ASYMMETRY IN SOLAR SPECTRAL LINES*

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Abstract. After reviewing observations of the spectral solar features originated either in the chromospheric layers or in the photospheric layers, from the point of view of the observations, and after having shown the strikingly discrepant set of interpretations that can be found currently in literature, a numerical experiment is performed in a case not too different from the solar case. It is shown that the use of the line bisector to determine, from the asymmetry of a single line, the trend of the velocity field might be considerably misleading, a fact which explains partly the results published in literature.

Clearly, asymmetries, in emission or in absorption lines, in a stellar or in a solar spectrum, can be due (if one excludes blends, or asymmetries of the instrumental profile, unsuitably corrected for) to *velocity* fields of some kind. On the other hand, the symmetry of a line does not exclude velocity fields, either 'macrovelocity fields', which can be such as to produce symmetric lines; or 'microvelocity fields', such that the integration along the line-of-sight, at any wavelength in the line gives place to a symmetrically broadened feature.

Therefore, the diagnostic of asymmetries might be insufficient to derive velocity fields; moreover, as we shall see, it will be quite difficult to make it unambiguous.

Both statements are leading to the conclusion that, in addition to asymmetry, other observable features will have to be observed: broadening, center-to-limb variation of broadening, intensification such as the one displayed by the height of the plateau of the curve of growth, and the like. Above all, very high spatial resolving power spectrograms should be able to allow us to disconnect the determination of the usual 'macro-velocity-fields', and to obtain them separately.

We shall briefly examine the two main types of observations – chromospheric and photospheric – of line asymmetries. We shall then look into the diagnostic problem, as it appears through the literature, and how we can see it now.

1. Chromospheric Features

1.1. The H and K lines of Ca II and Mg II

We shall refer to Linsky and Avrett's paper (1970) as one of the most complete bibliographical studies of the very numerous observations of H and K lines of Ca II in the solar spectrum.

The H and K lines, first observed (and named) in 1814 by Fraunhofer, are well known as the most conspicuous lines of the observable spectrum of almost all stars

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and galaxies. Figure 1 reminds the reader of some well known characteristics and notations referring to these lines.

The most remarkable feature of these two lines is their doubly reversed profile, well known since Hale and Deslandres, in the late eighties. The behaviour of this double reversal above spots, faculae, or at the limb of the Sun, has been studied in great details. We shall not mention these questions any more and shall send the reader back to Linsky and Avrett, as well as to the original literature.

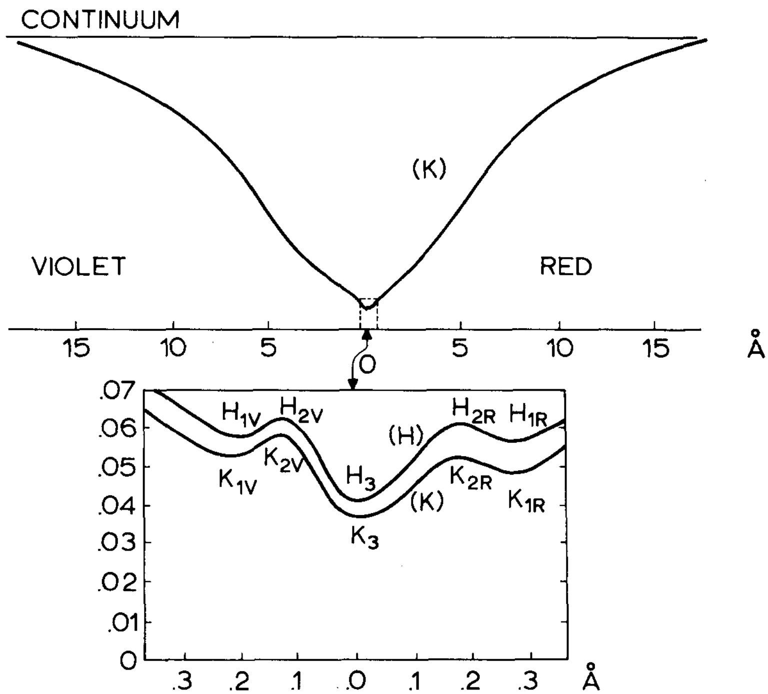

Fig. 1. *Usual notations relative to K-line. Top:* Small scale profile. The dotted rectangle is enlarged in the bottom part of the figure. *Bottom:* Large scale profile of both H and K (central parts of the profile).

The asymmetry of the doubled reversed peaks has been first observed by Jewell (1896), and studied in more details by Saint-John (1910). As a rule, it seems clear that the violet emissions K_{2V} is more intense than that of the red emission K_{2R}. *On the average,* the K_{2V} is displaced (to the blue, i.e. towards the observer, – if interpreted in such a simple-minded way) by 1.97 km s^{-1}, the K_{2R} component being displaced to the red by 1.14 km s^{-1}, the displacements being measured with respect to the center K_3 of the line. One interpretation is that the matter responsible for K_2 is rising, whenever the matter responsible for K_3 is falling. Of course, we shall come back on the analysis of this easy and early diagnostic.

Amongst the most significative studies made in the recent years, after decades of research, of such a dissymmetry, is the study by Pasachoff (1969, 1970). A rather good spatial resolving power allowed him to show that the above-described profile is only an *average* profile (this, we knew), but especially that the local profiles are extremely different from average, the standard deviation being considerable. According to the study by Pasachoff (a study which has been superseded in some way to the authors quoted at the end of this section), it seems that: (a) the 'normal' profile has only one peak, on the violet side; (b) the double peak feature occurs only in about 10% of the cases; (c) often, there are no emission peaks at all.

These characteristics can be derived from spectra such as the ones represented on Figure 2.

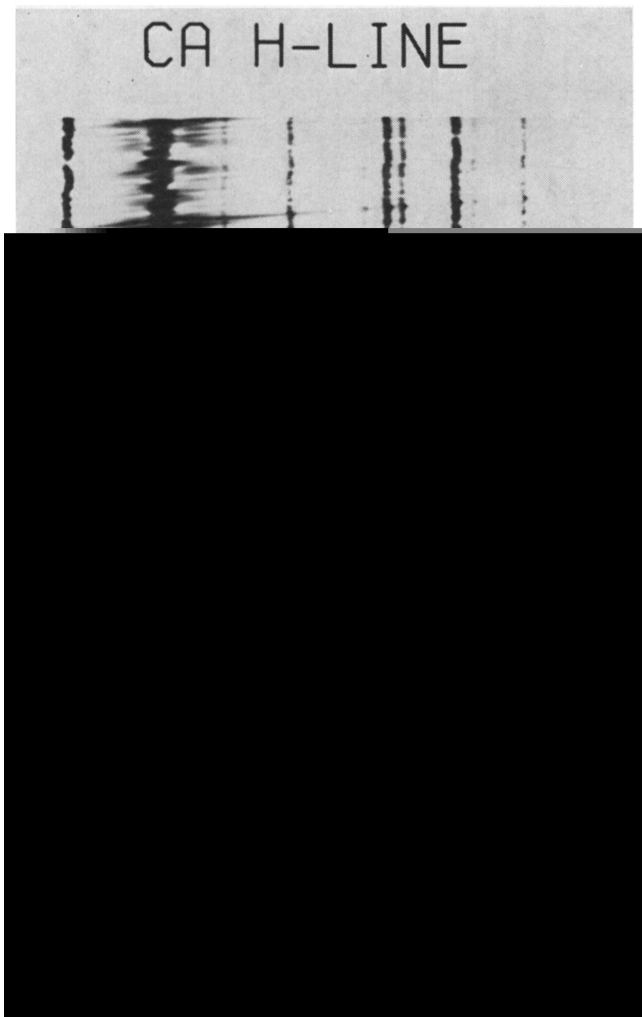

Fig. 2. *The H line (a section of it)* (from J. Beckers). Note the various aspects of the double peak asymmetries when going across the solar surface.

The asymmetry, with a given spectral, and spatial resolving power, at a point of the disk, is a function of time. This has been demonstrated clearly by Jensen and Orrall (1963) with a limited resolving power on the disk, and later confirmed by Pasachoff (1969), who found much larger variations. Spatial fluctuations of the two emission peaks K_{2V} and K_{2R} are badly correlated, – as could be expected from the above description by Pasachoff. The position of these peaks fluctuate; the rms value of these fluctuations is of the order of 0.04 for K_2, of 0.02 for K_3. The asymmetry of the center of the K line thus affects the spectral location of the peaks, and their relative intensity.

It should be noted that the K line asymmetry is diminishing towards the limb. However, off the limb, where the line appears only in emission, there are little indications on how the asymmetry behaves. It would be of course quite interesting to know this better.

High resolution (both spatial and temporal) observations are now the field of intensive work; those by Bappu and Siravaman (1971), Wilson and Evans (1971), and Wilson *et al.* (1972) are worth mentioning. In some cases the evolutionary behaviour of individual features is reported, and this offers clearly the possibility of testing in a more precise way the various theories of formation of the lines.

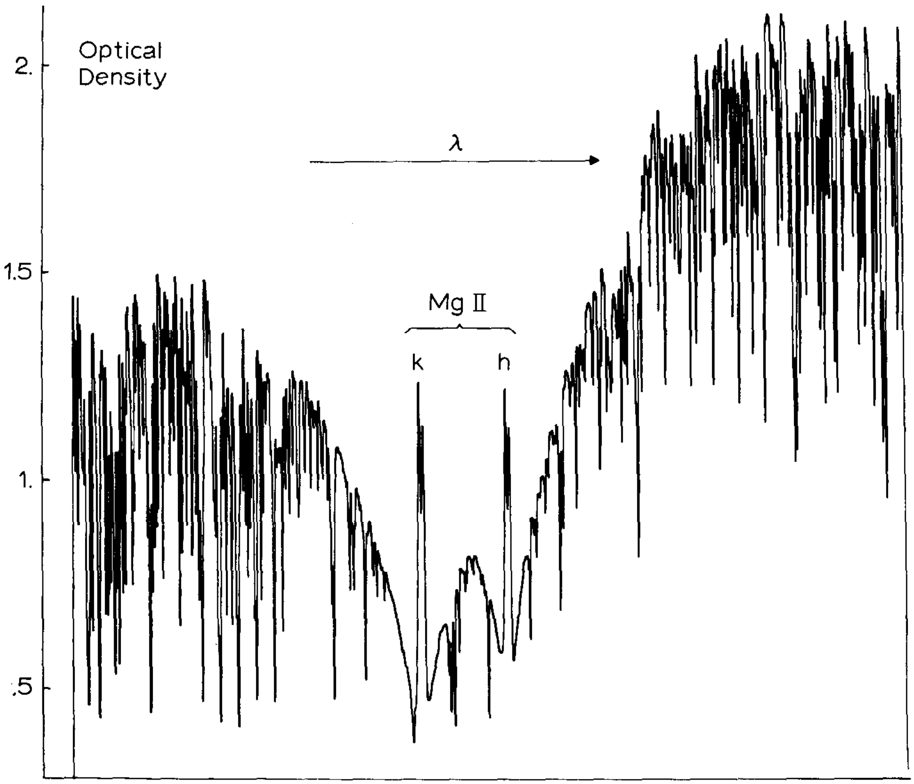

Fig. 3a. *Small scale profile of H and K lines* (after Lemaire).

