Recent Progress and Future Prospects in the Study of Stellar Atmospheres

By Dimitri Mihalas
Department of Astronomy
University of Illinois
349 Astronomy Building, 1011 West Springfield Av., Urbana, IL 61801, USA

I first met Bengt Strömgren 25 years ago, in the autumn of 1963, having just finished my degree work at Caltech, and just arrived in Princeton as a Higgins Visiting Fellow. Naturally it was quite a thrill to meet personally a man who had made so many brilliant contributions to my field of study. As I got to know him I also got deeper and deeper in his debt for the many important suggestions he offered me, and for the considerable encouragement he gave me. So it is indeed a great privilege and pleasure to be here today to honor his memory.

What I hope to do this morning is to give a brief survey of where we started in this field, where we are now, and where we are going. Needless to say, I will emphasize those topics I am most interested in and know the most about; so it is likely that another worker in this field might sketch a somewhat different picture. I purposely intend to keep the talk simple and nontechnical, but I hope I don't offend anyone by being too elementary.

Stellar atmospheres was a favorite (but probably not *the* favorite) topic of Strömgren, to which he made many important contributions, and to which he returned repeatedly. Like many branches of astrophysics, the study of stellar atmospheres is relatively young, and is still changing and growing rapidly. Although radiative transport theory got started early in this century, stellar atmospheres theory per se couldn't move forward until the 1920's when Saha enunciated his ionization law, and quantum mechanics was worked out to the point that it could be used to compute reliable atomic data. Today this field is no longer being held up by the need for fundamental breakthroughs in basic physics, and progress in many aspects of the subject has become relatively dependable as computers become faster and bigger, and as atomic data become more complete and accurate.

I. *Why Study Stellar Atmospheres?*

When I first came to Princeton I was so wrapped up in the subject of stellar atmospheres that I was not sharply aware of many other things going on in astrophysics. So it was a bit of a surprise one day when Ed Salpeter, who was visiting for a few days, asked me, in effect, "Why does one even bother to study stellar atmospheres?". His point was that, after all, a stellar atmosphere contains only 10^{-11} or 10^{-10} of the whole mass of the star, and is therefore obviously inconsequential for structural or evolutionary considerations except insofar as it might influence slightly the outer boundary conditions one used to construct a stellar model. (Recall that at that time it was common to use "zero" boundary conditions – density, pressure, temperature all zero – at the stellar surface).

Given his background and interests, Salpeter's remark is perfectly reasonable. But for others of us, these layers have a great deal of intrinsic interest, far beyond the fact that they supply a boundary condition for the star as a whole. These, after all, are the layers we can *see*, and which our instruments can measure and probe (remotely of course). It is by some kind of "suitable analysis" of the light received from stellar atmospheres that we may hope to deduce: (a) The physical structure of the atmosphere, e.g. the run of temperature, density, pressure, and ionization degree as a function of depth. (b) The chemical composition of the atmosphere (and presumably, at least in most cases, the composition of the star). (c) The strength and topology of any magnetic fields in the star's atmosphere. (d) Velocity fields as a function of depth, and thus something about the dynamics of the atmosphere.

Clearly there is a rich reward for the effort expended! And, as Strömgren so beautifully demonstrated, information about effective temperatures, surface gravities, bolometric corrections, and compositions can be coupled directly into the theory of stellar evolution. And the resulting information can be coupled into a knowledge of stellar kinematics to build up a picture of the dynamical evolution of the galaxy, again a favorite area of study of Strömgren's.

II. *Development of the Basic Theory*

I would like to give here a brief summary of the development of stellar atmospheres theory based on the paradigm that the atmosphere is a radiatively dominated boundary layer that connects an equilibrium (or at least local equilibrium) interior to empty black space. In the first quarter of this century Schuster, Schwarzschild, Eddington, and Milne had formulated the transfer equation, identified the basic physical processes leading to the absorption, emission, and scattering of radiation, and had developed the basic theory of radiation transport. In the process they had been able to solve approximately the problem of radiative equilibrium (energy balance) in a grey medium, and had thus derived a rough estimate of the temperature distribution in the solar atmosphere.

But further progress was virtually halted until the development of quantum mechanics in the middle to late 1920's. Then it suddenly became possible to predict reasonably accurate values for spectrum line strengths and continuum cross-sections, hence opacities. Reliable detailed analyses of line profiles became possible for the first time, and these led to believable estimates of atmospheric conditions (e.g. temperature, density) and even some fairly trustworthy abundances. Discrepancies between predicted and observed profiles spurred people like Eddington, Milne and Strömgren to make deeper investigations into the nonequilibrium nature of the absorption/emission/scattering processes in spectral lines, and of the effects of partial coherence on line shapes. Thus many of the basic ideas necessary for a physically complete picture of line formation were already in hand by the 1930's.

By 1940 Strömgren broke new ground by developing the "model atmosphere method". In this approach one constructs, numerically, idealized stellar atmospheres based on a set of simplifying physical assumptions. The basic assumptions typically made in most of the early work are:

(1) The atmosphere is stratified in *plane homogeneous layers.*
(2) The atmosphere is in *hydrostatic equilibrium.*
(3) The atmosphere is in *radiative equilibrium.*
(4) The material is in *local thermodynamic equilibrium* (LTE).
(5) The material is *grey.*

Before proceeding farther, it is worthwhile to consider the significance and implications of these assumptions.

Curiously enough, the first assumption, which sounds relatively innocent, is probably the most far-reaching, and is certainly the most difficult to remove. By postulating homogeneous layers we reduce the problem to a strictly 1D geometry with complete translational invariance. Thus all the differential and/or integral equations to be solved are one-dimensional, and are therefore easily handled by even modest computing capabilities. If one says instead that we must treat 2D or even 3D structures, then we are immediately faced with a much more difficult problem. Not only do we require knowledge of our variables on many more grid-points, hence a much larger computational capacity, but also in order to specify the atmospheric structure we will have to solve a much more complex set of hydrodynamic or magnetohydrodynamic equations. It is only now, and then almost exclusively in solar work where we have some idea of what the structures look like, that multidimensional structures are being considered.

The second assumption merely says that the atmosphere is static (no motions), in which case the hydrodynamic equations degenerate to a hydrostatic stratification. Assumptions (1) and (2) are intimately connected because if we have dynamics the layers are unlikely to be homogeneous, and conversely if we have 2D or 3D structures they are unlikely to be static (except, perhaps, in a theoretician's model!).

Because we say the atmosphere is static (and we can, of course, neglect thermonuclear reactions) there can be no hydrodynamic work terms or time-dependent changes in the material energy density, so the atmosphere has no choice but to re-emit exactly as much radiation as it absorbs, hence to be in radiative equilibrium. It is improbable that any stellar atmosphere is strictly in radiative equilibrium; yet calculations based on that assumption agree well (sometimes astonishingly well) with observation, so the assumption is almost always invoked.

The idea of LTE has its roots in the stellar interior, where it is manifestly a very good approximation. Basically it says that we may calculate all physical variables from the standard equations of (equilibrium) statistical mechanics using the local value of the temperature and total density. The only exception to this rule is the radiation field, which is allowed to be nonlocal, being calculated from a transfer equation. In a stellar atmosphere whence photons may freely escape into interstellar space the assumption of LTE is manifestly inconsistent. Indeed, very simple order of magnitude estimates show that the state of the material is strongly dominated by radiative rates in the more rarefied outer layers where collisional rates become small, and therefore the material has no choice but to depart from equilibrium. A correct evaluation of the size of these effects had to wait another 25-30 years, and in the interim LTE was an essential (i.e. without which no modeling could be done) assumption which (amazingly!) usually gave what seemed to be good answers.

