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## CONTINUATION OF DISCUSSIONS FOLLOWING TALKS BY PRADERIE AND DOHERTY

**Kuhi** — Now let's have a general discussion of Francoise Praderie's paper. Let's first discuss the question of what we do mean by a chromosphere from an observational point of view. One thing that bothers me a great deal is the distinction between a stellar chromosphere as we've come to think of it in the Sun and the changes that seem to take place as one goes from cool stars like the Sun to hotter and hotter stars in which the distinction between the defining characteristics becomes ever more vague, in separating out a chromosphere, an extended atmosphere, an extended envelope, and so on.

**Aller** — I think it is very- important to make, as you say, a distinction between a chromosphere on the one hand, and what have loosely been called extended envelopes and shells on the other. There are a number of objects in which the gradation from one to the other is certainly not clear cut. A good example is RR Telescopii. In that star you see a spectrum of ionized titanium and iron that looks qualitatively somewhat like the flash spectrum of the Sun. Superimposed on it, however, are increasingly higher levels of excitation; both forbidden and permitted iron lines, ranging on up from [Fe II] to [Fe VII]. In fact, [Fe VII] supplies the strongest features in the emission spectrum of this object. In looking at the spectrum carefully there seems to be no place where you can say everything of one or two levels of ionization should be assigned to an ordinary chromosphere and everything else is to be attributed to something else. There seems to be a steady gradation in excitation. It's almost as though we were looking at the solar spectrum, in the near UV region.

**Steinitz** — I would like more clarification of the definition of a chromosphere. One of the necessary conditions was defined to be mass flux, and it wasn't clear whether the idea was mass loss, or accretion, or just mass motion. Also could you clarify what exactly is meant by non-radiative energy transfer? Should this include or exclude specifically convection?

**Praderie** — I did not want to include mass *loss* as such as a necessary condition for a chromosphere, because I have no clear evidence that the mass loss is unequivocally bound to the existence of *a* specific region in the atmosphere. One can find mass loss as shown from the shape of profiles in lines of photospheric origin in some stars, whereas, in other stars, the mass loss is expected to occur only in the corona. So the mass loss itself I did not include in my discussion. Mass flux was meant as any net transport of matter in a certain region, of which maybe the mean value over time or over some distance can be zero. Now, concerning the non-radiative energy transport, it is not restricted to the chromospheric layers. In the photosphere you can have it too (turbulent, progressive waves, convection, etc.), but there is no dissipation to heat the thermal pool at that very place. So then I call chromospheric the region where the dissipation starts to act.

**Steinitz** — It is not clear how the observables which you discussed are directly connected, even though they were classified as direct and indirect observables with those criteria just now mentioned.

**Praderie** — **I** am aware that I have not clearly made a bridge between what one would wish to do, according to the scheme which was given in the introduction, and all the detailed observations which are available now. This is a diagnostic task which is far from being completed.

**Athay** — **I** think the question of the definition of chromosphere is very critical. We ought to use a definition that will allow us to talk about chromospheres with the least amount of confusion. I think that the way we defined it yesterday and today would lead to a maximum amount of confusion. The proposed definition requires a very careful interpretation of data and is not one which you can very easily go to from observations. We should define chromosphere for use in the literature as requiring the minimum amount of interpretation of data. I think it ought to be defined in terms of temperature reversal which you can at least hope to get to in a simple way. I don't see how an observer could ever get to an observable of mechanical energy flux. So, if we use that as the defining characteristic we'd have to restrict the observers from ever using the term chromosphere, leaving it only for the use of theoreticians.

**Kuhi** — **By** mechanical energy flux do you exclude mass loss then?

**Praderie** — I exclude it, maybe for convenience. In reply to Athay, I admit that we have apparently confused things by giving a definition which is bound to theoretical considerations; but it is my feeling that only from properly analyzed observations can you presume the presence of a chromosphere. I tried to show that if you have a positive outward temperature gradient it doesn't tell you enough. Even if you have none, you may miss the start of the chromosphere. I think one has to look for general definitions, not only for operational ones.

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**Thomas** — Here I also disagree with Athay Let me give you two examples. It seems to me that we should be defining things which the observers can use unambiguously when they look at data. My two examples are the atmosphere of the Sun and the atmosphere of a 15,000° star. The basic question for the interpretation of stellar atmospheres is, is it sufficient just to drop the assumption of LTE? Or must I also drop the assumption that there are only radiative energy sources? To me, a chromosphere is that atmospheric region for which I must drop the assumption of radiative equilibrium. This is very clear conceptually. From a purely observational standpoint what then is the situation? In the Sun, at r = 1,1 have a temperature of about 6000° I have a temperature minimum of about 4200°, judging from the observations. At a height of about 500 km in the chromosphere, the temperature is again about 6000° Now the maximum temperature one would get from radiative processes alone is about 5300°, based on the work of Cayrel, Frisch and others. Hence, for the Sun, we can infer the input of non-radiative energy. Now for the 15,000° star, pure continuum models give a maximum boundary temperature of about 9500°, based upon the work of Auer and Mihalis and the simple calculations of Gebbie and Thomas. The introduction of the effect of lines on populations may raise this value as high as 13,800° The clear cut observational question to be answered, then, is do the temperatures prevailing outward from the temperature minimum of the 15,000° star exceed the value predicted from radiative equilibrium models? If so, we can infer the dissipation of non-radiative energy and hence the existence of a chromosphere.

**Conti** — I would like to take a heretical view of the chromosphere by defining it in a simple way. Suppose we say that any time you see emission lines you have evidence for the existence of a chromosphere.

**Kuhi** — How would that allow one to distinguish between chromospheres and large scale extended atmospheres?

**Conti** — Maybe there is no essential difference, except in the scale. If a theoretician tells me that a chromosphere is present, I know that I'll see emission lines. The only question that remains is, if you see emission lines in Wolf-Ray et stars, Of stars, or early A or B stars, does it necessarily imply the existence of a temperature rise, mechanical heating or mass loss? I don't wish to go into a detailed theoretical discussion on this, but, as far as I know, where emission lines are seen, at least one of these three phenomena is always present. So we could have, as a working definition, that a chromosphere is a region in a stellar atmosphere which gives rise to emission lines.

**Kuhi** — Are there contrary views? I believe the problems for both the observer and the theoretician are much worse than Dick indicated.

**Underfill!** — I agree with Conti. However, I believe the problems for both the observer and the theoretician are much worse than Dick indicated.

**Kondo** — With regard to Conti's definition, I wonder if you would include close binaries in this category. They do have different problems than other stars such as those involving mass transfer and mass loss. Our balloon observations and OAO-2 observations show that j3 Lyrae has magnesium doublet emission, for example.

**Conti** — One could make exceptions but one could also use these to iiustrate the point. There are close binaries which have greatly enhanced H and K emission. X Andromedae is a fine example. Its emission lines are certainly chromospheric. And so we see that the chromospheric phenomenon has been accentuated by heating in a close binary.

**Underbill** — **My** definition of a chromosphere is that region of a stellar atmosphere that deviates from a simple model. Figure H-20 shows the predicted flux envelope for an ordinary 13,000°, log g = 4.0 model

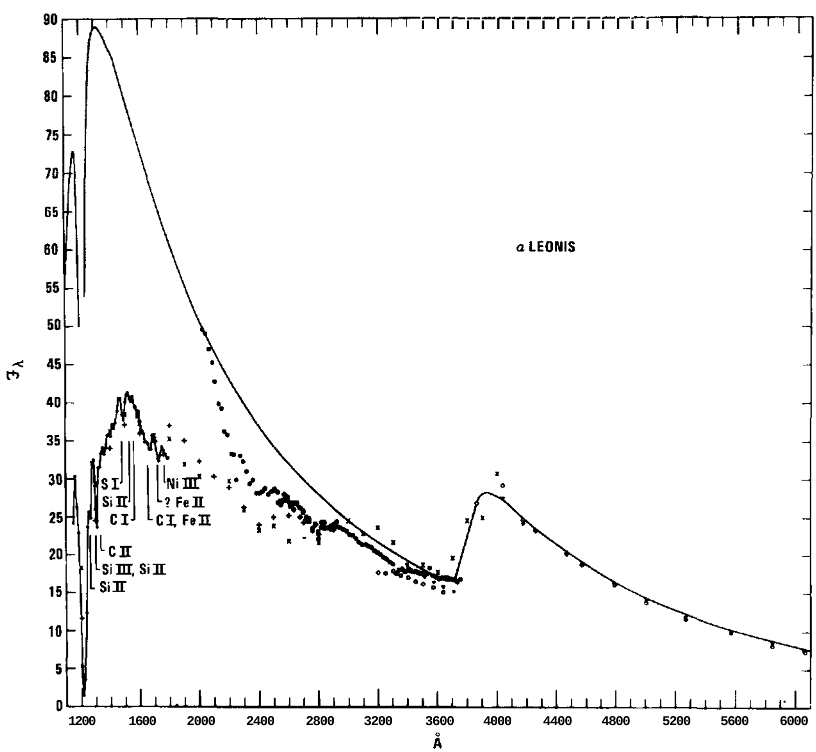


Figure 11-20

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atmosphere, calculated in hydrostatic equilibrium, in LTE, with the plane parallel assumption, etc. 13,000° is a fair choice of effective temperature for a B7 or B6 star like *a* Leo. The ground-based observed absolute fluxes are in units of $10^{40}$ ergs/cm$^2$/sec/A. As shown the model calculations fit the ground-based data like a glove. Also shown are the UV observations from OAO and from rockets. The OAO scanner 1 observations (3700-1800 A) are calibrated with the relative sensitivity function given to me by Savage. The rocket observations in the same wavelength range are tied into quite a decent absolute calibration and lie considerably below the OAO observations. I have concluded that the Savage sensitivity function must be in error and I have derived a new sensitivity function by forcing the scanner 1 data to fit the rocket observations. The short wavelength OAO scanner 2 observations have also been calibrated against absolute rocket fluxes. What I want to point out is that up to now we've been talking about the visual part of the spectrum which can be fit well with models, as long as you don't look at the results too closely. But as soon as you get into the ultraviolet below about 2800 A, the observed flux drops away from the model very rapidly. These results for a B7 star are similar to those I've also found for a B0 and a B3 star.

Something even worse is illustrated in Figure 11-21 which shows the observed flux for a rapidly rotating AOV star, 7UMa. The continuous line gives the flux envelope for a hydrogen line blanketed model, effective temperature 9750°, which fits the observations in the visible region. The observed flux shortward of 1800 A lies very much below the model, indicating line blanketing of a factor of about two. In Figure 11-22 is shown the observed flux distribution for Vega, which is also matched to theoretical fluxes in the visible. Now, you see a difference between those two AOV stars, one rapidly rotating and one not. For Vega, we have an *excess* of flux below 1600 A, with respect to the reference distribution (that of the model atmosphere), while for 7UMa we have a *deficiency* of flux with respect to the reference model.

Figure 11-23 shows rocket and ground-based observations of aCMa, fitted to the same reference model. Teff 9750° Again there is a lot more flux below 1600 A than you have in the rapidly rotating AOV star, 7UMa, but not as much flux as there is in Vega.

What I really want to say is summarized in Figure 11-24. Here are the three AO stars, or A1 in the case of aCMa, plotted with respect to the same model. You get considerable UV line blanketing in 7UMa; aLyr has a large brightness, or flux excess. It is 50% brighter than 7UMa at 1800 A or so; and aCMa lies in between. One would never have known that these three stars differ so much, from studying the ordinary ground-based spectral region, to which we have been fitting models. In Vega's far UV

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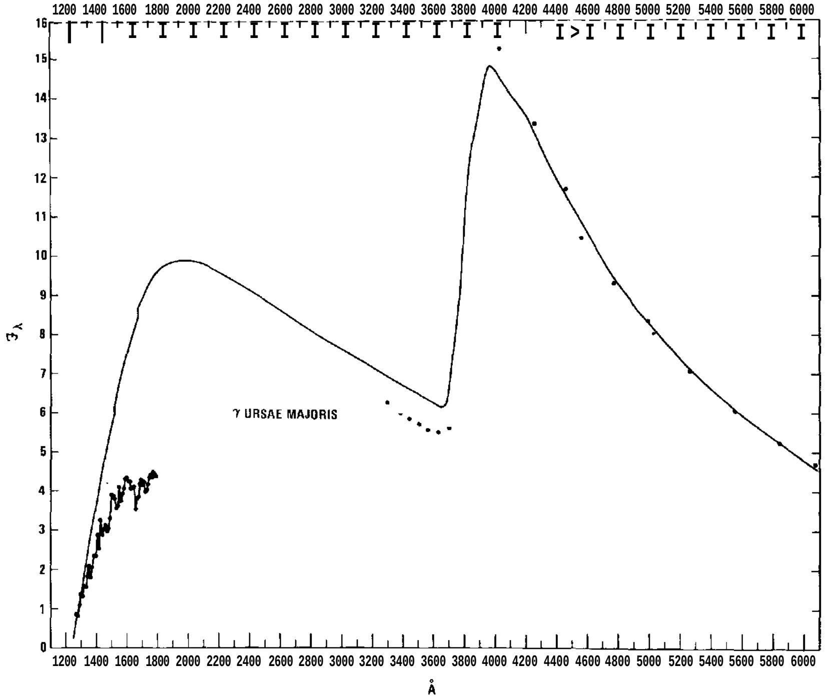


Figure 11-21

flux excess are we seeing a hot chromosphere or a companion? I really don't know. qLyr is a very funny star; it has been previously postulated to be double. The point is that around $T_{eff}$ = 10,000°, the predicted ultraviolet spectrum is terribly sensitive to the details of the model shortward of 3000 A. Nothing that we've been able to observe from the ground is nearly as sensitive. So the ground-based observer is up against a real problem in trying to determine if a chromosphere is present or not. Simple classical models predict continuously dropping temperatures and pressures as you go outward in the atmosphere. I defined, half jokingly, a chromosphere as being that region which reflects a departure from such simple models. Unfortunately, most ground-based observables are not very sensitive to these departures.

Hack - I would like to make a comment about the Conti definition of a chromosphere, having in mind the extended atmospheres of A-type supergiants. If we look at spectra of la supergiants we see Ha in emission, and according to the Conti definition we should say that these stars have a chromosphere. If we look at the spectra of Ib A-type supergiants we generally don't see Ha emission. But in both types we observe the same

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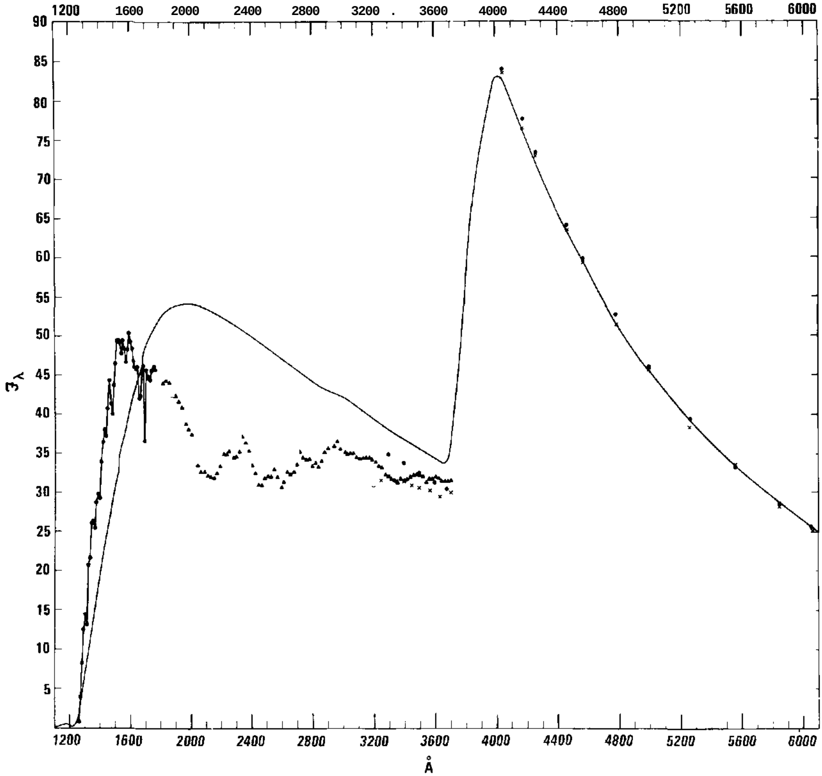


Figure 11-22

kind of radial velocity fields, and Balmer velocity progression, which indicate an expanding atmosphere. Hence, in my opinion, we must use the same definition (chromosphere, or extended atmosphere?) for both la and Ib atmospheres. The line contours are rather different in spectra of normal B-type stars and in spectra of *(3* Canis Majoris stars, 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 those of the normal main sequence stars. As a matter of fact there are some evidences that they are surface rather than atmospheric effects. Huang has shown that the sum of the equivalent widths of the components (measured at phases when the line is divided in two components) is equal to the equivalent width of the line (measured at phase when the line is single). He interpreted this fact as a proof that the components are not formed at different heights in the atmosphere, but rather in different parts of the stellar surface.