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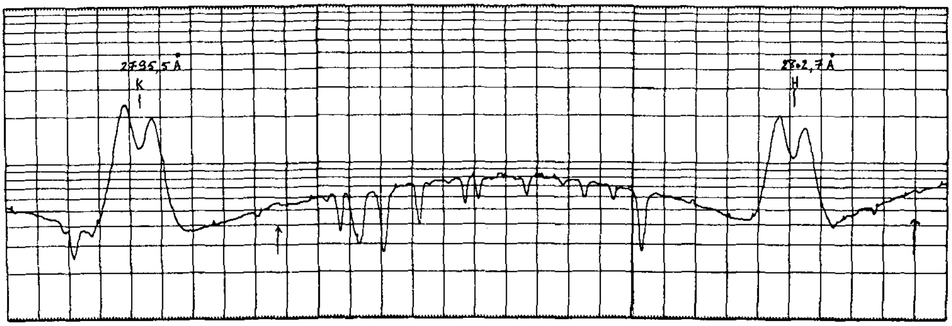

Fig. 3b. *Large scale profile of H and K lines*. Note the dissymmetries of these 'averaged-on-surface' profiles.

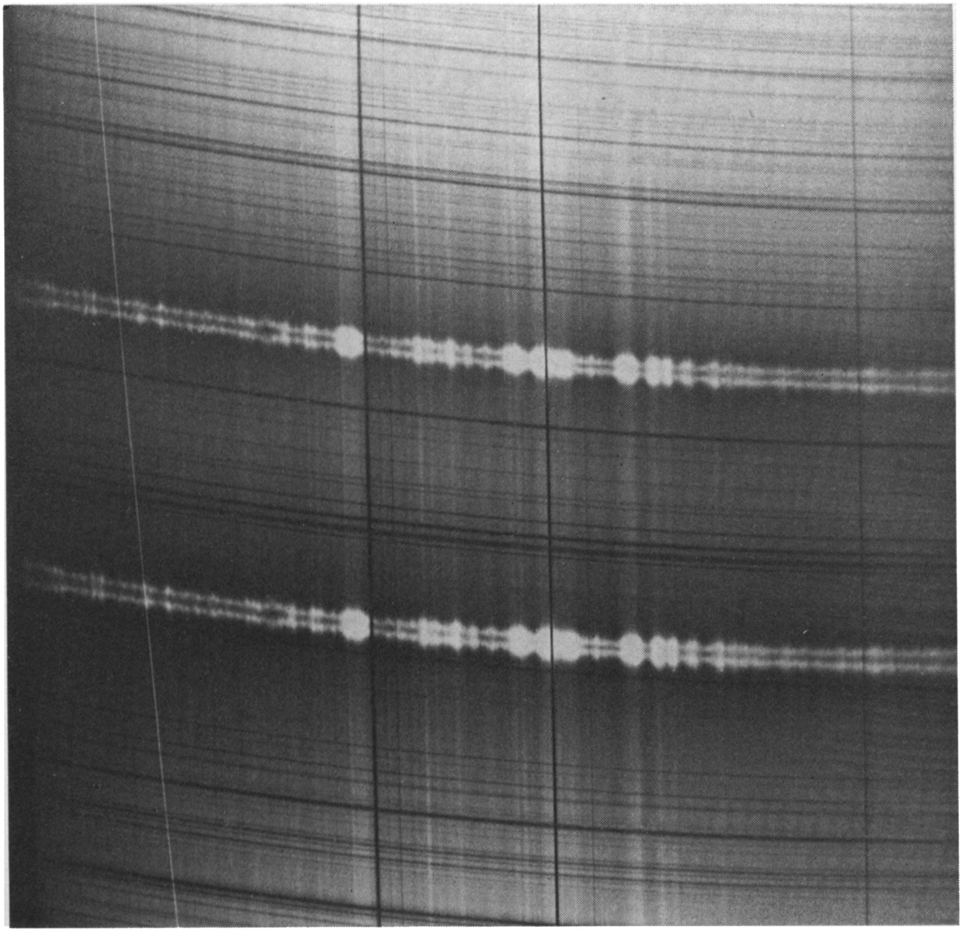

Fig. 4a. *Spectrum of H and K lines* (after Lemaire). Taken in balloon, at Gap, France, the 5.6.1972. Resolving power: spectral: 25 m Å; spatial: 2–3″. Exposure time: 17 s. Note the limb at the right side, and the increase separation between the two emission peaks.

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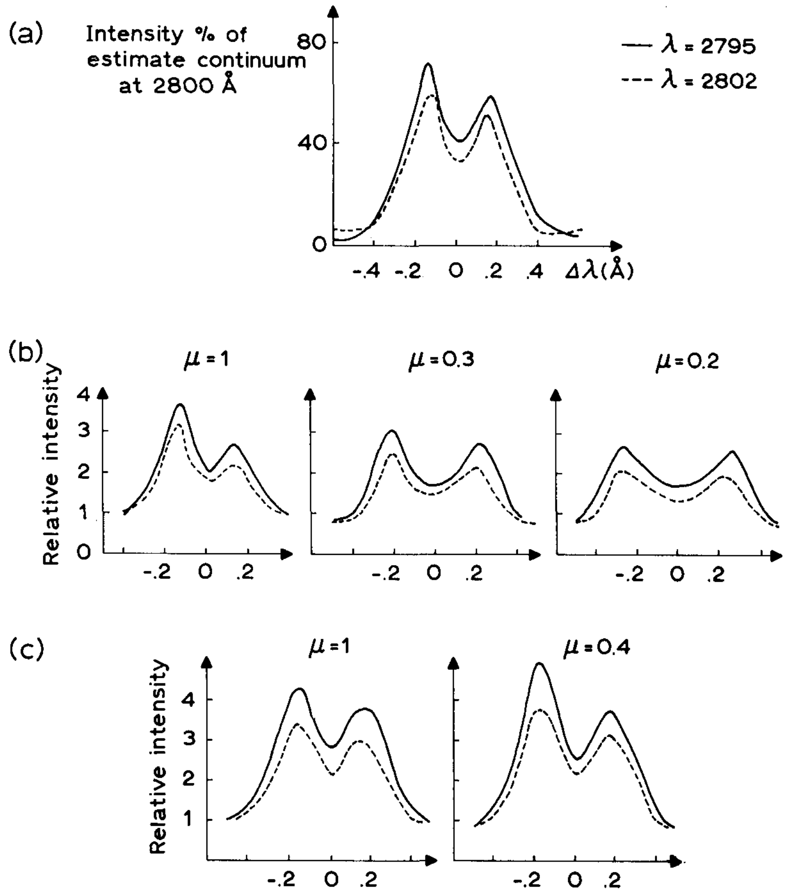

Fig. 4b. *Profiles of H and K lines* (after Lemaire). (a) Average of the central third of the disk on August 21, 1961 (Purcell *et al.*, 1963); (b) (c) (Lemaire, 1969) with a spatial resolution of 10″; (b) corresponds to quiet regions, (c) to faculae.

1.2. The H and K lines of Mg II

For these lines, formed higher in the chromosphere than the H and K lines of Ca II, the emission peaks are much more conspicuous (Figure 3). Their behaviour on the disk, notably from center to limb, with a relatively good resolution on the disk, is reproduced on Figures 4a, b. Both Figures 3 and 4 are taken from Lemaire (1969a, b, 1971).

Again, we cannot possibly attempt to describe fully all the observations, balloon-borne, or rocket-borne, dealing with the Mg II h and k lines. Lemaire (1969a, b, 1971) notes, as do Bates *et al.* (1969) the marked asymmetry of the line. Here, as in the case of Ca II, and certainly for similar reasons (but note that the Mg II lines are formed higher in the solar atmosphere), the violet peak is more intense than the red one. The asymmetry is decreasing towards the limb; in the meanwhile, the separation $K_{2V}K_{2R}$ is increasing from 0.28 Å (at the center) to 0.40 Å (near the limb).

It is not possible to say much more about these asymmetries: not only do they have a purely averaged meaning, but even so, the measurements are quite delicate, and the accuracy of the 'typical' profiles is not as large as desirable in this respect.

2. Chromospheric Features: the Interpretations

The discussion by Linsky and Avrett (1970) is so pertinent that we do not try to elaborate it much further. But their discussion is almost limited to one-dimensional models, and as we feel, unfortunately, that no satisfactory picture has been so far given to the observations abstracted in the above section, we will report here the analysis concerned with multi-components models. The actual suggestions follow those by Cram (1972). We shall certainly not consider them as satisfactory (they are even contradictory to each other) but, at least, we hope to reach partial (negative and positive) conclusions which will demand for complementary tests, or complementary analysis, both observational and theoretical.

(1) We have mentioned the idea that the region where K_2 is originated is moving upwards, the region where K_3 is originated being moving downwards. This interpretation, if we follow the suggested behaviour of homogeneous models, such as Dumont (1967), leads us to admit that, around $h = 300$ km, matter is moving up,

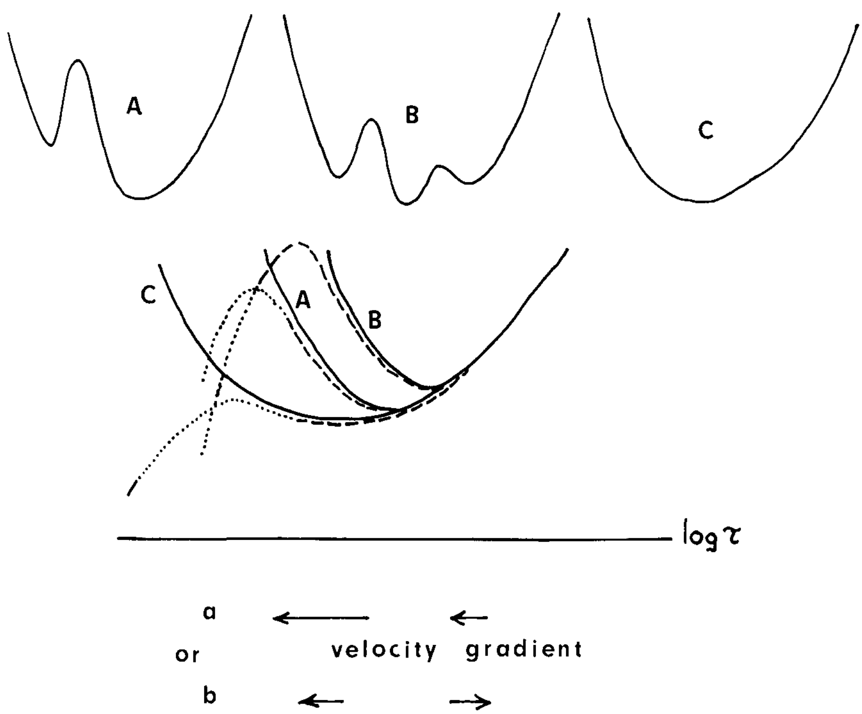

Fig. 5. *Possible interpretation for asymmetrical K-line profiles* (two layers type). *Top:* three types of profiles: A, B, C. *Center:* models corresponding to A, B, C; dotted line: source function. *Bottom:* possible distribution, along the τ-scale, of the gradient of velocity.

whenever it is moving down at the higher level $h=600$ km. As no local permanent increase of density, or no averaged increase of density, is indeed permissible, we have to exclude the combination of the 'homogeneous model' and the classical double motion interpretation of the asymmetrical emission.

(2) But can we exclude a 'two-columns inhomogeneous model'? It is less obvious that the idea cannot work either.... Indeed one can consider this suggestion in many ways. Either we can assume (as strongly suggested by Pasachoff's experimental evidence) that single emission peaks are the rule. Then, at a given point, it implies a gradient of velocity, in most of the cases, with an increase outwards, *and* a higher rise of temperature (in order to annihilate the classical decrease of the source function in classical models, built for some homogeneous chromosphere). In some of the cases, rise of temperature would occur higher, in others lower. This is shown on the Figure 5, highly schematically.