In the face of the mathematical complexity of the equations to be solved (typically nonlinear integrodifferential or partial integrodifferential equations), a numerical approach was (and still is) the only one possible. Strömgren realized that the calculation could not be mechanized at that time (computers didn't exist), but could be highly organized, which he proceeded to do. The resulting program was both powerful and flexible, but even with Strömgren's labor saving tricks it was very laborious (calculation on hand-driven or electrical desk calculators) and very slow. During the dark years of World War II Strömgren and his associates and students led the field and dominated it completely, becoming known as the "Copenhagen school" of stellar atmospheres. And after the war these same people spread out over Europe and North America, bringing the new techniques to a very wide audience.

Scientifically it was a remarkably fruitful time. In retrospect one marvels that so much could be done with such modest equipment and such crude algorithms. It is almost always the case in computational physics that the first few people who try to solve a new problem use old techniques, which usually turn out to be much less powerful and slower than the efficient methods that always seem to come once the problem is reasonably well understood. This remark certainly applies to Strömgren's pioneering efforts, which simply cried out for elaboration and generalization once automatic computation became possible.

In the 1950's the situation changed radically with the advent of electronic computers. It is almost impossible to communicate to the current generation of students, who have lived and played with computers all of their lives, what what a truly earth-shaking development even the most primitive "homemade" machines of the early 50's were! Possibly only one who has spent hundreds of hours doing a numerical integration by hand can have any idea what it meant to suddenly be able to do hours of work in seconds. Even with those early machines with their modest computational power one could begin to drop some of the restrictive assumptions that had been necessary earlier. For example it was possible to make nongrey models that allowed for the variation with wavelength of the continuous opacity (but omitted spectrum lines). And some ideas about how to enforce energy-balance (radiative equilibrium) in the atmosphere could be tested. For the first time we began to have at least a rough idea of the energy distribution of the hottest stars out into the ultraviolet. At about the same time exciting discoveries were being made using even the fairly crude models then available and primitive curve of growth techniques. In particular it was possible to establish the tremendous deficiency of heavy elements in the atmospheres of subdwarfs and thus map out some of the main features of cosmochemistry on a galactic scale. But in the absence of better models the analysis was effectively restricted to stars of near-solar temperature.

In the late 50's IBM released its famous IBM 7090, a transistorized follow-on to the 704 and 709 (which was not designed to satisfy the needs of scientists – not to mention astronomers! – but to provide a highly reliable machine for the US's Distant Early Warning [DEW] Line). A novel feature of this machine is that it came equipped with software: it had a compiler! And so the labor of coding diminished by a couple of orders of magnitude, and there was a virtual explosion of new work in the late 50's and throughout the 60's. Really good methods for solving transfer equations and enforcing the constraint of radiative equilibrium were rapidly developed. And with robust codes it became possible to make surveys of wide ranges of effective temperatures and gravities. Reasonably realistic nongrey continuum models were made from the O-stars down to the middle F's or early G's. Cooler than that line absorption becomes so heavy that models omitting lines are obviously seriously inadequate. From these efforts we were able to derive pretty good estimates of effective temperature as a function of spectral type. And it was possible to begin to make practical connections with observations by calculating, as a function of effective temperature and surface gravity, Strömgren's intermediate-band (*uvby*) colors or Johnson and Morgan's broadband (*UBV*) colors from the computed energy distributions of the models.

In the 1960's large grids of model atmospheres were constructed by several workers. And at about the same time Griem and his coworkers made a breakthrough in the broadening theory for lines of hydrogen and hydrogenic ions, and shortly afterward for He and hundreds of lines of most ions of astrophysical interest. For the first time astrophysicists were in a position to realize the promise of Pannekoek's early work and to use the hydrogen and helium lines as reliable temperature and density diagnostics (verified in the laboratory). In the early type stars (A-O) allowance could finally be made for the effects of blanketing by the lines of H, He, He+, and the strongest ultraviolet resonance lines of the astrophysically most abundant elements.

In this same era rockets were giving us our first glimpses of stellar ultraviolet spectra. A number of special-purpose codes were developed to make as accurate a fit to the H and He lines as possible.

In addition, some of the drudgery and inaccuracy of curve-of-growth analyses was relieved by codes designed to calculate profiles for many spectrum lines for a given model atmosphere, allowing fully for the depth-variation of an atom's excitation/ionization state, of line broadening constants and the Doppler velocity, and of the background continuum opacity. The codes were constructed so as to fit automatically the observed equivalent width of every line it was given and to derive an abundance estimate from each of the lines, which could then be averaged according to a prechosen weighting scheme. More sophisticated versions of this procedure included checks to see if the abundance derived from two (or more) ionization stages of the same element all gave the same result. If not, a new model could be chosen which was hotter and/or less dense, or the opposite. Likewise one could check the variation of deduced abundance with observed line-strength to see if it was necessary to introduce some ad hoc "nonthermal" velocity field in order to remove any correlation found. With these tools in hand quite a bit of new abundance work was done. Thanks to the increased precision achieved it was possible to discriminate relatively mild differences with fair confidence. For example, it was possible to show that Sirius had noticeably abnormal abundances relative to Vega, even though both exhibit what appears (at least to a casual inspection) to be a reasonably normal spectrum for its type. Moreover, attempts were made to analyze some stars which were much more exotic than hitherto attempted, for example the peculiar A-stars (Ap) and the metallic-line A-stars (Am). Not only did it emerge that each of these groups had characteristic abundance anomalies, but it was possible to demonstrate fairly convincingly that the atmospheric structure of the Ap stars is dominated by intense magnetic fields, and that elements are distributed nonuniformly into "patches" over the stellar surface. Likewise the first steps were being made towards unraveling the spectra of peculiar stars on the giant branch, and understanding the cosmochemical significance of their abundances. This is the subject of Professor Lambert's talk, so I will not pursue it further.

In the 1970's one can see at least three major lines of development of the theory. First, line-blanketed LTE models were pushed to a very high degree of realism as the quality and completeness of atomic data continued to improve (in some notable cases as a result of the efforts of the same people as were computing the models). Models were constructed with literally millions of spectral lines representing the first 3 or 4 ions of all the astrophysically abundant elements up through iron. In some cases extensive line lists from diatomic molecules were included. With the computing power available at that time (or even now, for that matter!) it was impossible to compute detailed profiles for all lines. Instead a couple of ingenious statistical schemes were devised to reduce the labor of the computation while preserving the final accuracy of the calculated energy distribution. In the *opacity distribution function* (ODF) *method*, one divides the whole spectrum into a fairly small number of bands (the goal is to use as large a band as possible while preserving accuracy). Then the total opacity (including the sum over all lines) is computed on a discrete frequency grid. Next, on the assumption that the band is narrow enough that the exact position of a line in the band doesn't matter, one rearranges the grid so that the opacity becomes a monotonic function of wavelength, resembling a "fat line". This smooth distribution is then sampled at a few representative points, and the calculation is done for each of these. The resulting reduction of the total number of wavelengths by two or three orders of magnitude makes the computation cheap enough to actually perform, while the effects of lines on both the emergent energy distribution and the temperature distribution in the atmosphere are still represented with good accuracy. Alternatively, in the *opacity sampling* (OS) *method*, much in the spirit of a Monte Carlo calculation, one simply chooses a wavelength at random, computes the total opacity at that point, and solves the transfer equation for the radiation field. As the number of sample points increases, each kind of point – pure continuum, weak line, strong line – is sampled with the correct frequency, in all parts of the spectrum, and the calculation converges to a stable result. Again, savings of several orders of magnitude are possible.

