PART VI

THE USE OF MODEL ATMOSPHERES FOR TEMPERATURE

THE USE OF MODEL ATMOSPHERES FOR TEMPERATURE-GRAVITY CALIBRATION

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Abstract. Regardless of the degree of elaboration of series of models, just *how* can they be used for calibration purposes? And how much is this calibration *sensitive to the* quality of the model theory?

These two questions are the basis of our discussion, which covers: I – The general principles of the use of model atmospheres in stellar calibration (1 – The two dimensional classifications; 2 – The use of the total luminosity; 3 – The cases of Vega and Sirius; 4 – The calibration of ST – T_{eff} relation); II – The failures of the two parameters model atmospheres (1 – The observational need for more-than-two-parameters classification; 2 – The abundance of elements, the line formation, and the model atmospheres; 3 – Various sources of unadequacy of models; 4 – Envelopes or shell features; their influence on model-building; 5 – The case of HD 45677. Diagnostic of early-type stars; 6 – Various unexplained spectral features); III – The present state of the model factory (1 – The classical models; 2 – New concepts in the description of a stellar atmosphere; 3 – New approaches in model making; 4 – Conclusions).

Résumé. Indépendamment du degré d'élaboration des réseaux de modèles, *comment* peut-on s'en servir pour la calibration des spectres? Et à quel degré une telle calibration est-elle *sensible à la qualité* des modèles utilisés?

Ces deux questions forment la base de notre discussion, qui couvre les points suivants: I – Principes généraux de l'utilisation des modèles d'atmosphère dans la calibration stellaire (1 – Les classifications à deux dimensions; 2 – L'utilisation de la luminosité totale; 3 – Le cas de Véga et Sirius; 4 – La calibration de la relation TS – T_{eff}; II – Les échecs des classifications utilisant des modèles à 2 paramètres (1 – Le besoin observationnel pour une classification à plus de deux paramètres; 2 – L'abondance des éléments, la formation des raies, et les modèles; 3 – Différentes sources d'inadaptation des modèles à la réalité; 4 – Caractéristiques d'enveloppes ou de couches; leur influence sur la construction des modèles; 5 – Le cas de HD 45677; le diagnostic des étoiles chaudes; 6 – Diverses observations spectroscopiques non expliquées); III – L'état actuel de l'usine à modèles (1 – Les modèles classiques; 2 – Nouveaux concepts dans la description d'une atmosphère stellaire; 3 – Nouvelles approches en vue de la construction des modèles; 4 – Conclusions).

The concept of model atmosphere has indeed made its first appearance in astrophysics as early as at the time of Secchi, when colors and temperatures were associated in a one-to-one relation.... Actually, the first models ever used, much later, were 'black-bodies'.

Very slowly, the development of the classification of stellar line spectra (independently of their colors), by the Harvard School, under the leadership of Pickering, A. Cannon, A. Maury..., in a sequence A, B, C... brought a considerable amount of new information. The discovery of the Saha ionization law, the identification of spectral lines it allowed, has helped to achieve this transformation of the Harvard initial sequence (completely artificial, based on a simple labelling), into the well known, and still in use, sequence: OBAF, etc....: this important step, to which such scientists as R. N. Russell, C. Payne... have contributed in an essential way, allowed to identify the spectral sequence with a *temperature* sequence; the stars were no more classed

B. Hauck and B.E. Westerlund (eds.), Problems of Calibration of Absolute Magnitudes and Temperature of Stars, 173–221.

according the Balmer lines intensities, but according their temperature. The new classification, together with its calibration in *T*, was not in a too bad agreement with the temperature scale based on the colors or on the energy distribution in the visual range.

We shall not go into a detailed study of the historical developments.*

But we should now mention that, as early as the first determinations of radii from interferometric measurements, in cases where parallaxes were known from trigonometric measurements, temperatures were determined from an estimate of the total flux – without much need of a reference to the black-body model, or to any other model (see Section 1.3).

We shall find, in the following, the simultaneous use of a similar 'double' methodology in the framework of the very sophisticated models and detailed measurements we can found now in the literature.

Let us come back to the Saha-law calibration of the Harvard classification.

It allowed immediately not only the removal of some ambiguities, but essentially the possibility of making easier the interpolation of any newly measured star in the grid of the comparison stars used for the calibration of the classification. It allowed therefore immediately the interpretation of any stellar spectrum in term of 'temperature'.