As interesting as this model may qualitatively be, we could not buy it, at various points of the disk unless: (a) the analysis of each observed profile gives weight to the

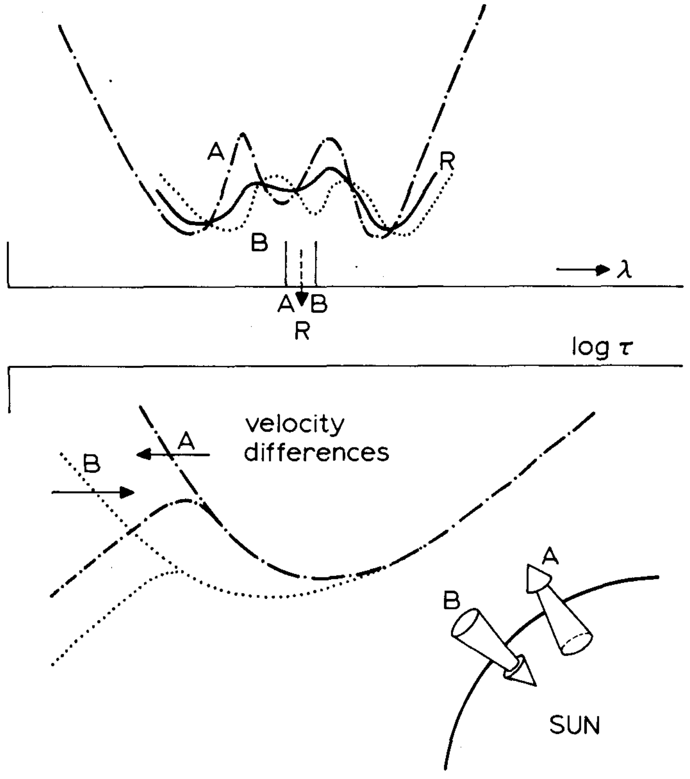

Fig. 6. *Possible interpretation for asymmetrical K-line profile* (two columns type). *Top:* Combination of profile A (rising) with profile B (descending). *Bottom:* Models on points A and B (the lower curve, in each case, represents the source-function). Obviously, this gives a profile of which the asymmetry is contrary to what is observed; an inversion of A and B and of violet and red gives a result similar to observation.

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assumed behaviour for the source-function, in a quantitative manner, as a function of the optical depth in the line center; (b) some tentative explanation is proposed on how is a point such as A or C heated more than point B. An evaluation, according to Pasachoff's statistical suggestions, on how much energy is needed to heat the 'usual' chromosphere described by situation A, is obviously a need at this stage. Let us note (Figure 5) that such a model implies, in any case, a continuous mass loss, at chromospheric levels. Let us note also that, in this model, the contradiction mentioned above (Section 2(2)) has also to be eliminated in a region such as B.

(3) Another way to look at the averaged profile is to admit indeed that there are some columns – i.e., essentially, that chromosphere differs from point to point. This point of view is not essentially, so it seems, different from that described in the preceding paragraph. However, in Section 2(2), we have suggested a gradient of velocity at any of the points A, B, C, suggesting that the effects observed, essentially, are local, and that there are asymmetries everywhere, locally. Now, we assume that the asymmetry comes from the averaging of different spectra, each corresponding to a point of the disk where the profile is perfectly symmetrical. The observed asymmetry of the profile comes here only from combinations of displaced symmetric profiles. This model is described on Figure 6, highly schematical also.

Of course, this behaviour seems contrary to Pasachoff's observations. But the latter are still questioned, as to their statistical significance: actually they should be, by some appropriate averaging process, equivalent to the 'normal' profile of Figure 1 – which they do not seem to be, according to their characteristic statistical features, quoted above in Section 1. The k Mg II lines observed earlier described, do not seem either – at the first glance – to agree with Pasachoff's description; but there, the lack of resolving power on the disk may be responsible.

Apart from this point, the model in question is quite plausible, in terms of the averaged profiles; and in terms of the center-to limb observed decrease of the asymmetry. But again, we have been only qualitative, and one should show, in a forthcoming study, that the source-functions in situations A or B are reasonably well in agreement with plausible models. Moreover, the 'infalling model', in B, should be understood, compared to the 'outflowing model', in A, in the sense that one should explain why the first one is heated in a different way, and how.

At this stage, our preferences do not go to any of the two suggested models, the 'locally asymmetrical' one, or the 'locally symmetrical' one, unless better computations are performed, and more significantly statistical study of the first structure of H, K, h, k lines achieved.

3. Photospheric Features: the Observations

Measurements of line asymmetry dealing with photospheric layers have been performed with an increased accuracy, from the earlier studies by Voigt (1956), till the recent works by Roddier (1965), to quote the most accurate one, in the authors mind. The Table I reminds the reader of the observational studies in question, and of the lines measured by the various authors.

TABLE I

Asymmetry in photospheric lines profiles

Author	Date	Designation	Line	E.P. (eV)	Remark
Voigt	1956	V1	Ni I 7789	1.9	center-to-limb
	1959	V2	Ni I 7798	3.5	
		V3	O I 7772	9.1	
		V4	O I 7774	9.1	
		V5	O I 7775	9.1	
Delbouille *et al.*	1960	D1	O I 7772	9.1	
		D2	O I 7774	9.1	
		D3	O I 7775	9.1	
Higgs	1960	H1	Fe I 6297.8	2.21	center-to-limb
	1962	H2	Fe I 6301.5	3.64	
		H3	Fe I 6302.5	3.67	
Müller	1961	M1	O I 7772	9.1	
		M2	O I 7774	9.1	
		M3	O I 7775	9.1	
Brault	1962	BR1	Na(D) I 5896	0.00	center-to-limb
Olson	1962	O1	Fe I 6173	2.21	
		O2	Ca I 6166	2.51	
		O3	Fe I 5935	3.94	
		O4	Fe I 7090	4.21	
		O5	Fe I 5929.7	4.53	
		O6	Fe I 5930.2	4.63	
		O7	Fe I 5927.8	4.63	
		O8	O I 7772	9.1	
		O9	O I 7774	9.1	
	1966	O10	C I 10700	7.4	5 lines
		O11	O I 7770	9.1	3 lines
		O12	Fe I 5930	4.63	
Roddier	1965	R1	Sr I 4607	0.00	center-to-limb
De Jager and Neven	1967	J1	C I 10754	7.46	
Boyer	1969	B1	Ti I 4535	0.83	
		B2	Ti I 4563	0.02	
		B3	Ti I 5210	0.05	
		B4	Ti I 5239	0.84	

In most of these studies, the type of asymmetry observed at the center of the disk is of a similar shape, essentially described by the 'bisector' line of the spectral feature. On Figure 7, we have superposed several of these bisector lines, and one sees clearly their *C*-shape, which seems to be function of the excitation potential of the line.

However, we must be very cautious in using these various observations; the instrumental errors are of many kinds. They have been well, and rather extensively, discussed by Boyer (1969), and *a priori* eliminated by the very astute experimental devices used by Roddier (1965) in his own measurements of the line profiles. By using a direct atomic beam (in which the dispersion of velocities is less than would be given by a temperature of 3 K), Roddier excites the Sr I resonance line by the solar light. The solar absorption line is swept by the beam, using Zeeman displacement. The wavelength measured is obviously the solar wavelength with respect to the laboratory wavelength –

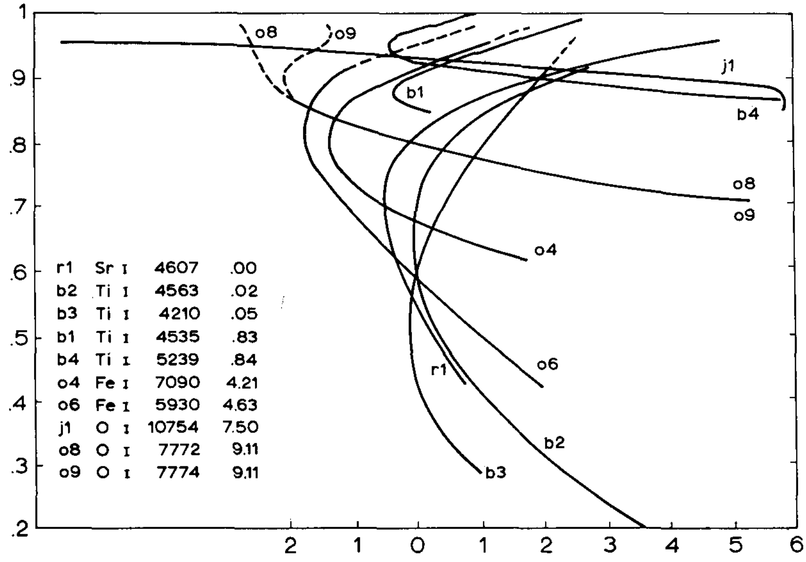

Fig. 7. *Dissymmetric observed profile bisectors* (composite picture, complied from results by Roddier, Boyer, Olson, de Jager and Neven, and Olson). Note the systematic behaviour with excitation potential (in electron-volts in the last column on the left of the figure, together with identification of the various curves).

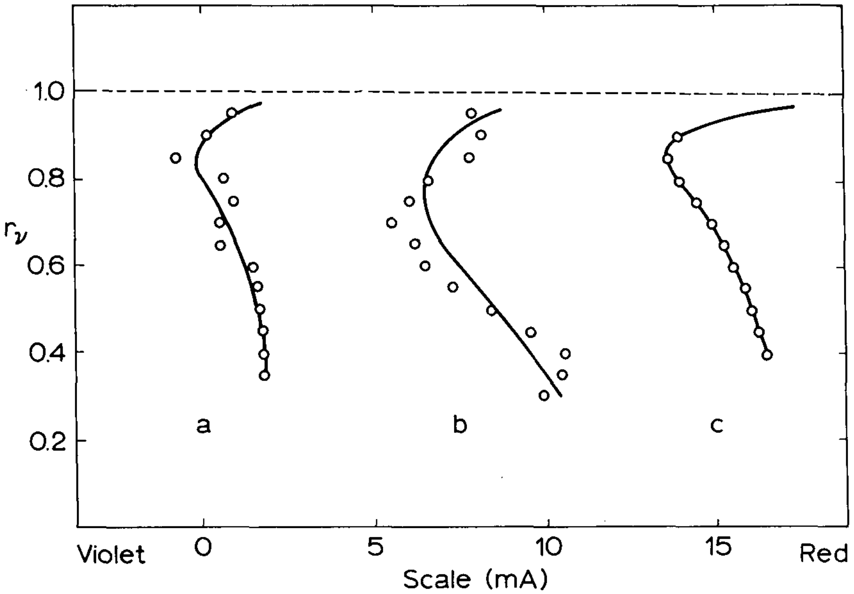

Fig. 8a.

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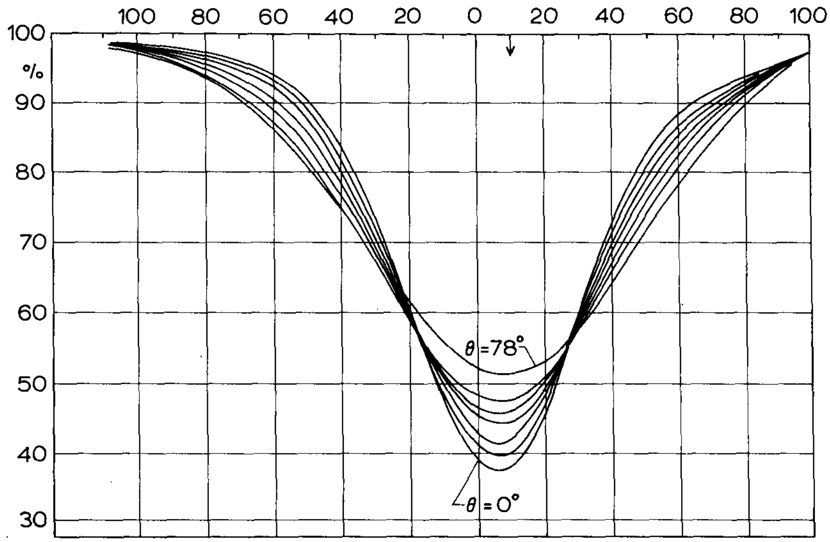

Fig. 8b.