Comparison of the computed results with observed energy distributions has been extremely encouraging. For example, Kurucz has fitted the energy distribution of Vega very closely, and we can now say that we know the effective temperature and surface gravity of that star to quite acceptable precision. More recently he has been able to fill in enough missing atomic data in the ultraviolet to achieve quite a respectable fit of his calculations to the observed solar spectrum, a nontrivial achievement! Likewise it is now possible to calculate accurate photometric colors from the theoretical energy distributions, and thereby obtain reliable relations between colors on the one hand, and effective temperatures and gravities on the other. Further, predicted values of ultraviolet fluxes, which are essential in understanding the energy and excitation balance in the interstellar medium, are becoming trustworthy, a point of importance, e.g., below the Lyman limit where interstellar absorption forever prohibits our getting observed data.

In a second major thrust, efforts were made to remove the assumption of LTE, and to compute models in which the excitation/ionization state of the material is consistent with the radiation field it produces. With the advent of the CDC 7600 it was clear that we had enough computational power to handle at least the most basic features of the problem. As mentioned earlier, the primary reasons the material goes out of LTE are: (1) densities, therefore collision rates, are very low in the outer layers of a star, hence radiative rates dominate in determining the excitation/ionization equilibrium; and (2) because of the optically open surface of the star, and the presence of temperature gradients, the radiation field departs from the equilibrium (Planckian) distribution function. In reality the two effects are coupled and feed one another: the radiation field, which results from the material's absorptivity and emissivity, in turn determines the excitation and ionization of the material, hence its absorptivity and emissivity. To make the problem worse, it is easy to show that because of the dominant scattering component in NLTE source functions, photons are not absorbed and thermalized after each free flight, but rather only after some huge number of scatterings. This results in an essential delocalization of the radiation field, and implies that the interaction volume within which the radiation field at one point can affect the material state at some other point, is generally also huge. A corollary is that effects from the boundary, hence departures from LTE, can penetrate very deep (a full thermalization length) into an atmosphere. Moreover, the radiation field in any one line influences not only the two levels it couples, but, because a change in that line will perturb the rate matrix, it influences all other levels in the atom as well, and thus the radiation fields in all other lines in the entire transition array of the atom. Therefore all lines in the transition array are more or less strongly interlocked, and photons may be more or less freely shuffled back and forth among them. This fact was recognized 30 years ago by Jefferies, who pointed out that any particular photon does not have a unique "identity", but in reality belongs to a collective "photon pool". Finally, over and above these couplings, there is a global coupling across the entire spectrum via the constraint of radiative equilibrium.

It was clear that a powerful new algorithm would be required to overcome these multiple difficulties. After some experimentation Auer and I devised the »complete linearization« method, which like the Henyey method of stellar interiors, solves linearized versions of the original nonlinear equations iteratively. Mathematically the method essentially amounts to a multidimensional Newton-Raphson procedure, and converges quickly if the original solution falls within the domain of convergence. In more physical terms, the method accounts fully (to first order) for the effect of a change in any variable at any point in the medium on any other variable at any other point. Further, it allows free redistribution of photons within the transition array (thus realizing mathematically Jefferies's photon-pool idea) and over the entire spectrum (as implied by radiative equilibrium). The method thus appeared to have several promising features, and experience soon showed that the promise would be met in reality. Application of the method was not without difficulties; for example it was very hard to find adequate collision cross-sections for even hydrogen and helium, the two simplest atomic systems (and the situation has not improved much even today).

The algorithm we implemented was a direct (brute force) solution, and consequently was quite expensive. Therefore we were able to make only simplified models. We did a survey of O and B stars, including opacity from H and He, and allowing only 6 spectrum lines for hydrogen, and one each for He and He+. Despite the primitive nature of the model atoms we were able to show that departures from LTE in O-star spectra were major (essentially because of the intense radiation fields at those temperatures). In particular we showed that LTE models predicted a spurious weakening of the H lines, and led to absurdly high gravities if a fit were forced to observed equivalent widths. Likewise, LTE models predicted too-weak He lines, which would lead to spuriously high He abundances if we forced a fit to observed equivalent widths. The NLTE calculations removed both difficulties at a single blow. As modest as that achievement seems now, it was the first demonstration that a major input of new physics led to markedly better answers (a comfort to those of us who believe that good physics is the price for good answers).

The calculation of the NLTE spectrum of an impurity atom in a given atmosphere is a great deal simpler than construction of the atmosphere itself. Thus it was easy to adapt the new method to a study of abundance anomalies, and the formation of emission lines. The hardest part of setting out on one of these projects was to find adequate atomic data, and also to choose a model atom that is complete enough to treat all the important transitions explicitly while remaining small enough to fit into the constraints of machine speed and memory size. Calculations of the He I spectrum of the B-stars showed that lines located in the traditional blue-violet region of the spectrum were practically unaffected by departures from LTE, thus validating earlier LTE abundance analyses based on these lines. In contrast the lines and the yellow to red regions of the spectrum were predicted much too weak by LTE, and the new calculations removed much (but not all!) of the discrepancy with observation. The different behavior of lines in the two different spectral regions can be understood by a very simple physical argument. A similar analysis of the Ne I spectrum in B-stars showed qualitatively the same effects. But now the strong lines which were used for abundance analyses are in the red spectral region, and the too-weak lines predicted by LTE leads to spuriously high abundances. In fact it turned out that when the NLTE analysis was done the abundance dropped from 5×10^{-4}, a factor of 5 larger than the accepted solar/cosmic value, right down to 10^{-4}, the cosmic value. Another mystery solved. And similar results for Si III/IV, and a few other ions.

For the O-stars an analysis of the Mg II line at $\lambda 4481$ showed major NLTE effects which had led to an overestimate of the Mg abundance in O-stars by a factor of 10. A more interesting result was that it was possible to reproduce the N III emission lines at $\lambda\lambda 4634,40,41$ as seen in stars classified as O((f)) by Walborn. The two puzzles were why these lines, transitions from $2s^2\ 3d\ ^2D$ to $2D^2\ 3p\ ^2P$ are in emission, unlike any other line in the (visible) N III spectrum, while $\lambda 4097$, the next line in the nominal cascade sequence $2s^2\ 3p\ ^2P$ to $2P^2\ 3s\ ^2S$ remained in absorption. The resolution of these puzzles was twofold: (1) It so happens that a double-excitation state $2s2p(^1P)\ 3d\ ^2(P,D,F)$ of N III exists just above the lowest state $2s^2\ ^2S$ of N IV. Because the double-excitation state is only a fraction of a thermal energy width above the continuum threshold for N IV, electrons can efficiently recombine into that state by dielectronic recombination. Then the doubly-excited state stabilizes via a $2s2p\ ^2P$

--> $2s^2\ ^2S$ transition of the inner electron, leaving the system in the $2s^2\ 3d\ ^2D$ state of N III, just where it is needed. The process is efficient enough that the upper state of the emission lines is sufficiently overpopulated to produce the observed emission. (2) The next step is that after 3d electrons decay to 3p, they do not preferentially decay to 3s, but instead to the $2s\ 2p^2\ ^2(S, P, D)$ states. These *two-electron jumps* normally unimportant, provide the essential drain from $2s^2\ 3p$. The reason is that by another accident of nature a $2p^3\ ^2P$ state, which *can* decay directly to $2p^2$, lies at almost the same energy as the $3p\ ^2P$ state, so that the two states become very strongly mixed. A similar mechanism also works in C III.

