PART I.

Questions of General Background and Methodology Relating to Aerodynamic Phenomena in Stellar Atmospheres.

Discussion.

Chairman: M. MINNAERT

— *Editor:*

Following Pecker's presentation of the preceeding paper, there were a number of questions raised by aerodynamicists simply on clarification of the astronomical jargon. Rather than reproduce this somewhat repetitious discussion, its context has been incorporated into the symposium proceedings by simplifing and expanding the relevent sections of the paper. Then, we pass to those aspects of the discussion concerned with the content of the paper in its bearing on the symposium.

— A. UNSÖLD:

Speaking of methodology in the philosophical sense, I am reminded of A. COMTE, who thought that the very principles of scientific inference would make it forever impossible to determine, *e.g.*, the chemical composition of a star.

Looking over some of the examples given of particular modes of astrophysical inference, I would remark that historically, things evolved in a rather different way.

As an example of quasi-empirical, quasi-theoretical inference, the discovery of « chromospheric turbulence » is quoted. Actually, this evolved as follows. I tried to explain (*Ap. J.*, **69**, 73 (1929)) the large width of the chromospheric Ca^{+} H and K lines as Doppler effects due to « turbulence », the word then being used in this connection for the first time, noting however that the motion might also be related to those of prominences. McCrea now noticed that the Pannekoek-Minnaert eclipse measures of a low density, or rather emission-gradient, in the chromosphere could be explained by including the hydrodynamic pressure of the mentioned « turbulent » motions.

As an example of wholly « theoretical » inference, the discovery of the hydrogen convection zone is quoted, whereas the actual happening differs.

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The earliest suggested energy transport in stars, by convection, was shown by K. SCHWARZSCHILD in 1905 to be inferior to radiation. But in 1931 I wondered how a static solar atmosphere in radiative equilibrium could exhibit sunspots and other signs of violent motion. The solution came from two sources. First, R. H. FOWLER had noted that ionization of an abundant element could depress the specific-heat ratio, so eventually start convection. Second, H. N. RUSSELL had shown it probable that hydrogen was much more abundant than hitherto supposed. Combining these two ideas immediately showed that the sun must have a convectively unstable layer.

Summarizing these historical notes, it becomes obvious that in all these cases just one, rather trivial, method was followed; namely, to find connections between facts or theoretical ideas which hitherto had appeared unrelated.

Turning to the astrophysical problems, the essential point about which the discussion centers is that in a stellar atmosphere we have two functions. One, the absorption coefficient, depends on frequency, temperature, pressure. The other, the source-function, also may depend on frequency and temperature. It seems to me that this three-term representation of the source-function summarized by PECKER is essentially equivalent to the phenomenological presentation previously used following essentially the procedures of K. SCHWARZSCHILD. That is, the source-function is divided into one part called true scattering, as an extreme case of non-equilibrium; and incoherent scattering, as an intermediate type; and one can add « extinction », a term indicating that the re-emission in a line follows the general behavior of thermodynamic equilibrium, but with a different scale-factor.

How can we find out something about these functions? I think that besides attacking the problem from a more or less formal viewpoint, another procedure may be not bad; viz., simply calculate a great number of different cases and see which depends on which. For example, we ask what is the influence of the kind of radiative exchange on the center-limb variation of the line-wings, examining the question from the scheme of true absorption, scattering, or extinction. Next, we ask what affects the dependence of the effect upon depth, or frequency, for weak lines, or for strong lines. All these points actually have been investigated in much detail.

On the other hand, one investigates the effect of a temperature gradient, of various kinds. Also, what deviations from thermal equilibrium can be well-matched simply by a suitable scale-factor on the Planck function? One can see quite clearly that some observational effects are strongly affected by deviations from thermal-equilibrium, and others not; so he can select a certain body of observations to investigate the effect. Of course, beside such a phenomenological approach, the kinetic approach — favored recently by some workers — remains important. I would emphasize that non-equilibrium calculations are very sensitive to whatever approximations one makes to the very detailed knowledge required of all the atomic parameters that enter. Kinetic calculations require knowing which process is the fastest, and which the slowest; missing one, everything may be completely wrong in a comparison with observations.

A typical example is the well-known story of the planetary nebulae. If one assumed that the general behavior of a planetary nebula is wholly determined by hydrogen, which is by far the most abundant element, he would obtain electron temperatures about 10^5, corresponding to the color temperature of the central star. Actually, the very few oxygen atoms present depress the temperature to the order 10^4. Such things can happen in ordinary stellar atmospheres, only we don't know yet. We recognize only quite a number of problems — *e.g.*, the Fe spectrum is so horribly complicated it is almost out of the question to deal with it kinetically. But why not deal with hydrogen? But how can we be sure that something is not going to happen, as in the planetary nebulae? I say this, not to deter anyone from making kinetic calculations — that would mean a complete misunderstanding of my remarks — but simply to indicate how terribly complicated they are, and how careful one must be. I would in general favor first a general idea concerning the importance of various kinetic processes and then try to justify or reject the more phenomenological approach.

For the aerodynamicists, I may use a comparison. If you have a complicated problem of turbulent flow, etc., you will probably never think of treating that directly by kinetic methods. But what one does is to justify the phenomenological Navier-Stokes equations, using kinetic methods, then for practical applications uses these phenomenological methods. To me, such a similar procedure in astrophysics doesn't seem bad.

— J.-C. PECKER:

First, I would note that we tried to describe only the logical structure of the various points treated, not looking too deeply into the historical pattern; maybe we were wrong.

Second, on the form of the source-function, a great deal of work can of course be done using the classical approach — using pure absorption, then pure scattering, etc. I would just give an example from the Meudon laboratory, worked out by Mme. PRADERIE (*), which shows how things can really be understood at once if one just uses the direct $S_s = B_\nu(T_{ex})$ approach. I measured several years ago equivalent widths of molecular CH lines, from center to limb on the disk; LABORDE at Meudon has recently made more accurate measures; the results are the shaded area in Fig. 1. How to compute this variation, from theory? Theoretical computations, using the classical

(*) PECKER, J.-C. and EUGENE-PRADERIE, F., *Ann. Ap.*, **23**, 622 (1960).

