DEFINITION OF THE PHYSICAL PROBLEMS CONNECTED WITH EXTENDED ATMOSPHERES

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ABSTRACT

The necessity of carefully defining the phenomenological basis for classification of atmospheres as being "extended" is emphasized, and four alternative bases for such classification are suggested (1) the necessity to include curvature terms; (2) the presence of an ejected shell surrounding a central star; (3) an observational discrepancy between predicted and observed density gradient; (4) an anomaly between predicted and observed phenomena in stars with "dynamic" atmospheres such as cepheids. A number of physical problems connected with the presence of an extended stellar atmosphere are then categorized according to these alternative bases.

Key words: extended stellar atmosphere, classical atmosphere model.

The general subject of this conference is "spectrum formation in stars with extended steady-state atmospheres." The theme of this first day is "definitions of the problems that exist." Because we use the term "extended atmosphere," we imply that these stars differ from the usual kind of star, which have atmospheres that are "nonextended." But we must recognize that most of our experience and physical intuition rests on our experience with these ordi-

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nary, nonextended atmospheres. There is the very real possibility that we may assess erroneously some problem on an extended atmosphere by applying an intuition that was developed in the study of nonextended atmospheres. So at the beginning of this first day I think we must be very explicit, even pedantic, in defining the terms we use to describe both the general subject and the problems that we think exist.

On this basis, I am not quite satisfied with the definitions Anne Underhill used, and the implications of some of her terms, because it seems to me that her approach is indeed wholly intuitive. For example, her most explicit definition is: "A star with an extended stellar atmosphere usually means a supergiant or a shell star." She then defines each of these stellar types in terms of certain spectral features, which we think we can interpret. But most of this interpretive process rests on our experience as to how these spectral features would be produced in ordinary, nonextended atmospheres. Again, Anne states: "We assume T_{eff} is the same for main sequence and supergiant stars. Then a spectral difference between these two classes reflects a pressure difference in the atmospheres, which implies a difference in radius, thus an atmospheric extension." and "We have certain types of lines in main sequence stars, and certain types in supergiants; some stars combine features of each--these we call shell stars."

There are strong implications in these definitions, but we are so accustomed to making them, when discussing ordinary stars, that we gloss over them. The balance of Anne's talk, logically enough, is devoted to a discussion of these spectral features and her reasons for thinking that our conventional, classical atmosphere [CA] models cannot describe them.

I myself think that there are many features that cannot be interpreted by the classical atmosphere models in stars whose atmospheres are neither "supergiant" nor "shell." Some of these features may arise because some parts of the atmosphere are more "extended" than the classical atmosphere model predicts. Other features may be anomalous on the basis of the classical atmosphere model, yet their appearance may have nothing to do with atmospheric extension.

So I would prefer to have a definition of an extended stellar atmosphere that is more directly tied to a pictorial, geometrical notion of "atmospheric extent." There are several possibilities for setting up such a definition, and I think we should consider them all. During this symposium we can use these categories as a reference frame. Perhaps we will find one particular category is preferable; perhaps we will find none are very satisfactory. I will outline my suggested alternatives; you may object and change them as you like.

A. DEFINITION OF AN EXTENDED ATMOSPHERE

We have several possibilities for this definition: one conceptual, from an a priori standpoint, within the framework of our ordinary models; one based on some circumstance that produced a configuration of an "unusual" type but to which we try to apply our "usual" thinking; one based on observations that directly contradict what we would expect from our "usual" models; and one based on some configuration we expect to occur but which we expect to lie outside the scheme of our "usual" models. I will summarize these, then ask which we might adopt.

For reference, first recall that the classical atmosphere (CA) is defined by: RE (radiative equilibrium) or CE (convective equilibrium); HE (hydrostatic equilibrium); and LTE (local thermodynamic equilibrium). Then we have four alternative classification schemes, each based on some distinction between the category and some property of the CA.

1. Distinction between an atmosphere where curvature terms need be included and an atmosphere where they need not be included. Other than this difference, we could hold to the CA model, if such a CA model could give an extended atmosphere.

Note that this was essentially the type of atmosphere considered by Kosirev, Chandrasekhar, and others during the 1930's when they were trying to find the change in the spectral distribution of emergent radiation due to the effects of extent and curvature. Note that they did not apply the CA assumptions literally, but adopted arbitrary density distributions to get the atmospheric extent; but they did retain, at least implicitly, the "physics" of the CA model.

