Part II.

General Summary of Results on « Astronomical Turbulence » in Stellar Atmospheres.

Discussion.

Chairman: A. Unsöld

(*Ed. Note*: The discussion opened, as it did in Part I, with questions by aerodynamicists on the astronomical jargon. A first part, devoted to clarification of the meaning of the stellar classification scheme in its implications on the physical characteristics of the stars discussed, is condensed and summarized. A second part began as a request for a re-explanation of the curve of growth, and continued as a question on the reliability of this technique. The explanation has been suppressed as duplicating material in Part I; the questions on the reliability of the technique have been retained as the beginning of the discussion proper. Miss Underhill has also added an explanatory section in her text, immediately adjacent to Table I, to clarify some of these points.)

Summary of physical implication of classification scheme, based on remarks by A. Underhill, A. J. Deutsch, E. Schatzman, A. Unsöld.

The spectral class of a star is specified by a letter; its luminosity class, by a roman numeral. The spectral class was originally a wholly empirical assignment, based on an empirical regular progression in behavior of the features of the spectrum of the star, pre-dating the development of theoretical understanding of atomic spectra. The Saha-Fowler introduction of thermodynamic-equilibrium statistical mechanics to describe the ionization and excitation state of the gas in the stellar atmosphere, treated as an isothermal region, showed that the spectral sequence could be interpreted in terms of a monotonic decrease of this atmospheric temperature from the « bluest » stars (type O) at one end of the sequence to the « reddest » (types R, N, S), at the other. Moreover, again applying thermodynamic equilibrium relations, roughly this same temperature value gave a good description of the distribution of energy in the continuous spectrum, and the total emission per unit surface area. A rough theory of radiative transfer through the atmospheric regions gives a reasonable

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quantitative representation of the direction of difference in temperature values—for given spectral class—needed to represent spectrum, distribution of energy in the continuum, and surface-flux of radiation. Excitation and ionization temperatures, T_{ex} and T_{ion}, refer to the line spectrum; color temperature, T_c, to the distribution of energy in the continuum; effective temperature T_{eff}, to the surface-flux of radiation. Roughly, temperatures vary from 30 000° for an O star to 2 000° for a K star, in the normal atmospheric regions. (This takes no account of chromospheric or coronal phenomena.) Any star with a temperature above about 10 000° is called « early-type »; anything cooler is called « late-type ». (For detailed discussion of all these points, cf. A. UNSÖLD: *Physik der Sternatmosphären.*)

The Russell-Herzsprung diagram was originally a wholly empirical discovery showing that total luminosity of the star and spectral class were not wholly independent, nor were they single-valuedly related. Three broad luminosity groups were originally found for a given spectral class: supergiants, giants, and dwarf or main-sequence stars—in order of decreasing luminosity. Only later was it found that these terms also refer to stellar dimensions, and that the pressure in the atmospheric regions varies oppositely to the luminosity, giving rise to measurable spectral differences, which permit luminosity classes to be established from spectral measures alone. The classes are now sharper than the supergiant, giant, dwarf categories: roughly, Ia and Ib refer to the first; II and III, to the second; IV and V, to the third. Pressures in supergiant atmospheres are roughly 100 times less than in dwarf atmospheres.

Current theories of stellar evolution regard the mass of the star as the basic physical parameter varying along the main sequence. A star condenses out of the interstellar medium, taking very quickly a place on the main sequence determined uniquely by its mass. The star remains on the main sequence so long as it generates energy wholly by thermonuclear processes, then moves off to the right of the main sequence into the giant or supergiant region depending upon its mass, and ultimately comes back to the left and falls into the white dwarf category. The essential point here is that the place the star occupies on the diagram is a unique function of its mass and the degree of exhaustion of its thermonuclear resources. (For further reference, cf. M. SCHWARZSCHILD: *Stellar Evolution.*)

— E. N. PARKER:

A question on the curve of growth. There are gradients of temperature, and various lines may come from different heights in the atmosphere. To what extent is the curve-of-growth analysis uncertain due to temperature gradients, and the fact that various parts of individual lines would come from various levels. What I am driving at is the physical significance of micro-turbulence. I am not yet convinced it exists, and I want to hear some arguments on this

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point. Can it be shown by formal calculation that we must believe in microturbulence as turbulence, or should we call it a discrepancy factor to be settled in the future?

— M. Minnaert:

Actually, for each line the curve of growth should be different, and should correspond to slightly different atmospheric layers. The flat section will come slightly lower or higher, and this might confuse us and modify somewhat the microturbulence found. But these are refinements which do not remove the well-established main effect of microturbulence found by everybody.

— A. Underhill:

I think the uncertainty due to the fact that really the physical quantities vary through the atmosphere, will introduce something of the order of magnitude of 0.1 in the log as a probable error in the velocity. One could of course refer to what we call microturbulence as a discrepancy factor. The factor was originally given the name « turbulence » because it was recognized that a random velocity, entering the curve-of-growth structure in the same manner as the thermal velocity, would act in exactly the correct way to remove the discrepancies from results based on thermal velocities alone.

— A. Unsöld:

We should follow Parker's question concerning the significance and accuracy of the micro-turbulent velocities ξ_{turb} determined from curves of growth somewhat further. The thermal velocities ξ_{therm} of heavier elements like Ti, Fe... in stars of medium temperature like the sun ($\sim 6\,000\,°\mathrm{K}$) are of the order of 2 km/s. If micro-turbulent velocities are added, the Doppler width $\Delta\lambda_{\mathrm{D}}$ of a line increases in the ratio $\sqrt{(\xi^2_{\mathrm{therm}} + \xi^2_{\mathrm{turb}})/\xi^2_{\mathrm{therm}}}$ and the almost horizontal part of the curve of growth moves upward in the same way. The question is, how accurate can its location be determined from the theory of stellar atmospheres in case of no turbulence? The answer is that the intrinsic uncertainties of the model atmosphere plus the errors of (reasonably good) measurements produce an inaccuracy in the height of the flat part of the curve of growth of about ± 30 percent. That means: Turbulent velocities of the order of 5 to 2 km/s or larger can be determined quite well and are certainly real. It should be noticed further that the observed micro-turbulent velocities are subsonic, relative to the velocity of sound in hydrogen, which is the most abundant element.

Next, I don't understand why Parker emphasizes so much the temperature gradients. In computations based on a model atmosphere, the temperature gradients in the atmosphere are taken into account.

— E. N. PARKER:

I find in the literature wide variations in estimate of temperature and temperature gradient in the region of line-formation. What does one use?

— A. UNSÖLD:

Well, there is the question of how accurate are the calculations of model atmospheres. Up to about 1946, we used so-called « grey models », based on the assumption of no frequency-variation of continuous absorption coefficient, and we know now that this simple assumption must be amended. The temperature actually decreases toward the surface faster than assumed earlier. However, the calculation of a really good model atmosphere is a lengthy job, and has been done so far only for a few stars.

— A. UNDERHILL:

Everyone is worried about details; and as UNSÖLD has emphasized, to obtain detailed answers requires much detailed analysis. The whole of the data I have presented has been obtained by straightforward and simple methods of analysis. A few detailed cases which UNSÖLD has worked out, and tried to improve by taking into account these physical details more correctly, has confirmed that these numbers give the proper order of magnitude. But please don't think that these numbers have all been ascertained by as detailed methods as he has mentioned; they have mostly been obtained by quite crude analysis.

— M. J. SEATON:

If one considers micro-turbulence inferred from curve of growth to be a discrepancy-factor, then at least one always gets positive turbulence velocities. If one ascribed the factor to non-LTE effects, would he expect the discrepancy to be always of the same sign?

— R. N. THOMAS:

Yes. Non-LTE effects make the line deeper.

— R. B. LEIGHTON:

On the question of spectral lines coming from different levels, it is not clear to all the aerodynamicists why lines coming from a higher state of excitation tend to be formed lower in the atmosphere than those from states of lower excitation.

— A. UNSÖLD:

The temperature increases as one goes deeper into the atmosphere—we do not consider here the chromosphere. Thus, excitation and ionization follow the Boltzmann and Saha equations, and excitation increases downward. (*Ed. Note*: Relative to the chromospheric influence on position of line-formation, cf. ZIRKER: *Ap. J.*, **127**, 680 (1958)).