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theory for pure absorption (equivalent to LTE) gives curve A. Pure scattering, gives curve S. The classical theory depends greatly upon choice of the model, and we could discuss this at length. But, I have no procedure

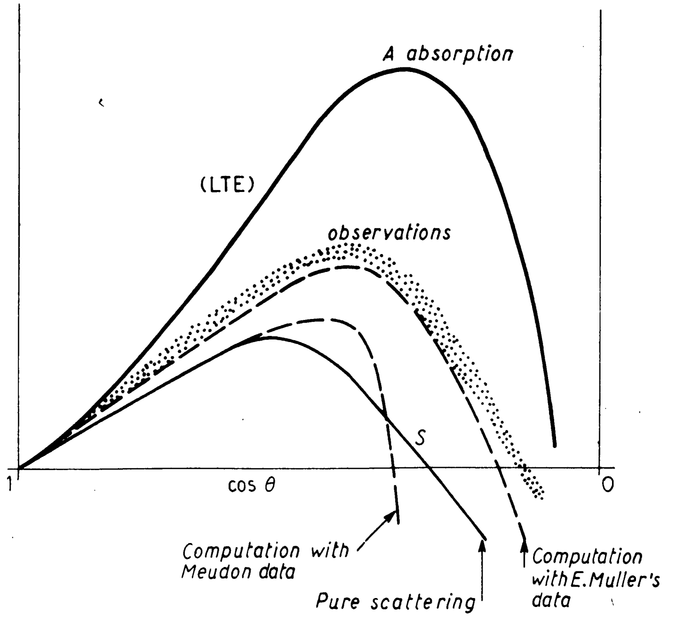

Fig. 1.

whatever that allows a choice between curves A and S, to distinguish between the effect of a choice here and of uncertainty in model, for such a complicated thing as a molecule. Now, however, turn to the new empirical method which has been set up to get T_{ex}, on the basis of the methodology described in my talk. It is an iterative method, giving you T_{ex} as function of optical depth, using the observed central intensities of some CH lines (cf. Fig. 2). From this set of T_{ex}, and $B_\nu(T_{ex})$ for the source-function one computes without further hypothesis the center-to-limb variation of equivalent width. Such computation has been done for two sets of measures; one, by Mme. PRADERIE at Meudon; the other, by E. MUELLER at Michigan. (The former set required a small correction for instrumental profile: the Michigan measure are undoubtedly better.) I give results from both sets of measure, in Fig. 1, to show that the result is quite sensitive to the data. T_{ex} lies about 300° higher than T_e at a certain depth from the Meudon data; and about 200°

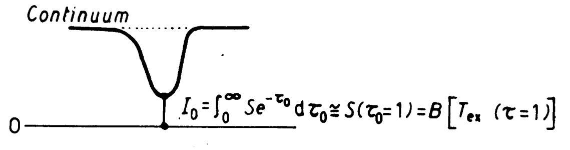

Fig. 2.

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higher from the Michigan data. Possibily the agreement with observations makes me overoptimistic. But I want to emphasize that when you treat the source-function as $B_\nu(T_{ex})$, you automatically take into account the interconnection of the source-function with the population of levels, which gives you a way to get $T_{ex}(\tau)$, and this result is impossible to achieve with the « classical » method.

— M. Minnaert:

I would summarize this interchange as follows. According to the situation, it may be advantageous: either to follow the *inductive* viewpoint and derive from the observations the sorce-function and atomic level populations; or in other cases to follow a deductive method, to start from models described by absorption or scattering and see how the empirical observation agrees; in still other cases the *kinetic* methods may be important, which make use of transition probabilities and collision cross-sections and are the final aim. While we can only be finally satisfied by a kinetic explanation, it is clear that this is for the moment very difficult, and one of the other methods may be of greater advantage.

— A. Underhill:

Most of the remarks made so far deal with the sun. It is possible to observe an isolated point on the sun; for the stars, you always receive the light from the whole disk. Therefore you always have one more integration to invert, and relating the observations to the theory becomes one stage more difficult. Next, considering the considerable observational uncertainties in details of line profiles, I wonder how much meaning can be attached to some of the intricacies of the theory which has been presented. How different are the predicted profiles from a simple theory, say LTE, and those from a more elaborate theory, which is certainly more correct? I agree that we generally simplify the physics because we cannot handle the more correct representation — but you must also remember this point is observational uncertainty.

— M. Minnaert:

Here, I think lies the advantage of the method favored by Unsöld because then you have calculated several possible models, and have derived the experimental consequences for each of these theories, so you can nicely see where the discrepancies with the observations exceed the errors of measurement.

— A. Unsöld:

It is a fortunate circumstance that using for theoretical interpretation of observation the two extreme cases of radiative exchange — true absorption and then scattering — the differences for most types of stars come out of the same order of magnitude as the usual errors of measurement. This seems to indicate that in stellar work of medium accuracy it is often not worthwhile to worry about fine details of radiative transfer, but just to go ahead.

— R. N. THOMAS:

I will give specific examples to the contrary. Would you admit Lyman-alpha in the sun as a good example or should I go to the stars?

— A. UNSÖLD:

I was only talking of stellar spectra of fairly normal types and in the usual visual range.

— R. N. THOMAS:

Then we should be in agreement that using solar observations, it is easy to distinguish between the LTE and the non-LTE predictions; between the classical method of assuming that a line is formed in some intermediate between pure absorption and pure coherent scattering, and a more precise detailed theory such as PECKER and I summarized. Thus it is not a question of what theoretical approach is *correct* — we can check that on the sun — but whether the detailed theory is too refined for the accuracy of stellar observation.

Turn to the stellar observations of Ca^+ H and K, by O. C. WILSON, whose interpretation is of strong interest to this aerodynamic-astrophysics colloquium. The observations show an absorption line with a self-reversed, emission core (M-shaped core). Observationally, the separation of the emission peaks increases from hotter to cooler stars along the spectral sequence. A number of authors have interpreted this relation to imply an increase of « turbulence » from hot to cool stars, assuming that the fine details of a transfer problem can be ignored so that the profile of the central « absorption core » simply reflects the profile of the absorption coefficient. If one wants to follow the logic suggested above, he would proceed to ask into a better interpretation of such a profile by saying he has three choices: either interpret on the basis of pure coherent scattering, or on the basis of pure absorption — that is LTE — or try to get a complete non-LTE theory. The questions the theory must answer are: how does the amplitude of the emission peak and the relative amplitude of the emission peak to the emission minimum depend on physical quantities actually in the atmosphere — velocity fields, electron density, etc. I assert that if I use coherent scattering alone, I will not predict the presence of the emission peak. If I use LTE, then I must assume that the temperature is first low, then high, then low going into the atmosphere (since $I(\nu) \to B_\nu(T_e(\tau_\nu))$, Sections 2.3, 3.1 of our summary-paper). On the other hand, going to the non-LTE interpretation, one comes to the conclusions that the features are explained by a monotonic outward rise in atmospheric temperature, and the relative amplitude of the emission peaks plus the separation of the emission peaks is a strong function of the temperature gradient in the atmosphere in addition to whatever velocity effect may exist. This picture would not come from either of the alternative two procedures, and it is hardly based on theoretical differences comparable to the observational uncertainty of the data.

— A. UNDERHILL:

To obtain emission — you have to have either an extensive body of gas bigger than the star itself, or a temperature that does not, as in a normal star, increase steadily inwards. Now your first two cases, which are very simple, can't possibly give a line like that. Therefore when you have an emission or something peculiar — you know that you have not just a simple case.

— R. N. THOMAS:

I only remark that here we have a good example — involving many stars — where the stellar observations are quite good enough to show the inadequancy of the several simple theories you would have us generally use.

— M. MINNAERT:

It is clear that if you make in the fashion of UNSÖLD the whole series of models, looking not only to LTE or to non-LTE, emission, absorption, diffusion and so on, but also inserting all possible values of micro-turbulence and macro-turbulence, a comparison with observation gives a very full possibility of judging about the model. Only, the question is whether it will not require a great amount of phantasy to combine all possibilities of nature, not forgetting anyone. On the other hand if you are able to interpret inductively the observed profile and to translate that into the source function and the atomic level populations then of course you must take into account the limit of precision of your observations and see whether you have reached the same precision in your theory. It the theories of Fraunhofer lines both methods of approach have been used. They have both their value and I believe that their respective merits have now been put forward sufficiently.

