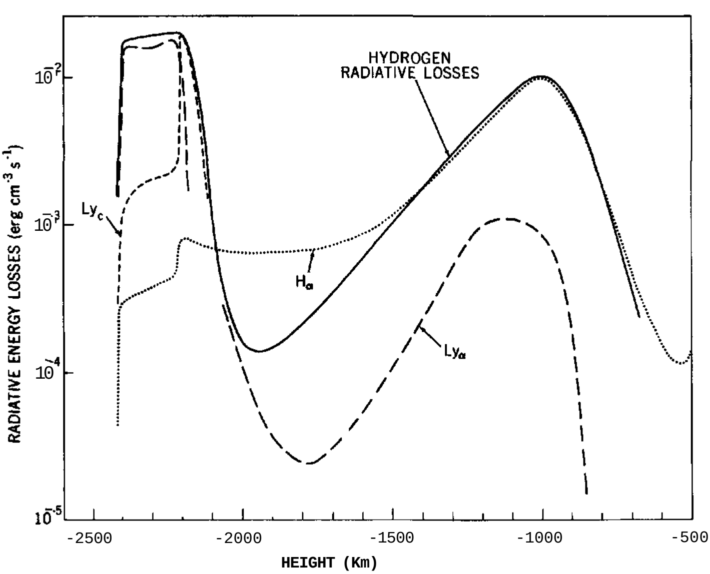

Figure 1-26

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Jennings — The loss rates should be proportional to the area under the curves you have drawn for Lyman alpha and H alpha. Do your results imply that Lyman alpha is giving up the largest part of the chromospheric energy loss?

Vemazza — Yes.

Skumanich - In addition to these results based on the divergence of the radiative flux you might find it interesting to compute the contribution of the divergence of conductive flux.

Vemazza — I understand that for a temperature of 10000°, Ulmschneider has computed the conductive flux coefficients in L.T.E. Given the extreme departures from L.T.E. I would be reluctant to base the conductive flux contribution on such results.

Ulmschneider — Using the temperature distribution determined from the Lyman continuum observation (Noyes and Kalkofen 1970, Solar Physics, 15, 120) one can compute the conductive flux. One finds that this flux is about 2×10^3 erg/cm^2 sec compared with the observed radiation flux of about 6.4×10^3erg/cm^{2}sec, (Friedman 1963, Ann. Rev. Astr. Astrophys., 1 59), the difference being due to mechanical and radiation heating. The amount of radiation heating through the absorption of Lya and Lyj3 photons in this region between the Ly continuum and Lya emitting regions appears now to be crucial for the existence of a temperature plateau. This may be seen as follows.

The radiative loss in the Ly continuum, Lya, Lyj8 regions is balanced by 3 competing heating mechanisms, thermal conduction, mechanical heating by shock waves and radiation heating. Of these mechanical heating becomes unimportant at greater height because, first, the increasing sound speed increases the wavelength, decreasing the strength of the shock wave and thus its dissipation, second, the dissipation of shock waves is a slow process and can not rapidly balance strongly increasing radiation losses. If radiation heating were also unimportant then thermal conduction would be the only significant heating mechanism. In the Ly continuum region the coefficient of thermal conductivity K, due to the increasing degree of ionization, is a decreasing function of temperature or height.

$$\frac{d\pi F_{Rad}}{dh} = \frac{d}{dh} K \frac{dT}{dh}$$

Thus through this equation any radiation loss and even zero radiation loss would lead to an increase of the temperature. This argument is especially valid in the main Lya emission region. In this region we expect a strongly rising temperature due to thermal conduction.

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On the other hand if radiation heating is appreciable then it could decrease the conductive flux leading to a temperature plateau between the Ly continuum and Lya emitting regions. For example if a radiative flux of Lya photons going toward the sun of about 2 x 10^3 erg/cm^2 sec were absorbed in the region between Ly continuum and Lya emission then assuming, for example, no emission in this region one could get

$$\frac{dT}{dh} = 0$$

as seen from the integrated version of the previous equation.

(note added in proof:) A numerical check of the importance of this Lya back heating was done after the conference by W. Kalkofen. He found that it invariably occurred in various different models so that the existence of a temperature plateau seems to be fairly certain although for reasons different than originally proposed (Thomas and Athay 1961, Physics of the solar chromosphere. Interscience, New York. p. 156).

Vemazza - (Note added in proof:) I referred to the conductive flux coefficient published by Ulmschneider (Astro & Astrophys. 4,144, 1970 which is calculated assuming L.T.E. Later, however, Ulmschneider kindly provided me with a more general conductive flux coefficient subroutine. The divergence of the conductive flux was calculated and was found to be insignificant.

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