**Kuhi** — I think that is the problem with a definition that says anytime we see lines in emission we have a chromosphere.

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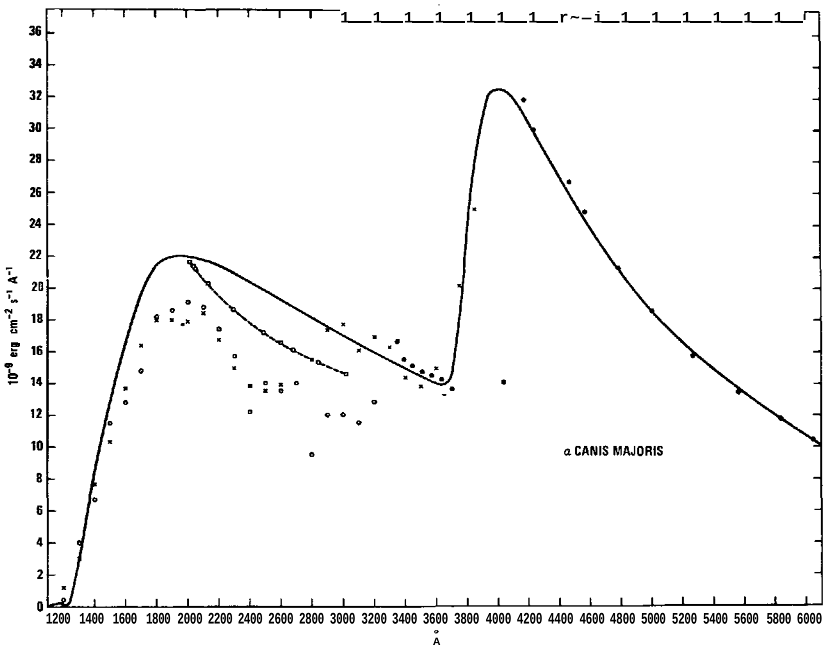


Figure 11-23

**Cayrel** — I am going to propose a very simple definition of a chromosphere, because I think it is too dangerous to have a definition based on assumptions you are making in your work. Very clearly, when people first defined the chromosphere of the Sun, they had the idea that when you look tangentially (at the solar limb) you get an optically thin situation in the continuum. So I would propose that the base of the chromosphere is where you have tangential optical thickness equal to one. There is then the problem of what kind of optical thickness we are using in the continuum. I would propose to define a wavelength Xo by Xo $T_{eff}$ = 0.288, and select a wavelength in the continuum which follows the spectral type or effective temperature. The other problem is what is the upper boundary of the chromosphere. In the word chromosphere you have "chromos" which means color, the idea being that when you look tangentially above this layer you are looking into lines. If there is a dominant line you get the color of this line. I would propose to take as an upper boundary r tangential = 1 in the strongest line of the spectrum which may be quite different in a cool star and in a hot star. In the sun I think that would be Ha. I don't know what the strongest line would be in hot stars. I think this would eliminate the problem of extended envelopes, because even in lines you are optically thin in envelopes.

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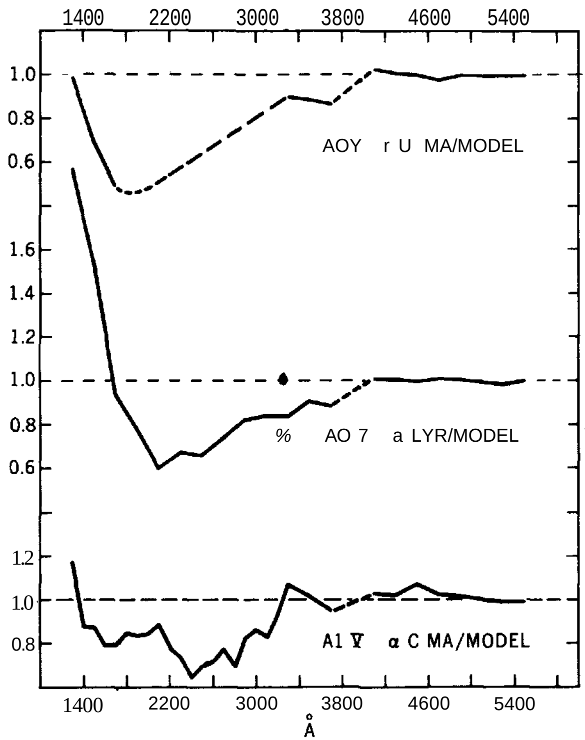


Figure 11-24

**Kuhi** — I'm not so sure that's true. There are stars with large envelopes that have optical depth much greater than one in emission lines.

**Cayrel** — **In** that case perhaps the chromosphere merges into the envelope.

**Auer** - **I** will be heretical about the definition of a chromosphere. Some objects are interesting because they have a chromosphere. If you ask the average graduate student what the solar chromosphere is he will say it is that region where there is an outward temperature rise. Would someone please tell me what is wrong with that definition. *There are lots of reasons for having emission lines. One of them is a temperature rise,* and that is one phenomenon that I would call a chromosphere. It is the simplest definition. There are problems with definitions that require mechanical heating. After all there are granules in the solar photosphere which are evidence of the presence of a mass flux. Are you therefore going to make the photosphere a part of the solar chromosphere by the mechanical motion definition?

**Kuhi** - We are dealing with observations here today and I think the question is simply how do we define chromospheres in stars from an observational point of view.

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**Auer** — I think the answer is clearly from phenomena related to a temperature rise. There are different ways to get emission lines, one of which is by means of a temperature rise. Certain lines will show emission because of this temperature rise.

**Kuhi** — So how do we go backward from observing emission lines to inferring the presence of a temperature rise? That is a little hard to do without good models.

**Auer** — It is hard to do, but that is not the problem of a clean definition of a chromosphere.

**Steinitz** — From an observational point of view, couldn't one say that a sufficient condition, not a necessary one, would be that you find emission lines with excitation temperatures higher than the color temperature of the star?

**Kuhi** - But one can think of stars that don't fit that.

**Praderie** — We have called a chromosphere a region where we find a temperature higher than that which you would expect in a radiative equilibrium. If we take all emission lines as characterizing a chromosphere, we can get into trouble because some of them, those formed by very specific excitation processes like fluorescence, will say something only about the radiation field and not about the gas kinetic temperature. Secondly, I also suggest that with Auer's definition of the chromosphere as being a region with a temperature rise outward, you have hidden the confusion within the definition, because you do not know what is the cause of the temperature rise at that place. I admit that I have not proved, in any of the indicators I have given, that they say something *directly* about the heating, except in the sense that Steinitz has just stated, i.e., whether the temperature derived is higher than some color temperature in the spectrum.

**Frisch** — 1 would like to know why we need a definition of a chromosphere. We need a word that everyone agrees about. Perhaps when we have many observations and people can do statistics, then we will need a definition. But now it is premature.

**Kuhi** — I don't really want a definition of a chromosphere. I would like to know the answer to the reverse question. If we observe emission lines in a star, are we necessarily observing a chromosphere?

**Magnan** — The only relevant point is, given a spectrum, can we determine the temperature vs. height relation. Also the definition of an emission line is not clear.

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**Pecker** — The main problem is that we have a hint that emission lines mean something. They might mean many things. The job of the experimentalist is to fish in the pond. The job of the theoretician is to take the fish and see if there is a chromosphere in the fish, or what amounts to a chromosphere.

Thomas — Clearly what you call something is not important. What we would really like to do is to understand what causes the structure of a star. We know the difference between the atmosphere and the interior in a vague sort of way The only reason one introduces atmospheric subdivisions is because different physical phenomena characterize these different subdivisions. We want really to determine what is the evolution of physical phenomena as I go outward in a star. In a very classical way the temperature and the density, by themselves, will suffice to describe everything, if I can make all of the standard classical assumptions. This isn't true if I go far enough out in an atmosphere. For example, in some cases there is a complete breakdown in the notion of describing a velocity field only in terms of a thermal component and a three dimensional macroscopic component. So, we should make some definition of atmospheric subdivisions which tell you what are the physical phenomena happening in those subdivisions.

**Underhill** — That is the physical approach and it is a logical and correct one. The problem for the observer is that he normally has to observe over a short wavelength interval. As we extend our wavelength region, we find we are observing different parts of the same object. A physical model which fits well the observations in one wavelength region may not fit observations at all in a different wavelength interval. Trying to extrapolate from one region to another on the basis of physical models is where we go astray. The observers are right to go after emission lines or extra deep absorption lines. In the ultraviolet, however, we have to take care that what we are calling emission lines are not really regions of residual flux between strong absorption lines in heavily blanketed regions. I'm not yet fully convinced of Ypji Kondo's arguments for seeing emission lines, but he really can't say anything else at this stage. With the kind of line blanketing I see at 20 A resolution in that region in OAO-2 scans, I wonder how much of the "emission" he sees is residual flux between lines. This is precisely the problem — the observations.The more observations we can get the more we're going to know. The theoreticians should proceed, but don't anchor yourselves to a fixed scaffolding of theory and get so fixed that the poor observers think it's there for good.

**Kuhi** — **I** don't think the observers have that problem.

**Athay** — So far I haven't heard two people give the same definition of a chromosphere. Let me be the first to support the definition Larry Auer

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gave, namely those situations where the temperatures rise outward in the atmosphere. That is a case where we can hope to give some simple diagnostic to the observer that he can use in saying a certain phenomenon indicates a chromosphere. If the theoreticians want to invent another word to describe a place where there is mechanical energy dissipation, we can leave that up to them.

O. Wilson - I've come to the conclusion after listening to this little hassle, that one man's chromosphere is another man's extended atmosphere. (Laughter and applause.)

**Kuhi** — **I** would suggest that we go on now to look at what the observations are trying to tell us. In her survey Francoise Praderie discussed many cases which we can cover one-by-one, starting with the question of excitation anomalies. Were there any questions of clarification about the Ca II H and K lines?

**Skumanich** — One should be very careful about listing universal criteria for chromospheres, when using the H and K lines. For example, one thing that was listed was intensity — age relationships which only apply to main-sequence stars. As I've shown in a study that has appeared in abstract form only, Call emission in the K giants is not an indicator of age. There is no kinematic difference, for example, between the emitting and the non-emitting K-giants.

**Kuhi** — But how about the pre-main-sequence stars? Not the T-Tauris, but those that are farther along than T-Tauris and almost on the main-sequence. Do you know what they do?

**Skumanich** — No, **I** don't.

**Kuhi** — Are there any other questions about the CA II emission in the Sun or in the stars?

**Linsky** — I would like to show some work by Tom Ayres, Dick Shine and myself at JILA. We have observed a few stars which are reasonably similar to the Sun in an effort to get absolute fluxes if it is at all possible. I'll start by presenting the data on Procyon which is an F5 IV star. Kondo mentioned that there is likely to be emission in Mg II H and K in this star. What I have here in Figure 11-25 is a low spectral resolution scan of the region including Ca II H and K. The units here are flux in ergs/cm$^2$/sec/hz at the surface of the star. I show this scan for two reasons: (1) to show that at low spectral resolution you see no emission in H or K and (2) to show how we calibrated our data in absolute units at the surface of the star. We took a 10 A interval centered at 3950 A and tied this through photometry to Vega at 5000 A for which an absolute flux is known. We put in the radius and parallax of the star to

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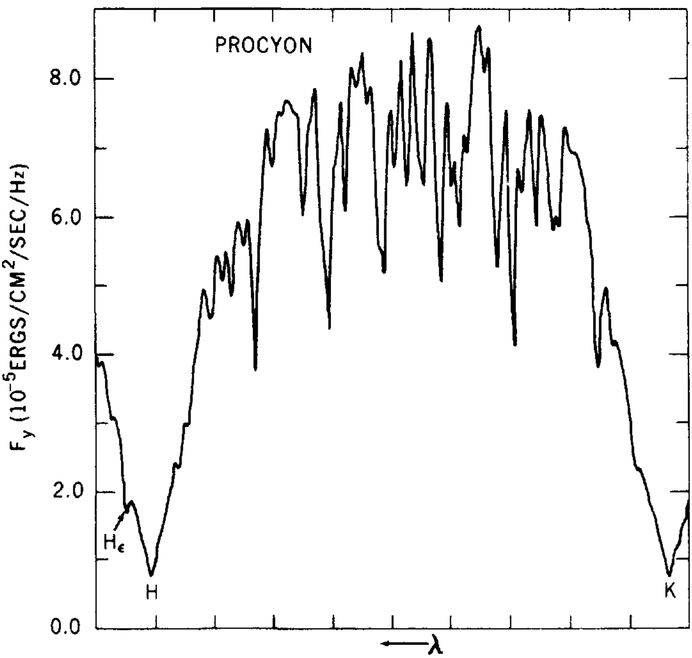


Figure 11-25

get absolute values of the flux at the surface of the star. Figure 11-26 shows a high resolution 7th order scan of the K line of Procyon. This is in the center of the line over about 1.7 A interval. The data have been filtered: This is data taken with the Kitt Peak solar tower and I might point out that this represents 5 hours observing. Again the units are flux at the surface of the star. On the right hand side of the diagram we have turned the flux units into an equivalent brightness temperature. In this scan we see a profile very similar to the K line in the Sun. There is definitely a reversal on the violet side, although such a reversal is unlikely on the red side. Also the brightness temperature corresponding to K^ is about 4900° If the minimum temperature in the Sun is about 4300°, which is the same as the brightness temperature in K1, and if one thinks of Kl as a good measure of the minimum temperature in the Sun, then we may indeed have a direct measure of the minimum temperature in Procyon. What is especially interesting is that the ratio of the brightness temperature in Ki to the effective temperature of the star is 0.745 for Procyon *and* the Sun.

It may well be that there is a scaling law which is applicable, wherein the physical processes that determine the minimum temperature in Procyon and the Sun are the same. So perhaps one could extrapolate at least over

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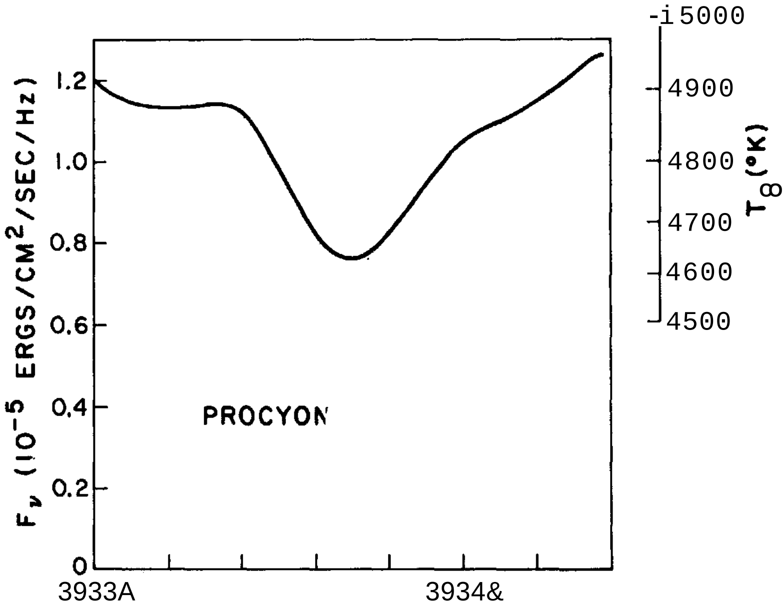


Figure 11-26

a limited range in the H R diagram to determine the minimum temperatures in related stars.

Figure 11-27 shows the K line profile of Procyon again, now in residual intensity units. Also shown is the K line for the Sun, now viewed as a point source. This is to show that the shape of the profiles is the same, although the Procyon profile is much broader. In addition the K line for Arcturus is shown; it possesses a much more significant double reversal. The data for Arcturus are taken from Griffin's Atlas.