— K. H. BÖHM:

PECKER has strongly emphasized the difficulties which occur if one has to determine the *source function* in a non-LTE situation.

One should also note that the same difficulties occur with regard to the interpretation of the absorption coefficient in the non-LTE case. Consider

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for instance the most general case of a subordinate line for which the source function varies with wavelength. Consider a three-level model of an atom, in which levels 2 and 3 are broadened, and absorption from 2 to 3. Level 2 may be reached by absorption from level 1; but if the source function is not constant within the line, this means there is no complete redistribution of the atoms over the level 2. This means the distribution of atoms over level 2 depends on the details of the radiation field in line 1-2. But the distribution in level 2 certainly influences the absorption coefficient in line 2-3. So one cannot interpret in such a case the frequency-dependence of absorption coefficient without knowing what goes on in the other line. Moreover deviations from the Saha's equation alone would lead *e.g.*, to deviations in the absorption coefficient and not in the source function. So it is my impression we need not only a better understanding of the source function but also of the absorption coefficient in the non-LTE case.

— R. N. THOMAS:

I certainly agree that one should study non-LTE effects on opacity as well as on source-function; I do not know how one could study one effect without studying the other. Indeed, our own studies of chromospheric non-LTE effects concerned opacity before they concerned source-function; we were led from the former to the latter. Except at such large optical depths that one does not see them in the line, a non-LTE effect on opacity invariably leads to one on source-function. I would only disagree that in the case you cited, the source-function is necessarily frequency-dependent. I would expect it to be ν-independent at least in the line-center, fixed wholly by non-coherence induced by the thermal velocity field; and any other broadening of the energy levels simply introduces more non-coherence in scattered radiation, thus less ν-dependence in source-function.

— E. BÖHM-VITENSE:

PECKER mentioned the method of determining velocity fields from the absorption coefficient, as used by DE JAGER and GOLDBERG, and later by UNNO to determine the depth-dependence of the velocity field in the sun (cf. Section **3**.1.1) I would emphasize that without knowing something *a priori* about the distribution of line-absorbing atoms, we cannot say to which level the measured velocities correspond. As PECKER pointed out, the method is probably useful for a Milne-Eddington distribution; viz., no depth dependence of ratio of line to continuous opacity and of profile of line-absorption coefficient. However, for a Schuster-Schwarzschild distribution — all line-absorption atoms concentrated in a thin layer — then all velocity averages are performed over this thin layer. However, the layer to which one sees at total optical depth $\tau = 1$ may lie below this thin line-absorbing layer.

Thus, the method gives $\bar{\alpha}(\Delta\lambda_i) = \bar{\alpha}(\Delta\lambda_j)$ from one pair of points on the two lines, leading to a $\bar{V}$ which we assign to the geometric level corresponding to $\tau = 1$. Another pair of points of equal intensity may correspond to a $\tau = 1$, thus to another geometric level, to which we assign the $\bar{V}_1$ determined from this pair of points. But, both $\bar{V}$ and $\bar{V}_1$ represent averages over the same, thin, line-producing layer; and if we do find a change in velocity, it would mean that there is a *very* steep gradient in velocity in the high atmospheric layers, in the case of our example. This situation is true mainly for neutral atoms of low excitation. So one must be very careful in using this method.

— M. MINNAERT:

Since in your example, the optical thickness of the line-forming layer must be less than 1, would you agree the method becomes useful for thicker layers?

— E. BÖHM-VITENSE:

No, because you still measure the mean of the velocities down to a certain point, and you still don't know the depth to which the mean refers. Also, in many cases you cannot very well compare the two lines because you don't take the mean over the same regions.

— J. WADDELL:

PECKER considered two points I should like to comment on. The first is the identity of the source function in lines of the same multiplet; the second is the frequency independence of the source function. Both of these points are subject to observational checks. Consider the case of the Na D lines. The emergent intensity on the disk of the sun for Na D_1 or Na D_2 is given by — using j to denote which line, and assuming a common source-function —

$$I_j(\mu) = \int_0^\infty S(x) \exp\left[-\tau_j/\mu\right] d\tau_j/\mu \,.$$

At any geometrical depth, x, the optical depth $\tau_2(x) = 2\tau_1(x)$ because the f-values of the two lines are in the ratio of 2 to 1. In this case

$$I_2(\mu) = I_1(\mu/2)\,.$$

This equation should break down in the wings of the line where the source function of the continuum becomes important and where the damping enters.

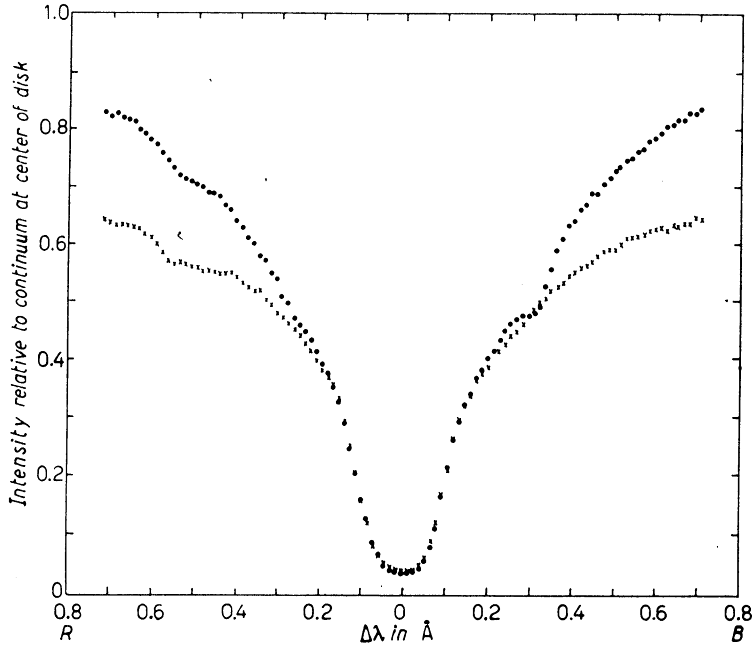

Fig. 3. – Comparison of $I_2(\mu, 0)$ with $I_1(\mu/2, 0)$ in core of lines (not corrected for telluric absorption). × Na D_1, $\mu = 0.50$. ● Na D_2, $\mu = 1.00$.

