of hydrodynamics as well as of transfer theory In any case further work is urgently required. First, high dispersion stellar spectrograms should be processed with care in order to define accurately and quantitatively what the properties of the chromospheric lines really are. Then the theoreticians will have to reproduce these lines as best they can, even if it requires the introduction of additional parameters.

I have tried to give here a brief but fairly complete view of the current status of the study of stellar chromospheres. We have learned a few things, but I think the subject is still in its very early stages and is deserving of much more effort on the part of observers and of theoreticians. To me, one of its most attractive features is the curiously large number of contacts with other astronomical fields to which it is able to make contributions.

CONCLUDING REMARKS FOLLOWING THE SUMMARY

Thomas — Dr. Wilson was asked to summarize the conference, as it is customary to have someone with wide experience and breadth of knowledge in the field close such a symposium as this on a note of perspective. It is not necessary that he be an expert on all the matters covered; one hopes only to hear some sort of encompassing "impressions" of what we, the participants, have been exposed to, and how well it "registered" to one having a broad background. I, personally, regret that Dr. Wilson chose not to do this, because I think that we would all have benefited greatly to hear his impressions. But I think that someone should try to do it, both for the sake of those who have tried to present a digest of ideas and for those of us who have just listened and commented. Otherwise, one may be left with what I consider the mistaken impression that there is only one type of chromosphere really worth much attention, the solar type, and only one set of indicators of the universality of the chromosphere phenomenon, those relating to the H and K lines. So, let me attempt a rather general summary

First, I can say in an overall way that I disagree strongly with Dr. Wilson on his assessment of the general importance of chromospheres. If I follow the logic of Dr. Praderie, in her presentation, that the properties of a stellar atmosphere may be discussed in terms of two kinds of fluxes — electromagnetic radiation and mass — then *conceptually* the chromosphere is that part of the atmosphere directly dependent upon a non-zero mass flux generating a mechanical energy flux.

Also, in a wholly *observational* way, the chromosphere determines the properties of the cores of most strong lines in the solar (and most of the stellar) Fraunhofer spectrum: not what I would call an irrelevant thing.

https://doi.org/10.1017/S0252921100500700 Published online by Cambridge University Press

Indeed, I well remember a discussion in the 1950's as to whether the solar chromosphere had *any* observational consequences on the Fraunhofer spectrum. And it was a major milestone in solar research when it was shown, unambiguously, from eclipse studies, *fust how many* solar lines observed on the disk were influenced by the properties of the chromosphere. As an indicator of the existence of a mass-flux, and as a determiner of the properties, of the cores of both strong and intermediate lines — I hardly consider the chromosphere as a "negligible" part of the structure of a star. If I venture to comment on the direction from which K. Gebbie, Pecker, Praderie, and I have been working — which has evolved into viewing the atmosphere as a transition region between stellar interior and interstellar medium — the chromosphere is again a most important region in this transition, from the direction Dr. Praderie emphasized. So, having tried to restore the role of the chromosphere into focus, let me try to survey what the invited speakers summarized for us.

Beginning at Day 1, which, in essence, was theory. Jefferies made two major points:

- How can one find the temperature structure of the chromosphere? He noted that there are two kinds of lines: ones which have collision-dominated source-sink terms, like the Ca^+ and Mg^+ H and K lines, and ones which have photoionization dominated source-sink terms like the Balmer series of hydrogen. In the case of the former, you can tell something of the T_e structure; this is true particularly in the case of an atmosphere with a temperature reversal. Such a reversal may produce a central emission core, and the central emission core may be, in turn, reversed wholly by radiative transfer effects. This is in contrast with the old L.T.E. interpretation which required a second temperature reversal to produce the self-absorbed core. In the literature, there are a lot of predictions of these kinds of effects, ranging from lines with no self-reversal to lines with self-reversals. You can make the self-reversals as strong as you want to, as wide as you want to, and the emission core as steep as you want to by "choosing" arbitrary distributions of T_e. For example, see Lemaire's thesis on the Mg II H and K lines. Now Wilson is interested in square profiles, i.e., profiles with steep sides to the emission peaks. Strictly speaking, there is no such thing as a "square profile"; it is "square" only to some accuracy Very steep sides on profiles have been computed, however, for particular atmospheric configurations, and they are in the literature. Furthermore such variations in steepness and behavior of the central core are found observationally in the Sun. Again, refer to the Lemaire thesis as an excellent compendium of observation and theory.

https://doi.org/10.1017/S0252921100500700 Published online by Cambridge University Press

- Jefferies second point concerned the observed emission lines and how their existence may relate to the existence of a chromosphere, emphasizing the distinction between intrinsic emission lines and geometrical emission lines. If we consider spectral regions where the continuum is depressed, we can have either kind of emission line. In the visual regions, where the continuum is not depressed, we obtain emission cores in absorption lines as a reflection of an intrinsic emission line. We can have any combination of these, depending on circumstances. The. following approach by Avrett permits a demonstration of these points.