Figure 11-28 shows additional data we obtained for Procyon near 8542 A (the pluses in the diagram). Note the central intensity in X8542 is the same for Procyon as for the integrated solar flux, although the Procyon profile is of course broader. We also observed Aldebaran (K5 III) where the profile is actually quite similar to the solar core.

**Peytremann** - How did you put the Sun on a flux scale?

**Linsky** — We put the Sun on a flux scale by taking the observations at the center of the disc and at a few JU points and doing an integration. We also took into account continuum limb darkening. It is sort of a fictitious, quiet Sun as we've ignored plages, active regions, etc.

In Figure 11-29, if again we go to Griffin's Atlas for Arcturus and plot the five Call lines on the same scale with residual intensity on the ordinate

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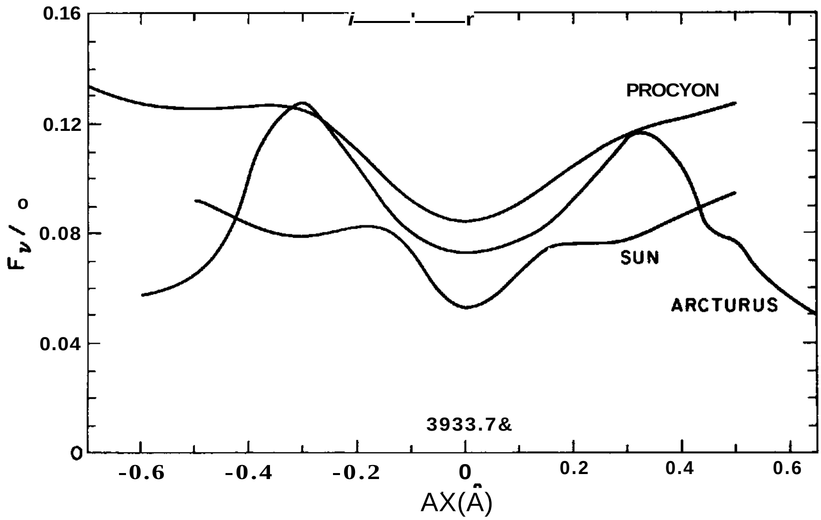


Figure 11-27

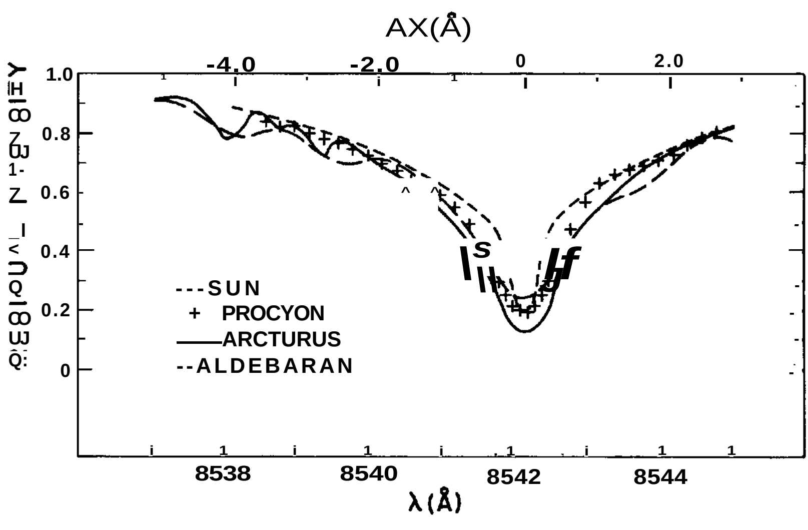


Figure H-28

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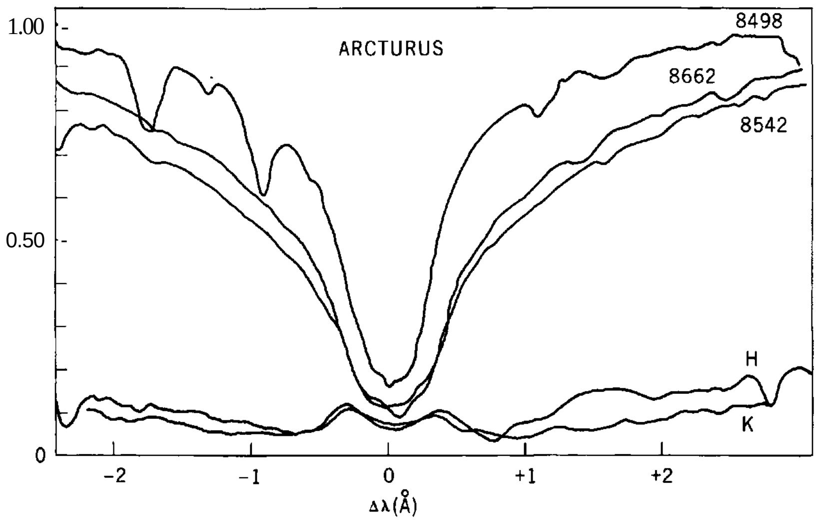


Figure 11-29

and a common AX scale on the abscissa, two things strike me as interesting. You'll recall that yesterday I showed observations of a very weak solar plage in the same five Call lines. There is quite a lot of similarity between that case and Arcturus. In a very weak plage in the Sun you get some emission in H and K (of course it is broader in Arcturus) and you get pure absorption lines in X8542 and X8662. In the weakest of the triplet lines, X8498, there is a hint of a central emission, in the plage. There is also a hint of an emission feature in X8498 in Arcturus, as taken from Griffin's Atlas. It may well be that X8498 is a very interesting line to look at in a range of stars, as an indicator of chromospheric emission.

**Athay** — Jeff, is it certain that X8498 does not have a blend in there?

**Linsky** — There are no known lines at the required wavelengths. It would have to be a very complicated blend, being the same in Arcturus as in plages, but absent in the quiet Sun.

In Figure 11-30 we have a low resolution scan of Aldebaran (K5 III), taken at Kitt Peak. The point here is that even at low resolution (20,000-30,000), you can see emission in the core of H and in the core of K. The emission is brighter in K than in H. The low resolution eliminates the $K_3$ feature.

In Figure 11-31 we have a low resolution scan of Sirius which shows that **Call H** and **K** exist in this star and that H is a small perturbation in the wing of He.

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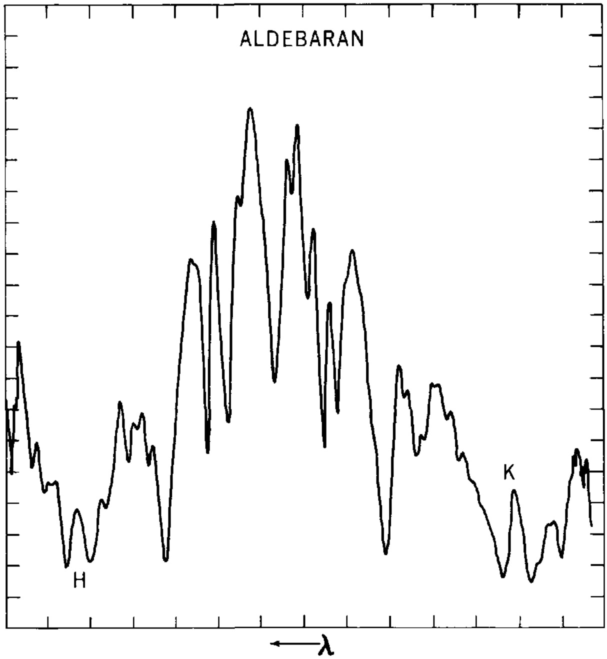


Figure H-30

Figure 11-32 shows a high resolution scan of Vega. This is really quite interesting. Here we are in the broad wing of $H_e$ with the wing decreasing in this direction. This is about 10-15 minutes worth of data taken while we were waiting for Procyon to rise. We didn't really expect to see very much in Vega, but it may well be that this feature seen on the red wing of the H line of Call is in fact an emission feature. This may indicate a chromosphere on a star as early as an AO dwarf.

**Praderie** — What is the wavelength scale?

**Linsky** — It is about 1.4 or 1.7 A for the full width. Before anyone takes this too seriously, I should mention the last figure, Figure 11-33. This illustrates the unfiltered data, for purposes of honesty This is the emission feature I was talking about. The data are very noisy and the observations should be done again. The emission hump does seem to be there in the unfiltered data and if you look then at the filtered data, perhaps the hump is there or perhaps it is not. I wouldn't stake my life on it. However, I wouldn't be surprised if Vega, which has already been mentioned as a star potentially with a chromosphere, indeed shows some emission in the Call H line.

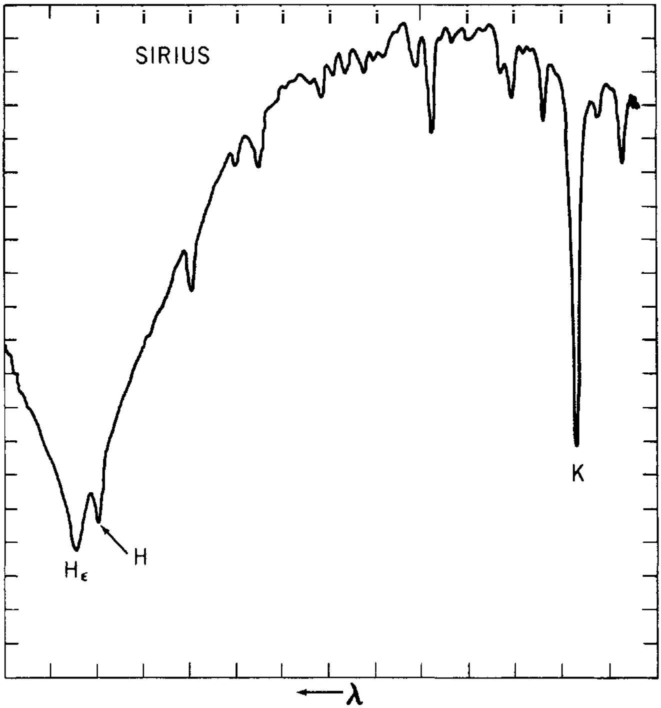


Figure 11-31

**Underhill** - Might the Call K line for Vega be double?

Linsky — We had intended to do both the H and K lines on Vega after we had seen data of this sort. However, it snows on Kitt Peak. We'll have to wait for our next observing program.

**Kuhi** — The next major topic covered by Francoise was also related to Call emission, namely the Wilson-Bappu effect. I have one question about this. It is always stated in the literature that a correlation exists between the absolute visual magnitudes and the width of the Call K emission. Has anybody looked to see if there is a correlation with absolute bolometric magnitude as well since the bolometric corrections are so small for these stars.

O. **Wilson** — I've never done that. I presume that there is a correlation, but it wouldn't be linear. I do not know what the correlation is. I've always used the visual because there the correlation is beautifully linear and therefore handy.

**Peytreraann** — I have some comments about the Wilson Bappu effect. Yesterday, Gene Avrett told you about some theoretical non-LTE com-

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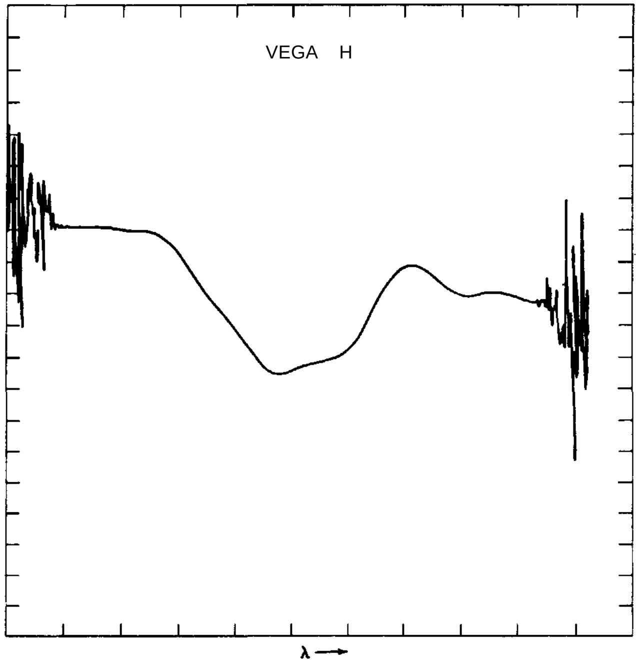


Figure H-32

putations we have done at Harvard on calcium line profiles. He showed some profiles which I will not show again now. Once we had these profiles, we tried to test them against one very well established observed effect, i.e., the Wilson-Bappu effect. The first question that arises is again one of definition, but this time it is a definition related to the observed quantity. The width of the Call K emission as defined by Wilson and Bappu (1957) is the difference in wavelength between what they call the violet edge and the red edge of the emission. If you have a theoretical profile you also need to define "an edge." On the top of Figure 11-34,1 show the red part of a calcium line with a flux scale on the ordinate and arbitrary wavelength units on the abscissa. I adopted three possible definitions of the width, which I call Wj, $W_2$, and $W_3$. Wj is the width at the minimum, $K_t$. $W_2$ is the width at half the flux difference between the maximum of the emission, $K_2$, and the minimum, Kj $W_3$ is the width at one quarter the height in flux units between the maximum and the minimum. This is important as will be seen in Figure 11-35.1 should add that if you measure the width on a photographic plate, even if you have the densitometry profile on the plate, you still are on a density scale. Even if you define the width on a density scale on the photographic plate, you still have to convert it back to flux units before

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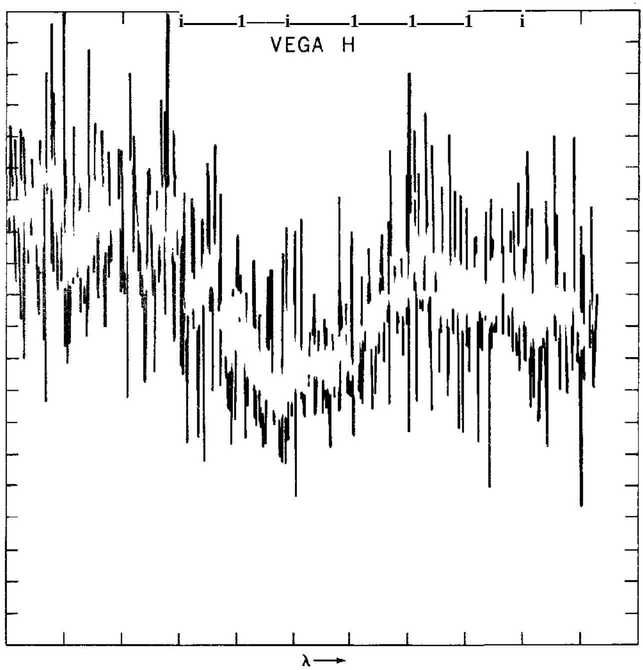


Figure 11-33

comparing it to theoretical calculations. Obviously, a densitometry profile is not going to look the same as a flux profile.