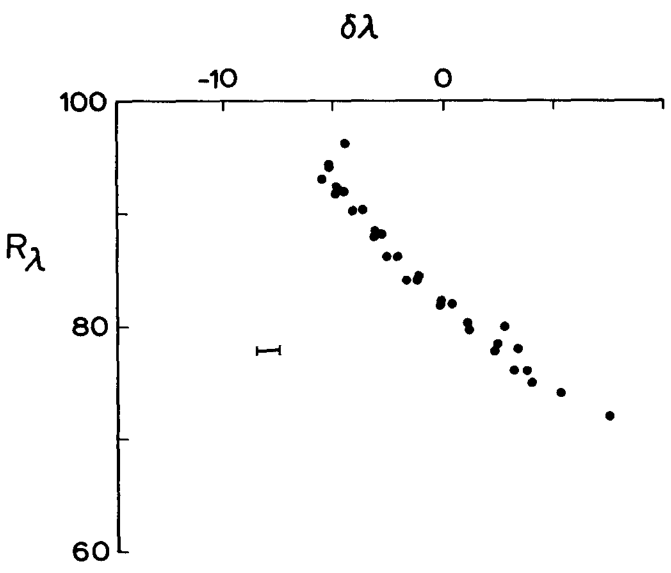

Fig. 8c.

Figs. 8a–c. *Dissymmetric observed profiles (bisector of profile)*. Some original data on some Fe I lines (a: Higgs), on the Si I resonance line (b: Roddier) and the infrared O I lines (c: Olson).

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a difference not so well determined in the classical conventional spectrographical studies (not with the same accuracy, at least...). We should, we believe, give the greatest weight to Roddier's measurements.

An additional important data, not always measured by the quoted authors is the center-to-limb behaviour of asymmetry. When available, the data have been plotted against μ, as $A(\mu)=(h_V-h_R)/(h_V+h_R)$, h being the half-width at half-line depth. It is probably necessary in this discussion to disregard the earlier measurements by Voigt (1956) so discrepant they were from the results by Delbouille *et al.* (1960) obtained later. We shall confine the relevant observations to Higgs' (1960, 1962) and Roddier's (1965), which seem more reliable so far as center-to-limb variation is concerned (Figures 8 and 9) – but they find quite a different result each from the other, quite systematic also. Our personal tendency, knowing the exceptional quality of Roddier's instrumentation, is to believe the results of this author. But, obviously, measurements are difficult, and we should worry here very much about the data.

We should also note (and this remark may still increase our ill feelings) that in some cases, noted apparently only by Higgs, the asymmetry is changing, at certain points if the disk, or at certain moments, in such a way as to even completely change the sense of the asymmetry itself. This comment may be linked with some of our conclusions in Section 5 hereafter.

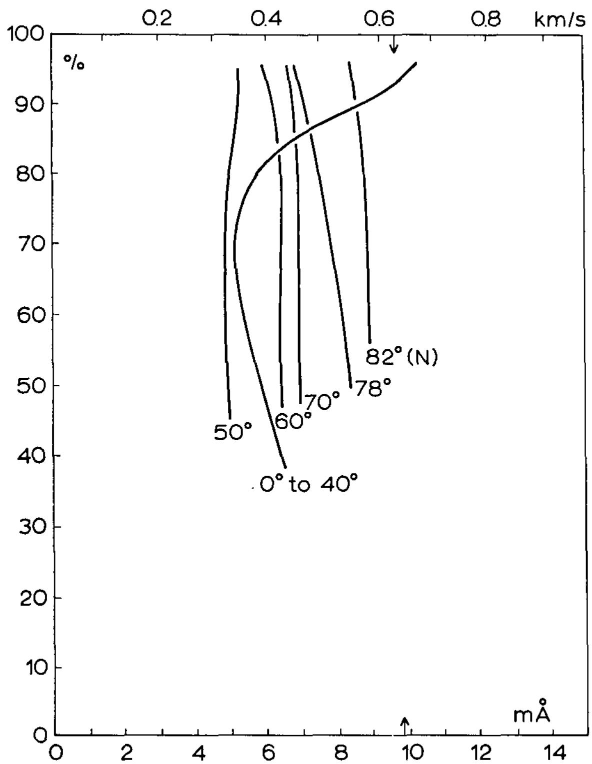

Fig. 9a.

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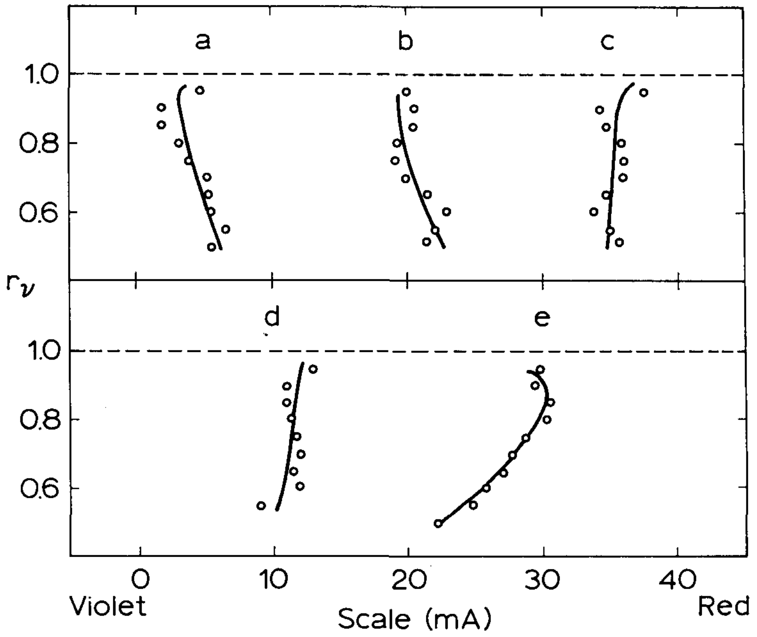

Fig. 9b.

Fig. 9a–b. *Center-to-limb variation of asymmetries.* (a) After Roddier (1965). The bisector of Sr I resonance line, from center to limb. (b) After Higgs (1962). Note the difference with Roddier's results.

4. Photospheric Measurements: The Interpretations

Naturally, the authors themselves have, in some cases, either alone or in cooperation, tried to achieve a satisfactory understanding of their data. However, one is struck by the severe factual indetermination, or uniformity, that is stemming from these papers. One cannot avoid being struck, also, by the lack of concern that is displayed by most of the authors about this non-uniformity, as if they had considered, in most of the cases, that they were the only ones to bring a sensible solution to a delicate problem.

We shall certainly not bring ourselves here any additional solution, and we shall limit our efforts to go through the literature and to gather more or less logically the various arguments, often accepted as proofs.

The first work of some relevance, in our opinion, is probably H. K. Böhm's theoretical paper of 1954. In this paper, he has shown first that three-temperature models (or three column models) although very unsophisticated (they were in LTE, RE, etc.) were not badly suited to interpret the abnormal limb red-shift earlier observed by Allen (1937) and by Adam (1959), and by others.

But Böhm did not study all the possibilities of such models, as he was not aware of the observed asymmetries of lines. Hence, we should consider that the more detailed computations concerning Böhm's model, or at least models similar to Böhm's, and

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relevant here, papers concerning not only indeed the amount of asymmetry, but also its behaviour, are those by Voigt (already quoted) and Schröter, 1957. The papers by Voigt and Schröter have been written almost simultaneously, and independently from each other, as clearly indicated in footnote 2, p. 172, of Schröter's paper.

On the other hand, it has been shown later by Jorand (1962) that several component models, as good they may be for some other purpose, could indeed correctly describe the shifts of line near the Sun's center, but that the extra redshift measured near the limb, in addition to the gravitational Einstein redshift, could not be accounted for (if real) by any such a model.

At least, Schröter (1957) was successful in predicting, or confirming, the behaviour of the asymmetry of spectral lines, and of some specific ones (Figure 10). Clearly, the behaviour he predicted for the disk's center was quite similar to that observed (see above, Figure 7). This theoretical work of Schröter was in fact the starting point of a whole series of experimental work, already quoted in Section 3. The Figure 11, due to Schröter (1955) shows finally how strongly different models can give indeed resulting spectral lines the symmetry of which is almost (but not completely) identical

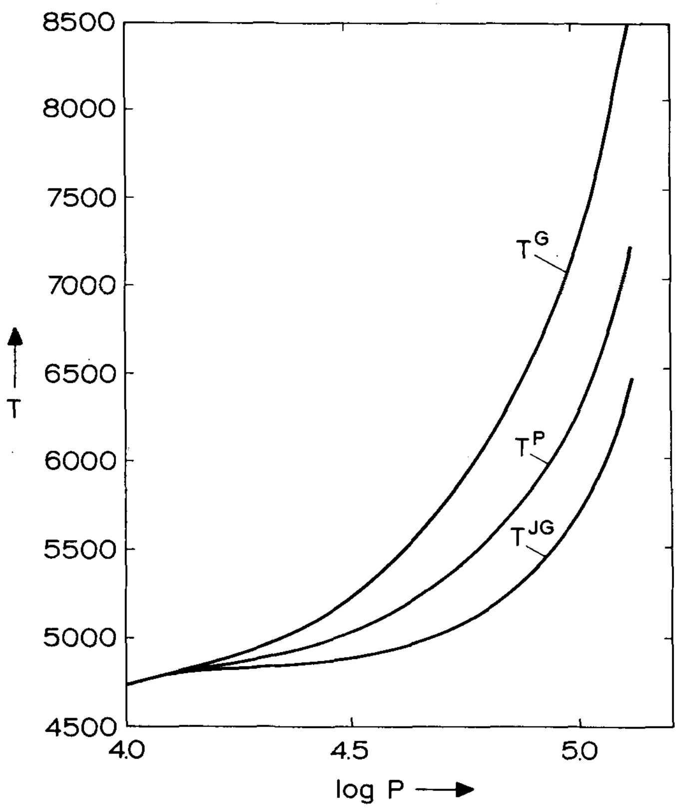

Fig. 10. *A two-column model.* After Schröter (1956). The letters G and JG correspond respectively to granular and intergranular region. T_P designates the one-column equivalent model. In abscissa, logarithm of pressure.