While the work just described certainly yielded a substantial number of results of astrophysical interest, it had to be unwillingly brought a close for two reasons: First, to attack more complicated atomic systems, or even do the ones originally surveyed definitively, one would need to include many more atomic levels and transitions. With the existing algorithm that would have required a large increase in computer power. Alternatively we would have needed a more efficient algorithm. Both of these conditions have been met by now. Second, the underlying models were simply not good enough to trust for the analyses contemplated. In particular, the available NLTE models were unblanketed, which implied that they would give unreliable photoionization rates, particularly in the ultraviolet; these, unfortunately, entered the calculation in a sensitive way. On the other hand it would have been fatal to use LTE models because then a spurious thermal radiation field in the lines and in major photoionization transitions would artificially drive the atomic system under analysis towards LTE. The only solution seemed to be to face the NLTE line-blanketing problem for thousands of millions of lines. And that was a daunting prospect! Indeed, at the time it appeared impossible; nevertheless, as discussed below, we now stand on the threshold of this very achievement.

In a parallel effort, stimulated mainly by the desire to analyze solar chromospheric lines, powerful techniques were developed to treat partial redistribution (i.e. partial coherence) effects on resonance line profiles (a topic Strömgren had touched upon in the 30's). In brief it was found that photons emitted in the line core are essentially completely redistributed over the core, while photons emitted in the wings are emitted almost coherently. This coherence produces significantly different absolute intensities in the wings compared to profiles computed assuming complete redistribution over the entire line profile. A result of this change is that the temperature minimum in the solar chromosphere as inferred from strong resonance lines comes out a few hundred degrees cooler, and in essential agreement with estimates made from far-infrared data. This point is interesting because the semi-empirical temperature turns out to be *lower* than the pure radiative equilibrium value, which is counterintuitive because one expects the minimum to be a the region where shock heating of the chromosphere first becomes felt. Only when full dynamical models of shock trains in the solar atmosphere became available did we realize that radiative losses from the hot compressed material in shocks outweighs the radiative gains in the rarefaction phases, leading to a net energy loss, hence cooling of certain regions of the atmosphere!

A third line of investigation in the 70's, spurred by observations of high-velocity winds from early-type stars, was the development of techniques for treating lineformation in expanding envelopes. Sobolev had, of course, already contributed his brilliant escape-probability method, which works extremely well in relatively rapidly expanding flows (i.e. where a line shifts by about a line-width over a photon mean free path). But a full transfer treatment is indicated for trans-sonic winds where velocities range from quite small to very large. All the early work assumed planar geometry (clearly inappropriate for a wind), and formulated the problem in the laboratory frame (with the exception of a remarkable paper by McCrae and Mitra back in the 30's which solved the transfer problem in the comoving frame of the material). In the laboratory frame one must keep track of photons in a frequency band twice as wide as the frequency shift produced by the maximum flow velocity, so the method is practical only for low-velocity flows, and not too useful for winds.

In the comoving frame one needs to treat only the frequency band actually covered by line absorption, which is a decisive advantage. The differential operator becomes more complex (it contains a frequency derivative) in this frame, but the problem can be discretized and solved by techniques appropriate to partial differential equations. Solution of the equations yields the scattering term in a two-level-atom source function. To treat multi-level atoms one replaces the complete set of coupled transfer and statistical equilibrium equations by a sequence of equivalent-two-level-atom (ETLA) problems and iterates the entire set of them to self-consistency. The simple ETLA iteration scheme works well in an expanding atmosphere (or at least a lot better than for a static medium) because the medium is expanding, so that expansion-induced photon escapes outweigh the reshuffling of photons via interlocking of lines. One thus obtains a powerful and general tool for computing the spectrum from a multilevel, multi-ion medium in trans-sonic spherically-symmetric expansion. This methodology has been used for a number of investigations, including analysis of the impressive P-Cygni lines from several elements in the ultraviolet spectrum of O-stars, and He II λ4686 in the visible.

In the 80's there has been a great resurgence of interest in creating efficient algorithms for solving various kinds of transfer problems, NLTE statistical equilibrium problems, and constructing model atmospheres. These new methods, coupled to the new generation of high-speed, large-memory machines (CRAY, ETA) make possible the rapid solution of hitherto unapproachable problems. Many of these developments are summarized in the two books "Methods in Radiative Transfer" and "Numerical Radiative Transfer" edited by Kalkofen (1984, 1987). The topics discussed include fast methods of solving the transfer equation, radiative transfer in spherical media, operator perturbation techniques, and transfer of polarized radiation. The "operator perturbation" methods ultimately derive from the elegant ideas of Cris Cannon, although they are now often cast in quite different mathematical form. Most current versions are based on an approach devised by Scharmer (1981). The basic idea is to use an approximate solution, which is then iterated to convergence. The method is easily generalized to NLTE line formation in moving media (Scharmer 1984), and to multilevel problems (Scharmer and Carlsson 1985).

Scharmer's method was modified by Werner and Husfeld (1985) to solve large statistical equilibrium problems, with up to 100 atomic levels in NLTE. And Hamann (1985, 1986, 1987) adapted it to solve statistical equilibrium problems for multilevel atoms in spherical expanding envelopes. By adding constraints of hydrostatic and statistical equilibrium Werner (1986, 1987) arrived at a very efficient scheme for constructing model atmospheres for material represented by model atoms having up to 100 levels. With this method Rauch and Werner have been able to evaluate the effects of various numerical approximations and assumptions about model atoms on the structure of, and line profiles from, NLTE stellar atmospheres. We now finally know how many atomic levels, angle-quadrature points, frequency-quadrature points, etc. are required in order to fit the high S/N data that we can obtain with modern spectrographs and receivers to its full accuracy. On the whole, it is amazing how well these methods work, and how large a speedup they yield. One senses that developments in this direction have not yet been exhausted, and that they hold much promise for the treatment of dynamical atmospheres.

A different kind of scheme has been developed by Anderson (1985, 1987) for computing lineblanketed NLTE atmospheres. In this ingenious method frequencies in all the lines and continua are cleverly regrouped into a small number of "blocks". As a result, the number of variables to be determined in the complete linearization scheme is much smaller, and therefore each iteration is relatively cheap. It is now possible to treat literally thousands of lines simultaneously, thus solving both the NLTE line blanketing problem and the statistical equilibrium problem for all interesting ions at the same time! And soon it may even be possible to do such calculations on a typical virtual memory workstation! This method is really a breakthrough, and may make it possible to produce large grids of NLTE line-blanketed models in the near future. Of course the construction of such models implies that we shall need huge amounts of atomic data: (1) continuum photoionization cross-sections, (2) line strengths, (3) line broadening parameters, and (4) collisional excitation and ionization rates. Work on items (1) and (2) continues at Los Alamos and Livermore, work on items (1)-(3) is being done by the UK/US opacity group, but work on item (4) still needs to be done. It is difficult to predict when all the data needed will become available. (Lack of adequate atomic data is also a problem Strömgren encountered in his work on stellar opacities and in constructing stellar interiors and atmospheres models!).