— W. B. THOMPSON:

Come back to Parker's original question. Could we be told exactly what physical assumptions underlie the curve-of-growth analysis, and what sort of calculations have been carried out? It seems to me that the hydrodynamic problem posed in finding these extremely high, very fine scale velocities is really a very severe one, and we should like to understand just how sure you are that these results are actually meaningful.

— A. UNDERHILL:

You put two types of questions forward. Let me answer the second: why are we sure that great velocities and peculiar velocities exist? The quantity called micro-turbulence is based on an analysis using only equivalent widths—the integrated line-profile. The line-shape does not enter. In my opinion, too much detail cannot be gained from this quantity; one can get only an order of magnitude of what may be called a discrepancy factor. The only way you can find detail is when you turn to macro-turbulence, the analysis of line-profiles. Here the sun comes into its own; stars cannot be studied in all desired detail.

— A. J. DEUTSCH:

I would present evidence for the reality of at least the larger values of micro-turbulence listed by Miss UNDERHILL. When the micro-turbulence gets larger thant $(2 \div 3)$ km/s, as it commonly does in giant stars, we then see it affecting the lines in two ways; it changes their equivalent widths and it also changes their profiles. The profile broadens by an amount which I think can be shown on rather simple grounds to be inadmissibly large to be accounted for in terms of temperature gradients in the stellar atmosphere. For example, in stars where we know that the atmospheric temperatures in the relevant layers are of the order of 5 000 °K, and the thermal velocities of the order of 2 km/s, nevertheless the line has an overall width of 5 or 6 km/s, while still not showing damping wings. I think the only way we can understand this is to suppose that, in addition to the thermal motion, there is another kind of motion; this has been called turbulence. I would like to concede that one does not have this kind of evidence for the sun or for most stars like the sun. But, as we pass to the stars where what we called the turbulence velocity becomes higher, we get a transition region where simultaneously we see the effect of the raising of the horizontal part of the curve of growth, and the widening of the line-profile, which I believe cannot be interpreted in terms of temperature gradients. Of course, when we go to the very extreme case of the super-giants, this becomes exceedingly obvious. There, however, the velocity has become sufficiently large that the principal effect lies in the broadening of the profile and we no longer have the principal effect in the equivalent widths. I think it is partly for this reason—that when we go to the cases of extreme turbulence we get this additional evidence from line profiles—that many astronomers have been reluctant to abandon the idea that there is a similar phenomenon at work here on the sun and in stars like the sun.

— A. UNSÖLD:

What you show, is that in every case where one has pronounced macro-turbulence, one has also—as he would expect—micro-turbulence, of somewhat smaller size. There remains the question of how do we establish the values of these turbulent velocities.

— M. KROOK:

I am correct in saying that the way one makes these calculations is to assume that there are no motions other than thermal, and neglects the lines, then computes the thermal structure of the atmosphere? Then using this thermal structure, you compute what the lines would look like, again in an atmosphere with only thermal motions? If you find a discrepancy, you assign it to turbulence? In other words, one does not calculate the formation of a line in an atmosphere in which turbulence is actually present, and may affect both thermal structure and line absorption coefficient?

— A. UNSÖLD:

Analysing a stellar spectrum is like solving a cross-word puzzle. You have quite a number of constants to determine from a great many observational data, and you begin from some starting approach and proceed until something doesn't check for consistency. Then you start again. It is difficult to explain the whole procedure quickly. The detailed analysis of one stellar spectrum by an experienced man takes about 2 years.

— W. B. THOMSON:

Can you calculate the atmospheric structure, taking into account the convection and turbulence?

— A. UNSÖLD:

In general, one does this. The influence of the turbulent velocities on the stratification of the atmosphere through the dynamical equations is rather small. The essential point in the analysis of a spectrum is first to get a reliable value of the temperature structure, because temperature enters in a very sensitive way into all the subsequent calculations. Subsonic micro-turbulence produces only minor correction on the temperature structure.

— E. N. PARKER:

Where can I find how this correction to the thermal structure is calculated? This is of interest to the aerodynamicist because this is the aerodynamic part of the determination of structure.

— A. UNSÖLD:

This point is unimportant for what Miss UNDERHILL has been talking about.

— H. PETSCHEK:

We are asking not only about convection due to turbulence, but also about the energy dissipation due to large-amplitude velocity. Can you prove what you said, simply?

— R. N. THOMAS:

The argument is that micro-turbulent velocities are about 2 km/s; thermal velocity, 10 km/s; thus Mach number, about $\frac{1}{5}$, so energy dissipation from micro-turbulence is small. The question remains about the implication of the macro-turbulent velocities quoted by Miss UNDERHILL, where the Mach number considerably exceeds one. The temptation among astronomers is to say: if we have to correct something, it lies in this latter aspect. This is a summary of a viewpoint, not a defense of it.

— A. UNSÖLD:

Agreed on the micro-turbulence. In the chromosphere and corona, where the heat dissipation is a vital point, there are of course a different set of problems, but these will be discussed later.

— W. B. THOMPSON:

If micro-turbulent velocities were observed to be supersonic, I think it would be an extraordinary thing from the hydrodynamic viewpoint. If they are subsonic, it would be surprising if they did not actually exist. So let me ask for complete clarification on one point. In *micro*-turbulence, not *macro*-turbulence, do you ever see extreme supersonic velocities?

— A. UNSÖLD:

From Miss Underhill's results, *micro*-turbulent velocities are of the order $(2 \div 5)$ km/s, which is the same size as, or larger than, the thermal velocities

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for the atoms observed, which are the heavier elements. But the sound velocity in the atmosphere is practically that for hydrogen, some $(8 \div 10)$ km/s. Therefore the observed micro-turbulent velocities are generally subsonic, and their dynamical pressure as well as their energy dissipation are of secondary importance.

(*Ed. Note*: The significance of such a generalization on the empirical values for micro-turbulence from curve-of-growth studies cannot be overemphasized. Miss UNDERHILL gives values up to 20 km/s for stars in luminosity class I*a*; the summary by K. O. WRIGHT, *Trans. Int. Astr. Union*, **9**, 739 (1955) gives variety of cases exceeding 10 km/s for a range of spectral classes. One asks the significance of such results, relative to Thompson's question, to the applicability of the curve-of-growth methodology, and to the accuracy of the empirical results.)

— G. ELSTE:

Consider a simple picture showing up micro- *and* macro-turbulence in an atmosphere with a single velocity field. There may be a large outward motion in deep layers which becomes smaller and smaller with increasing height. And at another point of the atmosphere there may be inward motion with a certain velocity gradient. Consider a line being effectively formed in a certain layer of finite thickness. Take the average velocity over this layer in both the upward and downward moving region. The difference of these average velocities will smear out the spectral line without changing its total absorption. This we call macro-turbulence. The scattering of the individual radial velocities within the layer around the average velocity in each region acts on the line like an additional kinetic temperature and changes its width as well as its total absorption. This effect we call micro-turbulence. For another line, having different excitation conditions, the position and thickness of the absorbing layer may be different, resulting in different behavior of the line.

— M. MINNAERT:

ELSTE has given a very precise account of an observational situation. These effects are observed at least as well at the limb as at the center of the solar disk. This shows that there are tangential as well as radial currents; apparently there is a field of large and small scale random velocities, which one should be inclined to connect with the occurrence of vortices, and which astrophysicists usually call turbulence.

— A. J. DEUTSCH:

It is my understanding that, as astronomers employ the terms, convection and micro-turbulence are not the same thing. I think that those of us who are persuaded of the existence, at least in some stars, of micro-turbulence are

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by no means persuaded that this is a convective kind of circulation. Isn't it true that the relevant layers in the solar atmosphere are in radiative equilibrium, so one does not feel that here it is convection? Thus, one should insist that he do not designate as convection all kinds of turbulence in stellar atmospheres.

— A. UNSÖLD:

This point will be covered in detail in Part IV-A, by Mrs. BÖHM-VITENSE, because the sun is the only star where these things have been studied in sufficient detail. At the present session, we do not yet consider the mechanism producing the observed velocities.

— E. SCHATZMAN:

Let me give a quick picture of the different kinds of motions we postulate, where they occur, and their relation to the observations. In the lowest observed atmospheric regions, we have a convective zone; above that a radiative zone; above that a chromosphere. The convective zone is the seat of convective motions which lead to the production of compression waves, which propagate outward, and they decay in the upper part of the radiative zone or in the chromosphere. The motions of the convective zone are usually supposed to appear in the curve of growth. The motions in the upper regions are probably the source of the line-broadening. In the case of Wolf-Rayet stars, we do not know the origin of such large velocities as are observed. In stars with extended envelopes, we have to consider the effect of the Keplerian motions of the envelope around the star.