The summary by Avrett showed what one could and could not do with various models, i.e., various *assumed* distributions of T_e. It was numerical experimentation. Its approach is one that Wilson could call upon to ask, *"Can* I, under any circumstances, get theoretically such-and-such a profile," and "How many kinds of circumstances can produce it?"

Now, as a comment on the bearing of these H and K profiles on our ideas about chromospheres, and as a bridge to Dr. Praderie's summary, let me quickly summarize the evolution of the past 25 years in our outlook.

In phase 1, the only star which had a chromosphere was the Sun. And the textbooks of that time (1950's) said that the chromosphere had absolutely no influence on the observed disk spectrum of the Sun. There were observations of line profiles which apparently showed (under LTE diagnostics) that the limb temperature was as low as 2700° K, in conformity with the LTE line blanketing calculations. That was the end of phase 1, essentially wiped out by the body of non-LTE theory applied to interpreting solar eclipse observations, which among other things showed such temperatures to be erroneous.

In phase 2, which Wilson's talk summarized masterfully, there were admitted other stars, besides the Sun, with chromospheres, and it was thought that these were essentially measured by H and K self-reversed emission cores. Recognition of these other chromospheres was an enormous step forward. Such stars occupy some part of the HR diagram, and about this part we have considerable "suggestive" information coming from those empirical relations which Wilson discussed. These tell us that there is some profound relation between the energy production by the star and that fraction of it which goes toward providing a chromosphere.

In phase 3, we advance to the rest of the HR diagram, so long as one makes synonymous the concept of a stellar chromosphere and the existence of Ca^+H and K line. No stars here were supposed to have chromospheres; cf. the 1955 IAU Symposium and comments by Biermann and Schwarzschild.

https://doi.org/10.1017/S0252921100500700 Published online by Cambridge University Press

In phase 4 we admit a chromosphere may exist in stars which do not have H and K as the major chromospheric indicators; and we begin an open-minded search for what these other indicators are. So, we broaden our sights, and we are here at this conference.

On Day 2, Dr. Praderie emphasized two conceptual points. First, a *necessary* condition for a chromosphere is a mass flux, taken in the broad sense of mass motion somewhere in the star. Second, a *sufficient* condition for a chromosphere is mechanical dissipation. She then described the direct observational evidences for chromospheres, only one kind of which is provided by the H and K lines. These H and K lines stand out in the minds of all of us because the lines are so well observed and because there is some kind of theory to interpret them. As you go to more complex atoms, there are complications, i.e., multilevel atoms, etc. One cannot predict theoretically all the features Dr. Praderie talked about, but she divided them into two aspects: excitation phenomena and ionization phenomena. For example, just the existence of helium lines on the solar disk and in the solar chromosphere tells us right away that there is some kind of anomaly. These are all direct observations. Praderie then went on to the indirect evidences for chromospheres — the existence of velocity fields of one form or another. This aspect might have been discussed by John Jefferies in his review of diagnostic techniques, but one must have a great deal of sympathy for why he did not cover these things, since our explanations and our analysis of the existence of velocity fields are extremely rudimentary so far. We take the direct diagnostics as giving some evidence for a temperature rise, and the indirect diagnostics as giving some evidence for the possibility of mechanical dissipation, which may then produce a temperature rise. While Wilson stated that there is general agreement among theoreticians on the necessity for magnetic fields for the transfer of this mechanical energy, I think this is a misleading statement. All the original work on chromospheric heating by mechanical dissipation ignored magnetic fields. One currently invokes magnetic fields to understand differential heating over the solar surface. I believe the question of the relation between mass flux and mechanical dissipation and magnetic fields is most important but badly understood at present. While simple correlations between the presence of magnetic fields and Ca^+ emission are excellent guides, a theoretician cannot afford to depend wholly on them. Agreed, one needs empirical relations to start and to be stimulated, but one needs to go *far* beyond that. Also, this coupling between the velocity fields and the H and K lines is a very strong point right now. The problem of interpreting the half-widths of these lines, and the Ha lines, and all the other lines Dr. Praderie discussed, is a very real one. It all comes down to indirect indications of chromospheres: the indications of potential chromospheric heating in the presence of velocity fields.

Doherty's summary put very well those aspects which have been exciting to all of us who had to live so long on the observations in the visual spectrum; viz, the enhancement in the "space" ultraviolet of all these things that one could only guess at from the cores of the H and K lines. The balloon observations of the enhanced Mg^+ emission cores provide a direct extension of the Ca^+ material. Then, we have in great profusion P Cygni-like lines showing evidence of outflow of mass, which links strongly to the theoretical work by Parker and subsequent work on the solar wind. When we find evidence for many lines showing P Cygni characteristics, plus many emission lines in stars which cannot be interpreted wholly in terms of geometrical effects, then we have enormously powerful chromospheric indicators.