III. *Improvements in Instrumentation*

It goes almost without saying that the inferences we make from a theoretical structure, no matter how grand, can be no more trustworthy than the data we analyze are accurate and complete. The improvement in effectiveness of modern techniques for collecting spectroscopic and photometric data, compared to the methods of, say, 30 years ago, has had as large an impact on our knowledge about stellar atmospheres as the advent of computers has had on theory. One of the really important events has been the arrival of truly linear receivers of excellent stability and great sensitivity (e.g. CCDs and Reticons), which, in addition, produce digital data directly. To understand the importance of this event one needs to recall the arcane procedure used to reduce photographic spectra: to obtain the data one had to microphotometer the plate, and then (unless some kind of special electromechanical device had been constructed to do the procedure automatically as the plate was scanned) apply the nonlinear transformation between density and intensity, sometimes by hand. The whole procedure was slow, tedious, and error prone. Nowadays one can put the data tape from the telescope directly on a computer and do the whole reduction procedure in a few seconds. The nonlinearity of the photographic plate and the difficulty in obatining an accurate calibration often led to systematic errors of the order of 10 or 20 percent. Such errors can introduce serious errors in any subsequent analysis. Modern measurements yield accuracies of about 0.1 %

Modern receivers have incredibly high quantum efficiencies in their most sensitive spectral regions, and now cover a range that was largely unavailable to workers of 30 years ago. Actually, the sensitivities are already so good that it is probably not realistic to look for major improvements in the near future. In addition, new spectrograph designs, particularly with echelles, make available vast stretches of the spectrum in a single exposure, whereas before one would have needed to make multiple time-consuming exposures. And these new spectrographs are typically being fed by larger telescopes. This is an area where we may indeed see large improvements in the next few years as mirrors of huge dimensions are constructed. Moreover, we do not require images of excellent quality for this kind of work: the telescope need only operate as a huge light bucket. And even here we may learn new tricks, for example multiplexing by using optical fibers to pipe light from several sources into several spectrographs simultaneously. And finally, the availability of digital data and the speed of modern reduction and display facilities makes real-time display possible. That can be used as a tool to save the most precious resource of all, the astronomer's time, by letting us know when the exposure is adequate (or warning us to stop if, say, we have pointed at the wrong object!).

It is probably hard to overemphasize the importance of the new wavelength bands now accessible to a stellar astronomer. The different picture we get of what a stellar spectrum looks like at ultraviolet, infrared, and radio wavelengths, compared to the visible, truly challenges (which is a polite word for "destroys") the usual paradigm upon which we have based most of our theory. In the ultraviolet we see intense chromospheric/coronal emission lines in late-type stars, and massive, high-velocity winds in early-type stars. So we must admit the need for nonradiative heating/cooling in the outer atmospheric layers, and must find acceleration mechanisms for the observed flows. Similar challenges arrive from the infrared and radio bands. The point is that in these bands we are typically observing the outermost layers of the atmosphere, where conditions may be radically different from those relevant in the visible. Instead of essentially static, homogeneous layers, the structure of the medium may be quite inhomogeneous, perhaps structured by velocity and magnetic fields, and likely subject to various kinds of (magneto)hydrodynamic heating/cooling phenomena. These views are so very different that they raise the question of "just what is a stellar atmosphere anyway?", a point to which we return at the end of this talk.

IV. *Magnetic Fields*

I would now like to say a few words about magnetic fields in stellar atmospheres. It is a subject I don't know much about, so I can be (mercifully) brief. From observations of the Ap stars we know that at least some stars have very intense, highly-organized, global-scale fields that most probably completely dominate the atmospheric structure. However, no one, to my knowledge, has put together a self-consistent model of such a stellar atmosphere, including the radiation field. One of the reasons this has not been done is that even for a static atmosphere the incredible richness of possible solutions admitted by the underlying Maxwell equations and hydrodynamic equations (even without nonlocal radiative transport!) is so great that we literally don't know quite where to start. We need at least some idea of the shapes and scales of the features we are required to model. And unless we someday get some kind of spatial resolution of a stellar disk, we are likely to be stalled for a long time because the parameter space to be searched is simply too large, and there is no guarantee of a unique result.

The situation for the Sun is quite different. Here we already have enough spatial resolution to see magnetic fields on a variety of scales. Some of what we see is quite surprising. Since the turn of the century people have known about the intense bipolar fields in and around sunspots. With the advent of the coronagraph and birefringent filter it became possible to observe spicules (jet-like surges apparently confined within a magnetic flux tube), active prominences (where we see streams of fluid motion along magnetic loops and arches), and quiescent prominences (material suspended in, and shielded from, the intensely hot corona by magnetic fields). In the early 60's Leighton discovered the supergranulation network: a network of magnetic field that outlines large-scale (30,000 km) convective flows. And magnetograms measuring magnetic fields with a resolution of a couple of seconds of arc (1500 km) became routine. By ingenious work in the 70's it was finally proven that the "general" magnetic field of the Sun actually consists of very thin (< 100 km), unresolved flux tubes, having fields of the order of 1500 gauss (almost as strong as a sunspot). These flux tubes tend to occur in +/− pairs in close proximity, so that a measurement in a large region averages over many pairs, and the "general field" of the Sun on a global scale appears to be only a few gauss. With space observations we discovered gigantic, magnetically-controlled, surges of material (superspicules) and explosive ejections of material from the corona. We also see that the corona is dominated by closed magnetic arches containing very hot, dense material, whereas the wind originates in cooler, less dense open-field regions. Presumably all of these facts also apply to other stars, at least those with gross properties roughly like the Sun. And in fact observations of solar-type stars do reveal regions like sunspots and solar plage.

Several important conclusions can be drawn from these observations. First, the higher we look in the atmosphere, the more the material is structured by the magnetic field, the more inhomogeneous a radial shell of material becomes (as we sample across different nonspherical structures), the more dynamical its behavior, and the more concepts like hydrostatic or radiative equilibrium become irrelevant. We do *not* see an evolution from a smooth photosphere to chromosphere to corona. The chromosphere is very inhomogeneous (in the sense that a photon mean free path can span several different kinds of structures). The transition region between the chromosphere and the corona is positively ragged, having very steep gradients and a great deal of small-scale structure. The corona is also quite inhomogeneous, and structurally dominated by magnetic fields.

Thus the radiation field we observe is actually the result of some very complicated nonlinear averaging over regions with extremely different physical properties. This fact not only complicates the analysis of the solar spectrum but has ominous implications for the analysis of stellar chromospheric and coronal structures, which can certainly be expected to be every bit as inhomogeneous as in the Sun. The problem is that we haven't the faintest idea what those structures look like! Magnetohydrodynamic modeling of the solar chromosphere and other magnetically dominated structures in the solar atmosphere is currently underway. At present it is quite oversimplified, though more and more realistic treatments are surely coming, because in the Sun we can at least see the structures we are trying to model, and can tell when our models do in fact fit the data. But we have essentially no guidance for other stars, and I personally believe that we should consider our present stellar chromospheric models as being only a *very rough* caricature of reality. Even stellar photospheres may not be "safe". For example, although a 1500 gauss field in flux tubes is quite strong enough to have serious structural and energetic implications in a star's photosphere, we wouldn't even be aware of its existence in other stars where we have no spatial resolution and can infer, at best, only global averages.