— E. SPIEGEL:

I want to draw attention to a possibly useful observational approach in the study of motions in stellar atmospheres. In the case of stars of spectral type near B0, there are convective instabilities near the surface due to the second ionization of He. It might be possible to detect the effects of the resulting motions on the spectra in the following manner: One might expect that lines formed principally in rising hot masses of gas are shifted to the violet while those formed in descending masses would show a corresponding red shift. The magnitude of the shift should be less than that given by the sound speed in such stellar atmospheres, about 20 km/s. On the theoretical side we know from the work of TRAVING on the star *10 Lacertae* that such motions could exist without disturbing the radiative equilibrium, and would ordinarily escape notice.

There are not many data which are available for such an investigation, but STRUVE has kindly provided some radial velocities for *10 Lacertae*. Fig. 1

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shows a plot of radial velocity against mean optical depth of formation as calculated by TRAVING. Lines for given ions have been grouped together and the size of the point in the plot is proportional to the number of lines measured. The asterisk represents 14 HeI lines.

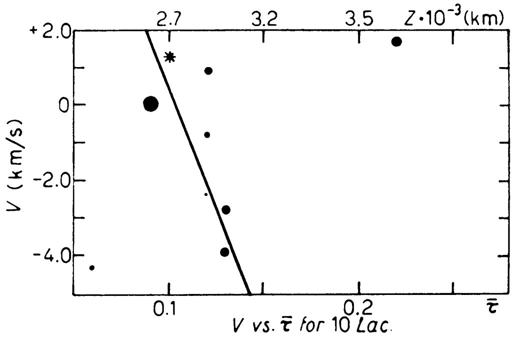

Fig. 1.

One sees that there is some indication that a correlation may exist in the suggested way. The highly discrepant point at $\bar{\tau} = .22$ is due to 6 lines of OIII which lie mainly in the UV.

Clearly, the data are not yet adequate for any conclusions to be drawn, but I would like to ask the observers whether they feel that with sufficient data, such studies might possibly be made definitive.

— A. UNDERHILL:

I question whether the size of the effect found is significant, relative to the uncertainties existing in the relative wave-length standards for the lines of HeI, CII, OII, OIII, and NIII used in the analysis. The velocity range found is 6 km/s, which corresponds to a 0.08 Å shift in the 4 000 Å region where most of these lines lie. So the relative positions of the absolute wavelengths of these lines must be known to higher accuracy than this 0.08 Å in order that the differential measures be meaningful. In the preface to the Revised Multiplet Tables, Mrs. SITTERLY carefully remarks that the several elements are not necessarily on the same wave-length system, owing to the experimental difficulties associated with producing the lines. It is my impression that small differences exist, of the same size as the 0.08 Å found here.

— E. N. PARKER:

Then do any radial velocities in the data presented mean anything?

— A. UNDERHILL:

Yes, because in measuring stellar radial velocities, the first thing you have to do is to set up a set of empirical wavelengths for each spectral type. In brief, you take spectra of sunlight reflected from the planets. You can compute the motion of a planet from its known position. Then you adopt a set of wavelengths that will reproduce the theoretical planetary velocities consistently within a fraction of one km/s. Then for each spectral class, and for

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each spectral dispersion used in observing, you establish a mutually-consistent set of effective wave-lengths. This is particularly important for the question of blends in the spectral lines. Consider, *e.g.*, the HeI triplet lines. To obtain the correct radial velocity, do you adopt the wave-length of the strongest component, or do you take the mean of the three. The effective wave-length may change by something like 0.1 Å depending upon your decision.

— E. SPIEGEL:

The second problem concerns the case of a binary star, *31 Cygni*, where one component, a K-star, has such an extended atmosphere that the ratio of the radius of the K-star to that of its B-star companion is of order 10^2. The orbital plane lies roughly in the line of sight, so we observe the B-star passing behind the K atmosphere, acting as a probe, which enables us to study the conditions in the extended atmosphere. One observes absorption lines produced in the B-star spectrum by the K-star atmosphere. The present conclusions are based on an analysis by ALLER and myself, of measures by MACLAUGHLIN, made at the next-to-last eclipse. The data are incomplete because of cloudy nights, and not made at the highest resolution now available. We tried to take the autocorrelation of the mean velocity along the line of sight, as a function of radial position. Such an autocorrelation depends not only on position but also on time, since it takes the B-star several days to move the distance between points, which is some $3 \cdot 10^6$ km. None the less, the autocorrelation function appears to be well-defined, dropping to zero at a distance of about $2 \cdot 10^7$ km, agreeing well with the figures Miss UNDERHILL gave. These results are tentative, and push the available data to the limit.

— S. S. HUANG:

The so-called « turbulence » as used by stellar spectroscopists is not necessarily the turbulence as understood by aerodynamicists. Therefore it is unfortunate, if not misleading, for astrophysicists to use the name « turbulent velocities » to denote some parameters which are introduced to interpret stellar spectral lines. Then, what is the meaning of the so-called micro-turbulent and macro-turbulent velocities which have been discussed by UNDERHILL and which have caused quite long discussion in this symposium? In order to clarify this point, we have to consider the nature of stellar spectral lines because, after all, the turbulent velocities in stellar atmospheres as used by astrophysicists are derived entirely from spectral lines.

Consider a point (x, y) on the stellar disk. From the theory of radiative transfer which was discussed extensively by many speakers yesterday, we can derive, in principle at least, the line profile of the emergent light at the point (x, y) as

$$I(\lambda, x, y, t;\ \bar{\alpha}, \bar{\beta}, \bar{\gamma}, \ldots, a, \ldots)\,, \tag{1}$$

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where t denotes the time of observation, while α, β, γ, ... denote a set of parameters depending on the layer of the atmosphere, *i.e.*

$$\alpha = \alpha(\tau)\,, \qquad \beta = \beta(\tau)\,, \qquad \text{etc.}$$

and a, ... denote another set of parameters depending on the particular line concerned but independent of the depth τ. In other words, a, b, ... are atomic constants characteristic of the spectral lines, such as the transition probability while α, β, γ, ... may be temperature, pressure, magnetic field, etc. Thus, α, β, γ, ... enter into $I(\lambda, ...)$ through the source function in a complicated way. Expression (1) is only a rough approximation in saying that the effect of $\alpha(\tau)$, $\beta(\tau)$, etc., on $I(\lambda, ...)$ can be represented by a single mean value $\bar{\alpha}$, $\bar{\beta}$, ... respectively. What we actually observe is

$$(2) \qquad F(\lambda, \alpha, \beta, \gamma, \ldots, a, \ldots) = \\ = \iiiint I(\lambda', x, y, t, \alpha, \beta, \gamma, \ldots, a, \ldots)\,\mathrm{d}x\,\mathrm{d}y\,\mathrm{d}t\,J(\lambda - \lambda')\,\mathrm{d}\lambda'\,,$$

where $J(\lambda)$ is a normalized function known as the instrumental profile. The integral in (2) defines three kinds of broadening, *i.e.*

1) physical broadening which is due to $\bar{\alpha}$, $\bar{\beta}$, $\bar{\gamma}$, ..., a ...,

2) geometrical broadening which is due to the integration over x and y,

3) operational broadening which is due to the integration over t and λ'.

With modern technology, we can reduce the exposure time to a very short interval and make the instrumental profile nearly a δ-function. Then we can neglect the operational broadening altogether and reduce (2) to

$$(3) \qquad F(\lambda, \bar{\alpha}, \bar{\beta}, \bar{\gamma}, \ldots, a \ldots) = \iint I(\lambda', x, y, \bar{\alpha}, \bar{\beta}, \bar{\gamma}, \ldots, a, \ldots)\,\mathrm{d}x\,\mathrm{d}y\,.$$

A transfer theory, of which the curve-of-growth analysis plays only a part, should explain the profiles and consequently the equivalent widths, of all lines —from very weak to very strong—by assigning suitable values to $\bar{\alpha}$, $\bar{\beta}$, $\bar{\gamma}$, ... in the atmosphere, while a, ... are in principle known quantities. The curve of growth is a method to determine $\bar{\alpha}$, $\bar{\beta}$, $\bar{\gamma}$, ... for the atmosphere concerned from a compromise over all lines.