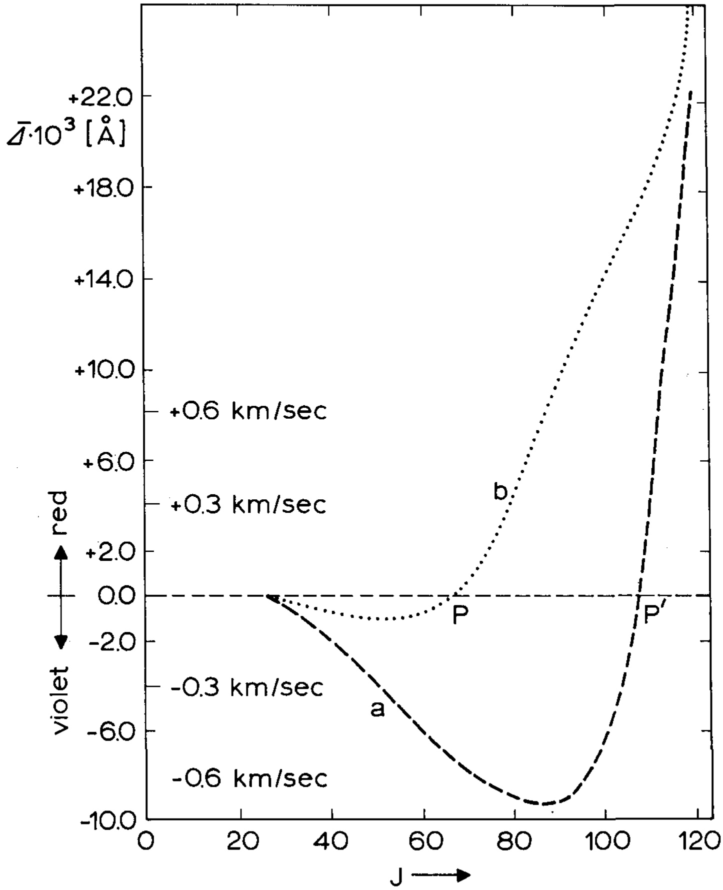

Fig. 11. *Asymmetrical profiles computed with two columns models.* After Schröter (1956). The figure reproduces the bisector line. In abscissa, the zero corresponds to the continuum; J is the line intensity ($J = 122$ corresponds to continuum).

to the observed one. But still the fitting is not as good as it should be, showing at the same time the impossibility of a correct diagnostic, and the undetermination of an 'almost correct' diagnostic... A point, which, applied to many other problems, is undoubtedly rich of deep significance....

Moreover, when Olson (1962, 1966) tried to perform similar work, the ΔT (difference between the temperatures of the rising and falling columns) had to be very different from line to line, a fact certainly far from being satisfactory (Figure 12). Olson, who was aware of the fine structure of lines, as revealed from high spatial resolving power spectrograms such as Schwarzschild's (1961), as deduced in any case from the high spatial pictures from Stratoscope I, studied by Bahng and Schwarzschild (1961), or by Edmonds (1964), did not find a good agreement between tthe temperature-velocity distribution that is needed to explain the dissymetries and the velocity

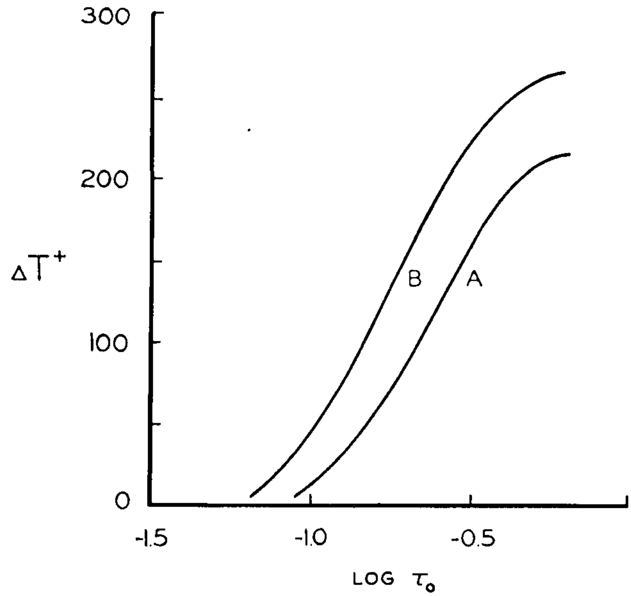

Fig. 12. *The temperature difference between the two columns.* After Olson (1962). A: for O I lines; B: for Fe I lines.

distribution which is inferred from direct fine resolution photographs. After elimination of telluric effects, or of differential Zeeman asymmetrical effects, Olson sadly concludes, at the end of his first paper on this question: 'the discrepancies are apparently still to be resolved'. By reading the subsequent literature, we do feel this conclusion as being still completely valid!

Let us note, incidently, that another effect studied by Olson (1966) is to be eliminated from the analysis: the very interesting suggestion by Noyes and Leighton (1963) of a mechanism of acoustic waves propagation does not seem to lead to the production of any real asymmetry of the profiles.

We must obviously, at this stage, notice that the difficulties of the fitting were obvious even without reference to center-to-limb observations; that all computations were strictly in LTE; that they introduced velocity fields as ad hoc parameters in an often non-physically consistent way. The whole problem of velocity fields of macro- and micro-scale has of course to be discussed at length in a broader context. But insomuch as profile asymmetries are considered, the undetermination in inferred velocity fields is showing in still a less pleasant manner than in the interpretation of the general symmetrized shape of the profile for reasons we hope to clarify in Section 5, hereafter. Let us, not leaving out, from the analysis, models that are trying to describe center-to-limb variations, now refer to the results of Boyer (1969, 1970).

This author comes back to the three columns model (in LTE), and is using the best 'up to date' models, at the time, i.e. the URP model (Heintze *et al.*, 1964) for the temperature distribution. But he lets free, as parameters, the respective area A_j of the

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three components (a parameter which indeed should not be free in such a way, because of the absolute need to correctly represent the continuum as well as the line intensities), and the microturbulence law, $\xi(\tau_c)$, considered as isotropic, and identical (but why?) in the three columns. The 'macroturbulent' velocities (V_1, V_2, V_2) are fixed in part by the area A_j and the law of mass conservation; assuming $V_2=0$, the only parameter $W=\frac{1}{2}(|V_1|+|V_3|)$ (different from $V=V_1+V_2+V_3$) is thus defining the convection. Boyer admits $W=0$ for $\tau<\tau_l$; $W=$const for $\tau>\tau_l$, i.e. in the 'convective layer'. All computations have been done in LTE.

The numerical experimentation performed by Boyer is interesting in that, in spite of an *a priori* very crude physical description, a good enough fitting is done with some adequate choice of the parameters. However, this is true only if one fits a *single* line. The second line does not fit any more – as already essentially noted by Olson (1962). Again, we hope that Section 5 will allow to understand why it is so. Boyer's model is looking very much like Olson's (Figure 13).

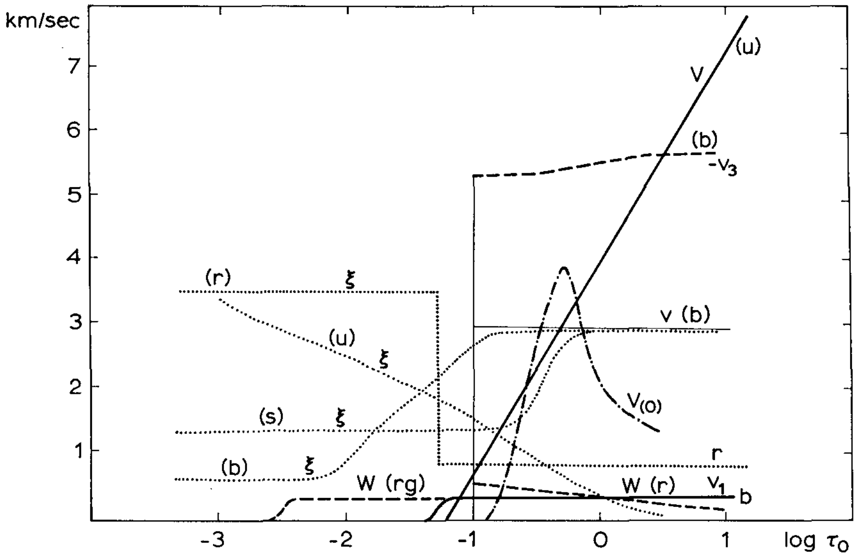

Fig. 13. *The variation of macro and micro-fields of velocities with depth.* This figure is compiled from some models obtained by different authors: r:Roddier, rg Roddier-Gonczi; o:Olson; b:Boyer; s:Schmalberger; u:Utrecht reference atmosphere. In dotted line, the 'micro-turbulence'. V_1, V_2 represents the velocity in moving columns (Boyer). V the average ($V_1+V_2=2V$) velocity. w the velocity in rising column ($w=V_1$).

Roddier (1965) and Gonczi and Roddier (1970) have, seemingly, looked into the problem with an attitude less obviously 'numerological'. In addition, they take into account the center-to-limb observations. They first noted the V-shape (in contradiction with the U-shape, generally predicted by theory) typical of the intervention of large velocity differences. Then they remark, as we did earlier, hereabove, that two simple types of models can account for observations: either large gradients of velocity;

or multicolumn models. But they note also that the line being generally violet shifted (with respect to the line center, properly located, account being taken of the Einstein shift), a neat convection is necessary, the non-zero upward observed velocity being an average value weightened by inhomogeneities. Then the center-to-limb studies impose anisotropic velocity fields: the convective cells indeed are not isotropic –, and this is showing clearly through the reduction near the limb of the violet shift. The widening of the profiles, contrarily, is due to an apparent increase of the line-of-sight component of microturbulence (either anisotropy in one of the n components – or decrease, inwards, of microvelocity fields).

For the various reasons listed above, the authors do not think that an easy conclusion can be reached quickly. Before going into the numerical experimentation, which obviously is needed, they can only suggest that according to published work, two kinds of models seem possible:

(a) *a two-column model* (or multi-column model). Essentially one column would move upwards, being slightly microturbulent; the other one would be moving downwards, the microvelocity field being there somewhat anisotropic.

(b) *a two-layers* (or with a strong velocity gradient) model: Essentially, the outer layer, highly microturbulent, would have very little, if any, average motion; the inner one has an average upward motion and is weakly microturbulent; no anisotropy is necessary; but evidently, in both regions, an inhomogeneous model has to be considered.

Model (b) was presented first by Roddier in 1965. In 1970, from about the same data, Gonczi and Roddier are favouring the model (a).

However, we may note several simplifications they made in their assumption before any experiment with the free parameters of the problem: (i) they assume that ΔT between the columns is not an essential parameter, and they give it the value zero; (ii) the area occupied by the two columns on the solar surface are equal and independent of height.

Gonczi and Roddier note first that, at the center of the disk, the two models are essentially equivalent; hence they do not accept easily the too limited conclusions of Olson and Boyer. But obviously, from center to limb, the model (a) is by far better. Hence their final choice. We must note that, as others already mentioned, they could not fit another line, (they have attempted to do it for C I 10691) with the same model....

Turning into details of the numerology of Gonczi and Roddier, they finally present the model of Figure 14, which indeed fits remarkably the observations made from center to limb by Roddier. There exists a small indication that $V \cos\theta$ (the apparent violet shift) is indeed to be supplemented, to fit the observations, by an additional violet shift term of the order of $-150\ (\sin\theta)^2\ \mathrm{m\ s^{-1}}$, the origin of which is far from clear: the authors think that supergranulation horizontal cells cannot account for it.

A source of error noted by Gonczi and Roddier is the existence of NLTE source-function: the abundance of Sr is found at the limb twice as low as at the center of the disk; but they comment that departures from LTE have no influence on the asymmetries and shifts.