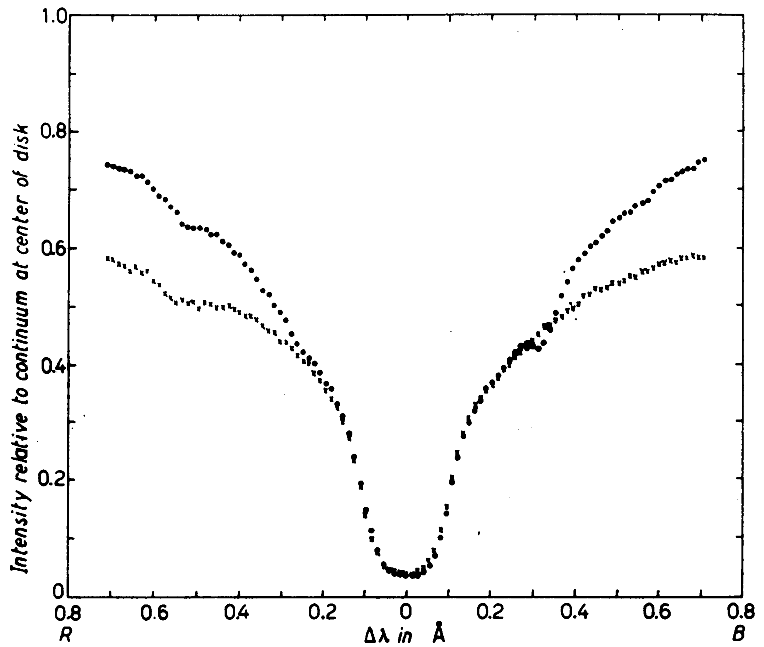

Fig. 4. – Comparison of $I_2(\mu, 0)$ with $I_1(\mu/2, 0)$ in core of lines (not corrected for telluric absorption). × Na D_1, $\mu = 0.40$. ● Na D_2, $\mu = 0.80$.

At the Sacramento Peak Observatory I have made center-to-limb observation of the Na D lines where the μ's were chosen in the ratio of 2 to 1. Figs. 3, 4 and 5 demonstrate the comparison. It would appear that the

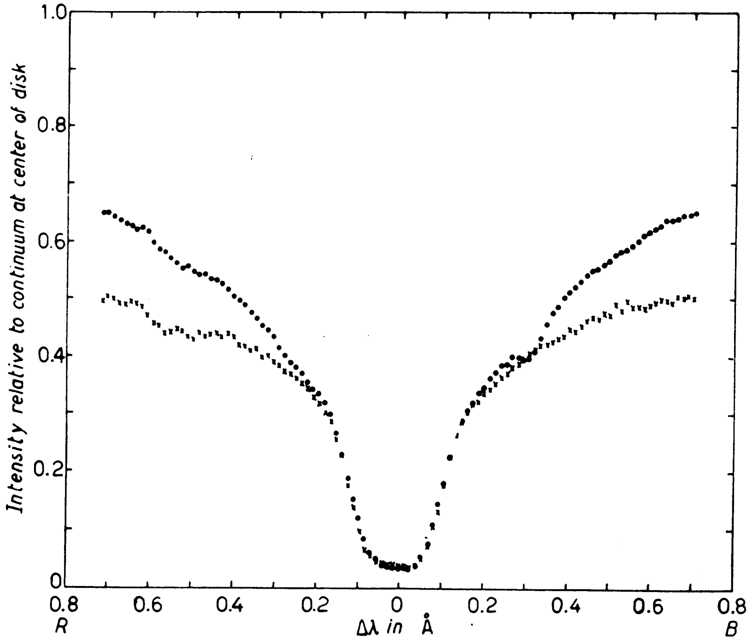

Fig. 5. – Comparison of $I_2(\mu, 0)$ with $I_1(\mu/2, 0)$ in core of lines (not corrected for telluric absorption). × Na D_1, $\mu = 0.30$. ● Na D_2, $\mu = 0.60$.

assumption that the source function of the Na D lines is identical, is a reasonable one. Next we use the intercomparison method at a particular μ to derive the Doppler width. Fig. 4 (*Ann. d'Ap.* **23**,921, (1960)) demonstrates the values of $\Delta\lambda_D$ derived for various intensities at the given disk positions μ. Goldberg's results for the Ca^+ H and K lines are compared. In both the H, K lines and the Na D lines at $\mu = 1.00$, we find that the Doppler width increases with depth into the sun and yelds temperatures in excess of 100 000°. GOLDBERG suggests that the intercomparison method is invalid if the source function is frequency-dependent; I feel this must be the case because we have excluded for the Na D lines that the fault might lie in different source functions for the two lines.

However, it would appear that the source function is completely non-coherent (*i.e.* frequency-independent) over the region where $\Delta\lambda_D$ is flat, namely for intensities up to 15 per cent of continuum. The Doppler width obtained in this region is on the order of .04 Å, yielding a temperature on the order of 5 400°. On the other hand, a temperature of 4 000° would

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permit a random « turbulent velocities » on the order of 1.8 km/s. This tentative conclusion is in agreement with UNNO. His results based on McMath-Hulbert Observatory tracings indicate also a decrease of turbulence with height.

— K. H. BÖHM:

I find the 3-term form of writing the source function very convenient if one considers resonance lines or very special types of subordinate lines. But for the general case of subordinate line, I cannot understand that it is useful to make such a distinction between lines of photoelectric type and collision type. Consider a many-level atom. The emission coefficient in line $(n+1) \rightarrow n$ is coupled to all other possible transitions within the atom, and therefore to the radiation field in all lines and the continua. But this coupling is usually not expressed explicitly (though it is certainly implicit) and I do not see how such a line can be characterized as either photoelectric or collision type. It has often been asked whether radiative equilibrium with coupling between many different levels would not under certain conditions lead to level populations close to the case of LTE even if the radiation field in the different lines is not given by the Planck function.

To answer for instance this question one might perhaps prefer a different formulation of the source function like *e.g.* that of HENYEY, which within a certain transition frame of approximation shows the coupling between the different transitions explicitly.

I should like to ask THOMAS whether he thinks that his source-function describes any situation or whether it is his own opinion that he really wants to have its application limited mainly to resonance or to certain types of subordinate lines.

— R. N. THOMAS:

I share your concern — and believe this is one of the chief problems facing us in discussing the source-function. Certainly everything we have done up to now is only for resonance lines — really only for a 2-level atom plus a continuum. Now there are two alternate ways of trying to extend the methodology that JEFFERIES and I have introduced for the resonance lines. On the one hand, one could do explicitly just what you mentioned, try to take all the transfer problems in all the lines into account. JEFFERIES has set up a chain-process-type attack in an attempt to investigate this problem (*Ap. J.*, 1960) in certain simple case. It essentially comes down to a set of simultaneous rediative transfer equations, which are very messy. From my own standpoint, I prefer to try to reduce the problem to an « equivalent 2-level atom » — write down the equations of statistical equilibrium for the 2 levels, taking into account all the transitions then try a perturbation

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treatment. The source-function is a ratio of 2 absolute rates; emission in the line : the absorption in the line. Retain these, and transitions to which the atmosphere is transparent, as absolute rates; introduce other radiative processes, requiring solutions of a transfer equation, as net rates, which you evaluate as perturbations, by iteration on the two levels to which they correspond. Therefore we have reduced these 2 exact equations of statistical equilibrium to an equivalent 2-level atom, using exactly the physical idea of the three terms we had before: one term is the radiation in the line itself; a second is the collision in the line itself — those are the only 2 direct processes; any other process, ionization and re-capture, or excitation to a higher level and cascade, is an indirect process, which you treat in exactly the same way as you did the terms to the continuum in the 2-level (plus continuum) approximation.