It is now clear that a velocity field due simply to thermal motion is not enough to explain the behavior of all lines. In other words, the determined velocity from the curve of growth is too large to be accounted for by thermal motion. The name « turbulence » is introduced to describe the excess velocity, and it is generally known as the « micro-turbulent velocity » in contrast to the « macro-turbulent velocity » which will be discussed later. The micro-turbulent velocity thus defined may not be a turbulent velocity in the sense of aerodynamicists. Indeed, the profiles—$I(\lambda, ...)$ in the case of the sun and $F(\lambda, ...)$ in the case of the stars—may be explained by other means than the introduction of a velocity parameter. This is what THOMAS and his associates are trying to do. There is no reason to object, *a priori*, to Thomas' approach.

Next we consider the broadening due to the integrations over x and y *i.e.* the geometric broadening. Because of the up and down motions at different points on the stellar disk, the integration over x and y introduces further broadening, which is said to be caused by macro-turbulent motion. Astrophysicists usually assign one single velocity to denote the magnitude of the macro-turbulent motion. It is obvious that one single parameter is not enough to describe the motions over the stellar surface. For example, axial rotation of the star behaves exactly like macro-turbulent motion so defined. A single parameter cannot show such a difference. Here a function instead of a parameter has to be introduced to define the mode of motion on the stellar surface. This function is called the broadening function, and may be derived from the study of line profiles.

Astrophysicists usually assume first the physical nature of the motion (such as rotation), then compute the broadening function, and finally compare the observed profiles with those that would be expected from the broadening function. In this way we are able to infer what is the nature of motion which broadens the lines in the geometrical sense. It is apparent that the so-called macro-turbulent motion is far from turbulence as we understand it in the laboratory.

— F. KAHN:

Let me describe a method to estimate the Mach number we can expect for micro-turbulence, in the sense that we have heard it defined. I proceed from the following principle: If we have turbulence at a given Mach number, then energy must decay, and must be removed quickly from the gas, otherwise the gas will heat up and reduce the Mach number. In the astronomical case, the energy is removed by electromagnetic radiation from the gas. The turbulence energy decays to thermal energy of the gas atoms, which can't radiate it away directly, but must pass it on to the electrons by elastic collision. Then the electrons excite the gas atoms by inelastic collision, which leads to the radiation. The rate at which the electron gas gets heated determines how quickly you can cool off the gas, because once the electron gas is warm enough, there is no difficulty in the electrons exciting the atoms to radiate. Therefore the rate at which the electron gas is heated must be large enough

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to balance the rate at which decay of turbulent energy heats the gas. So, we compute these two rates, and equate them.

First, with motions of typical velocity U, atmospheric density ϱ, and scale length L, the rate of decay of turbulent energy is given by $\varrho U^3/L$ per unit volume. In an atmosphere consisting predominantly of hydrogen, with concentration N and mass M, the rate is NMU^3/L. Next in the simple encounter of an electron and an atom, we can expect the increase in the square of the electron's speed to be of the order of the square of the speed of the atom; *viz.*, $m\,\Delta(v^2) \sim m\,kT/M$. m is the electron mass; T, the temperature; N_e, the electron concentration; S, the electron-atom elastic cross-section. Then the elastic collision rate between the electron and atoms is: $NS(kT/m)^{\frac{1}{2}}$. Combining these expressions, we find that the rate at which the electron gas picks up energy is: $N_e NS(kT/m)^{\frac{1}{2}}(mkT/M)$. Setting $a^2 = kT/M$—thus a is almost the speed of sound in the gas—and equating turbulent dissipation to heating of electron gas, we have

$$NMU^3/L \leqslant N_e NS(mM)^{\frac{1}{2}} a^3$$

or

$$U^3/a^3 \leqslant N_e SL(m/M)^{\frac{1}{2}} ;$$

the inequality sign denotes that turbulent heating of the atom gas must not exceed the rate at which energy can be transferred to the electron gas. This result can be connected with micro- and macro-turbolence fairly easily, because we speak of micro-turbulence in a particular spectral line when the length L — the scale of the turbulence — is such that there are many turbulent elements within a length corresponding to unit optical depth. If ε is the fractional number of atoms considered (relative to hydrogen) in producing the spectral line studied, and α is the absorption cross-section per atom, we thus require, for micro-turbulence: $(\varepsilon N\alpha)^{-1} > L$. Thus the above condition on U/a can be written

$$U^3/a^3 < (N_e\, S/\varepsilon N\alpha)(m/M)^{\frac{1}{2}} = (XS/\varepsilon\alpha)(m/M)^{\frac{1}{2}} ,$$

where X is the degree of ionization. If this condition is not satisfied, the gas must heat up until the Mach number, $\sim U/a$, drops sufficiently for the relationship to be satisfied.

For a rough estimate, we take $X \sim 10^{-6}$, $\varepsilon \sim 10^{-5}$, $S \sim 10^{-16}$, $\alpha \sim 10^{-17}$ and obtain $U^3/a^3 \lessapprox 1/43$ or $U/a \lessapprox \frac{1}{3}$. The value might differ from this, depending upon the cross-sections and the abundance of electrons.

— M. J. SEATON:

What happens if electrons and atoms have the same kinetic temperature — there is then no interchange of energy.

— F. Kahn:

The calculation would be better if the electron gas were considerably cooler than the atom gas. The formula gives the maximum rate at which the energy will be transferred.

— R. N. Thomas:

Several years ago, Krook, Bhatanagar, Menzel and I tried to see whether we could maintain a steady-state in a pure hydrogen atmosphere with $T_e \neq T_k$. (T_e, T_k being electron and atom kinetic temperatures.) We looked for a steady state by equating transfer of energy from atoms to electrons by elastic collision, to energy radiated from the gas, taking into account departures from thermodynamic-equilibrium distribution functions. We had to go to $T_k \sim 10^7$ before we could get as large a value as 1000° for $T_k - T_e$. So, it would seem to me you must take $T_e = T_k$ in your assumed circumstances.

— H. Liepmann:

I am worried, because this dissipation law is correct only for low speeds for which there is no coupling with an acoustic field. Dimensionally U^3/L may be resonable, but there could be a dimensionless coefficient of order M^{12}, for example, when Mach number, M, exceeds unity. Second, I do not understand why the gas should not heat up; if you have a normal relaxation time, the two kinetic temperatures should become equal.

— F. Kahn:

I do not think that the two kinetic temperatures necessarily are equal. I agree, however, that all kinds of things go wrong when U/a is large. I would not care to make estimates under those conditions.

— H. Liepmann:

I would prefer to see you write the complete equations of motion with the radiative terms in non-dimensional form, and then discuss the relation between the order of magnitude of the various terms. In this case, such a parameter as you introduced must occur, but it can't be the only one; there must be a term corresponding to turbulence dissipating directly into sound.

— R. N. Thomas:

This goes back to an approach Moyal and Uberoi made some years ago (Moyal: *Proc. Camb. Phil. Soc.*: **48**, 329 (1952); Uberoi: *Proc. Camb. Phil. Soc.*: **49**, 731 (1953)), and Clauser discussed something along these lines yesterday. I would agree that this would be the most satisfactory approach to the general problem of energy dissipation from a generalized « turbulence » and its relation to electron and atom kinetic temperatures.

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— F. H. Clauser:

You have assumed that the limiting process is the transfer of energy from atoms to electrons. But suppose the turbulence is buried deep within the atmosphere, so that even though you transfer the energy to the electrons and produce radiation, the radiation just bounces around, being absorbed and re-emitted. Isn't the rate-limiting process the ability of the radiation to diffuse to the surface and escape?

— F. Kahn:

You have raised a more general problem. I was talking only about the region where the spectral lines are formed, near the surface of the star, well above the photosphere. Any radiation generated there will escape; or, rather, any thermal energy turned into radiative energy is unlikely to be turned back into thermal energy.

— R. N. Thomas:

I disagree strongly. If you want to talk about energy dissipation by radiation in the lines, the biggest problem is the transfer problem. While the rate of radiative loss is certainly proportional to the rate of inelastic collisional excitation of atoms by electrons, the proportionality factor depends upon a solution of the transfer problem, and can be much less than unity. (Indeed, if you make the LTE assumption on the source-function, it vanishes.)