We shall not try here to criticize heavily this model. We have, so we believe, no

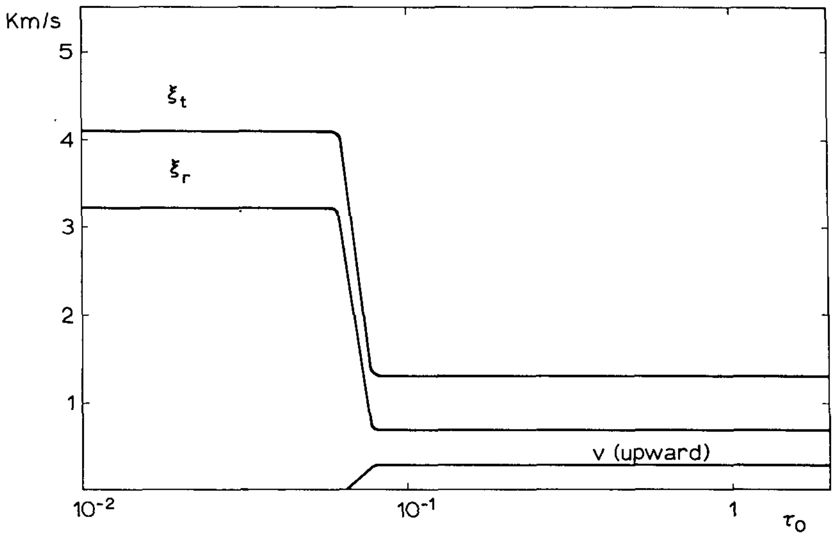

Fig. 14a.

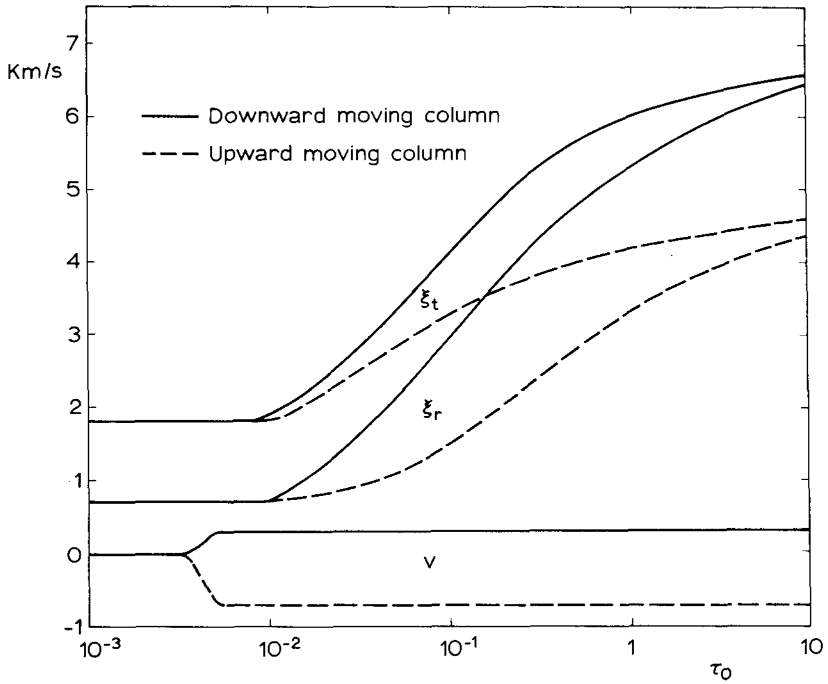

Fig. 14b.

Fig. 14a–b. *The Gonczi-Roddier discussion.* (a) A possible model (of two-layers type) with anisotropic micro-turbulence. (b) A probable model (of two-column type) with anisotropic micro-turbulence.

better one at hand, and many worse ones.... Indeed the convective velocity values, lower than usual, are fitting better the rms measurements of fine structure spectra. Also this model agrees with the observed fact that, in the wings, the contrast between dark and bright regions is larger on the red side than it is on the blue; this difference of contrast is reversed in the line center, and in the line core; this has been well observed and is compatible with the model of Gonczi and Roddier. Another important fact, observed, and of which the model gives a fair account, is also the fact that the profiles are widened in the dark intergranular regions.

To our knowledge (which is limited) no new attempts have been done in order to get new observations of asymmetries, and to deduce from them new models for the velocity fields of the solar photosphere; new velocity models certainly have been produced, but from other considerations. Obviously, many observations of high quality, and recently obtained, could be analyzed for it; but we did not find any relevant information from the best possible bibliographical report at the date, i.e. the 'draft' report of commission 12, prepared for the XVth General Assembly of the IAU (Sydney 1973)!

We shall now take the problem from an entirely different point of view, that of the general methodology of diagnostic in 'artificial' asymmetric line profiles.

5. Theoretical Line Asymmetries

5.1. Introduction

In view of the confusing state of the various interpretations of observed line asymmetries, it is clearly impossible to propound a new 'marvellous' and all-purpose methodology. The simplest diagnosis procedure, the so-called 'bi-sector' method, has been discussed by Kulander and Jefferies (1966) and, in the same spirit, we intend to set the problems by analyzing, from a theoretical point of view, the influences of some physical parameters upon the emergent profiles. The peculiar case of the solar H and K (or h and k)lines has been reviewed by Linsky and Avrett (1970) and here above, in such a way that we do not feel it necessary to consider these lines in detail, – but in a more general context, we hope to help everyone to ask himself the 'right' questions. In fact, these questions are always simple – though the answer is not! – and must not be hidden in the intricacy of the calculations. Fortunately the observations of line asymmetries lie in the heart of the problem of diagnosis, because they force us to introduce velocity fields very explicitely.

There, we cannot content ourselves by calling upon the help of that obscure parameter ξ: it is more natural and more consistent to search for velocity fields that account simultaneously for the width of the line and for its asymmetry. In this respect, it is to be expected that the study of asymmetries will help us in return to clarify the questions related to the formation of symmetric lines, because these symmetric lines are likely to be some average of many asymmetric ones.

At this point of the discussion, we cannot yet consider the mixing of two or more different emerging profiles: if we are trying to understand what is going on, then we must evidently limit ourselves first to the *intensity* emergent from a single line-of-sight. In other terms, the intensity is what concerns us, not the *flux*. This is, incidently, appropriate to solar problems at high spatial resolution, but also to some special cases. Let us consider the result of an integration along a simple straight line, and in order to introduce the notations, let us recall what this 'integration' consists of.

Consider first a local frame of reference moving with the fluid at some given point. The local frequency ν_L of a photon in this frame will be characterized by the distance

$$\Delta\nu_L = \nu_L - \nu_0 \tag{1}$$

from the absolute rest frequency ν_0 of the transition being considered. The thermal Doppler width $\nu_0 w/c$ corresponding to the thermal velocity $w=(2kT/m)^{1/2}$ will be denoted by Δ. If the distribution of the atoms in the upper state of the transition is a maxwellian distribution at the temperature T, the probability that a photon emitted in the line will be emitted at about the frequency $\Delta\nu_L$, in the range $\mathrm{d}(\Delta\nu_L)$, is:

$$\frac{1}{\Delta}\Phi\left(\frac{\Delta\nu_L}{\Delta}\right)\mathrm{d}(\Delta\nu_L), \tag{2}$$

where the profile $\Phi(x)$ is usually the Voigt profile, normalized to unity over $x= =(\Delta\nu_L)/\Delta$ in the range $(-\infty, +\infty)$. This profile depends only on the parameter $a=\Gamma/4\pi\Delta$, equal to the ratio of the atomic to the thermal broadening width. In what follows, a is always taken as constant, and for the sake of simplicity, we have dropped out from our notation the dependence of the profile upon this parameter a. In the numerical examples reported here, we have taken the well known approximate form

$$\Phi(x) = \frac{1}{\sqrt{\pi}}e^{-x^2} + \frac{a}{\pi x^2}, \tag{3}$$

where the second term on the right-hand side is to be added only for $x^2 > 1$. We have chosen $(a/\pi)=10^{-3}$, so that the second term dominates for $x > 3$.

The absorbing properties of the medium at the same point are characterized by the same frequency dependence given by the expression (2). We take as the depth coordinate the continuum optical depth τ_c between the surface and the point in consideration. This quantity τ_c can safely be assumed to be independent on the velocity field. The probability that a photon $\Delta\nu_L$ is absorbed in the line along the path $\mathrm{d}\tau_c$ is

$$\mathrm{d}\tau_L = \frac{\eta}{\Delta}\Phi\left(\frac{\Delta\nu_L}{\Delta}\right)\mathrm{d}\tau_c, \tag{4}$$

where (neglecting the induced emissions)

$$\eta = \frac{1}{\varkappa_c}\frac{h\nu_0}{4\pi}N_1 B_{12} \tag{5}$$

is the ratio of the mean line opacity to the continuum opacity $\varkappa_c$. The parameter depends only upon atomic coefficients and upon the population N_1 of the lower state but not upon the distribution function of the velocity of the atoms in that state. We shall take this parameter η as constant.

5.2. Adding a velocity field

Up to this point, the things are symmetric with respect to $\Delta\nu_L$. We assume now that each layer situated at depth τ_c has a velocity $v = v(\tau_c)$ with respect to an observer lying outside the medium and counted as positive when directed towards this observer. Along its path, the photon emitted towards the surface at point τ_c with local frequency ν_L is capable of being absorbed at point $\tau'_c\,(\tau'_c < \tau_c)$ where it is seen in the local frame moving with the absorbing material at the local frequency:

$$\nu'_L = \nu_L + \nu_0 (v - v')/c, \tag{6}$$

where $(v - v')$ is simply the velocity of the slab τ_c with respect to the slab τ'_c. Ultimately, this photon will be seen by the observer at the frequency:

$$\nu = \nu_L + \nu_0 v/_c. \tag{7}$$

From the preceding formulae, we write the line optical depth up to the surface for the photon emitted at point τ_c with the local frequency $\Delta\nu_L$ as:

$$\tau_L = \int_0^{\tau_c} \frac{\eta'}{\Delta'}\,\Phi\left(\frac{\Delta\nu_L}{\Delta'} + \frac{v - v'}{w'}\right) \mathrm{d}\tau'_c. \tag{8}$$

It is clear from this expression (8) that the photons emitted *symmetrically*, at local frequencies $\Delta\nu_L = +\delta$ and $\Delta\nu_L = -\delta$ suffer now a *non-symmetrical* history during the subsequent way to the surface. The reason is that the shift in the argument of Φ is never symmetric. A situation which is often considered consists in taking Δ' constant in the medium. Then for an expansion (i.e. $v' > v$ and $v > 0$), the argument of Φ is always algebraically decreasing so that its absolute value first increases on the violet side $+\delta$ and decreases on the red side $-\delta$. In that case, the optical depth is larger for the photon $+\delta$ than for the photon $-\delta$. But we want to insist on the fact that, even in this quite simple situation, we cannot infer immediately the sign of the asymmetry of the emergent profile. In fact, it must be realized that the local frequencies $+\delta$ and $-\delta$ are received by the observer at frequencies $+\delta + \nu_0\, v/c$ and $-\delta + \nu_0\, v/c$, i.e. symmetrically with respect to a quantity $\nu_0 v/c$ which depends on the velocity of the slab which has emitted the photons. So, by adding the contributions of many slabs of many local frequencies we lose inexorably the possibility to refer the various frequencies to some fixed frequency. The important consequence is that, in general: *speaking of the red side or the violet side of the emergent profile has no immediate theoretical signification.* In other terms, the 'line center' may be chosen at the point where the emergent intensity is minimum (i.e. 'observationally' chosen), but we have no way of saying, from a general and theoretical point of view, at which frequency this minimum will occur, except by treating completely the specific problems we may have to solve.