In Figure 11-35, I plotted absolute magnitudes as a function of the log of the half-width as defined by Wilson and Bappu. Before I discuss this graph, I have to say how we go from model atmosphere calculations to an absolute magnitude scale. The absolute magnitude is

$$\wedge = -10 \log_{10} T_{eff} + 2.5 \log_{10} g$$
$$-2.5 \log_{10} M + C_{bol} + \text{constant}$$

$M_y$ = absolute visual magnitude

$T_{eff}$ = effective temperature

g = surface gravity

M = stellar mass

$C_{bol}$ = bolometric correction

In model atmosphere computations I specify $T_{eff}$ and log g and also roughly the abundance — metal poor or metal rich. These three quantities

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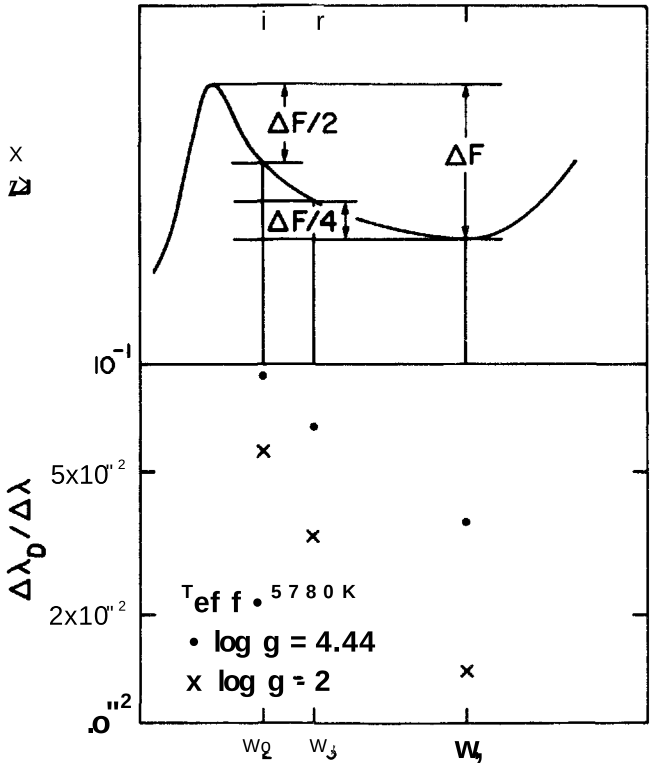


Figure 11-34

do not allow me to uniquely define the absolute magnitude, because I need the mass. I don't know anything about the mass in atmospheres that are roughly plane parallel. The bolometrie corrections can be taken from metal line blanketed models and in any event this correction is not too big in the range between $T_{eff}$ = 4000° and 6000° K. The main problem is how do we get the mass. We can start from evolutionary tracks in terms of gravity and $T_{eff}$; i.e., one looks at that star which at some point in its evoluation would have a specified $T_{eff}$ and log g. This star has a certain mass, which one uses to calculate *My*. Here we have to rely on evolutionary model calculations and that introduces another uncertainty. This solution is not necessarily unique because there can be a region in the HR diagram, corresponding to a $T_{eff}$ — log g combination through which stars of different masses can evolve. That is an uncertainty that can bring trouble.

We started with a solar model. We then calculated another model in which we just changed one parameter — i.e., the surface gravity — and left everything else as in the solar model. Avrett described yesterday how

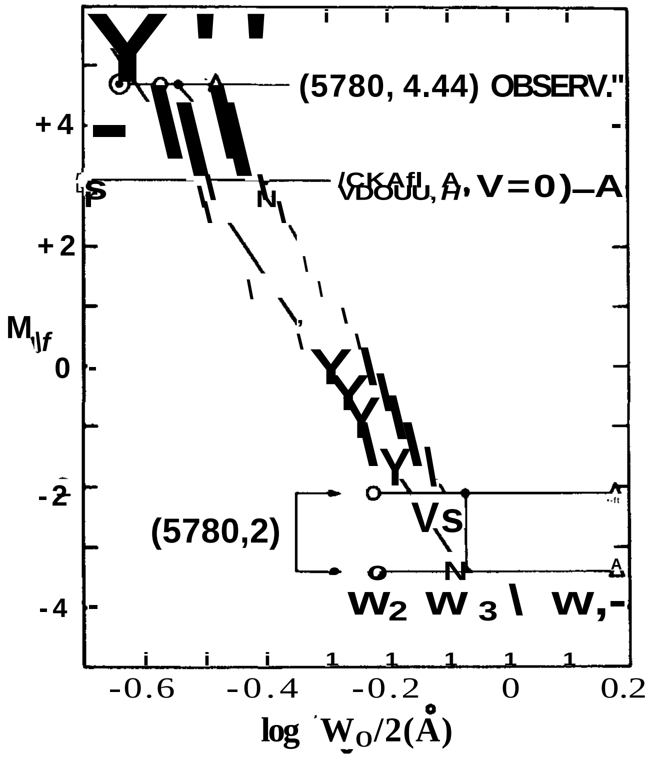


Figure 11-35

we re-scaled the temperature. There will be objections to the way we did this rescaling in order to have a chromospheric rise. For what we want to show, this is not an important problem. We just want a temperature rise in order to get an emission peak in calcium. It has been shown that the Wilson-Bappu effect is independent of the intensity of the emission peak. So whatever temperature gradient we take should give the right answer as far as the Wilson-Bappu effect is concerned. What we then have to prove is that it is also going to work for temperature gradients other than the ones we have adopted.

On this graph I show the Wilson-Bappu relationship as a solid line. The value for the sun given by Wilson and Bappu is indicated by ©. The open circle (o) corresponds to definition $W_2$,at half-height between $K_2$ and $K_2$ Wi (A) is at Kl. $W_3$ (•) is in between. The first thing that you can see is that the results one obtains depend significantly on which width definition one adopts. For the Sun the problem is not too bad, but for the giant (log g = 2) case, the definition adopted can change very significantly the results you get for the theoretical width. An extreme case is the

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model at $T_{eff}$ = 6500°K and log g = 4, where the emission peak is very narrow: (We have taken zero turbulent velocity in this case.) Then one has a very flat $K_t$ minimum. In such a case, one is in trouble because there is a tremendous difference depending on whether one adopts the definition W3 or W2, $W_x$ being obviously inappropriate.

In addition to the solid line, I have shown a dashed line which joins the points corresponding to definition $W_3$. In this case, the slope is roughly parallel to the observed effect, although there is a slight shift to the right. However, if one takes the giant ($T_{eff}$ = 5780°K, log g = 2) case, one sees that the calculated points between $W_2$ and $W_3$ bracket the observed relation.

For the model with $T_{eff}$ = 5780° log g = 2, and from evolutionary tracks (Iben, 1967) I derived a mass of 6M., which corresponds to $M_v$ = 3.4. In addition to this procedure I took a more direct route to get My. In a recent paper by Bohm-Vitense (1971), a star of luminosity class II has log g = 2. With this and the spectral type one can go to tables like the one of Schmidt-Kaler (1965), which then gives $M_v$ = -2.1 This gives two independent determinations of My. One sees that the observed width - luminosity relationship (solid line) is bracketed by the theoretical joints between definitions $W_2$ and $W_3$, and $M_v$ = 3.4 and -1.4 Within the uncertainties in the width definition and in the derived values of $M_v$, it would seem that we can explain the Wilson-Bappu relationship just in terms of an opacity effect. We did not put in any extra velocity fields. I do not say'that there are no velocity fields. But such fields may not be required to explain the Wilson-Bappu effect. These are very preliminary results which are presented here only because this meeting is supposed to be a working conference. Further calculations with various temperature-height relations are needed to confirm these first results, and to improve the shape of the emission peak. These investigations are currently under way.

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## CONTINUATION OF DISCUSSIONS FOLLOWING TALKS BY PRADERIE AND DOHERTY

**Kuhi** — Peytremann has given us a very interesting explanation of the Wilson-Bappu effect which did not require the velocity parameter suggested by others and relied entirely on the opacities. I wonder if there is any comment or discussion on this point.

**Rosendhal** — It should be pointed out that there is some observational evidence that velocity fields may have something to do with the Wilson-Bappu effect and other related phenomena. Referring to observational studies in the literature, in the case of the F stars, Osmer has empirically established that there is a correlation between the width of the infrared oxygen lines at 7774 and absolute magnitude. There is nearly a linear relationship for stars more luminous than absolute magnitude -2 or -3. He also finds that in this absolute magnitude range a change in the behavior of the turbulent velocity in the sense of an increase in the more luminous F stars, and that you can completely explain the dependence of the width of the infrared oxygen lines from the increase in turbulence in these stars. The second point which I think is important is that a couple of years ago a paper appeared by Bonsack and Culver who looked at the line widths and turbulence in the K stars. This was prompted by Kraft's observations of H# as an indicator of absolute magnitude through an analogous effect to the Wilson-Bappu effect. They also found that there was a correlation of turbulence as derived from the curve of growth with the width of H#. Therefore in two cases, namely that of the K stars and also the highly luminous F stars, there is some empirical evidence that velocities are relevant to the problem and that there is a relationship between the observed velocities and various types of luminosity indicators.

Peytremann — Many people who have tried to interpret the Wilson-Bappu effect in terms of velocities have thought that the widths represent velocity broadening in a direct sense and did not base their analyses on any sort of detailed model calculations to make sure that the broadening did not come about indirectly through some other intermediate mechanism. You mention Ha profiles, and I ask how you know that what may seem to be velocity broadened widths are *really* velocity effects.

**Rosendhal** — I didn't say Ha was broadened by velocities. I merely pointed out that the observed changes in the width of Ha are correlated with something which is associated directly with a velocity parameter, and that Ha exhibits a behavior analogous to the Wilson-Bappu effect.

**Kippenhahn** — The fact that a stellar atmosphere doesn't know about the mass but only about effective temperature and gravity has been a basic difficulty with the Wilson-Bappu effect. The situation is very similar in a quite different field in astrophysics, namely, in the explanation of the period -luminosity relationship of the Cepheids. There, as well as here, one needs information about the mass of the stars in a given region of the HR diagram, information which can only be obtained from evolution theory. Evolutionary tracks project the mass-luminosity relationship from the main sequence into the region of the evolved star, and, although there is some scatter, this procedure brought out the explanation of the mass luminosity relationship (Hofmeister, Kippenhahn, Weigert, 1964, Zeitschrift f. Astrophys. 60, 57; Hofmeister, 1967. Zeitschrift f. Astrophys., 65, 194). What Dr. Peytremann suggested this morning is very similar. When he assumed that for the red giant region stars of a given luminosity have a certain mass he assumed that there is a mass-luminosity relationship for evolved stars (which is not the classical mass-luminosity relationship for main sequence stars).

I wonder whether one would not get a similar phenomenon for the width-luminosity relationship as one encountered already for the period-luminosity relationship. In the case of Cepheids we know that stars which have undergone a different evolution like the W Vir stars (whose evolutionary history is still unknown) have a different mass-luminosity relationship when they cross the Cepheid strip and therefore have a different period-luminosity relationship. Similarly in the case of stars with Call emission: if another population of stars is observed in a certain area of the HR diagram they might have masses different from that of population I stars in the same region of the diagram. Should they not show a different Wilson-Bappu relationship? Can one look for this, or is the effect of different masses obscured by the effects due to different metal content?

**Athay** — There is, I think, an observational way of deciding whether the emission extends into the damping wings or is due to a velocity parameter. When Skumanich and I looked at the problem several years ago we found the same effects that Peytremann has described, but they implied that the line wing is producing the broadening, and that there is a correlation between the flux in the K emission and the width of the emission peak. If you increase the opacity in the chromosphere, that both broadens the peak and increases its flux, and I don't see how you can avoid that, at least for stars of the same age. Only if you deal with stars of different ages would you be able to destroy the correlation.

**Peytremann** — I agree that this correlation should not exist for stars of the same age, and, indeed, this point will be investigated.

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**Jefferies** — **I** think that in fact the answer may be with us already from some observations that were shown this morning. There are two things that determine the separation of the peaks used in the Wilson-Bappu relationship.

One is the Doppler width and the other is the optical thickness of the chromosphere. We should be able to differentiate between these two by using profiles of the H and K lines of ionized calcium and magnesium. Since these will have the same Doppler (velocity)widths, while the optical depths of the chromosphere in the two sets of lines will differ in proportion to the relative abundances, I think, therefore, that one should be able to determine the major contributor to the width from using a little theory and making a comparison of Wilson-Bappu relationships for the calcium and magnesium lines.

**Kuhi** — The Mg **II** relationship does seem to have a flatter slope but is based on only a few points.

**Linski** — An interesting result comes from looking at solar plages concerning the Wilson-Bappu relationship. Consider the relation between the K line width, determined say at the half intensity point between $K_2$ and Kj and the activity of the plage, both the width and intensity increase. From a weak plage to a strong plage, the width does not increase while $K_2$ does increase. I think the physical explanation of why this happens in the Sun would be of great importance in understanding the Wilson-Bappu effect.

Wilson — I would like to ask Jefferies a question about the Ca and Mg Magnitude — width relationship he discussed. If you look at two stars with the same luminosity but a different calcium abundance, presumably, you won't get the same results.

Jefferies — I can't offhand answer the question of what happens with different abundances, particularly with a different Ca to Mg abundance ratio from star to star.

Wilson — If you have one group of stars with a solar Ca abundance and another series of stars with, say, only one fifth that much Ca, would you expect to get two different magnitude-width relationships?

Jefferies — To the extent that the position of the bottom of the chromosphere isn't dependent on the Ca abundance that may be the case. Such a result may seem implausible, but so is the Wilson-Bappu relationship.

Wilson — There are many comments in the literature, as you know, about possible abundance effects but I think the evidence against such explanations is quite strong. I will have more to say on this in my talk at the end of the meeting.

**Pasachoff** - **I** have suggested in an Astrophysical Journal paper **(164,** 385, 1971) one more thing that helps explain the Wilson-Bappu effect in the Sun. The Sun is, after all, the star in which one can study how the actual line profile that we measure is constructed. If we look at Figure 11-36, we

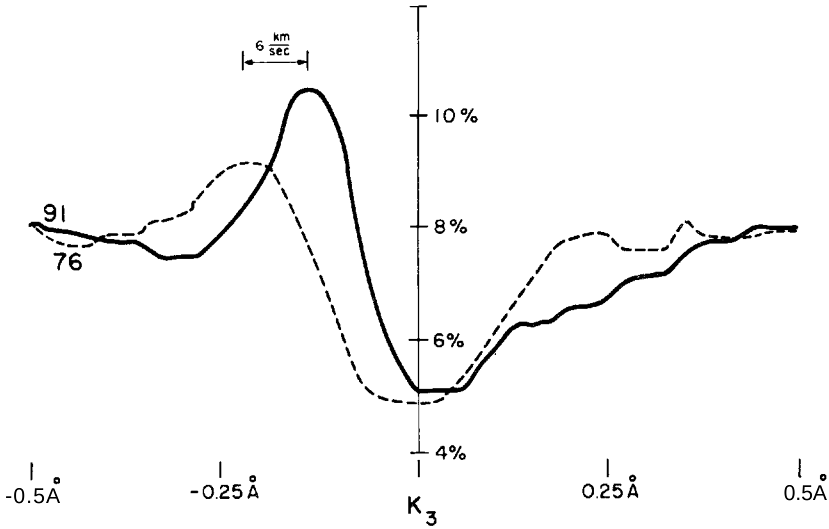


Figure 11-36

see profiles of two fine structure elements located about a second of arc apart from each other. One can see that each profile for the K line is very different from the profile for a neighboring element. These are what the supposedly symmetric double-peaked profiles look like under high spatial resolution. The $K_2$ peaks on the violet side of these two profiles appear in two rather different locations, a few hundredths of an Angstrom from each other. The statistics of how these peaks vary show that there is a contribution of several km/sec to the line width of the Sun. Similar contributions must also arise in the other stars we see.

**Magnan** — I think that the turbulent velocity is only a parameter that is put into the calculation for convenience. I think that the best indication for velocity fields comes from the asymmetry of the line. I think it is important to account for different intensities in the red and blue wings.

**Kandel** — I think that from the diagram that Pasachoff showed, the velocity differences in the separate cases would be assigned to macro-turbulence.

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**Pasachoff** - We all agree that the reason the averaged peaks have their observed separation and are asymmetric is still controversial. The simple models that just have Doppler shifts one way and the other can certainly be challenged on many grounds. While the peak displacements can be represented on a velocity scale, it is not necessarily the case that there are elements moving at these velocities.

**Jennings** — Praderie mentioned in her talk a correlation which has been published concerning calcium H and K emission, infrared excesses and polarization. Since the initial report quite a bit of work has been done on this at Kitt Peak and we have some results which differ from those which are in print. We have considered H and K, hydrogen, Fe II and other emission lines in late type giants and supergiants and we find the following results. If we plot the mean change in polarization vs. the ratio of intensity in the K line to that in the continuum we find that the stars break very neatly into two groups. Those that are intrinsically polarized show no Ca emission detectable at Wilson-Bappu intensity class II or greater. On the other hand, stars which do not show intrinsic polarization do show very strong Ca reversals. There is one star which tends to bridge this gap, *a* Ori. This star shows very weak polarization, and, as you know, Ca reversals.