— F. Kahn:

All I really want to say, is that the rate of transfer of energy to the electrons places an upper limit on the rate at which you are allowed to heat the atom gas by turbulent dissipation. You are saying that one should introduce a few more factors to make the inequality stronger. If you don't let the radiation go directly away, you heat the gas, and drop the Mach number of the turbulence. I think that what is really called for is more refined calculation.

— M. J. Seaton:

It is clear that Kahn's calculations represent an extreme upper limit on the energy transfer; in fact, it would probably be several orders of magnitude less. If you consider that electrons and atoms have a small difference in temperature, then ΔT rather than T enters the equation. But this ΔT would be very small. What has been done in Kahn's computations, has been to look at the energy transfer one way only, instead of looking at all the energy gain and loss processes. There are collisions leading to an energy transfer back the other way; and it is only when there are differences in the mean energies of the particles that you have a net transfer.

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— W. H. McCrea:

A question on Spiegel's discussion of *31 Cygni*. Can the aerodynamicists say what would determine the scale of the clouds in an extended stellar atmosphere if they be considered to be some form of turbulence?

— E. Spiegel:

Wilson and Abt studied the effects of the decrease of the intensities as you go into the eclipse and therefore made some inferences about density gradients. The figure I quoted for « cloud size » is of the order of magnitude of the scale heights, in those extended atmospheres. This is about the only relevant physical parameter I could suggest. Another aspect is that many people don't like to think of motion in the K atmospheres as clouds; for example, in one paper MacLaughlin refers to a network of prominences—and there might be some magnetic or other phenomena which determine the scale. That is a very difficult theoretical question to decide, but a very exciting one.

— A. Unsöld:

This is a viewpoint about which we should remind aerodynamicists quite generally. If looking at a « point » on an astronomical object we observe a spread in velocities, then of course we see at the same time a lot of objects lined up along the line of sight. These may be clouds, or, for instance, something like the prominences on the sun, which have nothing whatever to do with each other. And then it becomes senseless to speak of a continuous fluid motion. Such things can happen in astrophysics, but are usually not considered in aerodynamics.

Thompson will now summarize this session from the standpoint of the aerodynamicist; *i.e.*, what, from this astronomical material, appears to be of interest in aerodynamics.

— W. B. Thompson:

It has been assumed that I will summarize for you what I have understood, and then you can safely assume that the other hydrodynamicists have understood at least that much.

From the discussion so far it appears that the astronomer divides his interests into something called micro-turbulence—and something called, with even less justification, macro-turbulence. As I understand it, and I may be very unkind, the suggestion already advanced—that micro-turbulence is an artifice introduced to correct an erroneous theory of line formation—is not completely excluded. We have been assured by Unsöld that this is not the case. There do exist valid theories of line formation which demand the existence of micro-turbulence. What he means is not the sort of analysis described by

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PECKER and THOMAS, but a synthetic theory; the construction of a model atmosphere from which one predicts line shapes, and after the introduction of micro-turbulence secures very satisfactory agreement with observations. We have not yet, however, been exposed to the physical assumptions that have gone into this theory, and clearly it is not a complete *a priori* calculation based only on the microscopic properties of matter, for one does not know enough about atomic cross-sections—or even about the relevant processes involved to start from first principles. It is quite clear that at some stage simplifying assumptions have been made, Saha and Boltzmann equations, or local thermal equilibrium, and it would be interesting to see just what the simplifying assumptions are, and what physical arguments underlie the selection of these. These I am sure are familiar to all the astronomers present: they are not familiar to me, nor, I suspect, to some other of the physicists and aerodynamicists. It would be very enlightening if we could be shown the physical arguments underlying the theoretical calculations which have made inevitable the introduction of micro-turbulence.

Micro-turbulence is not so worrying now as it appeared at one stage of the discussion—when I and several others were under the impression that the evidence required supersonic micro-turbulence, which would have been rather hard to swallow. However UNSÖLD has presented the general conclusion that it is always subsonic—although the velocities are large.

A second interesting feature is that we know the scale of micro-turbulence. Since it is involved in the curve of growth, widening the line core, its scale is less than the mean free path of radiation, or the optical depth $\tau=1$. On this small scale, the high, almost sonic velocities required present serious difficulties, but I don't believe they are insuperable, particularly when you recall that the visible layers lie on top of a much hotter substratum. Scales: the impression I have is that $\tau=1$ corresponds to a modest length in the observable parts of the star, say 100 km.

(*Ed. Note*: There followed an interchange between THOMPSON, UNSÖLD, THOMAS on this question of scale, making the points: For strong lines, there is a variation of a factor 10^4 in scale over which the different parts of the line are formed. The distance $(100 \div 1000)$ km is a reasonable estimate for a length corresponding to $\Delta\tau \sim 1$ in the continuum, in the wings of strong lines, and in weak lines; so long as one does not consider stars with extended atmospheres.)

— W. B. THOMPSON:

A final observation about micro-turbulence. It seems that the only way of getting at the physical structure of things of this scale in the stars is by making model atmospheres and exploring the consequences of specific models. On the other hand, PECKER and THOMAS have suggested that for the partic-

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ular case of the sun, for which much more information is available, it may be possible to extract a good deal of detailed information about the physical system by examining lines directly, rather than by working from model atmospheres. Thus the sun is quite a different object for study than the stars, and I would have thought that in the present state of our ignorance it might be a good idea to concentrate on understanding the hydrodynamics of the sun, which I gather is fairly simple-minded, well-behaved star.

Now to macro-turbulence. If hydrodynamicists object to micro-turbulence —since in fact what is involved may be a superposition of unlinked sound waves rather than the turbulent field in which the velocity components are tightly interlinked, as CLAUSER described—how much more exception should they take to the term macro-turbulence, since here the scale is large—much greater than 1000 km. May I observe that the scale of the hydrodynamicists' turbulence contains parts which are independent of the geometric scale of the objects producing the turbulence. The scale will ultimately be determined by dissipative processes, nothing else. This is the meaning of turbulence as used by the aerodynamicists. It is a velocity field, which is coupled to itself through non-linear effects, in which energy is cascading from large scale phenomena into smaller and smaller scale phenomena. The scale with which you start is of course determined by the geometrical size of the atmosphere of the object which you are looking at, the final size of the small eddies is determined by dissipative processes and is the same in the laboratory as it is in the star, in so far as physical conditions are similar. That one sees micro-turbulence in those stars where macro-turbulence is also observed, suggests there is some passing down the scale—that there is some connection between these two things. But I do think they can be considered as distinct phenomena, with no necessary connection. It is just as well, because there seems to be quite a difference between them. In particular, macro-turbulence for some moderately pathological stars can be violently supersonic. That is, the Mach number is very great indeed. Miss UNDERHILL described this morning what is known about macro-turbulence in stars. It seems that a typical star exhibiting macro-turbulence has a large gaseous envelope around it, and seems to be a little unhappy in various ways—these stars are not steady, they seem to suffer from some sort of astronomical indigestion. So it is maybe not too surprising that we see large scale motion. Observe that the Mach number is very much greater than 1, but that the temperature used to estimate it was, of course, the temperature of the outer thin cool layers of the stellar atmospheres. Because of the possible scale of this motion, which can be anything from something greater than a few thousand km to something comparable in size to the entire star that one is looking at, from a hydrodynamic point of view this Mach number may be completely irrelevant.

Hydrodynamic behaviour on this scale is determined not by the thin cool atmosphere, but by the hot underlying matter of the star for which I suspect the Mach number to be less than one. The Mach number seems to me irrelevant to the macro-turbulence because the Mach number is with respect to the temperature of the thin, cool, skin on the surface of a star, and this surface temperature is completely irrelevant to the actual processes determining the motions which may occur in deeper layers. That of course can scarcely apply to these extremely tenuous atmosphere which have a very large volume indeed. As far as I can see from the evidence of *31 Cygni* presented by Spiegel, the atmosphere can be very much larger than the star itself; so that the transparent region you see is very extended and cool. There this consideration cannot apply. On the other hand, if there is a strong magnetic field in such a thing, the relevant Mach number should be with respect to the Alfvén speed, not to sound velocity and again it may be very much reduced.

— A. Unsöld:

Let me try to clarify some points, which have been brought up, in a kind of second approximation.