It may be that to get such an atmosphere under CA, we need to go to small values of $g_{eff} = (g_{dynamical} - g_{radiative})$, and problems of stability arise. But this is the direction of approach.

2. A distinction based on the presence of an "ejected" atmosphere or shell surrounding a central star and atmosphere where the configuration is considered to be now steady, or at least quasi-steady, but where there is no pretense that the configuration occurred as a CA evolution, just that a CA model, only slightly fudged, may now describe it.

A variety of objects could possibly be placed in this category: shell stars of all sorts; various nebulae including planetary nebulae, T Tauri shells, etc.

3. The wholly observational distinction between the predicted extent of a star with given g and T_{eff} and its "observed " extent. Then, assuming the parameters necessary to compute the CA model--g and T_{eff}--are reasonably accurate, the essential question is how does one specify the "observed extent." (We bypass for the moment the question of the implication on the model of the "microturbulent" parameter required for stellar spectral fit to the CA models).

The question of specifying the "observed extent" obviously underlies this whole possibility for defining extended stellar atmospheres. There are a number of possibilities for defining the observed extent:

(a) *Direct approach.*

"Tangential" observations, i.e., eclipse studies, that directly measure extent. Solar studies are obvious. Studies of stars like 31 Cyg, ζ Aur, and V444 Cyg give direct evidence of atmospheric extent.

(b) *Indirect studies of "extent" parameters.*

Studies of spectral features that <u>appear</u> to imply large geometrical extent: (1) "dilution" effects; (2) observation of an abnormally large number of Balmer lines; (3) some forbidden line effects (These last two are a combination of low-density and large geometrical extent phenomena.); and (4) interpretation of emission lines as coming from an extended stellar atmosphere.

(c) *Indirect studies based on the predicted effect of other parameters.*

Micro- and macro-turbulence values that are comparable to the compression-disturbance velocity in the atmosphere would be expected to distend the atmosphere.

Excitation effects that suggest anomalously high excitation, hence higher electron temperature, suggest greater atmospheric extent. (Note that some of these effects were what led to studies trying to find a UV excess in an extended atmosphere.)

Some years ago, the sun would have been included as the classical example of this type. After all, people thought that there was absolutely nothing in the non-eclipse spectrum to imply anything "anomalous." But now, with hindsight, we recognize that there are many things in the visual disk spectrum that suggest the following classification: e.g., the H and K line profiles, the λ10830 He line, the presence of the solar granulation.

4. Distinction between the "normal" CA models that we expect most stars to satisfy and whatever models are required to describe pulsating stars, ejecting stars, and other dynamically unstable stars which do, however, exhibit at least a quasi-steady-state configuration.

While we know what we mean by stars that exhibit an overall divergence from the strict CA model, we do not know in detail which these are. For example, we would now include the solar chromosphere-corona in the class of such atmospheres that the CA model will not satisfy, but several years ago we would not have done so. Further, the sun as a whole is thought, by most people, to satisfy the CA, and it is often argued that the chromosphere-corona are an essentially unimportant part of the solar atmosphere. (With which viewpoint some of us would take issue.)

It is often argued that at each phase of the cepheid pulsation the atmospheric configuration can be mimicked by a CA model with the proper choice of T_{eff} and g_{eff}.

It is sometimes argued that supergiant atmospheres satisfy the CA model. Other people disagree. The current satellite observations of their far UV spectrum show large mass outflow, and almost certainly a chromosphere-coronal phenomenon.

Despite the problem of identifying the stars, or the parts of the stellar atmosphere that might satisfy the distinction of being in the extended stellar atmosphere class, all seem to agree that the effect of a momentum input or a mechanical energy dissipation or both is to make the atmosphere more extended than it would be in the CA model. But in any event, a basic point is that the CA assumptions are violated. Thus in discussing an extended stellar atmosphere, we must carefully decide which of the above criteria we adopt in defining an extended stellar atmosphere (ESA).

B. SUMMARY OF THE PHYSICAL PROBLEMS CONNECTED WITH THE IDEA OF AN ESA

Clearly, the problems we will list and emphasize depends upon which definition of an ESA we adopt. Let us consider some of these problems according to the various alternatives given above:

1. *"Curvature" criterion*
   - (a) The main question is, Under what conditions can a CA model give an ESA?
   - (b) The second question is, What structural difference comes from such an ESA model?
     1. What difference does it make in a $T_e(\tau)$ model?
     2. What difference does it make in the predicted spectrum: I (λ, continuum); line profiles. We should consider this problem according to LTE, to be wholly consistent.
     3. What difference does such a model induce, in the expectation of non-LTE effects? Since such effects come wholly from inhomogeneity induced by the boundary, and the "kind" of boundary behavior changes with these curvature terms, it is an important question.