5.3. Introducing an important parameter

We now want to emphasize very strongly another physical fact expressed by the formulae (6) and (8). It is quite possible (and, generally, it is to be expected), that at 'some distance' from the emitting point, the value of the argument of Φ will depend primarily on the distribution of the velocities along the path, this distribution being or not of a random character. But this is absolutely wrong 'in the vicinity' of the emitting slab, where $v' \cong v$, so that the absorbing properties near the emitting point depends primarily upon the value of the local frequency $\Delta \nu_L$ which has random characteristics (see the formula (2)) independent of the velocity fields. In order to make the words 'at some distance', or 'in the vicinity', more precise, it is then necessary, even in a first crude analysis, to introduce some *characteristic length for the variation of the velocity* (or equivalently some *characteristic value of the velocity gradient*). In nearly all cases, this characteristic length is assumed to be 'small', but this is always a priori assumed: we think that this parameter must be introduced *explicitly* and that a convenient diagnosis must then be applied in order to determine its value. Let us illustrate these considerations with the aid of a model very close from that considered by Frisch (1969) and associates (Auvergne *et al.*, 1973). The medium is represented, for convenience by succession of different slabs of equal continuum optical depth Λ – (in Frisch's model, these lengths have a distribution which follows Poisson's law; in Auvergne *et al.* it is suggested that it can depend upon depth in the atmosphere), each slab i having a velocity v_i; the distribution of the v_i's is random so that the probability of finding a velocity in the range $(v_i, v_i + \mathrm{d}v_i)$ is:

$$\frac{1}{\sqrt{\pi}} \frac{1}{v_0} e^{-(v_i/v_0)^2} \,\mathrm{d}v_i . \tag{9}$$

The thermal velocity w is assumed to be constant, so that the line optical depth from the deeper end of the slab n up to the surface from a photon emitted at local frequency $\Delta \nu_L$ in this slab n is simply:

$$\tau_L = \frac{\eta \Lambda}{\Delta} \sum_{i=n}^{0} \Phi \left(\frac{\Delta \nu_L}{\Delta} + \frac{v_n - v_i}{w} \right). \tag{10}$$

So, at a given value of Δ_L, the quantity $(v_n - v_i)$ is effectively randomly distributed, but not for the first value, which is of course always zero. We expect then that the contribution of this first slab is the largest and thus cannot be dropped out without great care.

The quantity which is usually considered is the line optical depth of all subsequent slabs from $(n-1)$ to zero. When n is large, this line optical depth takes the value adopted in so-called 'microturbulent' situations:

$$\tau_L = \frac{n \eta \Lambda}{\Delta_1} \Phi \left(\frac{\Delta \nu_L + v_n \nu_0 / c}{\Delta_1} \right), \tag{11}$$

where Δ_1 is a new width which takes now into account the value of the characteristic velocity v_0 by the relation:

$$\Delta_1^2 = \Delta^2 + (\nu_0 v_0/c)^2 . \tag{12}$$

But the important point is that the residual optical depth of the emitting slab is independent of v_0 and is always equal to:

$$\frac{\eta \Lambda}{\Delta} \Phi \left(\frac{\Delta \nu_L}{\Delta} \right). \tag{13}$$

To give an example of application of these simple formulae, let us consider an LTE situation with a source-function $B = B(\tau_c)$, taking the values B_0, B_1,... in the different slabs. Then the slab n is emitting at the local frequency $\Delta \nu_L$ the total intensity:

$$I_n = B_n \left\{ 1 - \exp \left[- \Lambda \left[1 + \frac{\eta}{\Delta} \Phi \left(\frac{\Delta \nu_L}{\Delta} \right) \right] \right] \right\}, \tag{14}$$

which has then to be attenuated by the factor:

$$\exp \left[- n\Lambda \left[1 + \frac{\eta}{\Delta_1} \Phi \left(\frac{\Delta \nu}{\Delta_1} \right) \right] \right], \tag{15}$$

where $\Delta \nu$ denotes the frequency $\Delta \nu_L + \nu_0 v_n/c$ seen by the observer. The emergent intensity is then expressed as:

$$I(\Delta \nu) = \sum_{n=0}^{\infty} B_n \left\{ 1 - \exp \left[- \Lambda \left[1 + \frac{\eta}{\Delta} \Phi \left(\frac{\Delta \nu}{\Delta} - \frac{v_n}{w} \right) \right] \right] \right\} \times \\ \times \exp \left[- n\Lambda \left[1 + \frac{\eta}{\Delta_1} \Phi \left(\frac{\Delta \nu}{\Delta_1} \right) \right] \right]. \tag{16}$$

The great advantage of a formula of that type is that it contains explicitely two quantities: the value of a characteristic velocity v_0 but also the very important parameter Λ directly correlated to the scale height of the velocity distribution.

It is immediately apparent that the 'width' of the profile depends essentially on Δ_1 (i.e. on v_0), but that the value of the intensity is very sensitive to Λ. We expect to get, upon a 'background' given by the second exponential term of the formula (16), a very wiggly, and of course absolutely unsymmetrical, profile. We suggest that the 'true' situation is very similar to that one and we think that the majority of profiles that are observed are in fact the result of averaging such wiggly profiles. This is clearly demonstrated by spectra taken at higher and higher time resolution, both for the Sun and the stars. In that case, the 'theoretical' averaging must be done very carefully and in particular the kind of average (either over space or over time, or both) must be precisely specified. Formulae like (16) seem to be a sound basis for this task (we shall incidently remark that the papers by Huang (1952) and De Jager and Pecker (1951) were early attempts in these directions). From an observational point of view, it would be very desirable to obtain instantaneous and precisely located profiles: it is to be expected that the diagnosis would be easier if we were able to do ourselves our own averages, at our own will....

The formula (16) has been written for an LTE line, but it is easy to include NLTE effects in a model of that kind, by replacing the B_n's by appropriate S_n, in the line, different from that in the continuum. In fact, when the relative velocities of the different slabs are large (i.e. $v_0 \gg w$), each slab tends to be isolated and to build its own radiation field. In the limiting case, the source-function will depend no more on the characteristic velocity v_0, but only on the optical depth of each slab in this representation of the variation of velocity. This is of course a further argument to demonstrate the importance of that parameter Λ, which has the meaning of a 'correlation' distance. Even if this limiting case is seldom encountered (in the case of the Sun, we rather expect that v_0 and w are of the same order of magnitude), these kinds of effects may be present, especially for strong lines.

5.4. Varying the Thermal Doppler Width

We submit now to each one's thinking the results of simple calculations based on another model which is very schematic but, we hope, instructive, and all things considered, may be not very far from real situations. We have seen in the formula (8) that the argument of the profile $\Phi(x)$ is in fact a function of the velocity, but also of the thermal Doppler width. In order to illustrate the influence of this last parameter in the presence of velocity fields, we take a model in which the thermal width Δ increases towards the surface according to the law

$$\Delta = \Delta_0 \, e^{-(\tau_c/T)}, \tag{17}$$

where T is some scale height which has been chosen equal to 10^{-2}, i.e. just of the order of magnitude of the continuum optical depth before the chromospheric rise of temperature which we try to mimic by the law (17). We further assume that Δ remains constant when it has attained the value $\Delta_0/10$ (Figure 15). We consider now a velocity directed towards the observer and equal at each point to the Doppler velocity so that

$$v = w. \tag{18}$$

This choice is not entirely arbitrary: in fact, a rough relation between the thermal and the non-thermal velocities is to be expected from physical considerations, and moreover it is just when the equality between the two velocities holds that the effects become very significant. In fact, Sobolev's work (1966) demonstrates clearly that, when $v \gg w$, the problems become simple, because the photons do not diffuse in space. But when $v \cong w$, Sobolev's theory breaks down, and the entire problem must be reconsidered.

The formula (8) gives now for the line optical depth:

$$\tau_L(\Delta v) = \int_0^{\tau_c} \frac{\eta}{\Delta'} \, \Phi\left(\frac{\Delta v}{\Delta'} - 1\right) \mathrm{d}\tau_c', \tag{19}$$

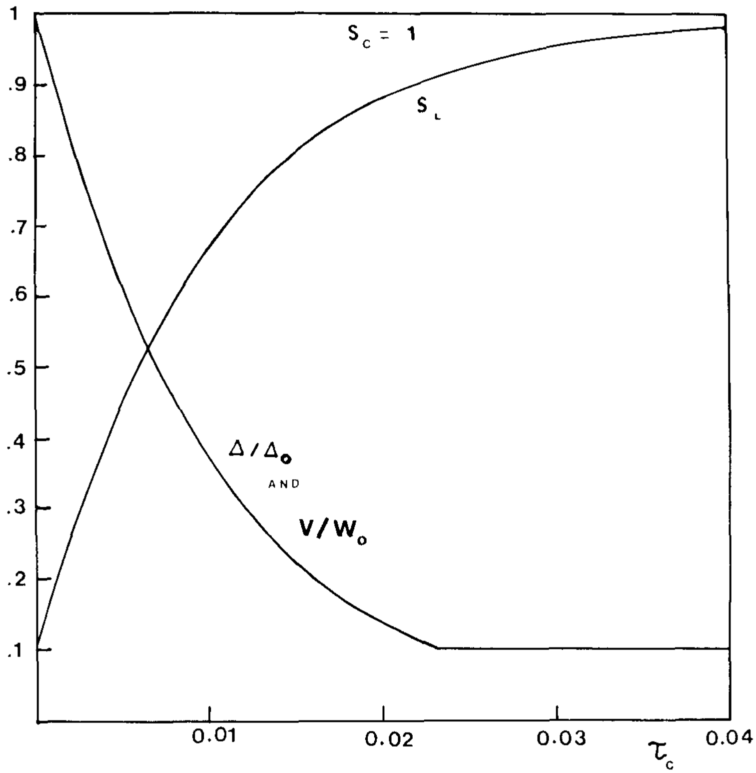

Fig. 15. *A Test Model* for source-function in continuum, and line, and for the variation of Doppler width. (Notations as in the text.)

where $\Delta\nu = \nu - \nu_0$, with the formula (7) being taken into account. From the formula (17), in the region where the thermal width is varying, we have

$$d\tau_c = T\, d\xi/\xi\,, \tag{20}$$

where $\xi = \Delta_0/\Delta$ is the dimensionless parameter giving the ratio of the surface thermal width to the actual one. It is then easy to express the total optical depth as

$$\tau(\xi, \Delta\nu) = \tau_c + \frac{\eta T}{\Delta_0} \int_1^{\xi} \Phi\left(\xi' \frac{\Delta\nu}{\Delta_0} - 1\right) d\xi'\,. \tag{21}$$

In the region where $\Delta = (\Delta_0/10)$ is constant, the optical depth is of course computed according to the formula

$$d\tau_L(\Delta\nu) = \frac{10\,\eta}{\Delta_0}\, \Phi\left(10\, \frac{\Delta\nu}{\Delta_0} - 1\right) d\tau_c \tag{22}$$

Concerning the source function, we have taken a NLTE case, with a continuum source function $S_c = B = 1$, and a line source function S_L decreasing towards the surface according to the law (Figure 15)

$$S_L = B[1 - 0.9\, e^{-(\tau_c/T)}] \tag{23}$$

so that the scale height of variation of S_L is the same as the scale height of variation of the thermal width. This fact also is likely to occur in the case of the Sun, even if the exact law is not exponential and even if the two scale heights are not exactly the same. The total source function at frequency $\Delta\nu$ is then

$$S_\nu = \frac{B\, d\tau_c + S_L\, d\tau_L(\Delta\nu)}{d\tau_c + d\tau_L(\Delta\nu)}\,. \tag{24}$$

The strength of the line is characterized by the quantity η, or better:

$$T_0 = \eta T/\Delta_0 \tag{25}$$

which is $\sqrt{\pi}$ times the optical depth at the center of the line in a static situation over a distance equal to the scale height T and for a constant thermal width Δ_0. We have considered some values of T_0 ranging from 0.5 up to 10. The value of this parameter labels the emergent profiles shown in Figure 16. The more usual parameter $\eta_0 = \eta/\Delta_0$ takes then values from 50 up to 1000: the latter corresponds to fairly strong lines.