I will consider first the question of macro-turbulence. One of the most exciting statements for the astrophysicist in this morning's lecture by Miss Underhill—based on new observation at the Dominion Astrophysical Observatory—was that the macro-turbulence that is observed in hot supergiant stars is always of the same order of magnitude as the changes that one measures in the radial velocity of the whole star, as a function of time, over longer times. That is for the first time a really convincing indication that these velocities are not connected with the rotation of the star, but are due to really irregular motions, which comprise considerable parts of the star. We have somehow to imagine that considerable parts of the stellar atmosphere move up and down in a rather irregular way. The detailed mechanism of course is far from clear. We may imagine that it has something to do with pulsation in higher modes. And that, of course, would come into perfect agreement with the viewpoint raised by Thompson, that relating these speeds to the velocity of sound for the temperature of the atmosphere in the usual way may have no sense. I think this is an important point which was not clear so far and which we should fix as a real result from this meeting.

Then comes the other question of micro-turbulence, where you did not quite feel satisfied about the explanation of the physical foundations. Perhaps I should say a few words more on these. I must attempt to explain briefly and in simple words a type of work which in fact is extremely circumstantial and lengthy, as I said this morning. Let us begin by assuming that we know the effective temperature, the surface gravity, and the composition of the star. Then we try to calculate the structure of its atmosphere; that is, how the

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temperature and the pressure depend on the depth. To begin with we assume a perfectly static atmosphere.

— W. B. THOMPSON:

You make essentially a calculation based on hydrostatic equilibrium. That must involve some assumption about transfer of heat. What other assumptions?

— A. UNSÖLD:

It is assumed that the energy is transferred in the higher layers entirely by radiation. For the deeper layers of the cooler stars convection is important too. These are processes which we can describe with sufficient accuracy. Then, if we know the dependence of temperature and pressure on depth, we can calculate for instance how the number of sodium atoms in a particular atomic state depends on depth. Next, we calculate the absorption coefficients.

— R. N. THOMAS:

You are assuming certain things when you calculate how occupation numbers depend on depth. Maybe you could mention the assumptions, and whether you have investigated their validity for the situation which you are examining.

— A. UNSÖLD:

We assume the Saha equation and the Boltzmann equation. Perhaps I should state the limitations of the procedure afterwards. I hope they become clear then. We calculate the atomic absorption coefficients for various lines as a function of depth. These are calculations which one can do nowadays fairly well from quantum theory, at least for a sufficient number of atomic states. Then we can calculate the curve of growth for these lines. Still without assuming turbulence. And now comes the process of fitting our calculations with the observations: We have to check on the one hand the temperature, and on the other hand the surface gravitation. Certain lines are more affected by temperature, and others by the pressure, which essentially depends on the surface gravitation. So we try to fix these two points by combining various observations. In that procedure the abundance of individual elements does not come into play. Then comes our important point—how to get the turbulence. First, we must draw the curve of growth in dimensionless units. Let us plot the measured equivalent widths of the lines divided by what one usually calls the « Doppler widths »; that is, the width of the absorption coefficient caused by the combined action of any motions which are there, which is thermal motions plus what we call turbulence. The abscissa is essentially the concentration of the atoms times the transition probability. If these quantities are plotted logarithmically, the linear part of the curve of growth becomes a 45° straight line; then comes the flat part, and then comes the damping part, with

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half the inclination of the first part. Now we take an element, which has at the same time very weak lines and lines of intermediate strengths. For these lines we know the ratio of the transition probabilities and so the distance between their points along the abscissa. Next we attempt to bring our « empirical » curve into coincidence with the « theoretical » curve of growth by shifting in horizontal and vertical directions. The horizontal shift determines essentially the abundance of the element while the vertical shift gives the ratio of the real Doppler-width (thermal motion plus « turbulence ») to the thermal Doppler-width alone.

— W. B. THOMPSON:

That gives you $\Delta\lambda_D$. Now you must have something to produce $\Delta\lambda_D$ and you invoke turbulence rather than departures from Saha or anything else.

— A. UNSÖLD:

Let the distribution of velocities ξ along the line of sight be $\sim \exp[-(\xi/\xi_D)^2]$ for the thermal part alone and $\sim \exp[-(\xi/\xi_t)^2]$ for what we call turbulence alone. Then we determine the ratio $(\xi_D^2+\xi_t^2)/\xi_D^2$. The temperature must be known from other parts of the analysis. As I said the horizontal shifts of curves of growth determine essentially numbers of atoms in certain atomic states and so one can use ionization—and excitation—equilibria for determining temperature and pressure. The essential trick in this type of spectral analysis is that one knows beforehand from a general study of the subject, which is of course a matter of some experience, that one line depends chiefly on temperature, another line depends chiefly on pressure. Also, one knows that the flat part of the curve of growth depends strongly on the velocities and then one combines the different observations. It is the experience with each one of our students that he complains first that a stellar spectrum has several hundred lines which he has to measure, and when he finally comes to the end of the analysis, he complains that this star has by far too few lines to determine all the parameters of the atmosphere. The essential point is that one uses one and the same set of plates for determining *all* the parameters—the effective temperature, the surface gravitation, the abundance of all the elements, and—if necessary—the turbulent velocity.

W. B. THOMPSON:

You have, of course, given this explanation with great care and it is very much like the other explanation that we have heard of the curve of growth. One point which still leaves some doubt in my mind, is the determination of the temperature itself. This has been done from things like Saha using the equation for ionization equilibrium, or relative line intensity and the Boltzmann equation. How sure are you of the validity of these determinations of the temperatures?

— A. UNSÖLD:

It is essential that we make clear about what temperatures we are speaking. In all our work we use as our characteristic parameter of a star the so-called effective temperature, which is defined as representing (in connection with the Stefan-Boltzmann law) the total energy flux per cm² $\pi F = \sigma T^4_{\text{eff}}$. With this parameter we can calculate, using the theory of radiative equilibrium, the real temperature at every point in the atmosphere. Here we assume that we know accurately enough how the radiative transfer is done.

Now recently THOMAS, PECKER and others have put more emphasis on the investigation of the higher layers of the solar and stellar atmospheres, where the assumption of local termodynamical equilibrium becomes worse and worse.

Let us try to get some idea about the boundary between the two alluded domains!

At an optical depth ≈ 1 (for the continuum) in the atmosphere, a fictitious observer would receive almost as much radiation from the outside as from the inside. Nearer towards the top of the atmosphere the radiation coming from outside becomes less and less and we receive radiation only from the lower hemisphere. So, if we go up high enough, we can certainly have significant deviations from thermal equilibrium. The question is whether these thin uppermost layers still contribute appreciably toward the production of the stellar spectrum. In general the smallest optical depth which is important for the explanation of the continuum and the equivalent widths of lines will be about 0.05. At such depths for the continuum, however, the optical depths for the stronger lines are still quite large and if one has some mechanism working towards establishing thermal equilibrium, *i.e.* exchange between different light-quanta, then just this radiative transfer will help a great deal towards establishing local themodynamic equilibrium. So for these layers the deviations from the Boltzmann equation (in general) are expected to be fairly small and for the Saha equation quite moderate. K. H. BÖHM has made some time ago (in an article for the new American Handbuch) estimates how, *e.g.*, in the outermost layers of the sun the ionization of iron will deviate from the Saha formula and it turns out that this effect is in general not large. In any case it will not affect the spectroscopic determination of the micro-turbulence. The matter becomes, of course, quite different if we go in the sun to higher layers in the chromosphere or still more in the corona. These are places where THOMAS likes to live and there things may be quite different. But these regions contribute little to the ordinary Fraunhofer spectrum which one observes on the solar disk or in stars.

— W. B. THOMPSON:

In determining the turbulence do you use the weak lines?

— A. Unsöld:

In order to determine the turbulence one must have lines which fit on the flat part of the curve of growth. You may see easily how strong these lines must be. Namely, the equivalent widths of the lines there is about 4 times the Doppler width. We saw that for purely thermal motion the Doppler widths for the metals are a few hundreds of an Ångstrom; so the mentioned lines come into the order of roughly one-tenth of an Ångstrom.

— W. B. Thompson:

Are such lines formed in that part of the atmosphere in which you are suspicious about thermal equilibrium?

— A. Unsöld:

No, these lines are formed in practically the same layers as the weaker lines and the greater part of the profiles of the stronger lines. Lines lying on the flat part of the curve of growth have almost rectangular profiles and their equivalent widths are determined by the points where their depth is $\sim 50\,\%$. These points of the line profiles however originate from quite intermediate layers in the atmosphere. So, I think, the measurements of turbulent velocities (within an accuracy of $\sim 10\,\%$) should not be affected by deviations from thermal equilibrium.