2. *"Ejected" atmosphere or shell*
   - (a) A major problem is to deduce the geometric distribution of the ejected material. One approach is to try to follow in detail the distribution of matter to be expected from a nova outburst, checking the inferred distribution, based on some sort of calculations, against the spectrum that should be produced by such a distribution. Another approach is to assume various degrees of "connectedness" of the distribution of material, and determine the "stratification" spectrum.
   - (b) It is equally important to ask whether the CA model or a simple modification can be applied. Clearly, here the non-LTE aspects associated with "dilute" radiation are important, but it is also important to ask whether such things as RE, HE, etc., can be taken over. So it comes down to asking what kinds of steady-state equilibrium can be stable for such configurations, taking into

account various kinds of interaction with the "parent" star--radiation, continuous small-scale mass ejection, etc.

(c) There are also many problems connected with predicting the spectrum from such an ESA. Again, these are connected with Session II of this symposium.

3. *"Observational discrepancy" with prediction of CA model*

(a) We made the point above that the primary problem is one of inferring parameters that either support or negate the CA predictions. Again the problem can be broken down:

1. Eclipse studies. Just as there was no real understanding of the implications of solar eclipse studies on the specific details of the outer solar atmosphere until a complete non-LTE diagnostics was developed, so there exists the same situation for eclipse studies of 31 Cyg, etc. Mainly, we have curve-of-growth studies. We need to see what is needed, in terms of data available, and develop the diagnostic methods.
2. Non-eclipse studies. Many of these studies are simply spectral studies and spectroscopic diagnostics, and thus come under Session II. Some of these studies are of the physical implication type and resemble solar studies, where, e.g. "observed" supersonic turbulence required high T_e. Thus we ask for studies of the physical consistency between the simultaneous "inferred" presence of various parameters. It is sometimes difficult to separate the above point and this point, just as in the solar problem, and they must be studied together. E.G., does an inferred supersonic turbulence imply a faulty diagnostics or a faulty inference on T_e?

(b) Emission lines: general approach. While this is logically part of the spectroscopic studies it should be broadened in implication to be mentioned separately. Basically the question we ask about emission lines is whether their presence can be interpreted to imply: (1) simply an extended atmosphere, and "classical" grad T_e, (2) the presence of a "specialized" mechanism, such as the Schuster mechanism, and again essentially a "classical" atmosphere, (3) the presence of a chromosphere-corona, thus mechanical energy supply, and definite departure from RE.

Thus the presence of emission lines is a class by itself when discussing spectroscopic diagnostics. It is also a bridge to alternative(4).

4. *"A priori" rejection of CA models*

The foremost problem is a conceptual one: What are the parameters that fix the state of the atmosphere of such a star? There may, of course, be several cases. But overall, the big difference between a classical aerodynamical situation and a classical stellar atmospheric configuration is the coupling of velocity field to internal degrees of freedom. In the aerodynamic case, coupling is through T_e (electron temperature), whose value is fixed by the kinetic temperature, whose value is fixed by the dynamical flow problem directly. In the stellar case, the coupling is through the radiation field, whose value is indirectly affected by the velocity field. The question is, In such cases as this model type 4 distinction between ESA and "normal" atmospheres, does this situation change with respect to the static case? The "classical" arguments on cepheids, etc., would say it does not change: you give g_{eff} and T_{eff} at each phase, with g_{eff} being the thing fixed by the dynamical problem--but the radiation field still fixes the local value of T_e, and the internal degrees of freedom, and so the spectral properties. Thus the atmospheric extent would arise from dynamical properties, not from increased T_e due to mechanical energy dissipation, in this "classical," RE, cepheid picture. The CA would not apply in its entirety, of course, only in this modified version.

But it is also possible that mechanical energy dissipation must also be included. In this case, we must determine the effect on T_e, and the coupling to internal degrees of freedom. In this case, the basic question posed is: How do you really describe the dependence of the local value of T_e on the quantity and quality of the radiation field and the local mechanical energy dissipation?

DISCUSSION

Pecker: It should be noted that, even in steady state, we should write time-dependent equilibrium equations. At a given location in the atmosphere conditions are fluctuating; they are steady only in a statistical way. Depending on the relaxation time of the medium, as compared with the characteristic time of fluctuations, it may be necessary to solve the time-dependent equations before integration over the time. The result could be different from the solution of time-independent equilibrium equations.