The Figure 16 shows the emergent intensity plotted vs the dimensionless parameter $\Delta\nu/\Delta_0$. The profiles are shifted as a whole towards the violet, as expected, indicating an overall approaching velocity. From an analytical point of view this is simply a

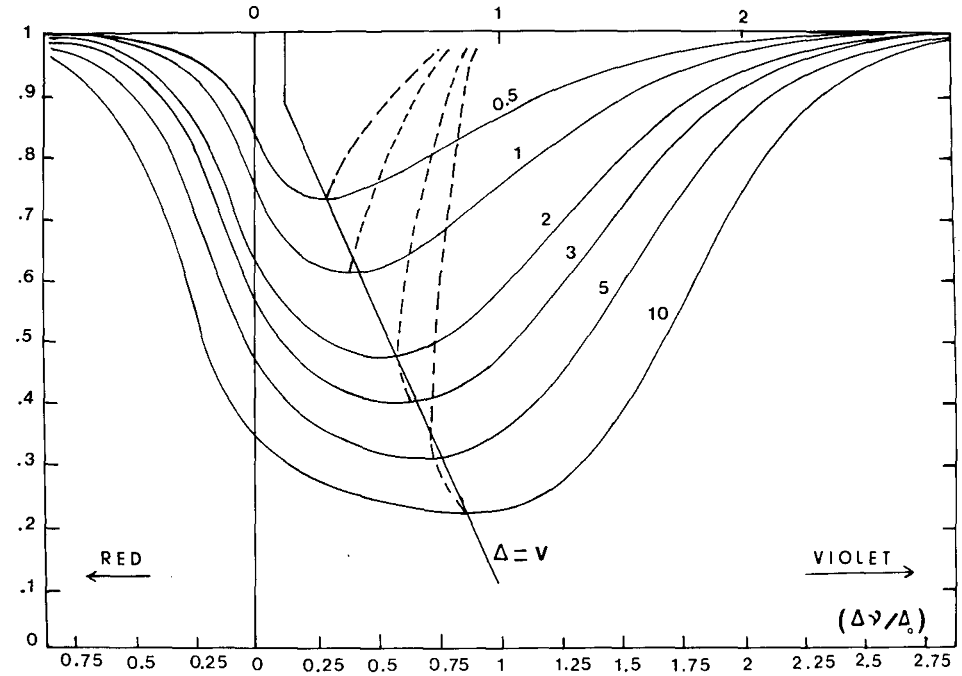

Fig. 16. *Asymmetrical profiles* resulting the test from model of Figure 15. (Notations: see the text.)

consequence of the (-1) term in the argument of the profile (see formula (21)): this negative term must be compensated by positive values of $\Delta\nu$ in order to produce a larger optical depth.

The second most evident feature in Figure 16 is the general trend for the bisector lines to be directed towards the violet when going from the line minimum up to the wings. So if we think that the wings represent deeper layers, we infer that the velocity *increases* in the direction of the increasing optical depth: this is just the contrary of what has been assumed. In other words, the bisector procedure fails entirely by giving the wrong sign for the velocity gradient. Again from the examination of the formula (21), it is to be expected that τ varies more rapidly with $\Delta\nu$ for negative values of $\Delta\nu$ (the argument is more and more negative) than for positive values of $\Delta\nu$. Thus the red part of the emergent profile is much steeper than the violet one and the bisector ought to have a positive slope.

The third noticeable feature is that, when increasing the strength of the line (see for example the curve $T_0 = 10$) there is a tendency to recover the true velocity gradient in the central part of the line but not in the wings. This leads immediately to the *C*-shape of the bisector, a fact so often quoted by the observers (see Figure 7). This point is of importance and we want to discuss it in more details. For purpose of comparison, we have plotted in the same Figure 16 the 'true' velocity (v/w_0) when a Barbier-Eddington-like relation is assumed, i.e. by simply saying that an observed intensity I is the value of S_L at a certain depth τ_c where the velocity has the value $v(\tau_c)$. By taking into account the formulae (20), (17), and (18), the relation between intensity and the 'true' velocity is then:

$$I = 1 - 0.9\,(v/w_0)\,. \tag{26}$$

We see that the agreement between the inferred velocity and the one that we call the 'true' one is very good at the line profile minimum. This implies that in this part of the line the integration runs over a small part of the medium, so that the velocity gradients have little importance, and we observe simply a certain depth at the rest frequency of the line in the *local* frame, but at a frequency shifted for the observer by the velocity of the given depth. This argument may be extended in the core of a strong line: there again, the velocity gradient is not seen but a non-zero local frequency $(\Delta\nu_L)$ such as to produce an optical depth of the order of unity is simply shifted by the velocity of the corresponding depth. This is of course the justification of the bisector procedure, which implies thus both the validity of the Barbier-Eddington approximation and the fact that the integration runs over such a narrow region that the effects of the velocity gradient on the argument of the profile Φ are eliminated. In the wings these two conditions tend to break down. First the line source-function reaches the Planck value (the continuum source-function) so that the Barbier-Eddington relation is no more valid (it is essentially a 'one-layer' situation, with $\tau_\nu = \tau^*$, and, on the contrary, we recover rather an opposite situation, of which the most appropriate approximation is by a single layer at a given geometrical – and not optical – depth $(z = z^*)$. Second, the region of integration becomes so large that now the variation of the argument of the profile Φ is also very large and we tend to 'see' the region which has the largest absorption coefficient: in the model under study, this region is just the surface, since the thermal width has its largest value Δ_0 thus giving smaller values of the argument $(\Delta\nu/\Delta)$ of the profile Φ. To sum up, we are seeing the *same* part of the atmosphere, both at the line profile minimum and in the wings: it is this situation which leads immediately to the C-shape of the bisector. These facts have already been noticed by Kulander and Jefferies (1966) and by Roddier (1964) in other cases.

We must then conclude that the observation of a C-shape for the bisector is just a strong indication of the failure of the bisector procedure, especially in the line wings, and we emphasize that this C-shape is more likely to be attributed to transfer effects along a single line of sight than to a complex mixing of two or more profiles emerging from different points of the solar surface. In other terms, we tend to be sceptical in front of the 'many-columns' models.

5.5. Conclusions

The first overall conclusion of this section is that all calculations show now very clearly that, in order to reproduce a given observed profile, the relation $\tau(\nu, z)$, i.e. optical depth vs frequency and depth, is, generally, of much greater importance than the relation $S(z)$, i.e. source function, vs depth. In other words it is often unnecessary to compute precisely the source-function if the first relation is unknown. Of course, one must be fully aware of the possible effects of velocity gradients upon the value of the source function, but we are now prepared to evaluate these effects by means of various methods such as those reported by Rybicki (1970). Actually, the work by Magnan (1968, 1970) shows that we can handle complex geometrical situations and physical conditions such as implying various frequency redistribution functions. But, as far as the observed profiles are concerned, and on the iterative path from one relation (z, ν) to the other $S(z)$, we are inclined to think that the most promising studies must now concentrate on the first one. This was often remarked by Athay (1970, 1972).

The second firm conclusion is the necessity of thinking always in terms of 'velocity gradients' and not in terms of 'velocities' alone: all this section was based on this point of view and in fact the introduction of the characteristic length parameter Λ is one way to do this. Related to this point is the importance of varying Doppler widths cause large variations of the dimensionless parameter $(\Delta\nu_L/\Delta)$ entering the profile Φ. We have demonstrated that this kind of effect must be taken into account even for 'photospheric' lines.

The third remark is that one cannot argue upon the simplicity of the models that we have discussed there to turn towards more sophisticated models including many parameters, but ignoring the fundamental facts that we have considered. In particular, we have restricted our discussion to the profile emergent from a single line-of-sight, but it must be realized that a subsequent summation upon many lines-of-sight includes implicitely the complexity of the situation along a single one! This seems evident, but it is also evident that the large majority of studies do not take properly into account the transfer problem along a single line-of-sight. It is thus an urgent need for the observers to specify what kind of average is truly observed, and this only requirement demands of course larger and larger resolution, both in space and in time. A high spatial resolution is possible only in the solar case; for the stars the situation is more difficult: there the geometry of the model becomes very crucial and determines for a large part the shape of the profile. The difficulties in establishing a diagnosis in this case have been discussed by Simonneau (1973).

6. General Conclusions

We shall conclude, obviously, by tempering the note of warning, which dominates this paper, by a message of long range hope.

(1) First of all, let us note that asymmetries in lines, often well determined from observations, are proving the existence of some velocity fields, either varying at a large depth scale, or else varying from point to point of the fraction of the solar surface under study (both cases commonly described as 'macro-velocity fields'). But, if we can assert that such fields exist, their diagnosis is terribly ambiguous. It is possible and even sometimes easy, to reproduce well some asymmetric profile by modeling the velocity fields. But we are forced to admit that the various solutions do not, even qualitatively resemble one another. The worse ambiguity is precisely the one above suggested: between a law $v(z)$ and a law $v(x, y)$, it is practically impossible to decide on the only basis of the existing observations, this being true both in the chromospheric and photospheric layers.

(2) Of course, this being said, we know that, *a priori*, a 'parametric' diagnosis is not what is needed. In the present state of knowledge of the physics of stellar atmospheres, taken in its broader sense (as suggested by Pecker *et al.*, 1973), it is relatively easy to solve the transfer equations, in NLTE, in complex geometrical or physical situations – *assuming* a given velocity field, even a complicated one. This is the basic reason why one does use a good physics of the source-function, and one derives from the observations the velocities, properly parametrized.

But the problem symmetric to that one, i.e. solving the hydrodynamical (or even MHD) equations in the stellar atmosphere, is in its very infancy! However, we might conceive that progress to come in this field will be done, hence removing a large part of the ambiguity. Then the diagnosis of measurements, using physical theory in a more and more self-consistent and complete way, will, ultimately use observations essentially as tests, or, possibly, to determine physical parameters of atoms and ions, the Sun becoming then nothing more than a big laboratory furnace, of known physical properties. This is still far from being the case, but we may at least hope that the earliest progress to come, not yet refined enough to make obsolete the use of diagnosis for the determination of velocity fields, will at least help to remove the ambiguities and undeterminations.

In shorter terms, now the theory gives $S(z)$, provided we know $v(x, y, z)$ and $\tau(z, v)$. The theory to come should allow us to determine $v(x, y, z)$ in a self-consistent way, not deriving it, rather badly, from observations, but more soundly from the equations of hydrodynamics and physics.

(3) The progress does not have to come only from improved theory. As we have said earlier, in Section 5 notably, better observations should complement the theoretical approach. A good guide to a better theory would indeed be given by very high resolving power (time, space, wave-length) observations of the solar lines, on the disk or off the limb. The problems evoked about the fine structure of K line are typical of what questions could be asked from the spectrographs. We know well that, fortunately, every observer is aware of this need. The efforts now under way are certainly going in that direction, and will soon provide us with new material to diagnose, with new guides to less ambiguous physical theories.

Therefore, our final conclusion will be very simple. It is a definite insistence on the necessarily parallel development on the three types of approaches already mentioned: the hydrodynamical (even MHD) approach; the NLTE transfer solutions, in presence of supposedly known velocity fields; and last but not least, the improvements of the resolution of the observations. Some kind of iterative process, injecting in the iterative loop the three types of data, is necessary in order to reach the same unified description of the astrophysical reality. A close coordination of work is necessary to improve the efficiency of the iterations. This implies from the part of the transfer people a clear conscience of the limitation of their theories, due to unadequate treatment of HD or MHD equations, from the part of the HD-MHD people a clear conscience of the coupling of radiation field with dynamical problems; from both a clear expression of what observations can provide, or must provide, them with what they need, and from the part of the observers, a coherent wish to observe quantities which add really new information according the requests of theory, instead of a bunch of unnecessary data.

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