— R. N. Thomas:

Let me try to put the points at issue into focus, recognizing the presence and prejudice of three kinds of interest at this symposium: *A*) an astronomer who is interested primarily in determination of chemical composition of the stellar atmosphere, and considers the presence of non-thermal velocity fields an unfortunate complication whose presence is to be eliminated from the analytical process as expeditiously as possible; *B*) an aerodynamicist who hopes to extend the range of his experience outside laboratory aerodynamics, thus is concerned with details of aerodynamic phenomena; *C*) a hybrid who is interested in the non-LTE phenomena attending a mixed situation of radiative transfer and « dissipating » velocity fields, thus wants details on everything. Then we must recognize that the methodology discussed by Unsöld is essentially aimed at satisfying *A*); it is essentially based on the supposition that the only effect of non-thermal velocity fields lies on the frequency-dependence of the absorption coefficient, such velocities have no effect on thermal structure of the atmosphere nor on atomic concentrations. That is, two procedures must be valid: 1) temperature distribution can be computed from radiative transfer of energy only, *no* energy dissipation from either « micro- or macro-turbulence » being allowed; 2) all occupation numbers of energetic states can be

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computed from thermodynamic equilibrium distribution functions, using the temperature computed from 1).

1) is certainly violated in regions where electron temperature, T_e, increases outward; for there some non-radiative energy input, presumably aerodynamic dissipation must occur. For the sun, this outward increase in T_e begins near τ (continuum) ~ 0.01. However, from the type analysis described by UNSÖLD, there comes no suggestion of velocity fields, in this region, larger than those he has just discussed as having negligible effect. So, one would conclude—using only the UNSÖLD type analysis—that there is no departure from validity of 1), and this conclusion would be erroneous. Indeed, the methodology of the same LTE approach has been applied by the UNSÖLD, and other, groups to obtain a monotonic outward *decrease* in T_e in the same atmosphere regions where other analyses, based on less-restrictive assumptions, show an outward *increase* in T_e.

2) must certainly be violated where 1) is violated, so it remains to compute the opacity in each line to show where it is formed, relative to the region where 1) is violated. But since 2) is violated everywhere above T_e(min), at least, such opacity calculations can only be made on a non-LTE basis. We have shown that such non-LTE calculations sometimes increase, by several orders of magnitude, the opacity computed from the LTE approach; so the latter will often seriously err in predicting what regions suffer from the non-LTE effects, even if they had been successful in predicting where 1) is violated. Also note that 2) may be violated even in regions where T_e does not suggest aerodynamic dissipation, the violation coming from anisotropy of radiation field. PECKER discussed yesterday empirical evidence for such failure for intermediate and weak lines, of the type considered by UNSÖLD. We have shown theoretically and empirically such failure, for strong lines.

Further, note that the atmospheric range over which a line-profile is formed may be enormous, more than 10^4 in optical depth for a reasonably-strong line. In consequence, it may well be that in certain cases the curve of growth, based on total absorption in the line, averages things out so well that it indeed « suppresses » the value, and effect, of such things as velocity fields, non-radiative dissipation, and non-LTE effects—leaving only a reasonably-good measure of chemical abundance. But then, it is hard to place much reliance on physical interpretation of « velocity parameters » derived from it. The point is, we require, before passing final judgement, much more investigation of the curve of growth from the standpoint of including at the outset the presence of all these neglected factors. Again, PECKER has referred to the preliminary work at Meudon along these lines.

So to some of us, it has appeared that the information required by groups *B*) and *C*) above comes best from analysis of line-profiles, particularly the central

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regions. Then, we must look carefully into two questions: the validity of the methodology used in the analysis of line-profile; and the question of computing the opacity, to say where the line is formed, relative to the continuum and other lines. A good example is the large amount of current work interpreting the central profile of Ca^+ *H* and *K* terms of turbulence, non-LTE effects, etc.

— A. Underhill and A. Unsöld:

Yes, but this doesn't affect our equivalent widths. In the central part of the line, profiles can be measured only very roughly, due to plate grain and lack of resolving power. And, we don't discuss *H* and *K*; we talk about FeI, TiI, TiII, CrI and things like that.

— M. Minnaert:

If you take photoelectric records such as are obtained, *e.g.*, at the McMath-Hulbert Observatory—take Fe, Ti, Cr if you like—you will find the curves are quite smooth. What you refer to are old-fashioned photographic methods, modern methods are photoelectric. Theories must be adapted to modern methods, and not to old-fashioned methods.

— A. J. Deutsch:

I should like to give my impression of why it is that there are such strong disagreements on this subject among astrophysicists. I think the working philosophy for the astronomer who actually does a curve-of-growth analysis, perhaps of the kind that Unsöld just described, has been at least historically, something like this. He is perfectly content to start with the thermal Doppler widths. When he plots his equivalent widths in a curve of growth, he then finds that he gets one curve of growth for iron, and a slightly different curve of growth for titanium, and still another curve of growth for sodium, and another one for calcium, and so on and so forth. And at this point he asks what is the least complication that he can introduce into the theory of stellar atmospheres which will enable him to reconcile these apparent discrepancies. He comes up with the answer that he can introduce a single new parameter which has the dimensions of a velocity. The Doppler width $\Delta\lambda_D$ associated with this velocity replaces the various thermal Doppler widths, and is the same for all the atoms which are considered. Then all the observed points move nicely on to the same curve of growth.

Now Thomas is going to explode in a minute, and I think it needs to be added that there are astronomers—and Thomas is by no means the only one—who say that this is no true, that even after he has made this adjustment he will get significant systematic differences between different atoms. Now this is where the difficulty lies. Some astronomers insist that the present theories are entirely adequate to satisfy the observations at hand; and there are some extremely competent astrophysicists who maintain this position with respect to most of the lines in the solar spectrum. And there is another group who say, no, if we use the best photometric measures we have and the best of the other relevant data, we still get discrepancies which cannot be reconciled with any choice of the Doppler parameter; we must change the source function.

Now, I should like to point out that in addition to the question of the precision of the photometry, which may be involved here, there is also some question as to what should be used for the scale of abscissae. The doubtful parameter is the oscillator strength, or f-number. f-numbers in astrophysic play a critical role. They have done very notorious things to us in the past. You have the uncomfortable feeling that they are still doing very unpleasant things to us at the present time. The answers that we get from the curve of growth may depend very sensitively on the numbers that we take for the oscillator strengths. These are difficult to determine precisely. Some astronomers prefer to take their oscillator strengths from one source, and some to take their oscillator strengths from another source. Some astronomers assert that by using a more suitable set of oscillator strengths, it is possible to remove the discrepancies that are cited by the people who insist on the necessity of changing the source function. I cannot take a position on this question; I do not know. But I suggest that this may be a fair appraisal of the reasons for the wide disagreement which you will find among astrophysicists at the present time, about the necessity of abandoning the relative simple equilibrium model which most astronomers have been content to use in the past.

— A. UNSÖLD:

I agree with DEUTSCH on the viewpoint that one must be extremely conservative in using oscillator strengths. Then we have been frequently talking about deviations from thermal equilibrium. No doubt such deviations exist; but opinions are divided on their importance. In any case we should make clear that we are dealing with two quite different problems. Imagine first a perfectly quiet atmosphere in purely radiative equilibrium. In its outermost layers there is no radiation coming from outside, and that will lead to deviations from thermal equilibrium. On the other hand, if we have an atmosphere with motions (from whatever cause), their velocities will increase outwards and we get energy transfer also by mechanical motion, *e.g.*, by dissipating shock, or hydrodynamic waves, etc. Such effects may produce again deviations from thermal equilibrium which may be rather different from these mentioned first. It might clarify the discussion if deviations from thermal equilibrium having quite different physical background would be distinguished from each other.

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(*Ed. Note*: For a discussion in terms of such a distinction, cf. the PECKER-THOMAS paper, section on the two categories of source-function for a 2-level atom.)