Since then, much has been done in using always more elaborate models to calibrate stellar classifications. This sentence essentially defines what will be the topics I intend to cover:

(a) Regardless of the degree of elaboration – physical or numerical – of a series of models, just HOW can they be used for calibration purposes? What are the PRINCIPLES involved in this use... when facing the stellar spectra, gathered in the large piles of tracings on our desk? and:

(b) Just how much are the results of this calibration technique SENSITIVE to the degree of elaboration of the models, to their quality? Nowadays, just how far have we proceeded in this elaboration? And what do we have to do in order to make a better progress, independently of the progress of the observations?

As I feel we have indeed MUCH TO DO, I want immediately warn seriously the observers that, still for a long time, they should be very cautious in using models, implicitely or explicitely, whomever they may come from; and I certainly do not intend to give them recipes they could blindly apply....

1. General Principles of the Use of Model Atmospheres in Stellar Calibration

They have been described often (see Strömgren, 1963; De Jager, 1955; Pecker, 1955; Van Regemorter, 1959, for example). But I feel that, at least shortly, I should try to reproduce once more the basic organigrams, with very few modifications... (Figure 1).

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* See, in the bibliography, articles of books marked with the sign*, which give some historical view or some bibliographical study, on this question.

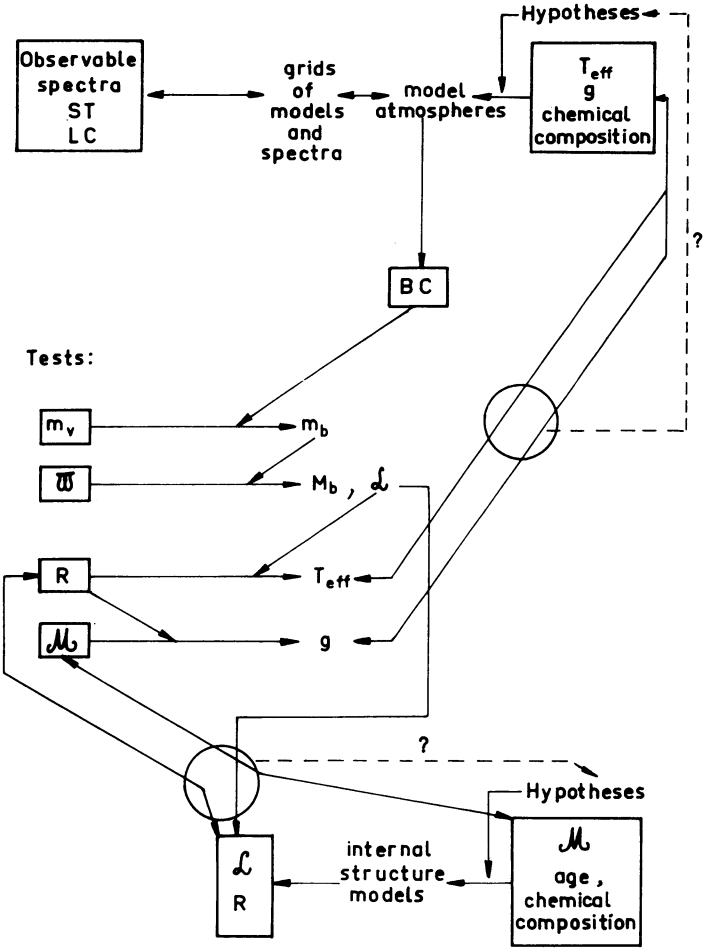

Fig. 1. The organigram of $T_{eff} - g$ calibration procedures. On the left side, the observed data; on the right, the hypothesis of the theory, and its parameters. The question-marks refer to the tests: Are the R, $\mathcal{M}$ values directly deduced from observations compatible with the T_{eff}, g values deduced from spectral data and atmospheres theory? Are the $\mathcal{L}$, $\mathcal{M}$, R values deduced from observations compatible with those deduced from internal structure theory? In other terms, are the observations compatible with either one or the other of the two sets of hypotheses and theories?