— K. H. Böhm:

Let us assume you use the iterative procedure for calculating a particular subordinate line, say the Paschen α line, corresponding to a transition from $n = 4$ to $n = 3$. Now for an atom in the level $n = 4$ usually the probability for making a transition to, say $n = 2$ or $n = 5$ is of the same order of magnitude as the probability for making a transition to $n = 3$. Now you start with a 2-level atom consisting of $n = 3$ and $n = 4$ and use an iterative procedure, though the probabilities of transitions to other levels are of the same order of magnitude. I don't see how this procedure converges.

— R. N. Thomas:

The idea is that you express the 3-4 transition as an absolute rate, and the 2-4 transition as a net rate; thus the *relative* size of the latter becomes very small. If I can get a rough approximation to its value, there is a *hope* that error will not perturb the solution too much. Work along these lines has just started. Some of it is described in Chap. 9 of the Chromosphere monograph (Thomas and Athay (1961): cf. bibliography in Pecker-Thomas paper).

— G. Elste:

First, a remark on the question of a more realistic model of distribution of absorbing material, raised by Mrs. Böhm-Vitense. The cores and flanks of medium strong lines are formed in a layer about 150 km thick, compared with a thickness of about 300 km for the whole photosphere. Plot the contribution of each depth to the I_ν-integral, *vs* $\log \tau_c$. You find bell-shaped curves, with half-width about $\Delta \log \tau_c \sim 1.4$ (Fe, $\chi \sim 4$ eV). For a line twice as strong, the position of the contribution curve shifts upward about $\Delta \log \tau_c = -0.4$. So the layers over which one has averaged in the two cases overlap considerably. The $\log \tau_c$, to which the $\Delta\lambda_D$ found by Unno's procedure refers, may be the center of gravity of the contribution curve. This is not necessarily the depth at which the value of source-function equals the emergent intensity, as used by UNNO.

Second, I have a result bearing on a choice between depth-variation of velocity and ν-variation of source-function. Restrict attention to such weak lines that the only broadening mechanism for the absorption coefficient is Doppler; use $T_e(\tau_0)$ obtained empirically from the continuum; and assume $S_\nu = B_{\nu_0}(T_e)$ to compute line profiles, without introducing any turbulent velocity. These computed profiles are only some $\frac{2}{3}$ as wide as those observed. One choice for an explanation lies between introducing non-thermal motion and introducing a ν-independence for S_ν.

I think, a priori one can say that the total source function in the line $(S_s + r_\nu S_c)/(1 + r_\nu)$ can differ from that in the continuum, and therefore can have a ν-dependence, only where the total absorption coefficient departs non-negligibly from the absorption coefficient in the continuum. If one has a thermal motion according to the kinetic temperature T_e in the photosphere, the line absorption coefficient — which has to be added to the continuous absorption coefficient — has a width which is only $\frac{2}{3}$ of the width of the observed line. How can the source function differ from that in the continuum over a range larger than the one where the absorption coefficient is different? This is only possible if there exists an additional widening such as non-thermal, so-called « turbulent » motions provide. So as a first approximation, a non-thermal velocity field with two unequal components, horizontal and vertical, each depth-independent has been introduced (ALLEN, WADDELL). These two quantities are fixed by two observed quantities, central dip and half-width. We are able to fit the center-to-limb variation of the equivalent width and half width and to reconcile computed and observed profiles at the limb, but there still exists a discrepancy in the profile at the center of the disk as shown in Fig. 6. The question is do we revolve this discrepancy by introducing a depth-dependence of the vertical motions, or do we try a ν-dependent S_ν? I believe we can exclude the latter unless, in addition to the ν-dependence, an anisotropy of the source function is introduced, because the discrepancy only appears at the center of the disk but vanishes at the limb.

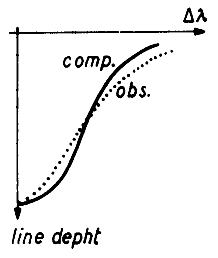

Fig. 6.

— *Ed. note:*

Write

$$I_\lambda = \int (S_\lambda \exp[-\tau_\lambda/\mu]\tau_\lambda/\mu) \, d \ln \tau_\lambda .$$

Quantity in brackets is the contribution function, on $\log \tau_\lambda$ scale, for I_λ.

— J. Waddell:

Pecker spoke of the difficulty in distinguishing between depth dependence and angular dependence in turbulence. Let me show why I believe it is possible to distinguish between the two effects. Consider the contribution function of the central dip of two lines, A and B, at two positions on the disk ($\mu = 1.00$ and $\mu = 0.35$).

Line A at disk position $\mu = 0.35$ is formed at the same optical depth as line B at disk position $\mu = 1.00$. Nevertheless both lines A and B are represented with a constant radial component of turbulence of 1.8 km/s and a constant tangetial component of 3.0 km/s. If the increase of Doppler width to the limb were interpreted as an increase of turbulence with height above the photosphere for line A, it would not be possible to compute the correct half width of line B at the center of the disk.

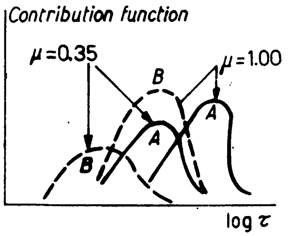

Fig. 7.

Since this somewhat idealized case is similar to the results I obtained at the McMath-Hulbert Observatory, for 11 lines, I feel that one is able to distinguish between the angle and depth dependence. Further, when one looks at the variation of the half widths with μ, he finds a strong increase near $\mu \sim 1$, leveling off at smaller μ, for many wide lines. This is an immediate clue that we are concerned with a μ effect and not an optical depth effect; for optical depth effects show themselves most strongly quite near the limb. It is possible to represent the observations with a depth dependence of the form

$$\nu^2 = A^2 - (A^2 - B^2)\tau^2 ,$$

but unlike the μ dependence, the values of A and B depend on the line chosen. However when I used non-isotropy, I could explain eleven lines with the same 2 constants; 1.8 km and 3 km (cf. Waddell: *Ap. J.* **127**, 284 (1958)).

— J.-C. Pecker:

In presenting the summary-introduction, I mentioned the possibility of such effects as discussed by Waddell originating in non-LTE effects. At Meudon, we have been looking at Unno's graph of his photospheric results, giving rms velocity variation with depth, which show that the points coming from the lines of Ti and those from Cr fall systematically on opposite sides of his mean curve. This result is easily interpreted, if we assume Unno made (implicitly) a mistake, computing the optical depth of each of these lines in the classical way. Now if one introduces non-LTE considerations from a purely empirical standpoint, he finds that this dispersion of points is entirely due to neglected non-LTE effects (J-C. PECKER and L. VOGEL: *Ann. d'Ap.*, **23**, 594 (1960)).

— J. WADDELL:

Note that previously the increase in Doppler width with decreasing μ had been explained as a depth-dependence — turbulence *increasing* with height in the photosphere. Unno's results go just in the opposite direction. I do not think we should argue whether he is right or wrong. I would only note that his results are a smaller magnitude motion, compared to those from line-broadening to the limb. Unno's results do not explain my observations, nor are they an alternate picture. Also, they are based only on observations made at the center of the disk. Our interpretations are compatible.