— G. ELSTE:

May I give an example of improved « classical » methods; *i.e.* no departures from LTE and no turbulence has been used in the model atmosphere of *τ Scorpii* which ALLER, JAGAKU and I were looking at some years ago. I call it an improved method because not only the abscissae of the curve of growth but also the run of $\log W_\lambda/\lambda$ as a function of $\log \tau$ was calculated theoretically for Si III and Si IV. As a result the observed points agree very well with the calculated curves leading to the same Si abundance. But look

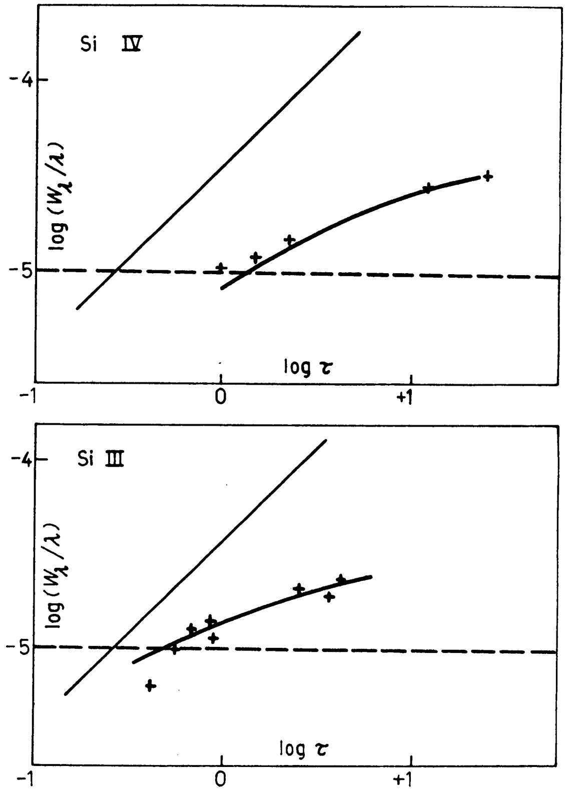

Fig. 2. – Curves of growth for silicon in *τ Scorpii*.

at the position of these curves of growth. There exists quite a difference between Si III and Si IV. While the Si III-curve does not differ much from the common Milne-Eddington curve, the Si IV-curve runs below it and in the region where one expects « weak » lines it does not at all reach the 45° lines.

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This behaviour can be understood by the rapid increase of the number of Si IV-ions in deep layers where these lines are formed and saturated, and above this layer there still exists continuous emission. This note may be a warning, because there are cases in which the different positions of the points for different ions are interpreted as difference in turbulence velocity.

— R. B. LEIGHTON:

I have a question concerning stars with extended envelopes. May we assume that the objects that have been studied, that show large turbulent velocities, are typical for *all* supergiants? Or have we merely looked at a limited sample, which could give us misleading results?

— A. UNDERHILL:

That is a very difficult question to answer because very few supergiants have been observed. There is nothing about those supergiants which have been observed that is particularly different from any other supergiant; but each supergiant is a bit of a character of its own. I think you could conclude that the observations give a reasonable representation of what any supergiant might be expect to be. You just cannot make statistics from a handful of observations; though astronomers try very hard most of the time.

— M. J. SEATON:

I am still not clear on the precise attitude that PECKER and THOMAS hold on these questions. Yesterday we had some terrible warnings about all the uncertainties, attending use of the «standard» methodology and I think it would perhaps be useful if PECKER and THOMAS could make it plain just what they do accept. From a question asked this morning, I almost had the idea that the whole concept of micro-turbulence was rejected, or at least that source function uncertainties are so large that no information about micro-turbulence can be obtained. But is this really their point of view?

— R. N. THOMAS:

Our viewpoint is very simple: Do not take literally the result of any observational interpretation unless this is made on the basis of the most complete physical theory you can construct. UNSÖLD's book on stellar atmospheres is still, to me, the best that exists—because he worked very hard to insist that one put all possible physics into astrophysics. We are trying hard to extend this viewpoint into fields he did not consider, the non-LTE and aerodynamic-dissipation aspects. The detailed theoretical results obtained thus far are exploratory, and limited in scope—mainly limited to the central regions of strong lines—but they have introduced strong changes over results based on the LTE approach. Relative to the micro-turbulence derived from curve-of-growth studies, I would only repeat my remark of a few minutes ago on the possible « averaging-out » character of the gross curve-of-growth approach, its insensitivity to effects found in regions its result presumably cover, and resultant suspicion on the physical meaning to be attached to parameters derived *a posteriori* from it. I see little point in proceeding to construct aerodynamic theories to explain an inferred velocity field, until I am sure that the existence of such a velocity field is consistent with the basis upon which I have derived the theory used to infer the presence of the velocity field.

— A. UNDERHILL:

The rather large motions that appear in all types of stars do not differ greatly. Different amounts of radiative energy flow through the stellar atmospheres, but you get about the same magnitude of velocities. It is to me extremely interesting that the magnitude of the velocity appears to depend far more on the size of the atmosphere than on the absolute value of the energy flowing through it. Practically all the discussion today has been concerned with astronomical micro-turbulence; I wonder if I may infer that no really interesting aerodynamical problems are posed by this other aspect, that seems to me a rather interesting field?

— F. H. CLAUSER:

Frankly, I am not very clear on what you are asking. I think that those of us who have been associated with turbulence in the laboratory feel that turbulence is not a definition—it is not an invention—it is not a catch-all. It exists as a reality and it has an existence that is forced upon us by observations in many different fields under many different conditions for a variety of fluids, and I think that we are interested in the fact that it exists under your circumstances. We find that turbulence exists so universally that it would not surprise us at all if in every star you found turbulence. That you find micro-turbulence that is subsonic, I think has been aptly expressed by the statement: we would be most surprised if you did not. The fact that you find macro-turbulence with very large velocities, again is not surprising to us. Just how much our interest is, I am not quite sure; because it is not clear to me, at least, what you are really measuring, with macro-turbulence.

I have been sitting here thinking about how, in the laboratory could one generate supersonic macro-turbulence; and it suddenly occured to me that we have a lot of it, every place. For example, supposing that I were to take the ordinary wind tunnel—a simple, ordinary wind tunnel in which the flow comes in at very low subsonic speeds, goes through a nozzle at sonic speeds, and accelerates to supersonic speeds. I put glass walls on it, turn it at a slight diagonal angle so that you get a component in the line of sight, turn a light through it and allow you to analyze only the total light that comes from the entire tunnel including the supersonic and subsonic portions. And you will get supersonic macro-turbulence. We would no more call this turbulence than the man in the moon, but it appears to me that you would call it turbulence—you would call it macro-turbulence. Now I think we are a little unhappy about this. Because we feel that turbulence is very real, and even though we cannot define it precisely, we find that there is a large range of phenomena under which it is clear cut that it is turbulence.

Now, let me tell you a few things that are not turbulence. For instance, a random sound field. If you were to generate pulsations on the walls of this room, you would get a velocity field in this room, and I do not believe that anyone of us would call it turbulence. If you look out at the surface of the lake and see the surface of the lake moving up and down, we do not call that turbulence. There are a large number of such things that we do not call turbulence. Now, let me tell you some of the things that we do call turbulence. I think one of the most startling things that we find is the following: turbulence, like pregnancy, is all or nothing. There is no such thing as half-turbulence. For example, we used to believe that turbulence could die out, and get finer and finer, so that you just get less and less of it as you went out into the field that adjoins essentially a large mass. But as we got more sophisticated instrumentation, that could resolve in both space and time, we found that the border between the turbulent and non-turbulent parts of flow was very sharp and very distinct. The only reason that you thought that you had less turbulence was because your instrumentation for a small fraction of the time was immersed in a turbulent field. We also observe that in the transition that took place between a laminar flow and a turbulent flow you got bursts of turbulent and bursts of non-turbulent, flow. Then we begin to look more and more to see if we could ever find a case where the turbulence simply died out; and to my knowledge, we have never found such a case. In every case where the instrumentation has been adequate and proper, the boundary between the turbulent and the non-turbulent fields is very sharp and very distinct. And, the sharpness of the boundary seems to be comparable with the smaller eddies; as near as we can tell—the characteristics of the turbulence carry right out to the boundary. Now, if we look at these things optically, say we shine light through the turbulent wake of a bullet, the boundaries at the edge are as sharp and clear as any of the finest eddies that we find. The reason, that I say all this is because we try it with liquids, and we try it with gases—we try it with non-Newtonian fields that do not have linear viscosity laws. We try it with compressible phenomena. We try it under a great variety of circumstances, and we find that turbulence is a very real phenomenon. It is not an invention of ours. It is not a catch-all, just to include anything else you do not know. This is what you appear to be using it as. This makes me, at least, unhappy.