I think that if the theoreticians are to be criticized, it should be in a tough but realistic way. And the tough way is that the theoreticians have not provided simple, straightforward models, both of the physical concepts underlying all this non-LTE diagnostics and of the physical concepts underlying mechanical heating and really non-equilibrium thermodynamics, in such a way that the observer can both see it clearly, and can sit down and make simple-minded approximations in order to interpret these space observations. Non-LTE theory is not conceptually that complex. That is my summary of the first two days.

In some sense, the third day was the real meat of the conference to those of us who are concerned with the definition of a chromosphere in terms of mechanical heating. The preponderence of thinking in this symposium has been to define a chromosphere in terms of a T_e-rise, because we know what that predicts.

This is the real focus of the conference so far as many of us are concerned. But, we are staggering. We have some kind of diagnostics developed; we have enormous numbers of observations; we have from Wilson and his co-workers enormous stimulation so far as one kind of chromosphere is concerned, that kind centered on the H and K lines as a diagnostic tool, suggesting, in the Wilson-Bappu relationship, that there is some correlation between the intrinsic luminosity of the star and that part of the mass flux which provides a mechanical energy dissipation to heat the chromosphere. How do we explain it? If you go back to the early days, when these very first suggestions on mechanical heating were made by Biermann, Schatzmann, and Schwarzschild, then we have very naive ideas, to which reference has been made today One goes on from there to ask: how do I produce, first, the flux from a given internal convective structure; how much flux do I produce; how do I get up into the regions of hearing; and where do I heat the atmosphere? There were strongly technical discussions on day 3, and certainly, those presenting

https://doi.org/10.1017/S0252921100500700 Published online by Cambridge University Press

the discussions did not bring us all up to their level. But there were two interesting summaries: One was by Jordan who summarized the applicability of various approximations on when a sound wave becomes not just a sound wave but something strong enough to produce heating in the atmosphere. That is the sort of investigation we need to explain the Wilson-Bappu relationship. Jordan summarized the current thinking on that kind of approach. The emphasis lay on the basic physics. The second summary by Delache was an attempt to go back from that standpoint and to ask, what do I do when I talk about those phenomena which produce a chromosphere or a corona? And you start from the very basic thesis by Parker that you can't have quiescent stars, so long as you do not have a constraining boundary in some sense. He went on from there to develop what possible kinds of structures one could have, recognizing that the Parker stellar wind means that all the way down into the star some kind of a mass flux must exist, no matter how small in the deepest layers. This is the kind of approach one needs to begin to make some kind of theoretical structure. If I only try to say that all I have is a variety of motions of unknown origin in the solar atmosphere, and it is their resultant that produces the observations, introduced in an ad hoc way, I go to a situation similar to terrestrial meteorology. It is like saying there is no point in making a first-approximation model of the terrestrial atmosphere because I can not reproduce *all* the local phenomena that you see when looking out the window of an airplane — lightning discharges, beautiful clouds with periodic structure, enormous plumes, etc. The answer to that viewpoint is that it is simply defeatist. One has to do the best he can to start. What do we do? First we make a spherically symmetrical model of the stellar interior, and then a spherically symmetrical model of the stellar atmosphere, not because we believe that is the last word; but each time we made a model, we should say, "That model is good to some degree of accuracy " We make models to be compatible with the observations, good enough to achieve internal physical consistency; and then we try to reproduce our observations. All Day 3 was trying to tell us was the accuracy to which we know the basic physics; namely, how much mechanical flux is put in the atmosphere, how much is stored, how much is propagated, how much is dissipated to the accuracy that we know initial boundary conditions; all in the hope that, with this knowledge, we can use those results on two things — mechanical dissipation of energy and velocity fields.

On Day 4, Kippenhahn gave what I consider to be a fine complimentary summary of the work that Wilson has presented here. Kippenhahn gave essentially a theory behind this particular kind of chromosphere, based on the internal structure of particular stars. He presented for us a very beautiful, complex, "flow diagram" of the linkage paths between mass loss, angular momentum loss, magnetic field from the turbulent dynamo and its relation to differential rotation and the convection zone, and stellar evolution. Somehow, he suggested these are measured by g and T_{eff} — myself, I have a hard time seeing how these two parameters suffice — but this probably just reflects my own ignorance, which is a good admission for a summarizer to make.

That is what we have had in the conference: some diagnostic techniques; a summary of observations of different kinds of chromospheres that appear to exist; a summary of the theory for some very particular effects, namely the aero-dynamics as we know it today; and a summary of the observations of some particular stars, following a summary of the relation of the interior structure of certain types of stars to chromospheres.