— M. MINNAERT:

There is one point which was mentioned in the speech of PECKER where the aid of the aerodynamicists will be especially useful for us. That is the question whether supersonic turbulence is possible; we have an aerodynamicist prepared to give his opinion on this point.

— F. H. CLAUSER:

When asked if I would talk about whether supersonic turbulence were possible, I said that I was not prepared to answer the question. MINNAERT said it's always the same with aerodynamicists, that any question we ask them has not been worked out, so give your beliefs.

In order to make any meaningful statement, I feel that I will have to go back a bit with our concepts to lay a little foundation. Suppose that we were to be confronted with a velocity field, of a single component species. Later on I'll come back and try to give an indication of what happens when many species are present. Such a velocity field for this single component is specified by the three components of velocity: U, V and W, — each as functions of x, y, z and t. But for simplicity of illustration in the example that I shall quote to you, I shall assume that there is a single velocity component, U and that it will be a function of a single variable x.

I try to avoid those questions that mean the difference between a vector variable in a vector space with a time variable parameter, and those which you can talk about, a scalar variable in a scalar space. There are many problems which revolve around the difference between vector and scalar variables, but I think I can avoid these without any controversy. Now — so far as the aerodynamicist is concerned, there is an important fact of life that emerges and covers a good deal of our thinking and background — that

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is, those particular common fluids that are given to us to work with, such as air and water, have such a low viscosity that if you were to compute the Reynolds' number from any realistic combination of parameters, you invariably get a very large number. (The Reynolds' number is defined as a velocity times a length divided by the kinematic viscosity — viz., the ordinary viscosity divided by the density of the fluid.) In fact to get Reynolds' numbers of the order of 1, you either have to go to such microscopic sizes for reasonable velocities that you can't use ordinary instrumentation any more, or you have to go to such low velocities with ordinary dimensions that you haven't any instruments capable of measuring them any more. Basically this means that in those characteristic features that we see in turbulent flow, — eddy sizes, etc. if we take any characteristic velocity and any characteristic length, and divide by kinematic viscosity we invariably get large numbers. Now this is the background on which I base the next part of my remarks. You all are familiar with the difficulties of analysing non-stationary random functions. If you simply start with truly non-stationary random functions, it's hard to know just where to begin, and invariably you make assumptions that the random function must be stationary or some such postulate. Now the way that many of us start is the following. Supposing that we were in fact given this simple plot: one velocity component against a single variable x. If we take a cut at a given instant of time through the fluid and measure the result, we get some such random function as this.

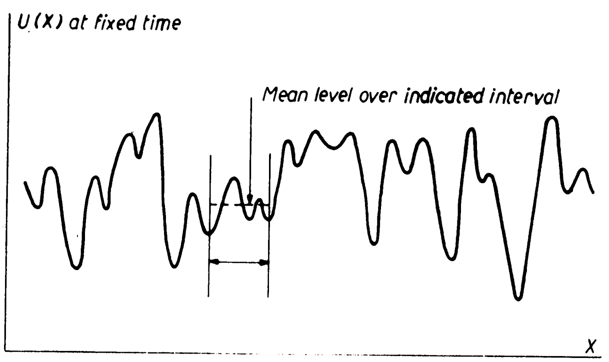

Fig. 8.

Now supposing, to be definite, we were to select, to begin with, a characteristic size — which I will vary in a moment — but pick out a given size and say that we shall analyse a sample of this length. We see that its velocity has a mean level, and around that mean it has fluctuations. If we look at the dynamics of such a system, we find that if we consider those particular parts of a fluctuation that are small compared to this length, then from a physical point of view I have an eddying motion taking place. The fact that I've picked the mean here, as the start, implies that I no longer ask about an absolute magnitude of velocity but simply have a glob of matter here in space that is undergoing an eddying motion, and I walk along with it, so that it looks to me as though it's standing still on the average. And within that chunk of matter, there is a complete hierachy of eddies. Since I've restricted my consideration to something this big or smaller, I can't talk about bigger eddies. They do not exist for me — I've simply cast them out by simply agreeing to go along locally with this glob. You are all familiar with the classical concept of viscosity and what it means. If I have a streaming motion in which different layers are moving at different velocities, and a sub-motion takes place — it will carry with it momentum from one layer to another. Now, in this problem that I consider, I find that there are these small-scale motions taking place, doing exactly this interchange of momentum, exerting an effect of viscosity. Let me tie this in now with my fact-of-life of a moment ago, that the ordinary, thermal-motion, viscosity is quite small. Physically what is happening is this: I have a glob, which has a characteristic motion within it that is distinguishable if I look at sizes that big, but there is a random smaller motion, still large in scale compared to the thermal motion, but much more important in transferring momentum within the glob than is thermal motion. This smaller sub-motion is acting as a viscosity to the glob itself. In this way energy is transferred from the large-scale motion down the size-scale to thermal motion. We express this quantitatively in terms of what we call a power spectrum. It is essentially the square of the amplitude of the ordinary part of the spectrum. Plot here the wave number against the energy per unit wave number size, and we find that for a characteristic turbulent motion we get a curve that looks something like this.

I've been talking about what goes on in a central portion of the spectrum — the part indicated by an arrow. It is an empirical fact backed up by a number of theories that F varies as $n^{-\frac{5}{3}}$ in this region. This result emerges purely dimensionally if you make the assumption that all of the energy transfer is taking place by the smaller eddies acting as viscosity for the larger hierarchy.

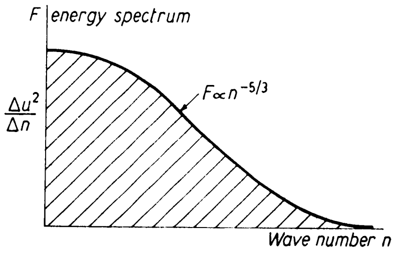

Fig. 9. – Power spectrum.

But now consider the two extremities of the curve. First if I consider large n, small eddy size, I find F decreasing continually. If I multiply the amplitude — which is a kind of velocity — by n^{-1}, which is a kind of length, I can form a kind of local Reynolds' number, which gets smaller and smaller. There comes a place where this is of the order of the number computed from thermal viscosity.

It is at that point that the energy is no longer transferred from the bigger eddies to the smaller eddies, but now begins to be transferred by viscous action to heat. So we have the concept: energy in the big eddies gets transferred to even smaller eddies down the chain, and eventually is converted into thermal energy.

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But at the other end, if I progressively increase my length scale — invariably I come to the scale where there is something feeding the energy in. In laboratory experiments it is invariably a solid wall, an obstacle, a wing, a propeller blade, or it can be convective eddy sizes, Benard cells. It is at this point that the statistics seem no longer to be appropriate; you get a field that you do not analyse statistically any more. You begin to search for the non-linear driving mechanisms that feed the energy from a moving, solid wall, or a convective cell, etc. That dimension, we are missing. Our representation breaks away from this curve when you reach sizes that are particular to the boundary layer, the wave size, the cell size, etc.