In short, we must always keep in mind that our usual theoretical paradigm for a stellar atmosphere may fall far short of reality, and that in one star we may actually have several quite distinct "atmospheres". These are difficult problems, and we may have to content ourselves with partial answers for a long time!

V. *Dynamics*

In some stars the dominant phenomenon seems to be ordinary hydrodynamics (i.e. not magnetohydrodynamics). A very basic pattern observed in the Sun (and therefore probably present in solar-type stars) is granulation: individual time-dependent convective cells that rise into the photosphere, radiate, and die, typically in the form of an "exploding" granule. Positioning a slit on an image of the solar surface one observes "wiggly" spectrum lines, each line responding to the quasirandom up-and-down motions of the granules. For a solar-like stellar atmosphere, where we have no spatial resolution, we would observe the spatial average of these profiles, and would get a broadened line profile. This broadening has customarily been called *microturbulence*, a terminology that has often sparked intense arguments. The appellation "micro" is appropriate in the sense that the individual fluid elements are of the same order of size as a photon mean free path. However the quantity being measured is certainly in no sense what a hydrodynamicist means by "turbulence", but is simply the ensemble average of what may actually be completely laminar flow. The distinction becomes critical as the inferred "turbulent" velocities approach the speed of sound. One of the most impressive accomplishments is solar/stellar physics in the past few years has been the development of hydrodynamic codes that accurately model the convection in 3D, and allow for radiative transfer, and magnetic fields (Nordlund 1985, Stein and Nordlund 1989). These computations yield fairly realistic maps of both the intensity fluctuations and velocity fluctuations typically seen in granulation. This work has already greatly deepened our understanding of the solar atmosphere, and similar work is likely to add tremendously to our knowledge of the atmospheric properties of other stars.

On larger spatial scales one sees the solar atmosphere oscillating, with about a five minute period, in an extremely complicated and rapidly changing spatial pattern. We understand these motions today as the result of a superposition of thousands (millions?) of high-order nonradial pulsation modes of the solar envelope, although we do not have a good picture of what acutally excites and drives them. In the solar case we can identify the modes because we have spatial resolution and can carry out the required spatial transforms of the data; but this information is again unavailable for stars. It may prove possible ultimately to detect a few low-order modes, but in the meantime all we observe for stars is something called *macroturbulence* which presumably represents flows on scales much larger than a photon mean free path, changes the shape of a line profile without necessarily changing its strength. Sensitive fourier transform techniques applied to high signal-to-noise profiles now permit a dissection of the stellar velocity field into rotational, microturbulent, and macroturbulent components. But it must always be borne in mind that "micro" and "macro" "turbulence" give us at best only two characteristic points on a distribution function, and certainly do not provide enough information for unique hydrodynamic modeling.

Regular radial pulsations of stars driven by the He+ ionization zone occur in a fairly narrow strip in the H-R diagram, and a variety of semi-regular and irregular pulsations occur in late-type giants and supergiants. The underlying mechanism, an oscillation of the ionization front inward and outward, is by now well understood, and models of these phenomena can be used to place useful constraints on the theory of stellar evolution. However the atmospheric effects of the pulsations and the outward propagating shocks they drive have been relatively little studied theoretically. Part of the reason is that present atmospheric models of pulsating stars aren't very good. The codes are almost always Lagrangean and use the diffusion approximation in treating the radiation flow. As a result of coarse zoning and inadequacies of the diffusion approximation the computed emergent flux has large spurious "bumps" and "wiggles", and is not too reliable. These are not the result of numerical instabilities, but of a failure to resolve, on the computational mesh, critical structures such as the emerging shocks and the ionization front. Until the time that this difficulty can be overcome, we will not get even accurate light curves from the models, not to mention spectra! That's a pity because good spectra, showing fascinating shock phenomena such as line splitting and emission, have existed in the literature for over 25 years. But it is no surprise that the problem has not yet been solved because it would require a full NLTE treatment in a dynamical atmosphere (in which the important structures had been resolved!). That is a rather daunting prospect, yet I believe that it is within reach within the next decade provided that we do solve the computational problems with the dynamics.

One of the most interesting and successful developments of stellar atmospheres theory over the past 15 years has been the working out of a theory of radiatively driven winds, which are observed ubiquitously in high-luminosity stars. It was known from the outset that radiation forces on the continuum opacity alone could not drive a wind. Lucy and Solomon (1970) showed that direct radiative momentum deposition in strong resonance lines in the ultraviolet could, in fact, induce a transsonic flow, and Castor, Abbott, and Klein (1975) quickly showed that the observed mass-loss rates could be matched when one accounted for hundreds to thousands of lines in the spectrum. The first version of the theory has been elaborated and refined to include effects of the rotation and finite angular size of the star, a complete line spectrum, improved ionization balance, line overlap, and multiple scattering of photons back and forth across the volume contained within their successive resonance surfaces. This last point has turned out to be particularly important, because a photon has a very large energy relative to its momentum (compared to massive particles), and thus it can be "robbed" of its full momentum many times before its energy gets "used up". Thus photons can actually multiply their driving effect manyfold, and calculation shows that even the large mass-loss rates of the WR stars can be driven in this way. A related phenomenon is that because the expanding envelope can backscatter excaping photons back into the stellar photosphere where they thermalize, the star suffers a kind of "wind-blanketing" and backwarming which may have important consequences for the visible spectrum. This effect is now known to be so large as to have a major impact on the derivation of effective temperatures, surface gravities and abundances from O-star spectra.

I think that most people would agree today that CAK theory provides a pretty good basic description of the supersonic part of the flow, in particular of the acceleration mechanism that produces the high observed terminal velocities. Nevertheless there exist at least two outstanding problems that are being worked on actively at present: (1) The energetics in CAK type models is rudimentary; indeed the radiative scattering is assumed to be strictly conservative, and the gas is therefore adiabatic (or forced to be isothermal by fiat). This is probably not a serious problem for the supersonic flow region, but may be of much greater, even dominating, importance in the subsonic flow. And it should be remembered that it is the subsonic flow region that determines the mass loss rate, because once the flow passes sonic, no amount of energy or momentum input can alter the mass flux (only the terminal velocity). Accurate modeling of this flow regime, including an accurate treatment of the radiative transfer, full energetics of the gas, rotation, and perhaps magnetic fields, remains to be done, and may occupy us for some time. (2) Another important aspect of radiatively driven winds is that they are unstable. Small disturbances in the flow rapidly grow as they are swept downstream, and produce strong shocks. The interaction between radiation forces and the shocks seems to be reasonably well understood. The shocks are strong enough to produce very high temperature plasma that can emit soft X-rays. Early attempts to explain the X-rays in terms of a thin corona failed, and pointed to a source distributed throughout the wind, in agreement with the present picture.