1.1. THE TWO-DIMENSIONAL CLASSIFICATIONS

On one side, we have the observed data. For all stars, they are essentially *spectral data*, and they give only relative values of the variation of intensity across the spectrum. The spectral data are indeed of a great complexity. All classifications (from the rougher ones, purely qualitative, well adapted to faint objects, to the precise quantitative classifications, which can be used only for the brightest stars) are selecting spectral data as *spectral 'criteria'*, as sensitive as possible to the physical quantities that seem to be relevant to the authors of the classifications.* Some are good indicators of temperature, some others of pressure, – for example, the width of the metallic lines, function of collisional broadening, is decreasing when the pressure or the density decreases; for example, the ratio of He II to He I lines, when observable (early-type stars), is increasing with temperature. But already now, we feel some ambiguity: other parameters are influencing the line width, such as magnetic fields, microfields of non-thermal velocities; etc.... We shall have to come back later on that kind of difficulties, which may lead, when ignored, to misleading conclusions.

At least, one can say that it can be, and it has been, reasonably asserted that (apart from some irregularities or misfits, on which we shall come back) all spectra can be fitted into a *two-dimensional classification*, all spectra can be defined by the given datum of the *spectral type* (ST) and of *the luminosity class* (LC), with more or less accuracy, according the ways they have been measured, and according their brightness. This is a purely empirical result, at least as it appeared twenty or even ten years ago, and is the basis of the well deserved success of the MK classification, for example. It means that the models that are intended to fit the observations should also be defined by two parameters, and that, in general, will be sufficient.... This idea has been, for years, of a great help; nowadays, it looks more like a severe and artificial limitation, which has been often acting as a brake against any progress in the interpretation of stellar spectra.

Of course, the first parameter to use is the *effective temperature* T_{eff}, which defines the total flux of energy which crosses each square centimeter of the atmosphere during each second of time, assuming implicitely (this assumption is essential: think of H and K lines, think also of fast rotators, such as those described by Maeder at this meeting!) the fact that this flux is the same at all points of the stellar surface:

$$F = \int_0^\infty F_\nu \, \mathrm{d}\nu = \frac{\sigma}{\pi} T_{\text{eff}}^4 . \tag{1}$$

This quantity has not the meaning of the temperature of any given well defined region in the atmosphere; it just reminds its user of the paramount importance taken in the early days of spectral classification by the black-body representation of the spectrum. Here, it is related essentially to the flux per centimeter square of the stellar surface and does not need to be linked with the total luminosity.

The other parameter is, naturally, the *gravity*, g, which defines the pressure p in the atmosphere, provided hydrostatic equilibrium is assumed to be present. Then, we have:

$$\mathrm{d}p = -g\varrho \cdot \mathrm{d}h . \tag{2}$$

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* We do not want to give here any exhaustive list, and we send back the reader to the classical papers of Fehrenbach (1958), Keenan (1963), Strömgren (1963), to the article by Schmidt-Kaler (1965); some more recent systems include the Spite system (1966), the Barbier-Morguleff-Gerbaldi (1972) system, oriented towards metallicity indices, and the IR systems (see Pecker, 1971), oriented towards envelopes criteria.

As the optical depth (at a frequency ν) is defined by:

$$\mathrm{d}\tau_\nu = -\,\varkappa_\nu \varrho\,\mathrm{d}h\,, \tag{3}$$

one sees easily that

$$\mathrm{d}p = (g/\varkappa_\nu)\,\mathrm{d}\tau_\nu\,. \tag{4}$$

If the opacity is nearly constant, then p is nearly equal to $(g/\bar{\varkappa})\,\bar{\tau}$, and, – everything being the same (a dangerous sentence: 'toutes choses égales d'ailleurs' en français!),– well defined by g, at an optical depth $\bar{\tau}$ near unity.

Building models with a given T_{eff} and a given g, assuming some more or less natural assumptions, of physical nature, about the nature of the stationary equilibrium reached in a stellar atmosphere, and their analytical expressions, about the symmetries of the geometry,... but assuming that nothing else differed from star to star, one is thus able to build artificial spectra depending upon two parameters. Then it is possible either to assign to a given star (used as a calibration star) a value of T_{eff} and g, or to assign, to each set of values of T_{eff} and g, a combination ST-LC, using for building these correspondences *any* set of two criteria used by the observers to define ST or LC, or measured in the spectrum of the particular calibration star under study.

Let us assume now that this operation is easy and unambiguous* (!!!). Let us note essentially here that, apart from the physical uncertainties on the physics of the atmosphere itself, our theoretician has been led (by the insistance of the observer) to admit *a priori* that nothing else but T_{eff} and g varies from star to star. In other terms, he has forced himself to admit a unique chemical composition, no variation of the magnetic fields (as they cannot be computed from T_{eff} and g), the unicity of rotation (for the same reason), perfect similitudes in the envelopes or shells, etc..... One sees now that this classical methodology might be seriously misleading; this, at least for two reasons:

(a) the 'criteria' might be depending upon additional parameters, even if carefully selected;

(b) the models themselves might, through some physical coupling, depend upon neglected phenomena, and might therefore be completely inadequate.