A. B. Underhill made a guess of "what lines are sensitive to non-LTE conditions." She proposes three categories. This seems to me very dangerous. As soon as we suspect that non-LTE physics is necessary, we have to investigate, in each particular case, whether LTE might be used as a sufficiently good approximation.

The Balmer lines should be added to the list. The studies by Feautrier, for example, show that b_1, b_2 are far from equilibrium.

In the case of the hydrogen lines in supergiants even a non-LTE theory does not solve all problems. A. B. Underhill mentions that we could consider a smaller hydrogen abundance to reconcile observations and theory (i.e. β Ori and others). But what about departures from hydrostatic and radiative equilibrium? These could give rise to an increase in temperature and variations of g_{eff} in the outer layers of the atmosphere and should be seriously considered.

Underhill: If the temperature increases with height in an atmosphere, you may find emission components in some lines or indications for higher excitation temperature in the lines. But the spectra of B-type supergiants seem to indicate that there is a very low excitation temperature.

Menzel: Are there some similarities to the solar chromosphere?

Underhill: It is very difficult to observe chromospheric effects in the light integrated over the disk, even in the case of the sun.

Pecker: If the lines show low excitation temperature, you can only say that there is no increase of the source-function. It is wrong to conclude there is no increase of excitation temperature.

Underhill: There is no known mechanism for heating a chromosphere, for hydrogen and helium are already completely ionized in the outermost layers.

Kalkofen: The model calculations by Mihalas and Feautrier do show an increase of temperature in the UV.

Underhill: An increase of 2000° for an average of 20,000° K is too small to show conspicuous effects.

Menzel: I disagree with the statement that the assumption of LTE gives only absorption lines. Strictly, if the temperature of the gas equals that of the photosphere, the spectrum will contain neither absorption nor emission lines. If the photospheric temperature is greater, we obtain absorption lines; if the temperature of the gas is greater, the lines appear in emission.

I am not implying that departures from LTE do not exist. Cyclic transitions, perhaps involving metastable levels, must cause such departures. However, they may not be as great as certain non-LTE theories seem to require. Such neglected phenomena as non-static atmospheres, flares, "star spots," magnetic fields, atmospheres with non-uniform temperatures and dynamical flow can produce what appear to be significant spectral anomalies, as interpreted by conventional theories.

Most proponents of non-LTE theories of stellar atmospheres attribute the supposed spectral anomalies to the interaction of the radiation and collision fields, a source function differing from that of Planck radiation for a given temperature, or non-Maxwellian distribution of velocities of colliding electrons, plus the interactions and resonances--real or fancied--between different atomic constituents.

We don't have to go very far to see that this model simply does not work. The sun is an excellent example, as I first demonstrated about 40 years ago, not from any model but from the observations alone.

I have always insisted that observations are fundamental. Models are important, but they must be subservient to the observations rather than to traditional or classical concepts of how stellar atmospheres should be constructed.

Just look at the sun! The normal Fraunhofer spectrum is consistent--at least roughly-- with that from a gas at a temperature of about 5000°K. The chromosphere, a layer of gas contiguous to the reversing layer, contains strong lines of ionized metals, of helium, and of ionized helium. In the old days-- and by "old days" I mean the very ancient pre-DHM era, which no one here other than I can recall--some astrophysicists claimed that the chromospheric spectrum was entirely in accord with the existing theory. They pointed qualitatively to the Saha formula and naively suggested that low pressure was responsible for the presence of ionized helium in the chromosphere, a fact that I later showed could be explained only on the basis of high temperature.

Miss Underhill calls attention to many lines she doesn't "like," because they "behave anomalously." Anomalously, that is, according to her models. But I think we need to postulate the existence of truly distended atmospheres--stellar coronas, if you will. In such regions we shall expect to find numerous "anomalous" conditions, such as shock waves, magneto-hydrodynamic flow, and atmospheric irregularities. No wonder the spectra seem to be anomalous. But the fault lies in our models, not in the stars.

Underhill: What I don't like is calculating those lines with LTE-theories. I do like those lines because they do give us a clue to what is going on in particular processes. I cannot agree with Dr. Menzel that it may be possible to heat the extended atmospheres in layer sections, giving each section a temperature and then going on with LTE calculations. The critical lines do come from levels so widely separated that you cannot justify Saha-Boltzmann relations. The reason is that in early type stars the particle densities are too low (of the order of 10^{12}-10^{14}) even in main sequence stars. Saha-Boltzmann relations are reasonably valid if the particle density is of the order of 10^{16}.