Further, Dyck and others have discussed a correlation between polarization and infrared excesses, so we can also add infrared excesses to the graph. Combining these two pieces of data we interpret this to mean that those stars which are surrounded by circumstellar material do not tend to show Ca H and K emission. We have also looked at other emission lines, notably Fe II and we find that the result holds for these lines; i.e., stars with infrared excesses and intrinsic polarization do not show Fe II in emission. The only star which does is a Ori. But again this is a case having very weak polarization, and very weak infrared excesses. It should be noted that this particular relationship conflicts with that originally mentioned by Geisel who suggested that the presence of Fe II is accompanied by infrared excesses. We find this not to be the case. It is also interesting to note that among the stars which do not show polarization none currently show hydrogen emission nor have we been able to find any reference in the literature to hydrogen emission among these objects. On the other hand, approximately 50% of those stars which are high polarization objects have shown or are showing hydrogen emission. Also, this is the strange type of emission which is shown by Mira variables, i.e., having a distinctly anomalous decrement. Two cases of this type presently in emission are Z Ursa Majoris and RX Boo where we find that Ha and H/3 are missing, HY is weakly in emission, H5 strongly in emission, He is missing, and H8 through H10 are weakly in emission. The explanation for this seems to be that these lines arise far down in the

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photosphere, and are affected by strong overlying absorption. This is the current status of the emission line vs. grain indicator correlation.

**Kuhi** — In defense of Susan Geisel's comments, I think that in her paper she certainly did not mean to imply that 100% of stars that showed Fe II emission had infrared excesses. I think her batting average was around 80%.

**Jennings** — Among the late type stars the correlation seems to be exactly the opposite. If you find Fe II emission you do not find infrared excess.

**Pecker** — Your measurements all refer to rather cool stars, those showing the K line, and Susan Geisel picked primarily Be stars. For Be stars do you still find the correlation between Fe II emission and the absence of infrared excess?

**Jennings** — I meant only to say that Susan Geisels correlation is reversed in the case of late type stars. Her correlation seems perfectly valid for stars of early spectral type.

**Boesgaard** — What data do you have for the Fe II emission lines for late-type stars and how many stars did you observe?

**Jennings** — Of thirty stars or so, seven or eight showed strong polarization and for these we found no iron emission and none seems to be reported in the literature. Fe II emission is fairly common among those stars which don't show polarization.

Leash — We do not seem to have directly observable indicators of the chromospheres in early type stars. I wonder if Praderie has any opinion on whether the lines of Si II at 4128 A and 4130 A might be a good indicator of chromospheres in B stars?

**Praderie** — I have tried to determine the dominant terms in the source function for the Si II resonance multiplet at 1808, 1817 A in A and B stars. The source function is collision dominated. I don't know the situation for Si II 4128 A and 4130 A, and have not considered B stars.

**Heap** — I would like to suggest the O stars as candidates for having chromospheres on the basis of observations by Slettebak in the 1950's. Slettebak measured the broadening of lines in O-type spectra and found that there Was no O star whose spectrum shows lines sharper than about 75 km sec. His sample was large enough that he should have been seeing some of these stars pole-on. He concluded that there was some intrinsic velocity broadening, eg. turbulence, present in early O stars. Also, Aller's plates of planetary nuclei having O or Of-type spectra show at least 75 km/sec broadening. Hence, there are no O stars, young or old, that have sharp lines. This is a serious problem because of velocity of 75 km/sec is about twice the speed of sound in the atmospheres of hot stars.

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Underhill — For the O stars there is no difficulty in explaining the hydrogen line widths at least, but you are correct in stating that sharp lines are not seen in O star spectra.

Kuhi - Also we must consider the problem of radiation pressure in these very hot stars, which may be very efficient in forcing material away from the star. This could prevent the formation of a chromosphere.

Boesgaard — I wish to report on the ultraviolet Fe II emission line in a Orionis. It is perhaps too bad to leave the Ca II emission line which is the one thing everyone seems to agree on that indicates the presence of a chromosphere. Inasmuch as Francoise Praderie implied that the Fe II emission lines may be formed in a circumstellar shell, when I talk about these Fe II lines I should adopt Olin Wilson's feeling about a chromosphere: one woman's chromosphere may be another woman's extended atmosphere. In any case a Ori offers ample proof of both a chromosphere and an extended envelope. It does show the calcium emission and it certainly shows blue-shifted circumstellar cores in zero-volt absorption lines. These Fe lines were first discovered in 1948 by Herzberg (Ap. J. 107,94). There are about 17 observable lines from multiplets 1, 6, and 7 of Fe II. These lines occur in the region 3150 A to 3300 A which makes it very difficult to look for them in cool stars since they radiate so little energy that far in the ultraviolet. About the best candidates are *a* Sco and a Ori and even these require long exposure times for high dispersion studies. Bidelman and Pyper (1963 P.A.SP 75, 389) looked at something like 6 M stars, one MS star and a carbon star for these lines.

Figure U-37 shows an ultraviolet spectrum of a Ori at 3.3 A/mm taken at the Mauna Kea Observatory 225-cm telescope. The iron emission lines are indicated there. Of those 17 lines about 7 are so badly mutilated by some kind of overlying absorption that little can be learned from them. (A figure in Doherty's talk showed profiles of two Fe II lines: one with a strong self-reversal and a second line which has a high laboratory intensity but which is too mutilated to give any radial .velocity information.) The feature at 3 228 A looks like a double emission line but is actually a strong emission line with a central absorption reversal. The line at 3277 A is an example of a strong emission line with a weak self-reversal. The lines in the region around 3167 A are among the weakest lines with no reversals.

I measured the radial velocities on four separate spectrograms taken over a period of a year from November 1970 to December 1971. The absorption lines give a radial velocity for the photosphere, *a* Ori is known to have a variable radial velocity as the photosphere seems to be pulsating. The velocity there is about 21-22 km/sec and shows a range of about 4 km/sec. Measurements were made to determine radial velocities of the absorption lines, the emission lines, and the self reversals; the

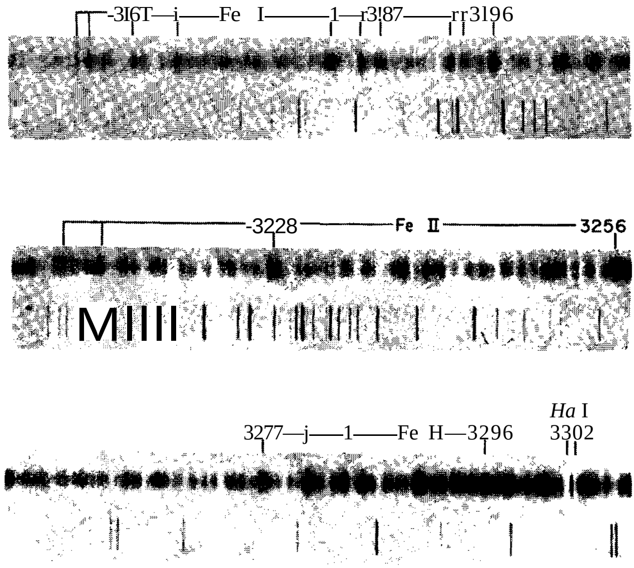


**Figure 11-37**

results are shown in Figure 11-38. The first panel shows radial velocity measurements and probable errors on 4 plates for the absorption lines. This variation is what is expected for *a* Ori for the photosphere; it shows a range of a total of 4 km/sec. The average velocity is about 22 km/sec. The next part of Figure 11-38 shows the velocities for the emission lines. The large dots at the top are from the seven strongest emission lines in the spectrum; this looks like the velocity is constant for those emission lines. The region where the emission lines are formed does not take part in the photospheric variations. The four small dots below that are the radial velocities of three weak lines. The probable errors are similar to those for the strong emission lines but are not shown for the sake of clarity. The third part in Figure 11-38 shows the positions of the reversals.

Now if you have looked at the scale on the left you may be perturbed by the fact that these emission lines show a red-shift. That usually indicates *infalling* material. If the chromosphere or envelope is expanding, I find it difficult to understand such a shift, but Grant Athay has assured me that it is possible, even in an expanding atmosphere, to get red-shifted emission lines. If you look at the average of these velocities, the emission lines are red-shifted by about 5 km/sec relative to the photospheric lines. The reversals, except in the one case of KE-33, are slightly blue-shifted within the emission features. That we can understand as cooler material farther out in this expanding atmosphere. So the reversals are about 3

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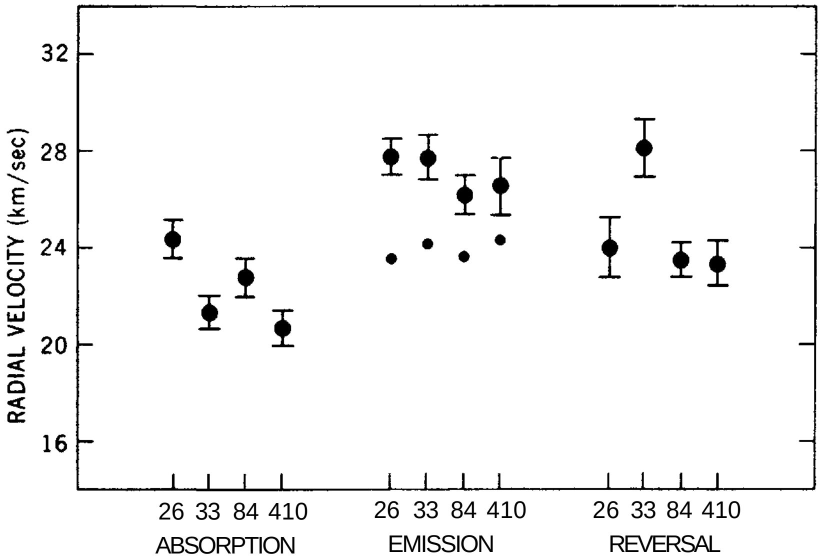


Figure 11-38

km/sec to the red of the photospheric lines or about 2 km/sec to the blue of the emission lines. Incidentally, in the same star Olin Wilson long ago measured the velocities for the calcium emission and the K2 features are also shifted to the red by 4 km/sec.

Determinations of relative intensities, half-widths, and intensities of the reversals have also been made. For Figure 11-39 I have averaged emission intensities that are eye estimates on the four plates that I have and plotted them against half-width, that is, width at half intensity. There is a linear correlation between the intensity and the breadth of the line. The scale shown on the right in the figure is in km/sec; the weakest lines are about 20 km/sec in width and the strongest line has a width of about 85 km/sec. Figure 1140 shows the relationship with the reversal intensities. Not all the lines are self-reversed; those are the weak ones and the reversal intensity is zero. The medium intensity lines have medium reversals and the strong line at 3228 A has a very strong central reversal. This figure is again the *average* intensity from the 4 spectrograms. There are plate-to-plate variations so reversal intensities for medium-strength lines range between 1 and 3, but none are ever called 4. For individual spectra these diagrams show linear correlations without the discontinuities seen in this averaged diagram. If we look again at the km/sec scale for the widths, the unreversed lines have an average width of about 30 km/sec. The middle ones have widths of about 60 km/sec and there is the one strong one at

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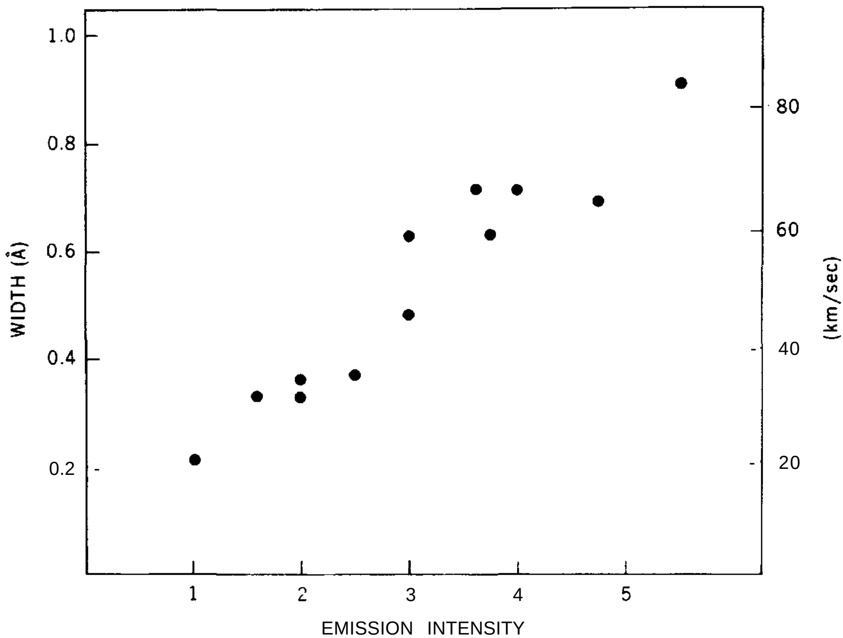


Figure 11-39

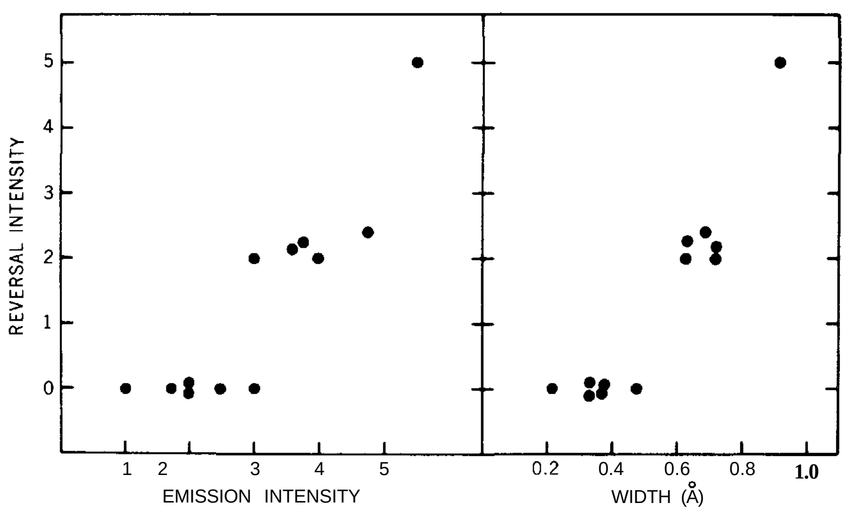


Figure II-40

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85 km/sec. The width, $W_o$,. measured by Wilson for the ionized Calcium line is 170 km/sec.

Figure 1141 depicts profiles of some of the lines. The first one, 3166.7 A is an example of a weak line; 3196.1 is one of the medium strength lines

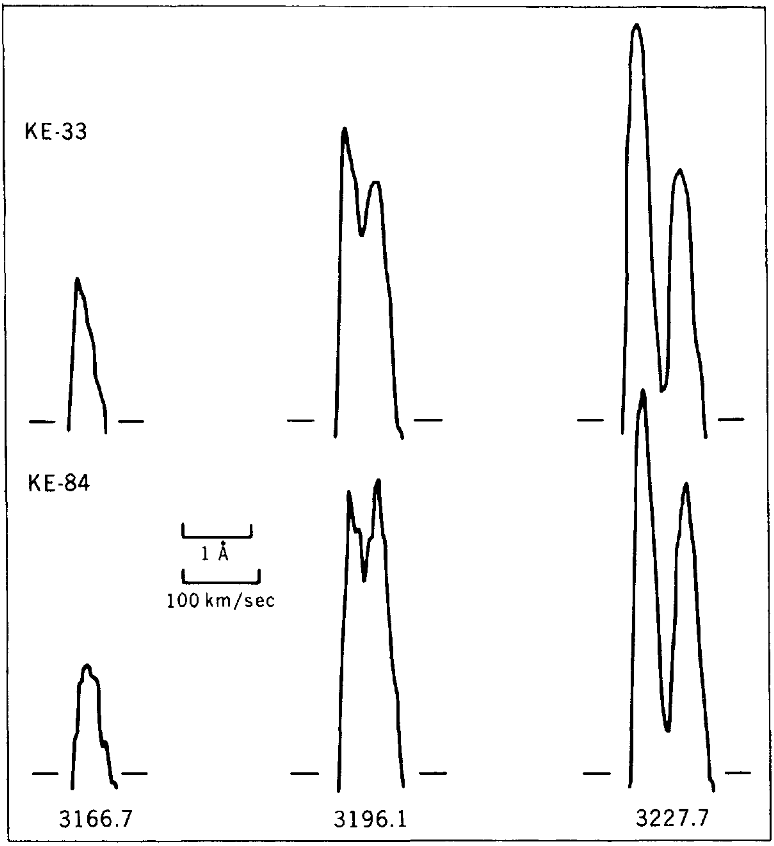


Figure 11-41

with a self-reversal. Also shown is a strong line, 3227.7 A, with a strong self-reversal. The upper and lower set of profiles are from two different plates taken several months apart. For the self-reversed lines on KE-33 the blue^ peaks are stronger than the red peaks, and the weak line is asymmetric. There is some variation with time in the exact structure of the iron emission lines in this star. All the lines in KE-84 seem more symmetric like the examples in Figure 1141.