1.2. THE USE OF THE TOTAL LUMINOSITY

The use of models, to determine the T_{eff} and g of a given calibration star, or to determine the relation ST, LC vs T_{eff}, g, is possible if the spectrum is known with a sufficient resolution. But it does not need the use of any other measured data than the spectrum.

Another well-known approach is possible, using not the spectrum, but other observable quantities – parallax, magnitude, eventually mass.

The effective temperature can indeed be linked with the total *luminosity* of the star, if its *radius* R is known:

$$\mathscr{L} = 4\pi R^2 \sigma T_{\mathrm{eff}}^4\,. \tag{5}$$

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* A severe difficulty comes in particular from the fact that many much-used criteria are not so well defined in terms of the profile of the filter utilized for the measurements.

But let us note that this relation is ambiguous, as T_{eff} can indeed, when treated as an atmospheric parameter, depend upon the point on the stellar surface.... So (5) *defines* a T_{eff} which may be different from the T_{eff} in (1).

Let us note also that this $\mathscr{L}$ is only *radiative* energy; mechanical energy however may be important, and is generated as well in the central parts of the stars.

This quantity $\mathscr{L}$ is linked naturally with the stellar absolute magnitude M, which depends not only upon the flux per cm^2 of the stellar surface, but also upon the size of the star:

$$M_b = -2.5 \log \mathscr{L} + \text{constant}. \tag{6}$$

The subscript b is used for 'bolometric'; but we can hereafter delete this subscript, M being clearly the magnitude corresponding to the total flux of the star. The constant in formula (6) is determined by the reference to the solar values. For the Sun, one has (Table I):

$$M_{b\odot} = 4.62$$
$$\mathscr{L}_\odot = 3.866 \times 10^{33}.$$

TABLE I

Basic data used in this paper

Sun	ST. LC	G2 V		
	$R_\odot$	$= 6.960 \times 10^{10}$ cm		
	$\mathscr{L}_\odot$	$= 3.866 \times 10^{33}$		
	$\mathscr{M}_\odot$	$= 1.989 \times 10^{33}$ g	$M_b = 4.62$	Allen (1963)
	T_{eff}	$= 5.785$ K		
	$\log g$	$= 4.44$		
Sirius A	ST, LC	A1 V		
	δ	$= 0\overset{\prime\prime}{.}00612$	$\pm\, 0\overset{\prime\prime}{.}0001$	Brown *et al.* (1967)
	ϖ	$= 0''375$	$\pm\, 0\overset{\prime\prime}{.}006$	Jenkins (1952)
	m_v	$= -1.43$	$\pm\, 0.02$	Allen (1963)
	$R/R_\odot$	$= 1.75$		from δ,
	R	$= 1.22 \times 10^{11}$ cm		
	$\mathscr{M}/\mathscr{M}_\odot$	$= 2.28$	$\pm\, 10\%$	Allen (1963)
	$\mathscr{M}$	$= 4.54 \times 10^{33}$ g		
	$B-V$	$= 0.01$		Allen (1963)
Vega	ST, LC	A0 V		
	δ	$= 0\overset{\prime\prime}{.}0037$	$\pm\, 0\overset{\prime\prime}{.}0005$	Brown and Twiss (1956, 1964)
	ϖ	$= 0\overset{\prime\prime}{.}123$	$\pm\, 0\overset{\prime\prime}{.}008$	Jenkins (1952)
	$R/R_\odot$	$= 3.23$		
	R	$= 2.25 \times 10^{11}$ cm		
	m_v	$= +0.05$	$\pm\, 0.02$	Allen (1963)
	$B-V$	$= 0.00$		Allen (1963)
Bolometric correction, in the A0 region				
	BC	$= -42.5 + 10 \log T + 29000/T$		Allen (1963)
	(general interpolation formula)			

Therefore the constant is equal to 88.587 ± 0.01, the error being essentially due to the internal dispersion of the whole magnitude system, involving the comparison of the Sun's magnitude to the stellar standards.