The difference between the atmospheres of early type stars and the sun is, that in early type stars the density of the photosphere is comparable with the density of the solar chromosphere. In extended atmospheres the densities must be even lower.

Wellmann: It seems that the lack of adequate mathematical methods to treat the NLTE case is a severe handicap for the explanation of many observations related to stars ranging in type from the supergiants with minor deviations from LTE to extreme NLTE-objects like planetary nebulae, as well as certain Be stars, WR stars, and novae. The complete problem is to solve the (infinite) system of linear equations N_i = const together with the transfer equations for the radiation densities u_{ik}. It might be possible to introduce new special functions adapted to this task. This is a challenge to the theoreticians.

Underhill: It should be possible to reduce the infinite system to only a few equations by neglecting many transitions for physical reasons or by tying the higher levels to the continuum. In addition it might not be too bad to assume black-body radiation density for the secondary lines and restrict the transfer problem to one or two lines. How to proceed is a question to be answered by the theoreticians.

Wellman: I should like to point out that the formation of emission lines might be strongly influenced by the velocity distribution in the atmosphere.

Hillendahl: In calculations recently completed for supergiant atmospheres the line shapes (core depth, width, wings and equivalent width) were found to be quite sensitive to the velocity distribution in the atmosphere. Because of this sensitivity it is difficult to see how one can draw conclusions about the importance of non-LTE unless the velocity dependence has been taken into account.

Thomas: Let's be sure that questions on symbiosis are not questions of the method of description rather than physical facts. Since Menzel's work in the 1930's it is clear that the sun is a symbiotic object in the above sense: T = 3400° for NaD-lines, 25000° for He II. But recent work using non-LTE and non-radiative equilibrium has shown that this "apparent" symbiosis is not symbiosis at all; it is just a deficiency in our methods.

Pecker: If there are objects between the supergiants and the stars with large shells, i.e. planetary nebuale, it is not a priori necessary that we find the same mechanism. The only reason for intermediate objects could come from stellar evolution.

Wellman: The gap is filled by novae in different states and by Be stars of different types.

Rybicki: The question of the appearance of emission lines is a question of the behaviour of the source function. Independent of LTE or non-LTE conditions it is the projected area that gives rise to emission lines. So it is important to consider the geometric structure.

Thomas: The point is not whether an extended atmosphere can produce an emission line only by geometry--but whether an observed emission line implies such a geometrical origin uniquely. It is possible to have an intrinsic emission line in a non-extended atmosphere.

Rybicki: If you are going to obtain the mechanism of emission from the observations you must be sure what kind of mechanism you mean. In the case of supergiants you cannot get an emission line, if you do not have an increase in the source function.

Groth: A. B. Underhill postulated that the atmospheres of some B-type supergiants of luminosity class Ia are hydrogen-poor. Our studies of supergiants (α Cyg A2 Ia, ϕ Cas FO Ia) show that the atmospheres of these stars do have normal hydrogen abundances. The Balmer series of hydrogen breaks off at n = 32-34, as one would expect for supergiants of low electron density. Using an LTE model with variable microturbulence, I found excellent agreement between calculated and observed hydrogen lines (H_γ-H_{10}) for α Cyg.

Estimates show, that the luminosity of α Cyg is very high, M_{bol} being of the order of -8^m. ϕ Cas is one of the most luminous stars of our galaxy ($M_{bol} = -8^m.9$). We don't yet have a model calculation of its atmosphere, but the appearance of hydrogen lines is quite normal. The high luminosities of α Cyg and ϕ Cas imply that these stars are evolved stars, probably further evolved, than the B type Ia supergiants. The mass of α Cyg and ϕ Cas can be estimated to be 30-40 solar masses.

Hillendahl: In the calculations mentioned above it is unnecessary to assume a turbulent velocity. The dispersion in atmospheric velocities, which occur as a result of hydrodynamic motion, fulfills the role of the turbulent velocity.

Houziaux: To fit the Balmer line profiles you introduced a change in He/H abundance. Did you find an influence on the shape of the continuum?

Underhill: I used the schematic models by Böhm-Vitense, who has only calculated the continuum. Line calculations have not been done.

Groth: At Munich we are working on the analysis of spectra of a number of supergiants (Ia, Ib) with spectral types A and F. In two cases (α Cyg, η Leo) we already have model calculations.