Supernova envelopes are another example of coupling between radiation and hydrodynamic flow. The strong shock created in the bounce of the envelope from the collapsed core is supercritical, and strongly dispersed by radiation. At the time of shock breakout and unloading an intense burst of radiation emerges. Current codes describe these phases rather crudely, using, at best, some kind of flux-limited nonequilibrium diffusion, and usually not event that. A proper transport treatment of the radiation, taking into account that the opacity in the envelope is typically completely dominated by electron scattering, would be fruitful, and seems easily within reach. Modelling of supernova spectra is considerably more well advanced. At present the codes solve the full transport problem for an expanding spherical envelope, using a relativistic formulation that allows one to account for all velocity-dependent terms. In general one obtains quite convincing fits to the observed spectra, and it has been possible to infer both physical conditions and element abundances in the ejecta. An unusual complication, not normally met in ordinary stars, that enters is that element abundances in the envelope may be stratified because of the effects of thermonuclear reactions. At later stages a supernova envelope becomes very distended, and NLTE effects must become quite important owing to both dilution of the radiation field, and low densities. A few exploratory investigations have been done thus far, and this looks like a good area for rapid development in the future.

The situation with novae is much less satisfactory. Modeling a nova spectrum is extremely difficult because of the complicated geometry of the emitting medium: two stars, a disk, an expanding envelope, and perhaps other structures. There are no easy, but good, approximations: The material certainly does not have a 1D symmetry. A 2D calculation (difficult enough) fails because a disk in the system precludes axial symmetry around the axis joining the stars. So nothing less than a full 3D calculation is needed, and this appears to be beyond our present capabilities. Some novel Sobolev type treatments have been developed in the USSR; these may fill the gap until a full transport method becomes available. It will be interesting to see how much progress can be made on this problems in the next ten years.

VI. *Geometry*

We usually take for granted that we know the basic geometry of the objects we are trying to model, and we tend to forget how restricted our methods really are. As often as not our assumption is not really true, and it seems worthwhile to mention here a few points about global geometry. The solution of 1D transfer problems, whether in planar or the spherical geometry, is now well understood and needs little comment. One aspect of the spherical case worth mentioning is the question "What is a stellar radius?". In the planar limit the thickness of the atmosphere is so small compared to the stellar radius that the definition of the radius is unambiguous. But in a very distended envelope the radius of the surface of unit optical depth in the most transparent continuum may be a small fraction of the surface of unit optical depth in a strong line (or continuum edge). Consequently the size of the emitting surface of a star can be a strong function of wavelength, and it is not at all obvious what the usual stellar interiors convention of choosing the stellar radius to be the radius of the surface of Rosseland optical depth = 1 (or 2/3) means. It has always surprised me that those who make models of giants and supergiants, have not attempted to eliminate possible ambiguities and problems by using detailed model atmospheres for their outer boundary conditions.

For a rotating star we expect some kind of oblate spheroidal shape. We thus have

2D axial symmetry, and methods for treating radiation transfer in that symmetry exist. (At present they are rather clumsy and expensive, but it is reasonable to expect significant improvement over the next few years). These methods might also prove useful for, say, single (proto)stellar objects with bipolar jets, with or without an accretion disk. Axial symmetry would also be appropriate to weakly interacting binary stars if each star has a rotational symmetry around the line joining their centers (e.g. the stars are prolate spheroidal). Likewise for any diffuse matter in the system. But as the members of a binary get closer and interact strongly, the stars become distorted, and they become immersed in a common envelope of unknown shape. In addition, as sketched in bewildering detail in Otto Struve's old book "Stellar Evolution", there may be a considerable amount of radiating gas in disks, spirals, and streams. We are still a long way from being able to calculate the radiation field from such geometrically complex objects, except possibly by Monte Carlo methods. And even if that were not the case, we would still have the deeper problem of knowing what structure to choose to model!

VII. *Spatial Resolution*

I have alluded several times above to the problem that we have no spatial resolution for stars and therefore cannot choose a theoretical structure uniquely. Indeed, except for the Sun we cannot really answer even the question "What does a stellar atmosphere actually look like?" The things we would like to know are quite basic: We would like to know what the limb-darkening is for a given star, especially giants and supergiants, and most late-type stars. We want to know whether we can see convective patterns like granulation and supergranulation. We would like to know if there are sunspots, flux tubes, and magnetically-dominated active regions. We would like to know if the star has a chromosphere and corona, and, if so, how inhomogeneous they are. We want to know if the star has analogues of spicules and prominences. We want to know what the huge regions, which appear to be gigantic "starspots", on RS CVn stars really are. We would like to know what the magnetic field structures in an Ap atmosphere are, and what the regions of differing element abundances are and imply. We want to be able to detect nonradial pulsation modes. And we need even gross geometric information about the distribution of gas in close binary systems.

What are the prospects for ever getting this information? One immediately thinks of interferometry. Until now this technique has yielded only stellar diameters, which one might regard as the "zero-order" term of limb-darkening. At the present there are projects underway to improve greatly the amount of information we can recover from speckle interferometry, and to build a powerful new Michelson interferometer. Some of the information described above requires only a few resolution elements on the stellar disk, but the rest requires many. Even though I am hardly an expert on stellar interferometry I strongly suspect that we will not obtain the kind of information needed from ground-based observations because of the problem of overcoming seeing effects at long baselines. Beyond ground-based interferometry lies perhaps a huge telescope or a gigantic interferometer in space or on the moon. Such an instrument would be incredibly costly, and would take a very long time to build, even if it were ever approved. So at the risk of sounding unduly pessimistic, I would guess that at a meeting like this held, say, 20 years from now, we will have little, if any, *direct* new observational information about the structure of stellar atmospheres, and would still be talking about the same problems.

Of course there are some indirect techniques, but these have only a limited capacity to obtain the kinds of information we want, and always require considerable reduction and interpretation. In addition it may be possible to get some information, for example about granulation-like inhomogeneities, from numerical simulation, but then one is introducing theory into the process of deciding what kinds of structures must be considered by the theory, which may lead to circular reasoning. In short, it seems to me that we are not likely, with a high degree of confidence, to make major improvements in the structural assumptions underlying the models and that we will still be working with highly idealized and oversimplified models for some time to come. I therefore think that it will behoove all of us (especially the model builders!) to remember the significant limitations of our models, and to use the models with caution in areas they are unequipped to address.

VIII. *Prospects*

It seems appropriate at this point to adress briefly the questions "How good is our modeling, and where do we go from here?". I think that it may be fair to say that the quality of a model is, like beauty, in the eye of the beholder. Those who have developed the numerical techniques that have moved us so far forward in the past two decades can rightly be proud of the fact that we can now actually solve many problems, where before we could only make rough guesses. But those aware of all of the physics left out of the formulation, and those who have looked at solar observations long and hard in order to learn what a "typical" star might look like, will quickly point out the flaws. I would like to steer a middle course.

First of all, the place where we do best is near (or below) the main sequence, where stars have moderately compact atmospheres. I think it is safe to say that we essentially understand continuum and line formation in the photospheres of normal upper main sequence stars, at least to the extent that purely radiative models are appropriate at all. Thus Kudritzki and his coworkers, for example, have been able to demonstrate a very close correspondence between the computed spectrum of O-stars and high quality observations. It now appears that we can deduce effective temperatures, gravities, and abundances for these stars and can model their ultraviolet spectra with good reliability. On the other hand, it has long been known that most upper main-sequence stars show a considerable amount of "microturbulence", "macroturbulence", or both, in their atmospheres, along with rapid rotation. There is no question that hydrodynamic motions occur in the atmospheres, but we do not yet know what they are (waves? nonradial pulsations?), nor do we know whether they play an important role in either the energy or momentum balance in the atmosphere, or create inhomogeneities that have a significant effect on the radiative signature we receive. Unfortunately there doesn't seem to be much work being devoted to these important questions at present, and so it is impossible to guess when we might learn the answers.