So far it looks as though I am not talking at all about supersonic turbulence, but I had to get this background laid for it. Now, we have this velocity field, which so far I have treated as a continuum. Supposing that we were to use even smaller instruments in making our measurements. If we were to do so we would find that the curve does not look like this. To be specific, supposing that I were to introduce a little cork ball in the fluid, and actually trace out its motion in order to determine the velocity field, then introduce progressively smaller and smaller ones. Now, to a certain range, I will map out this velocity field, but as the ball gets small enough there comes a time where the Brownian movement begins to be perceptive. Then I get a change in the curve, a tail is added for large n — above the tail, I have what I call turbulence; below it, I have the thermal motion.

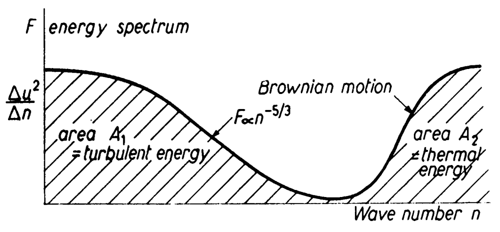

Fig. 10. - Power spectrum.

Now, this is essentially $\Delta u^2/\Delta n$ plotted against n. So the area under this « turbulent » portion of the curve, A_1, gives me the turbulent energy, u^2. The area under this « thermal » curve, A_2, gives me the velocity of sound squared, a^2.

Now the question about supersonic turbulence in our view arises in the following way; the turbulence would be supersonic if this area A_1 became larger that this area A_2. Is this possible? And what new concept would happen if it were to become so? Now in the first place, all of our laboratory experiments have been in cases in which this area A_2 is considerably smaller than A_1; the ordinary cases of subsonic turbulence.

But let us look and see what happens now if we were to approach the case of supersonic turbulence. I would do this not by making A_1 bigger — rather hold it constant, simply decrease the temperature, and see what happens. When looked at this way, there is a unity that did not exist before. The energy of one eddy was fed into a smaller eddy, and a chain arose. But there came a break in the chain; this part of the curve at the break was not drawn; I simply said I had to invoke a new mechanism, viscous dissipation, which fed the energy into the thermal part.

Now I do not have to do that. I can talk about this whole process as part of a single curve, and simply say that the energy is being continuously transferred down the curve and into that portion of the curve which is the thermal energy. So I can apply the whole process throughout the entire curve. If we go toward supersonic turbulence, not by making the velocity larger but by making the velocity of sound smaller, the amplitude in the thermal region will come down. There is a coupling between these two regions, so the amount of viscous dissipation at the onset of the thermal region is reduced. So in the process of following another curve in the thermal region, the position of the curve in the turbulent region is raised (cf. Figure). To reduce the viscosity, all you do is to have the eddies extended to smaller scale. So the net flow of energy down the cascade of eddies is still the same. As we continue to decrease the viscosity, the « minimal » position of the curve simply moves out even farther.

Eventually, the smallest eddies become comparable to the mean free path and there is really no clear distinction between eddies and thermal motion anymore.

Originally, almost all work was done on incompressible turbulence. The advent of high speed flight forced us to consider the effects of compressibility on turbulence, and as usual, the effect of compressibility wasn't just to shift thing a little — it meant going back to some very fundamental ideas. You go through the formal process of setting up the Navier-Stokes equations, including the conductive and convective heat transfer equations, the equation of continuity, etc. — they are horribly non-linear, almost hopeless to try and solve. But for small perturbation of the flow, you make the following assumption — you assume that essentially you have a uniform background field on which you superimpose fluctuations of temperature, pressure, density, velocity, etc. This gives you a set of partial differential equations of high order, which you can linearize by saying that the fluctuations are small compared to certain basic quantities — *e.g.* the velocity of sound, the fluctuation must not be large compared to the basic temperature, etc. And in this process you find the high order partial differential equations can be split so that the total field of variation can be represented as a set of solutions of several different partial differential equations.

We interpret that as saying that the full compressible Navier-Stokes equations have solutions that represent different modes of behavior. Now if you examine these modes of behavior, you find that one of the equations is essentially the equation of sound modified by certain small terms in which you have simultaneously occurring density fluctuations, temperature fluctuations, velocity fluctuations, etc. In addition there is a second set that essentially corresponds to shearing motion. And these equations have all the characteristics that we have associated previously with incompressible turbulence. There is a third set of equations that essentially correspond to heat transfer, but this is a convective type of heat transfer, and it is not ordinary heat transfer in which there is no velocity present. It turns out there still are velocity- temperature- pressure-, etc., fluctuations, but in different proportions — the velocity is much smaller now. And it turns out that as long as the fluctuations are small, any random field can be categorized in these three parts — so much eddy-turbulence, so much random-sound, — and so much hot spots which are diffusing. But as one increases the level of the fluctuations, non-linear terms begin to appear, and cause what had been neatly categorized as eddy-turbulence to begin to feed over into the random sound, and also over into the conductive, convective transfer.

Now there comes a point where you can no longer say that this particular thing is eddy-turbulence, and this particular thing is random sound, and that particular thing is a set of hot spots randomly distributed throughout the field. Now we believe that your physical intuition about the existence of turbulence and about the existence of sound waves and about the existence of hot spots and so on is not an arbitrary one; that there really is something in nature, that this mathematical splitting goes into your intuition, and that you find that there is this splitting. But, we think your intuition begins to fail you under strong conditions, where the amplitude becomes so large that you can no longer split those things and say that is sound waves, this is turbulence, and so on.

I should like to say that aerodynamicists have done some additional work on the effect of magnetic fields. We find that as soon as magnetic fields are present that will influence the flow field, the order of the equations increases and they become even more complex. A typical new phenomenon that enters is the waves first discussed by ALFVÉN. These appear as an additional type of mode for the fluid.

So, when we ask if supersonic turbulence is possible, the first question that arises is whether, with such large amplitudes, as would be required by supersonic turbulence, the various modes of behaviour can be separated out, or do they necessarily become strongly intertwined. Also, the question arises as to whether, in supersonic turbulence, the turbulent motion can be separated from the thermal motions, or do they blend together.

In astrophysical examples, it would appear from what has been said before, that an additional complication is present. This is the possible streaming of various constituents or species through each other. In the case of electric currents, we have a single example of such a situation, because the current represents a streaming of the electron through the ion and the

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neutral particles. In astronomy it appears possible for the various atoms to stream through each other. For such problems, relatively little work has been done. It is quite possible that if such cross-streaming takes place on a large scale, gross instabilities can occur, and turbulence would appear, driven by other new interaction mechanisms. This will bring in wholly new concepts that are not contained in our present experience.

— M. MINNAERT:

On the case of different kind of atoms, we astronomers would have given as the criterion — different kinds of atoms will have different velocity of thermal motion, inversely as $(\text{mass})^{\frac{1}{2}}$, but the same velocity of turbulence.

— G. ELSTE:

One has to be very careful whith such a simple criterion. If he deduces different turbulent velocities, or different thermal velocities from different widths of different lines, it is not clear that the results refer to the same atmospheric region. Especially, this is true in extended atmospheres, or prominences, where regions of completely different physical conditions lie along the same line of sight.