The observed equivalent widths of metallic lines cannot be explained, if we assume an LTE model with constant microturbulence. The abundances of weak and strong lines, even within one multiplet, differ by a factor of 10-100. If we introduce an increasing microturbulence with decreasing optical depth we get excellent agreement for the abundances of weak and strong lines.

Using a curve-of-growth analysis, we find for all supergiants a damping parameter log 2α of the order of -1 to -1.5. We know that there is only one effective damping process for low density atmospheres, namely radiation damping which should give log 2α approximately -2.5 to -3.0. Again there is a discrepancy by a factor of 10 to 100 between observed and predicted damping constants, which can be explained by the variation of microturbulence.

Underhill: Do you have spectra of high dispersion, so that you can compare the predicted turbulent velocities with the line profile?

Groth: Normally it is impossible for supergiants to prove the predicted microturbulence by the profiles. In all stars studied the shape of the profile is due to rotation or macroturbulence with a Doppler width larger than that of microturbulence.

Menzel: The departures from LTE must not be very large. If you assume changes in atmospheric structure, it is possible to fit the observations using only very small deviations from LTE.

Mugglestone: Does the dependence of microturbulence on optical depth change the line core or the wings?

Groth: The central depth of the lines becomes smaller and the wings get broader, as in the case of higher damping, if you assume that the equivalent width is the same as in the case of constant turbulence velocity.

Hillendahl: Using the microturbulence in the calculation of line profiles does mean that you introduce an additional parameter. I propose to take another free parameter. Using a linear velocity distribution you get the gradient of velocities as a new parameter. I believe that you will find the same result as before.

Groth: It is possible that a velocity gradient in the atmosphere could give similar line profiles to those which you get with microturbulence depending on optical depth. But a velocity gradient must give rise to differential displacements of lines, which are formed in different optical depths.

Pecker: Describing a velocity field by a given number of parameters is an ambigious procedure, as long as one does not define the shape of the function. For instance the functions

(a) $\xi = \xi_1 \qquad o \leq \tau < \tau_\ell$

$\xi = \xi_2 \qquad \tau_\ell \leq \tau < \infty$ (step function)

(b) and $\xi = a + b\tau$ new line have a completely different shape and are not equivalent.

If one tries to represent a solar line at the center of the disk, one can get a fit either using (a) or (b). This has been shown clearly by Roddier and Gonczi for lines of SrI.

Underhill: To get a good fit of the observations, it is necessary to use as many lines as possible. It is important to use lines of different excitation and ionization potential to find unique solution.

Hillendahl: My proposal to use a linear velocity distribution comes from the result of calculations for supergiants type Ia where I got this result.

Thomas: Do you know the mechanism producing the velocity field?

Hillendahl: The velocity field is produced by a rarefaction wave, which is excited by a shock front. (Detailed description of the processes in Hillendahl's paper.)

Pecker: D. H. Menzel has rightly attracted attention to the fact that improvement of fitting the observations by theory can be done either by working on non-LTE analysis with a classical model, or by using LTE and an improved model (i.e. an improved T_e and n_e distribution).

I do not challenge this fact. Actually a given set of observations can be accounted for by a variety of situations. This means there are indeterminacies in the problem. One must realize that the several discrepancies we have are not necessarily of the same nature. If we consider the theory of line formation, we do know the physics well, the main difficulty being either numerical or bad knowledge of physical parameters. I cannot say anything about the models. We don't know the physics of heating processes, or the effects of gravity waves etc. Therefore in a first approximation one should always use NLTE analysis for which the physics is unambiguous. The next step in learning more about the physical mechanism involved in the departures from radiative and hydrostatic equilibrium must be the comparison between the observations and the result of theoretical non-LTE treatment.

Böhm: In the case of plane-parallel atmospheres the progress of the non-LTE theory has been very large. Recently we have made NLTE calculations for spherical atmospheres. We found these calculations to be very complicated, so that they can be done only in extreme cases, i.e. planetary nebulae.

Hummer: It is possible to do non-LTE calculations in spherical geometry. If the velocity gradients are very large, then this becomes fairly simple. This has been worked out by J. Castor at JILA and has been applied to He II in Wolf-Rayet stars. Work on small or zero gradient has been performed by

Mathis at Wisconsin and Skumanich at Boulder.

Pecker: I agree that non-LTE problems can be handled mathematically for plane-parallel and spherical atmospheres, but the main problem is to understand the physics of extended atmospheres.

Thomas: The physical principles for non-LTE atmospheres are well known. It is important to use them in the right way.

Böhm: I am not sure that the physics is so simple if you take a large number of coupled terms.