We also seem to be able make qualitatively correct models of OB-star winds, and to estimate mass loss rates, though we certainly do not yet have self-consistent models including instabilities and shocks. And recently Hamann (1985) and Hillier (1987a, 1987b) have been successful in fitting the spectra of Wolf-Rayet stars, and in deriving a semiempirical model of a WN envelope. Nevertheless we must remember that the structure of the subsonic flow region has yet to be worked out, and the impact of nonradiative energy inputs (e.g. nonradial pulsation) from the interior of the star has yet to be understood. For all we know now, the ultimate driving force behind the wind might be photospheric and subphotospheric motions (as argued by Thomas). Things are much worse for the Be stars. Not only is there some kind of ill-defined flow (disk-like?) whose morphology we don't know and whose cause we don't really understand, but there is ample observational evidence by Doazan and her coworkers indicating that the Be phenomenon is episodic, recurring at irregular intervals. Furthermore they show that Be stars are not "special" objects in the H-R diagram (e.g. rapid rotators near "breakup" velocity), but that even "ordinary" B-stars can become Be-stars for a time, and vice versa. In my opinion, we have yet to take even the most basic steps towards a realistic model for Be stars.

As we go down the main sequence, line-blanketing becomes increasingly severe and at some point our present NLTE models fail because they do not yet account for line-blanketing adequately. We may be able to overcome this problem in the next few years thanks to Anderson's new method. But even if we can get through the G-stars, the K-stars and M-stars promise to be much harder because of molecular blanketing, and larger convective and magnetic effects on the structure of their atmospheres. In all cases we need to answer someday the question of to what extent convective/magnetic inhomogeneities in an atmosphere "average out" (nonlinearly!) so that we can describe the atmosphere as being "effectively homogeneous", and to what extent we must consider the atmosphere to be composed of physically distinct components.

If we turn to giants and supergiants, my reaction is to wring my hands. I know, of course, that some brave people have modeled such stars, but I personally feel that it is a problem of almost hopeless difficulty for our present tools and paradigm. From the great size and low densities of these objects it is obvious that departures from LTE will be extreme. Furthermore, from the small irregular fluctuations in their light, colors, and of their line profiles, it is obvious that there is a great deal of hydrodynamic motion going on, and indeed that the atmosphere may be "unstable" in the sense of local areas jittering around randomly with relatively large velocity amplitudes. It may well be that the key word is "random", and that we will need to develop some kind of *statistical models* of these stars, and a statistical transport theory (on which important work has already been done) to compute their emergent spectrum. I think that it is essentially impossible to predict the rate of progress here, but given the general reticence of astronomers for dealing with hydrodynamics, I am not too optimistic about it being large.

Likewise, prudence (or cowardice?) has delayed my mentioning stellar chromospheres and coronae. Here I have a rather pessimistic view: I think that until we have a detailed physical understanding of the solar chromosphere and corona; until we have some kind of empirical indication of the topology and strength of magnetic fields in stellar chromospheres and coronae, and of the nature of the material inhomogeneities they induce; and until we have the ability to calculate the magnetohydrodynamic behavior and NLTE radiant output of 3D structures, possibly embedded in an ambient wind, then I think that we are likely to make little if any real progress. I know of course that there have been many papers, often based on IUE data, written about stellar chromospheres. But, with all due respect to their authors, I personally think that we do not learn much of permanent value from them because they invariably assume a model that is manifestly extremely oversimplified.

Finally, we should return briefly to the very basic question "What is a stellar atmosphere (anyway)?". I have tried to make it clear that a real stellar atmosphere is, in general, radically different from the typical theoretician's conception of it. The primary reason that this is so is, I believe, that none of us has ever seen a stellar atmosphere (other than the Sun's), and so we can oversimplify with a clear conscience. Further, we don't know how far out in the atmosphere we have to go before interesting and important things stop happening. Is just the first few scale heights above optical depth unity enough? Or do we need to go out through the (inhomogeneous) chromosphere and corona? How about the wind? (Certainly needed for WR stars!) Is going out to the sonic point enough to avoid further effects of the wind on the star? (Not for an O-star in which the wind-blanketing heats the underlying atmosphere to a much higher temperature and changes the emergent energy distribution!) What about dust shells? Are they really separate objects surrounding the star, or should they be considered to be a part of the atmosphere, given that the optically thick shell can absorb a significant fraction of the stellar flux at some wavelengths and re-emit it at others? And what about binaries: when does one star stop and the other begin; what about a common envelope?

In no case do we know how to answer these questions precisely enough to formulate unique equations and algorithms. But that is not surprising because these questions are not easy, and the answers cannot be expected to be "tidy". And so for the next few years, at least, we will have to content ourselves with partial answers, which, nevertheless, if properly understood, can still guide our research. In any case I want to stress that the field is in a state of rapid change. The next 20 years or so will certainly be an era of rapid growth in our knowledge about stellar atmospheres; one that will surpass the past 20. It will be a time which, I am sure, that Strömgren would have enjoyed greatly.

Conclusion

To conclude, I would like to end this talk on a more personal note. Bengt is gone now. And there are many here who will miss him deeply, whether as friend, teacher, or colleague. But I would like to remind all of us of the old saying that "*No man is truly dead until the day his name is last spoken*". On that count Bengt will remain alive amongst us for many, many, years to come!

References

Anderson, L.S. 1985, *Astrophys. J.* **298,** 848.

Anderson, L.S. 1987, in *Numerical Radiative Transfer,* ed. W. Kalkofen (Cambridge University Press) p. 163.

Castor, J.I., Abbott, D., Klein, R.I. 1975, *Astrophys. J.* **195,** 157.

Hamann, W.-R. 1985, *Astron. Astrophys.* **145,** 443.

Hamann, W.-R. 1985, *Astron. Astrophys.* **148,** 364.

Hamann, W.-R. 1986, *Astron. Astrophys.* **160,** 347.

Hamann, W.-R. 1987, in *Numerical Radiative Transfer,* ed. W. Kalkofen (Cambridge University Press) p. 35.

Hillier, D.J. 1987a, *Astrophys. J. Suppl.* **63,** 947.

Hillier, D.J. 1987b, *Astrophys. J. Suppl.* **63,** 965.

Kalkofen, W. (ed.) 1984, *Methods in Radiative Transfer* (Cambridge University Press).

Kalkofen, W. (ed.) 1987, *Numerical Radiative Transfer* (Cambridge University Press).

Lucy, L., Solomon, P. 1970, *Astrophys. J.* **159,** 879.

Nordlund, Å. 1985, *Solar Physics* **100,** 209.

Scharmer, G.B. 1981, *Astrophys. J.* **249,** 720.

Scharmer, G.B. 1984, in *Methods in Radiative Transfer,* ed. W. Kalkofen, (Cambridge University Press) p. 173.

Scharmer, G.B., Carlsson, M. 1985, in *Progress in Stellar Spectral Line Formation Theory,* ed. J.E. Beckman and L. Crivellari (Reidel) p. 189.

Stein, R.F., Nordlund, Å. 1989, *Astrophys. J. Letters* **342,** L 95.

Werner, K. 1986, *Astron. Astrophys.* **161,** 177.

Werner, K. 1987, in *Numerical Radiative Transfer,* ed. W. Kalkofen (Cambridge University Press) p. 67.

Werner, K., Husfeld, D. 1985, *Astron. Astrophys.* **148,** 417.