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The time variation for the Ca line structure is shown in Figure 1142. The solid line is from KE-33 taken on November 14, 1970, while the dotted

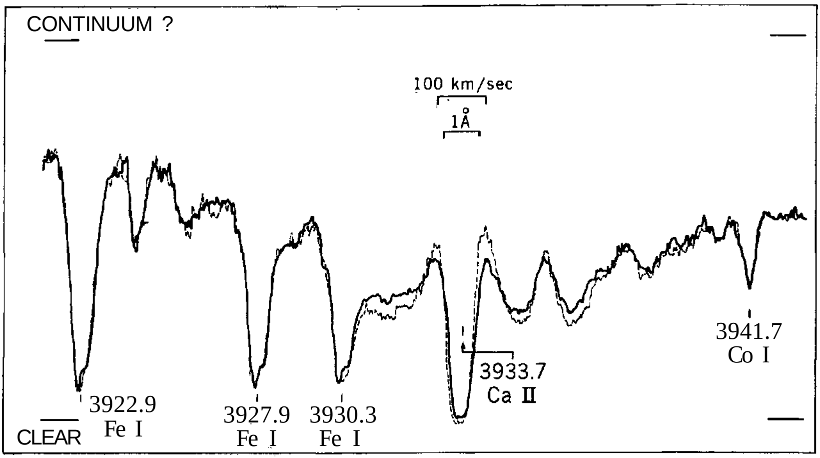


Figure 11-42

line is from KE-410 on December 6, 1971. There is a very broad, shallow $K_x$ and $H_t$ in this star. A continuum point about 17 A to the ultraviolet from this line is indicated at the top of the figure; the actual continuum is probably higher. The line-center position shows that the $K_3$ core is slightly blue-shifted. The $K_2$ emission peaks show on either side of $K_3$. Wilson's data give a slight red-shift for the emission peaks, +4 km/sec. You can see that there are some variations in the Ca intensities and in the $K_2$ blue-to-red relative strengths. For KE-410 note that the *red* peak is stronger than the blue.

There is a large amount of information available about chromospheres in the iron lines. There are many lines for one thing, at least 10 that are particularly useful and about 17 that give some information. For a Ori the photospheric lines show the expected velocity variation while the constant-velocity Fe II emission lines are red-shifted by about 5 km/sec for the average of the strong lines. The weak emission lines are about +1.5 km/sec and the reversals are about 2.5 km/sec to the red of the photospheric lines. The red-shifts are small relative to the line widths. The widths for the weak emission lines are 30 km/sec and that corresponds to an average red-shift of 1.6 km/sec. The stronger lines range from 60 to 85 km/sec in width which corresponds to a greater red-shift of 5 km/sec. The red-shifted Ca II $K_2$ lines have an even greater width of 170 km/sec. The

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line widths correlate with emission intensities and with the strengths of the self-reversals. Another interesting aspect is the time variations that are present in both the calcium and iron lines.

I would greatly appreciate a theoretical explanation of the observed red shift.

**Magnan** — I would like to describe the profile I call "standard" for an expanding atmosphere. This profile is characterized by a blue-shift of the core and an enhancement of the red emission. The effects are reversed in the case of a contraction. These features are a consequence of a differential Doppler shift along the path of the photon. This shift is due either to a differential expansion in plane-parallel layers or to the curvature of the layers in the case of a constant velocity of expansion.

**Underhill** — If you take the hydrogen lines in a Be star, invariably the strongest Balmer lines are red shifted with respect to the others, and the velocity of expansion is something of the order of 50-80 km/sec not enough for escape. It is usually stated that Ha is coming from a region of smaller outward velocity. The Fe II lines observations might be explained in a similar way.

**Wright** - This diagram (Figure 1143) is probably the best example we have which shows satellite absorption lines of the K line obtained during the chromospheric phases, prior to first contact, in the spectrum of 31 Cygni. This series was taken at the time of the 1961 eclipse; we hope to obtain another series this summer, chiefly at egress in July. At the beginning of the series, the B spectrum fills most of the K line of the K spectrum and the Kj and $K_2$ emission features can be seen. The central chromospheric absorption, in general, becomes gradually stronger as eclipse approaches. A major feature is the appearance of additional satellite lines which come and go. Perhaps the most interesting is the one shown on August 7-28 which showed in nearly the same position for three successive nights when the projected distance of the B star was more than two stellar diameters from the limb of the K star. The feature disappeared but another one appeared again in September and similar effects could be seen right up to eclipse, though after first contact the normal broad absorption of the Ca II K line of the K-type star dominates the spectrum. Similar effects have been noted at eclipses of 32 Cygni and £ Aurigae; at times I have suspected three or four satellite lines though they are usually weak and sometimes difficult to distinguish from the grains of the photographic plate. The explanation in terms of one or more clouds or prominences in the outer atmosphere of the K star, moving at different velocities, which absorb the light of the small hot B star, still seems to me to be reasonable. These observations seem to confirm to a

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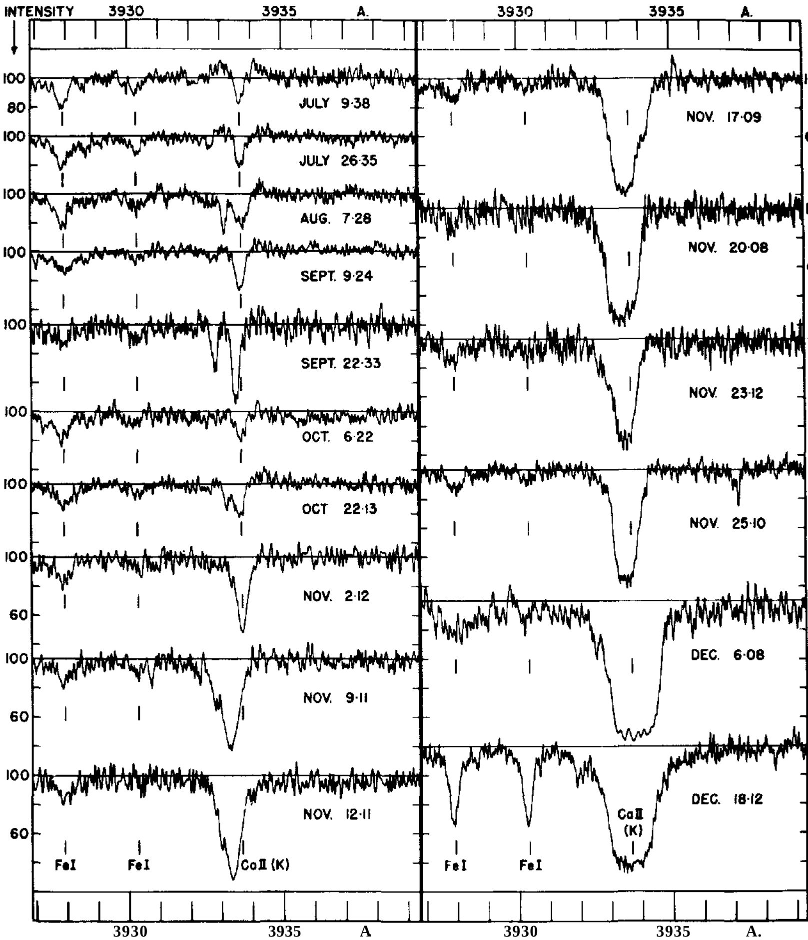


Figure 114 3

certain extent the type of phenomena about which Anne Underhill was speaking.

**Boesgaard** — One point is that these are small shifts compared to the halfwidths. So in fact, there may be more blue-shifted *photons* since the shift is only 5 km/sec while the half-width of the line is 60-80 km/sec and the line-profiles are asymmetric.

**Kuhi** — One of the problems that has been mentioned is that of mass outflow from the star and its detection by specific line profiles, asymmetric lines, P-Cygni profiles, anomalous line widths, etc. Is there discussion on this aspect of the problem?

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**Kandel** — Are we talking about mass flux as an essential part of a chromosphere or only about mass motions of some sort, i.e., velocity fields, as being a chromospheric phenomenon? These are two different questions.

**Kuhi** - In Praderie's talk she specifically avoided discussion of mass loss and I think we would like to do that as well since that gets into the problem of extended envelopes and other questions. I should mention Roger Ulrich's defining point this morning, which he didn't get the chance to make, that maybe the outer boundary of a chromosphere is the point at which the material is no longer gravitationally bound to the star. This would eliminate from the discussion all very extended envelopes.

Cassinelli — I would like to point out that it is not necessary to have mechanical energy deposition to have supersonic mass loss. John Castor and I have recently calculated expanding model atmospheres for early type stars. The atmospheres approach the usual static behavior at the base and have supersonic expansion farther out. The only form of energy deposition required for the flow is absorption of radiation.

**Pecker** — We have been speaking of extended atmospheres and the chromospheres of other women — it seems to me that the point is that an extended atmosphere is defined by its departure from hydrostatic equilibrium so that what is necessary for making an extended atmosphere is to have an additional *momentum* input, while the chromosphere is distinguished from the photosphere by having an additional *energy* input.

**Kuhi** - Okay, I guess I'll buy that.

**Conti** — There are many Of stars for which the X4686 of He II line (3-4 transition) is seen in emission and it has always been a mystery why this is so. In at least one star, f Pup, the rocket UV observations by Stecher also show the line He II (2-3 transition) at XI620 in emission. And now there have been some observations of the infrared line XI0124 of He II (4-5 transition) of that same star by Mihalas and Lockwood, and that line is also in emission. We have however, the He II Pickering series (4-M transitions) in absorption in this star. So some mechanism is over populating the ion up to level 5 and then causing cascading down through the other levels. According to the recent models of Auer and Mihalas, they were unable to get the X4686 line into emission and' they were certainly unable to get XI0124 in emission in any kind *of plane-parallel* model. So it seems very clear that at least for f Pup and presumably for all of the Of stars in which you see X4686 in emission, you must have some sort of extended envelope. If there was a planet from which some f Puppians were watching their Sun, and there was a solar eclipse by an

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appropriate moon, they would certainly see *chromospheric* emission lines in He II, but that's an aside. The main point I want to make is that when you see A4686 in emission, there is some sort of extended envelope around the star.

The star I want to talk about now is $6_X$ Ori C. As some of you may know, this is the central star of the Trapezium and the star that excites the Orion nebula. I have some spectra to show of this star. Figure 1144 shows

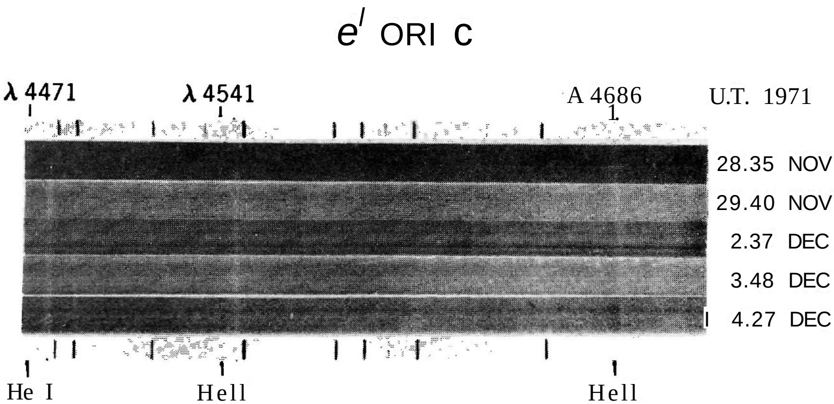


Figure 11-44

the spectral region of X4471 and 4541 of He II. These show just as absorption lines on these spectra, taken on five nights during one week. Note the appearance of X4686, the first two nights. Then a couple of nights later we see an emission at X4686. The emission is violet displaced and the absorption is red displaced, and we call this an inverse P Cygni profile. As most of you know, a P Cygni profile is one which suggests that material is flowing out from the star. Therefore an inverse P Cygni profile suggests the opposite. Figure 1145 shows the profile of X4686 on the first two nights. The absorption line is undisplaced with respect to the other absorption lines and then after three nights we see the emission on the violet and the absorption on the red side. What this suggests on the face of it is that there is material which is falling into $6_t$ Ori C. Sometimes material is accreting and other times it isn't. That is an interesting phenomenon for a star that has excited a gigantic nebula which is apparent to the naked eye. There are a number of physical problems connected with that process, and I think the line formation problem is the presence of accretion is in itself an interesting problem for astrophysics. The terminal velocity for material falling in is about 1100 km/sec and the infall velocity, roughly given by the absorption profile, is

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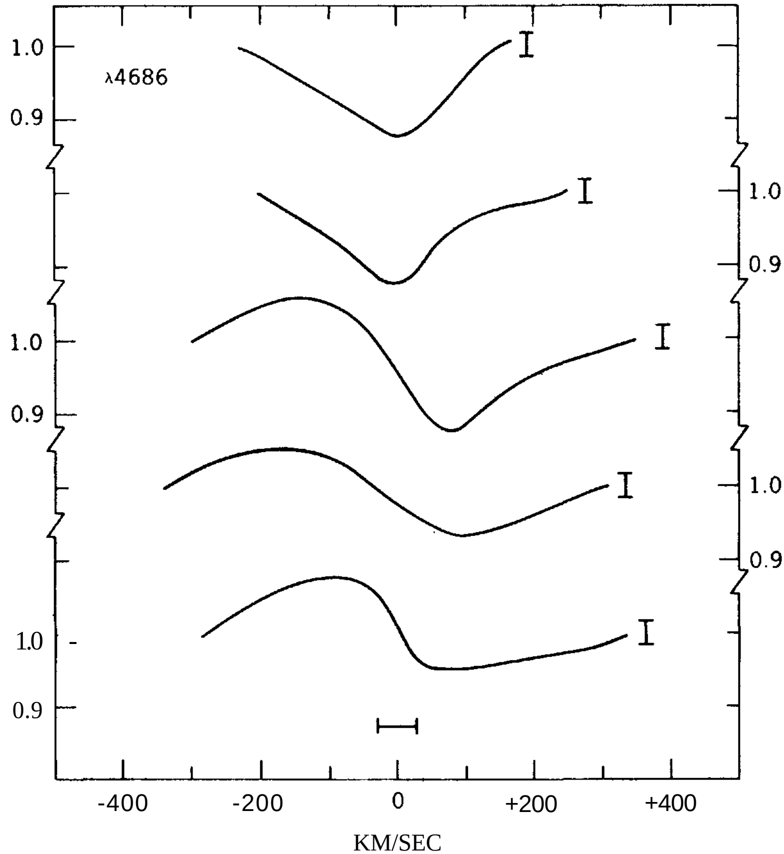


Figure H-45

something like 10 or 20% of that. So it isn't coming in with full force; presumably radiation is braking the fall, but it is definitely accretion. That should lead to some interesting problems of interpretation.

Wilson — Some of those fljOrionis stars are binaries, are they not?

**Conti** — This star is listed as a spectroscopic binary Upon searching the literature, you find out it is called a spectroscopic binary by Frost, et al. They give it this identification on the basis of "large" velocity variations, back in the 20's. Then you find that the senior spectroscopist, Struve, (and Titus in 1944 also studied this system) could find no velocity variation that could be blamed on binary motion. The plates I have, which are these five and another eight or so, all show no velocity variations.

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**Wilson** — That might be why the famous Wolf-Rayet star that was an eclipsing binary stopped.

**Conti** — Once a binary always a binary

**Kuhi** — Yes, but it stopped eclipsing.

**Conti** — But it didn't stop being a binary.

**Pasachoff** — Let me show you some observations we've been making with the 100-inch telescope on Mt. Wilson, using the 32-inch camera of the Coude spectrograph at 6.67 A/mm. Wilson and Ali (P.A.SP. 68, 149, 1956) observed the helium $D_3$ line a few years ago and reported a probable detection of $D_3$ in four stars, namely e Eridani, 61 Cygni A, *K* Ceti and X Andromedae. The first three are dwarfs and X Andromedae is a spectroscopic binary with a strong chromosphere. However, they were able to measure the position of the supposed $D_3$ line for three of the stars, and found that they were displaced some 0.4 A or so to the red. Since this region is confused by the presence of some water vapor lines all around $D_3$ (at X5875.44, 5875.60, and 5876.12, for example, with $D_3$ at X5875.64 right in the middle) the evidence was still incomplete.