— J. C. PECKER:

And, one must be careful in saying non-thermal motions show no correlations with properties of atoms — *e.g.* gyromagnetic motion when magnetic fields exist.

— M. MINNAERT:

Let us not complicate things — neither by magnetic fields nor by speaking of different regions. If we have a small mass of gas, astronomers try to separate thermal from turbulent motions by looking at the difference between lines of different atoms. Must we assume this method is oversimplified?

— E. SCHATZMAN:

I want to comment on Clauser's nice description of so-called supersonic turbulence, in connection with several astrophysical facts or theories. Consider first the problem of the heating of the solar chromosphere by sound waves. It is usually accepted that we have in the convective zone some eddies, that some compression waves are produced by these eddies, and that these compression waves get out and refract in the upper part of the solar atmosphere. After a few wave-lengths these compression waves are transformed into minor shock waves of small amplitude — and we have in this region a random noise of small shock waves. When we use fairly reasonable estimates of the amount of energy dissipated, we do not find at the shock front a velocity jump larger than 2 km/s. With an amplitude of 2 km/s and a distance of 10 s between 2 successive fronts, we have quite enough energy for explaining the heating of the solar chromosphere. There are minor differences concerning the theory but general agreement.

We are not yet in the region Clauser described as supersonic turbulence: we have compression waves of a quite well understood nature, and if there is any turbulence, it is produced as a secondary effect by these random shock waves, and is a small effect.

Second, turning to the stellar case Struve has observed cases, in which (*Ap. J.* **104**, 138 (1946)) velocities inferred from line-profiles are much larger than one would expect on the basis of results from the curve-of-growth method. It has been interpreted by saying that we have very big masses in motion, and we integrate over the whole disk, like integrating over a series of small atmospheres moving at random at the surface of the star. Then, this profile is actually supposed to be a profile due to macroscopic motion. But what is remarkable is that from the excitation of the lines we have a temperature of the order of 7 000°, which corresponds to a sound velocity in hydrogen of about 8 km/s, while the profile corresponds to about 25 km/s. This is undoubtly macroscopic motion with velocities larger than sound velocity. It was a great temptation to extrapolate the above-mentioned theory of acoustic heating of the chromosphere to the case of a giant star with a large convective zone, purely phenomenologically. We suppose a large production of sound waves in the convective zone, and we suppose that the amplitude reached by the shock waves in the region where they are seen in the spectrum is large. But then it is undoubtedly the case Clauser has mentioned, when we cannot distinguish between random shock waves and so-called supersonic turbulence. I would only note that using this phenomenological theory, basing calculation on the assumption of a constant ratio of the mechanical flux to the light flux, and using an elementary theory of the line formation we could explain the width of lines in the case of four stars, the material I had in hand 10 years ago. It would be worth trying again with the material collected by Miss Underhill. Naturally astrophysicists would welcome any kind of improvement of the theory, so that we could abandon this phenomenological theory for a reasonable and sound theory of the random motions in such a case.

— E. N. Parker:

I would like first to emphasize a point that has been made many times before, when an astronomer uses the word « turbulence », he is not necessarily thinking of the same phenomena as the aerodynamicist. He means any motion that is not microscopic in the kinetic theory sense — but on the

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other hand which is of smaller scale than his own visual resolution. It might very well be ordered motion — rising and falling columns, such as Benard cells. With that point in mind, I want to discuss a single case of supersonic motion — let its scale be smaller than our ability to resolve visually so that I many legitimately call it « astronomical turbulence » — I think we can come to a specific conclusion for this special case. Suppose that I have a boundary with a semi-infinite medium on one side, and suitable machinery on the other side of the boundary to generate waves. These waves may be compressional sound waves — they may be transverse hydro-magnetic waves — they may be longitudinal hydro-magnetic waves. The principle in every case will be the same. I generate waves at the boundary and the waves propagate outward into the medium. Let the initial amplitude of the waves be 100 km/s. I use these numbers because they are not inappropriate to the solar corona. Let the temperature of the medium be 8 000 °K, so that the thermal velocity is $\sim$ 10 km/s. Now regardless of whether the wave is transverse or longitudinal, if it has such an enormous amplitude compared to the thermal velocity of the gas, it will steepen its front. The steepening will go on until halted by some sort of dissipation mechanism. If the wave were a sound wave, it would quickly become a shock wave. With the Mach number at 10, the temperature behind the shock wave will be much larger than ahead. The thermal velocity would be less, but of the same order of magnitude as, the 100 km/s fluid velocity in the wave. So as the wave sweeps off to the right the atmosphere returns to something like hydrostatic equilibrium. The original pressure is approximately restored and the temperature is some sizeable fraction of a million degrees Kelvin.

If the disturbance were isentropic — the original temperature would be restored; the irreversible character of the disturbance at such a large shock strength lets the medium retain a temperature near the peak value — so long as we neglect electromagnetic radiation.

Now if the gas should radiate fairly rapidly, relative to the interval between pulses, then the temperature which may have jumped a million degrees behind the shock will rapidly settle back to its 8000° equilibrium value. Thus when the next pulse is generated with 100 km/s velocities, it again will be supersonic, Mach ten, and repeat the cycle. Thus in this assumed case, an observer would see a temperature which is most of the time, over most of the area, 8 000°. He would find superimposed velocities of the order of 100 km/s. The observer would — in the sense that I said astrophysicists use the term — say that he had supersonic turbulence.

Now on the other hand, suppose that the pulses come sufficiently quickly that the million degrees does not have time to cool down by radiation between successive pulses. Then the next 100 km/s pulse is not very supersonic; it will dissipate energy — but not nearly as much. The next pulse will dissipate even less, and it will travel, of course, very much farther through the medium before it dies out. The medium remains near 10^6 °K. The point that I want to make here is that an observer looking into the gas would now see only sonic motions. He would see 100 km/s material velocities in a million degree gas, which is roughly Mach one. He would not have supersonic turbulence, as opposed to what he would see in the more rapidly cooling gas.

In the case of the solar corona, the temperature relaxation time is very long, somewhere on the order of a fraction of a day. And if one thinks of shock waves or hydro-magnetic waves coming up into corona every few minutes then the cooling between shocks is slight. One has, then, a situation where he would have subsonic turbulence, by virtue of the fact that the temperature will rise to that point where the waves no longer form steep fronts and irreversibly dump energy into the medium. This is the effect which at least some of us think is very important in the heating of such things as the solar corona, what may control the temperature of the corona, the temperature rises to that point where the waves no longer are supersonic. One must worry about this particular condition in any star or situation where he sees what looks like supersonic « turbulence ». Can you in fact keep the temperature down enough so that the « turbulence » is supersonic, or will the « turbulence » simply raise the temperature of the medium so quickly that you could not maintain it at a supersonic velocity?

— *Ed. note:*

(For a more detailed discussion of the « piston » problem used as example here, cf. the summary by WHITNEY in Part III-A. For a summary of the viewpoint that « supersonic astronomical turbulence » represents a blend of higher atmospheric temperatures and non-random macroscopic motion, cf. Chapter 1, *Physics of the Solar Chromosphere*, THOMAS and ATHAY (1961)).

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