Since that time, Vaughan and Zirin (Ap. J. **152,** 123, 1968) have published a paper with image tube observations of XI0830. Thus we now know for a variety of stars what the velocities may be. In fact, a dominant red shift effect does not appear. Some stars do show such velocities, but they are not always in the same sense. Figure 11-46 shows the triplet energy diagram. The XI0830 line comes from the metastable 2s triplet state and the X5876 triplet goes from the 2p state, 1.14 eV higher, up to the 3d state.

We have observed a variety of stars, a few A and B stars but mostly G, K and M stars. Just as Wilson could not specify results for the M stars because there were too many lines in this region to know whether what is seen is $D_3$ or not, we also had to limit ourselves to the G and K stars. But we do not know which stars have strong XI0830. |3 Draconis, for example, a G2 II star, has 1000 milliangstroms of XI0830 according to Vaughan and Zirin. Zirin has some more recent, unpublished observations showing twice that equivalent width. Looking at the $D_3$ wavelength on Figure II47, you see that there is no line there. For j3 Scuti, similarly, there is no $D_3$ radiation. For X Andromedae it is a little trickier. There is an iron line about an Angstrom to the red, which could broaden the total profile, but there is probably a line near the basic $D_3$ frequency. X Andromedae has 1000 milliangstroms or so of XI0830 and it is, of course, a spectroscopic binary. There is even a hint on one plate of possible emission around $D_3$, though that certainly remains to be confirmed.

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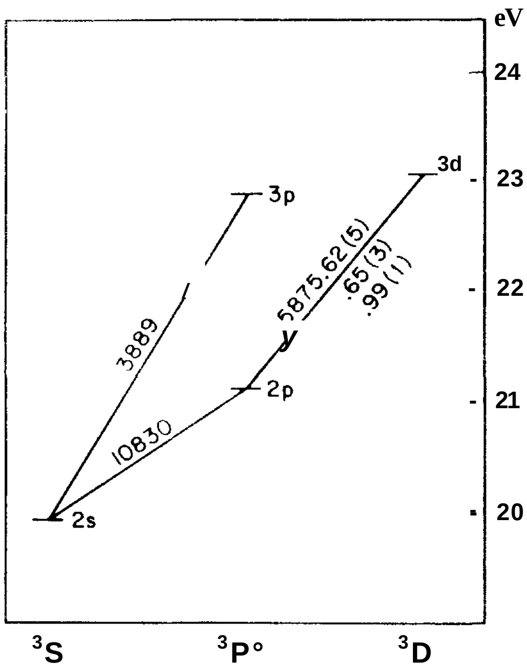


Figure 11-46

X Cygni, a K5 lb star, does not show clear $D_3$ in that region. We have to follow these stars at different times of the year to use the different radial velocities to separate out the atmospheric contamination. We are continuing this project.

Figure 1148 shows some of the spectra. First of all, for j3 Orionis at the top, there is a strong $D_3$ line. It is not "chromospheric," according to my definition of a chromosphere, for this is a B star and I would tend to call it a hot atmosphere. The other stars do not show this line, except for X Andromedae, which does show a possible faint line and even possible emission on this plate. However, 0 Draconis, a G2 \\ star, may have twice the XI0830 of X Andromedae yet it does not show $D_3$ absorption, certainly not of the magnitude of X Andromedae.

So what I would really like are comments on theoretical calculations of the relative intensities one expects for XI0830 and $D_3$. You might expect $D_3$ lines to be down by a factor of perhaps 10, calculating with a dilution factor of 2 for an atmosphere of about 6000° K. The ratio will change as we go to cooler atmospheres, but it would be nice to have some more exact model calculations from all the people calculating grids. We would also be happy to have suggestions for additional stars to observe in our continuing observing program. Elliot Lepler, a graduate student at Caltech, has cooperated in this work.

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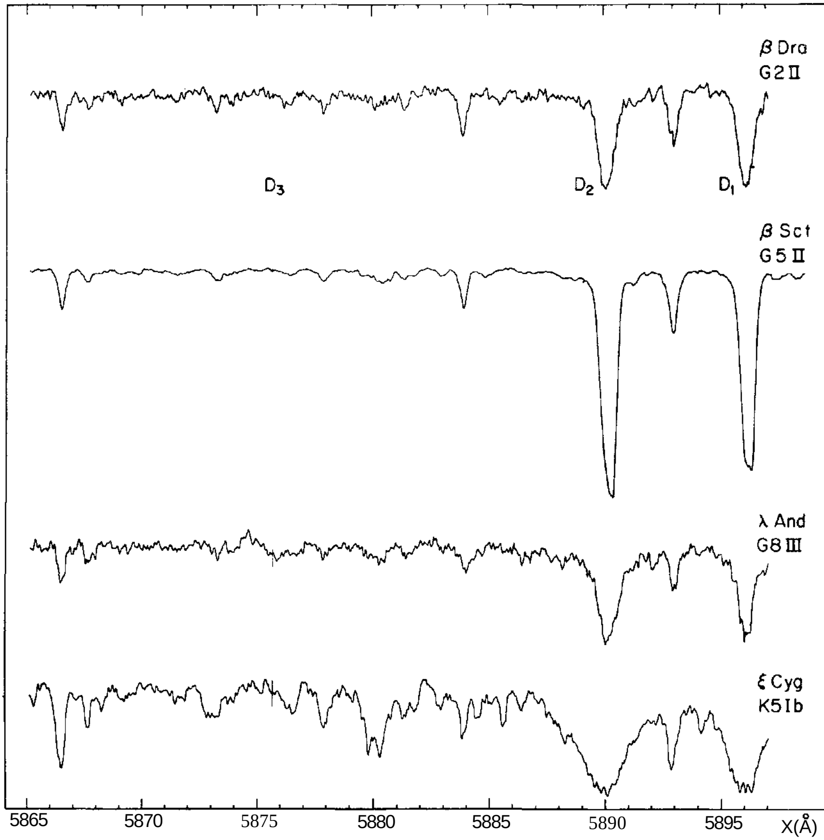


**Figure 11-47**

**Fosbury** — In Vaughan and Zirins' original paper they suggested that you are more likely to see 5876 in the slightly earlier type stars, i.e., the F stars rather than G. This is the case of Zeta Doradus again.

Pasachoff - Vaughan and Zirin did comment that they found XI0830 in one F star which surprised them. I have not observed F stars yet for $D_3$.

**Underhill** — If you are looking for chromospheres in the F, G, and K stars, certainly in the low chromosphere, where there are reasonable densities, the most prominent ions are singly ionized metals and some of the neutrals. We already found out that non-LTE applies here because the density is a bit too low for LTE to apply Most of the resonance lines that we would want to look at are located in the ground-based region of the spectrum. Doherty showed us that it is very difficult to get an observable flux in the UV but there are really not too many low-

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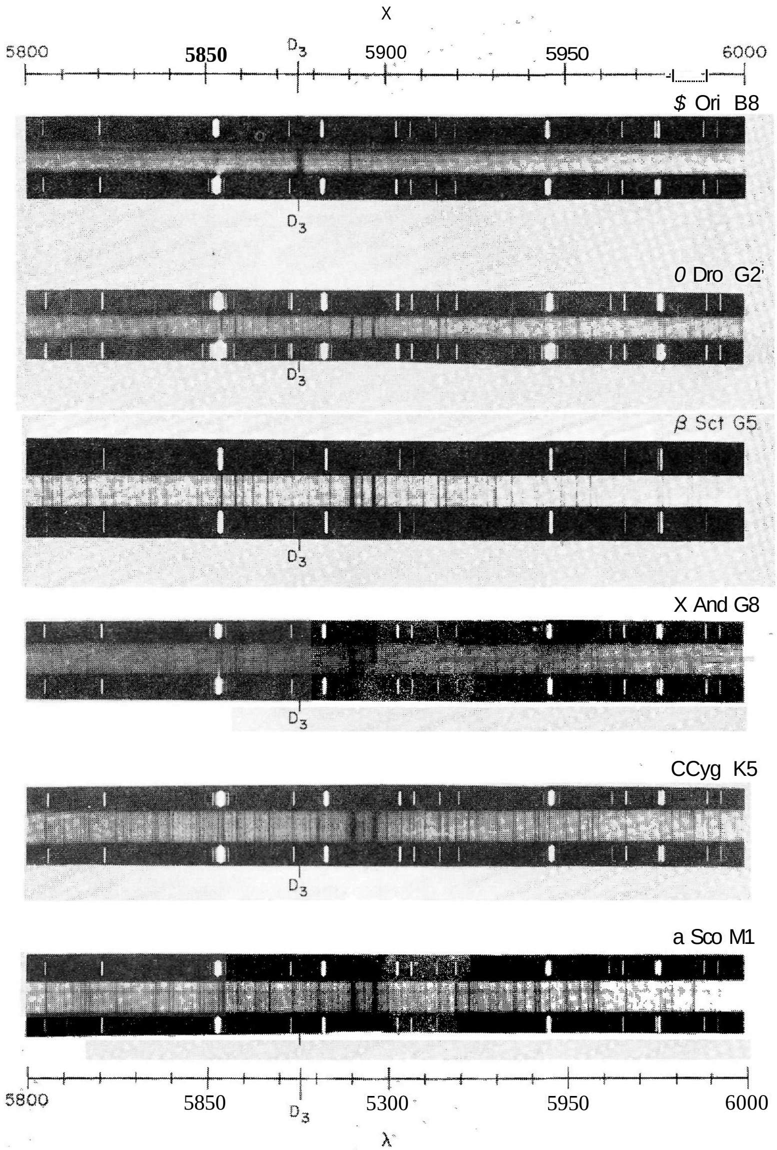


Figure 11-48

chromosphere resonance lines there, so we are not too badly off down to about 2000 A which is an easier region to observe than the region below 2000 A. I think the region 2000 A to 3000 A is important because there are a lot of Fe II, Cr II, etc., lines. If you go to the A stars you get Si II, C II, between 1000 and 2000 A. So the near ultraviolet is not a difficult region for good observations of stellar chromospheres, and it is tragic that we have not got before us any observational capability for the near future to observe such regions. We are going to have to rely on balloons and rockets which have limited capabilities.

**Kondo** — **I** would like to point out that high altitude balloons can be useful for observations down to about 2000 A and do offer long observing periods in comparison with rockets. Residual extinction can still be a problem, however, for observations of certain types, particularly near 2500 A where the absorption due to ozone is high.

**Doherty** — It might help if I pointed out that the slide I showed with the decrease of many magnitudes was for broad band measurements. In the case of the later stars, these results do not show any of the emission lines that might be stronger. The fact that we have a measurement at all of Ly *a* Arcturus is somewhat remarkable and is evidence that Ly a is a very strong line in that region.

**Gros** — Through an analysis of the observed radiation of Sirius (A LV) in two wavelengths located as far as possible in the ultraviolet spectrum, we have tried to derive information on the thermal structure of the superficial layers of the atmosphere of this star. We have used measurements made by Carruthers (1968) at $\lambda_1$ = 1115 A and $\lambda_2$ = 1217 A.

- *Analysis* — Applying the Eddington-Barbier approximation and assuming that the source function at the observed wavelengths follows the Planck function, we have deduced the temperature gradient between the layers ($\tau_\lambda = 2/3$), where the radiation at $\lambda_1$ and $\lambda_2$ is formed from the knowledge of the ratio of the observed fluxes $F(\lambda_1)/F(\lambda_2)$. To get the depths of formation at 5000 A for the radiation at the relevant wavelengths, we need a model to start with: we have adopted an LTE, non-gray for the continuum, radiative equilibrium one.

- *Application* — The first approximation model is homologous to a model due to Strom, Ginerich, and Strom (1966), with an effective temperature $T_{eff}$ = 10486°K and a surface gravity of $10^4$ The chemical composition was deduced from the study of lines in the Sirius visible spectrum: silicon is overabundant by a factor of 17 relative to Warner's (1968) solar abundance. The observations (Carruthers, 1968, Stecher, 1970, OAO scans) and the theoretical spectrum from the Strom et al. model are plotted in the Figure 1149. We must point out that, in the absence of an absolute calibration for the OAO data, we have related them to the ground based observations of Schild, Peterson and Oke (1971). The

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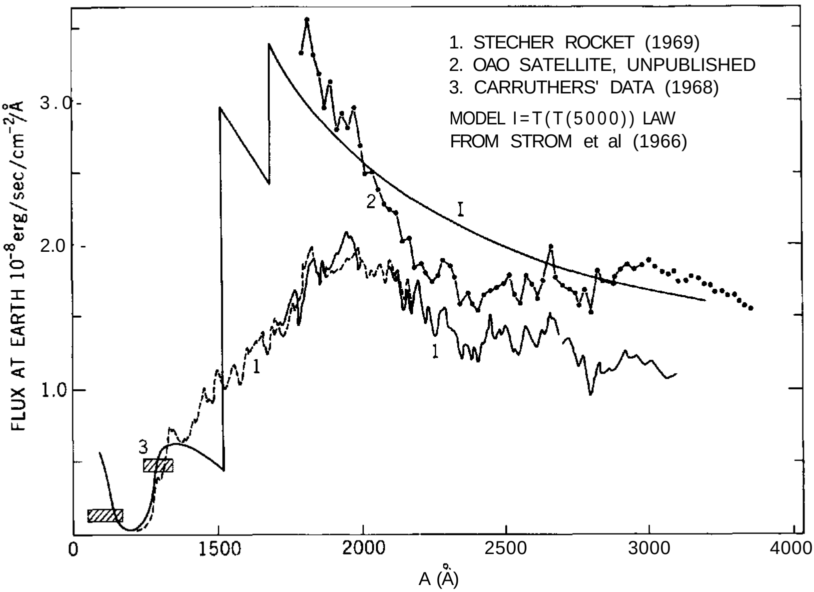


Figure 114 9

comparison between the observations and the predicted fluxes shows that the spectral region around Ly *a* is well fitted by this model, the computed flux is too low between 1300 A and 1520 A, and at 1520 A, a discontinuity due to photoionization from the $^3P$ level of Silicon is present (A m = 2.07 mag). This discontinuity is not shown by the observations. This discrepancy had been pointed out by Gingerich and Latham (1969).

The model allows us to compute the depths of formation for the continuum at each X as shown in Figure 11-50. Note that the violet side of the Balmer discontinuity is formed at about the same depth as the UV radiation at wavelengths greater than 1430 A. This is the main difficulty of the application of the present method to stars as hot as Sirius.

One gets the following results :

$X_x$ \= 1115 A　　　T(5000) = 0.086

$X_2$ ← 1270 A　　　T(5000) = 0.066

$^A\ ^T e$ = + 190°K

Hence we derive an increase of the electronic temperature $T_e$ in the outer layers starting at r (5000) = 0.086.

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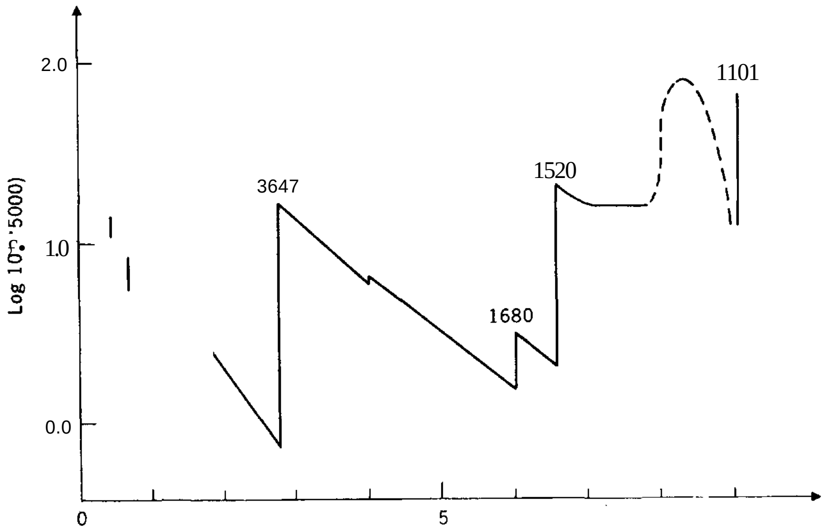


Figure 11-50

Figure 11-51 shows the semi-empirical model obtained from the Strom et al - one by modification of $T_e$ (r (5000) above T (5000) = 0.086). The characteristics of the predicted flux from this model are shown on Figure 11-52.

- A good fit exists for the region around Ly *a*.

- Between 1300 A and 1520 A, there is a small excess of flux, which is compatible with the presence of strong lines shown by Stecher's observations.

- The computed Si I discontinuity at 1520 A is still too large, but it has been decreased by a factor of 2 (A m = 1.26); the same is true for the Si I discontinuity at 1680 A. We have shown elsewhere that the discontinuity at 1520 A is blurred by the strong Si II doublet, at 1526 A - 1533 A.

- This model is too hot to fit the observations (OAO spectrum) between 2510 A and 3647 A. Moreover the computed Balmer discontinuity is too small (D = 0.27), as can be seen on Figure 11-53.

We conclude that this attempt is not completely satisfactory in two respects:

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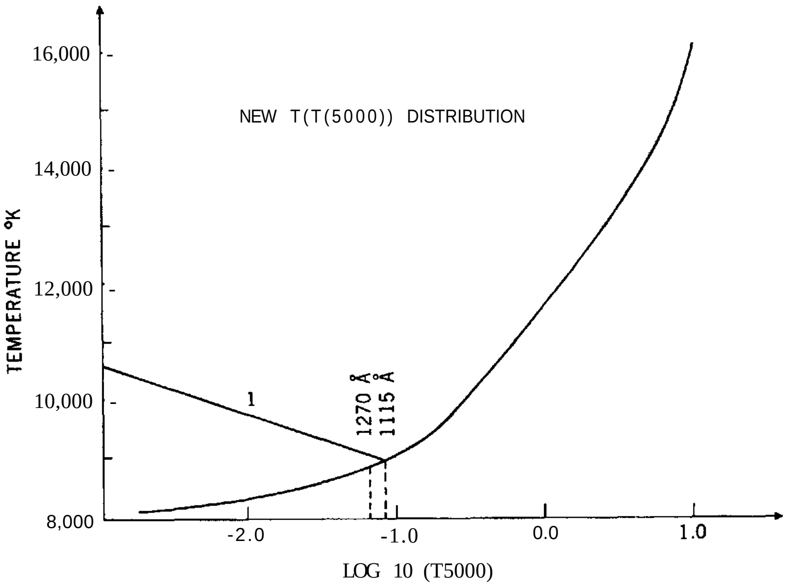


Figure 11-51

- The analysis has been carried out with a purely LTE source function for the continuum at 1115 A and 1270 A; at those wavelengths the opacity is actually due simultaneously to the wing of the Lyman a line, and to the CI and Si I continua; the total source function implies then the knowledge of the departure coefficients in hydrogen, carbon, and silicon atoms.

- The depths of formation for the radiations we use in our analysis depends on the model we choose to start with. If this model has a lower temperature $T_0$ at the surface we can hope that the concerned layers will be higher in the atmosphere and that we will so avoid affecting the formation of the blue side of the Balmer discontinuity. A complete multiple iteration must be performed.

We thank Dr. A. D. Code and Dr. R. C. Blen for having provided us a spectrum of Sirius in the region 2000 — 3500 A, from the **OAO** satellite.

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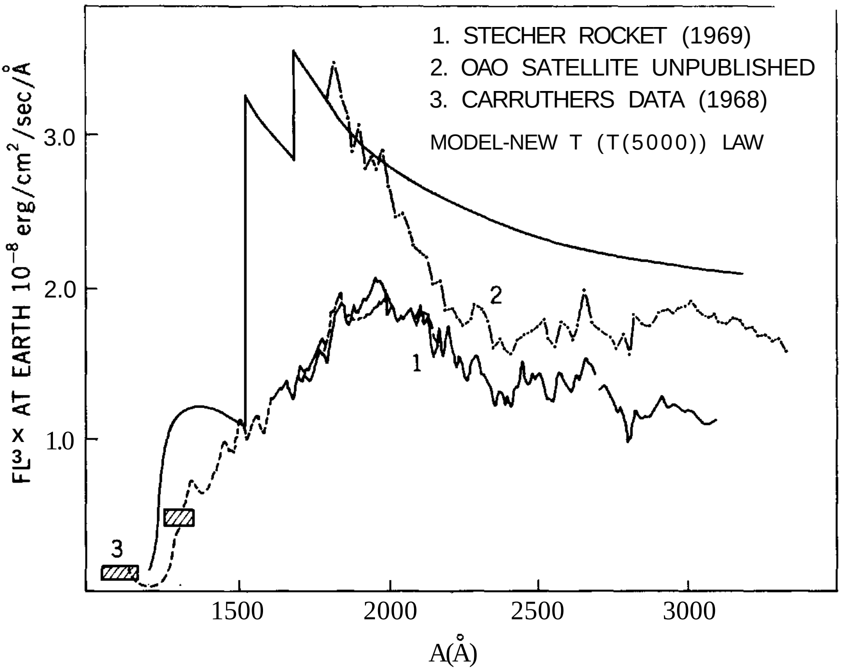


Figure H-52


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CONTINUATION OF DISCUSSIONS FOLLOWING
TALK BY PRADERIE AND DOHERTY

Underbill - I have some of the observations of Sirius from OAO and from ground-based work. The OAO results tend to be overexposed so I have not used them. There are Stecher's rocket observations, and Dennis Evans from the Goddard Optical Astronomy Division has done an absolute calibration of the rocket scans with effectively the same instrument but with independent absolute calibrations. This material was presented last summer but has not yet been published. Those two sets of measurements agree within the uncertainties of transfer to absolute intensity, within 15%, say. The OAO results for this star can't be calibrated as well as rocket data. I have not heard from Savage in Wisconsin what he thinks of my revision of his sensitivity curve based on the rocket data.

Sacotte - In the preceeding talk, M. Gros had some observations in the Lyman *a* range and she obtained some models. In this work, we are

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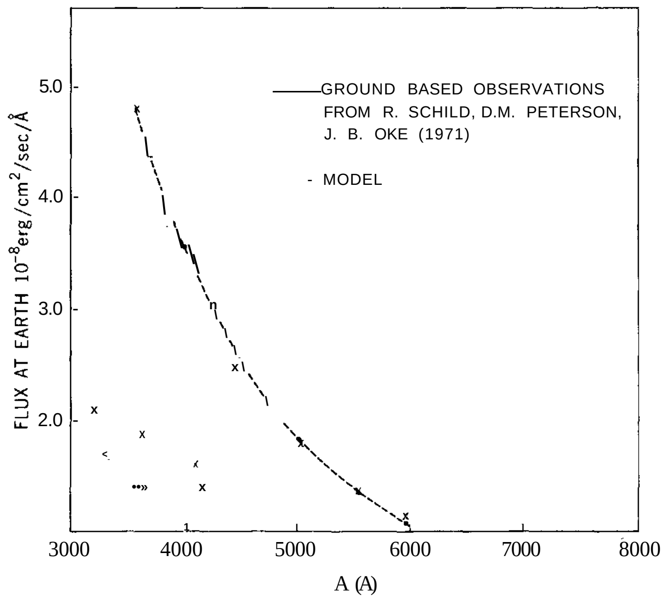


Figure 11-53

departing from models. We compute some synthetic spectra and compare them with observations in the range 2000 - 3000 A, and then use characteristics of the models for comparison. We used the OAO results and believe the calibration is accurate to about 20%. To compute a synthetic spectrum we compute the emergent flux every 1 A or 0.5 A and then convolute the emergent flux by the apparatus function, and we obtain a spectrum directly comparable to the OAO spectrum. Line calculations are made in LTE. We assume that the source function is the Planck function and we use atomic data from various sources. We use a broadening constant 2 times the classical value plus the effect of broadening by hydrogen and helium. The first graph, Figure 11-54, shows the OAO spectrum and the comparable synthetic spectrum. The model used is by Strom, et al. From 2000 A to 2500 A we have an important disagreement, but in both spectra we notice important absorption features. At X2500 A, we introduce in our line computation the data on Fe II by Warner and the agreement is much better as a result. The level of observed flux is reached and every feature is well reproduced. The second graph, Figure 11-55, shows a similar computation based on the model of M. Gros, and here the agreement still is not good. We can reproduce various changes in the spectrum but the flux levels are not in agreement, and we can see some emission levels in our calculation. All we can deduce

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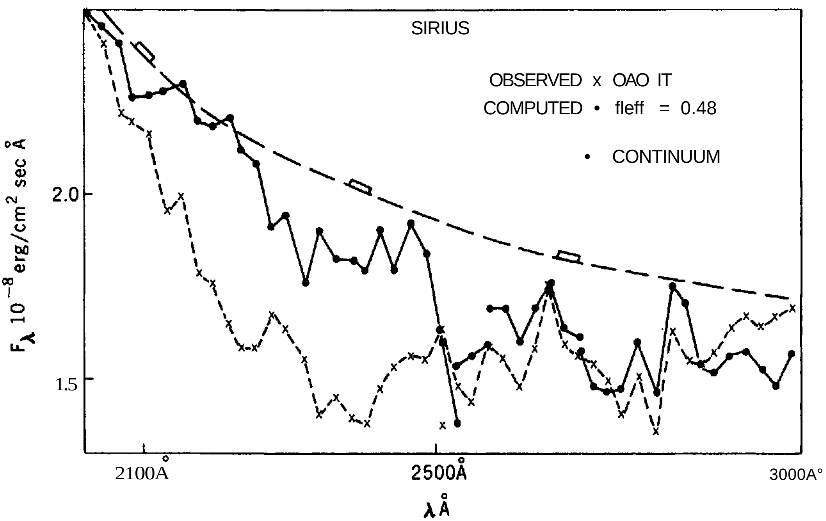


Figure 11-54

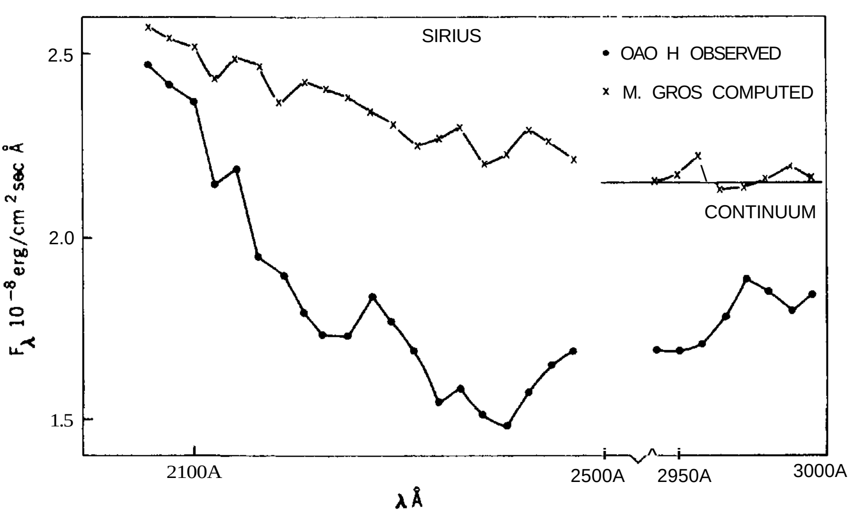


Figure 11-55

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from the calculation performed with the semiempirical model of M. Gros is that the location and the importance of the rise in temperature deduced from the Ly $\alpha$ region in a Al Star have very sensitive effects redward of 2500 Å to the Balmer discontinuity.

**Giuli** — I would like to amplify a comment made earlier by Kondo on the use of balloons in UV astronomy to about 2000 A. Current operational balloons can carry telescopes with easily twice the light gathering power that any Aerobee type rocket can carry. At altitudes of something like 40 km the signal strength or recorded signal per unit time is just as strong as that of a rocket and on top of that you have the advantage of an entire night's observing time. For some reason astronomers seem to have missed out on many of the recent developments. Cosmic ray physicists have been using balloons for a long time, but there are really only about three astronomy payloads that have seriously considered ballooning for ultraviolet astronomy. I would like to encourage those of you who are seriously interested in ultraviolet astronomy from balloons that there are several places around the country which can offer advice, based on experience, such as the Gehrels Polariscope group in Tucson or our group at the Manned Spacecraft Center.

**Bonnet** — We have also been using balloons to perform ultraviolet studies of the Sun.

**Peytremann** — Such balloon experiments have been carried out for many years by smaller countries with limited research budgets, and these were often considered secondary relatively unimportant experiments compared to the orbital ones. There is perhaps some irony in the renewed interest shown here in ballooning.

**Giuli** — Yes, my comments have been directed to the American astronomers. It is ironic, but understandable, how balloon astronomy has been neglected in this country. Our space program was funded suddenly, and, as a consequence, funds were suddenly available for rocket payloads.

**Afler** — Can balloons take payloads above the ozone?

**Giuli** - Reliable balloons can carry 500 kg payloads to 4042 km. Smaller telescopes could be carried reliably to 44 km, but to go much higher requires a tremendous increase in balloon volume, and hence cost and risk. Also, the larger balloons obscure a larger portion of the sky about the zenith.

**Kondo** — It is also important to realize that the state of the art in carrying out these experiments has advanced significantly in recent years. Sophisticated pointing and stabilization systems used in our experiment are examples of such an advance. One can also benefit greatly from the

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capability to monitor in real time the spectrum being scanned, using only the integration time needed for this purpose, and then moving on to observe other objects. The flexibility we now have in carrying out observations was not available a few years ago.

**Kuhi** — That's an important point; ballooning is not restricted to photographic recording of data.

**Underhill** — **I** agree with these possibilities for good observations down to 2000 A or so. Most of the emphasis in ultraviolet astronomy has been on the hot stars and on the wavelength range below 1700 A. I have felt like a voice crying in the wilderness saying that more stars can be observed and more interesting things in the 2000-3000 A range than have received attention so far. But let us not, please, lose sight of the fact that you really need a spectroscopic satellite, such as we have described as SAS-D, up there to observe all sorts of stars for a long time. Balloons and rockets have their place, but satellites are required for comprehensive observing programs.

**Bonnet** — I would like to add a comment on balloon spectroscopy. We took advantage of balloons to look at the solar spectrum but had no means at that time to look at stars, due to the lack of a good pointing system. It appears possible to observe at balloon altitudes in the range below 3000 A down to 2700 A. Below that wavelength you have a strong absorption by ozone and at lower wavelengths competition between absorption by molecular oxygen and ozone. However, there is a reasonably transparent region between 1900 A and 2300 A and, furthermore, this region of the solar spectrum is very interesting because of the presence of the carbon emission line at 1994 A. Detailed observations of this line shows that it is emitted in very limited regions, probably corresponding to spicules on the Sun. If this is confirmed it would be possible to look for spicules in stellar spectra by observing the carbon line using balloon spectroscopy. This line is quite strong and might help in identifying a rise in temperature in the outer layers of a star as well.

Jefferies — In Hawaii we have been making ultraviolet spectra of the Sun from a rocket, and with a resolving power of about 200,000. One of the lines that we have observed is a line of S I; this displays a very curious distribution over the Sun. It is extremely strongly limb brightened in our spectra. I forget the exact wavelength, but I was wondering if any of the stellar observers have seen this. It is a reliable observation and seems a definite indicator of some sort of chromospheric emission.

**Kuhi** — Apparently no stellar observers have seen this line.

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**Fosbury** — Can I comment on the point that Jefferies just made. Let us refer to the diagram for the Ca H and K lines — and the Mg II emission lines. I am trying to find out whether it is a Doppler effect or an opacity effect. I know there is an Fe line blending with the H lines, but can you not do that with the H and K lines separately? You have a factor of 2 in oscillator strength.

Jefferies — In principle you should be able to do so, but, in practice, I don't think it will work. I think that we need a much larger factor than 2 between the optical depths to show a difference of the kind you mention. I think that the factor should be about 10 between Mg and Ca.

**Pasachoff** — May I make a plea for not confusing the spectroscopic notation for H and K, which refer to Calcium. I suggest that we find other names for the Mg resonance lines.

**Kondo** — We are provisionally calling those lines the 2795 line and the 2802 line. However, we might also consider alternative ideas such as use of "h" and "k" suggested by Skumanich.

**Kuhi** — Fraunhofer's notation ends up by P, so we could use P and Q. They are resonance lines and the least confusion is caused if we refer to them by wavelength rather than by the Fraunhofer notation which has caused enough confusion.

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