The radius R and the gravity g are also related by a relation which brings in the *mass* $\mathscr{M}$:

$$g = G\mathscr{M}/R^2 . \tag{7}$$

Clearly the knowledge of M (i.e. of $\mathscr{L}$) would then bring the additional knowledge of the radius, and of the mass $\mathscr{M}$ of the star, if T_{eff} and g are known from the spectrum, – even if one does know directly these quantities, as possible in a few cases (binaries...). If we know $\mathscr{M}$ through the study of a binary system, then we have the possibility of an interesting check of the consistency of the whole analysis.

But in order to proceed in such a way, again, we need some additional use of the model, and a very essential one indeed. For the observed quantity is not the *total absolute magnitude* M, but the *visual absolute magnitude* M_v, if we know, through the measurement of the parallax ϖ, how to deduce it from the *visual apparent magnitude* m_v.

To extrapolate the visible spectrum to unobservable parts, i.e. to compute the bolometric correction ($-BC = M_v - M$), we need the models. The well-known relations involving magnitudes are:

$$M = m + 5 + 5 \log \varpi \tag{8}$$

$$m - m_v = M - M_v = BC . \tag{9}$$

There is here some ambiguity. The absolute magnitude is often coming not from trigonometric parallaxes, but from '*absolute magnitude spectral criteria*'. This is the case, for example, for the O-stars: in their case, the number of stars of known parallax, that could be used for calibration, is so small as to almost forbid their use. The absolute magnitude criteria are often used to assign to a star a LC: but indeed the latter is still purely a quantity characterizing only the atmosphere, and quite independent of the radius. If it found that the LC is also a total luminosity criterion, it means essentially that there is, at least in the samples in use, a strong statistical correlation between the three quantities T_{eff}, g, and $\mathscr{L}$. Two of them seem to be sufficient to define the third one, in practice.

This implies that there exists an $\mathscr{M} - \mathscr{L}$ relation, independent of R. We know from empirical evidence that indeed this is actually the case.

This could imply also that there is a singly T_{eff}, g relation, independent of the luminosity: but this is not true: we have several sequences in the HR diagram. The fact that actually, we have such sequences implies thus another relation. On the surface of Figure 2, which is the locus of stars in the (T_{eff}, g, $\mathscr{L}$) space, stars do not appear everywhere: a second additional relation therefore must exist; and this could be a $\mathscr{M} - R$ relation.

Actually, these two additional relations are given by the theory of stellar interiors, and they both depend upon the three quantities $\mathscr{L}$, $\mathscr{M}$, R. Empirical evidence however gives some additional weight to their existence, as it is well-known.

The model atmospheres, together with the use of measured spectra, are thus allowing some determination of T_{eff}, g, BC, (which may depend, and indeed does, of the models in use). They do not tell anything else. If we use any observational luminosity criteria, in addition, to claim we know $\mathscr{L}$, it means therefore that we *assume* some internal equilibrium relation without saying so. We assume such relations are valid at least for both the stars used for the sampling of the classification and for the star under study.

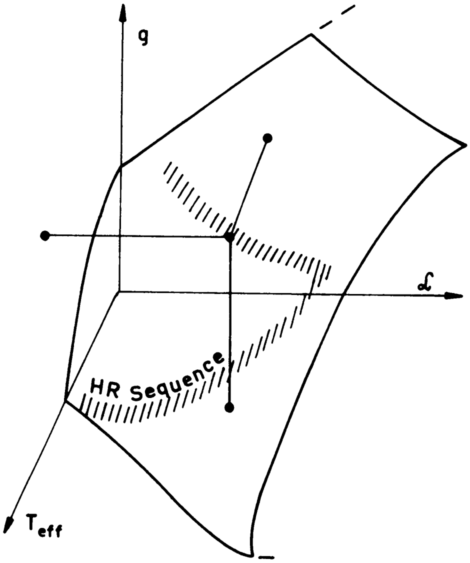

Fig. 2. Two-dimensional sequences in the T_{eff}-g-$\mathscr{L}$ space. The empirical possibility of two-dimensional classification forces the representative point of a given star to fall on a single surface; the existence of sequences on the HR diagram forces the representative point to fall on some limited bands of that surface.

The trouble is of course the statistical character of the various phases of the procedure. A given star might always not satisfy the internal equilibrium equations (star not yet on the main sequence, already having left the main sequence, etc....) – or the atmospheric traditional equilibrium equations....

1.3. THE CASE OF SIRIUS AND VEGA

In both cases, accurate knowledge, and particularly, direct determinations of the radius R, and a relative adequacy of the model atmospheres should allow checks.

We can essentially compute T_{eff} from the measured data of magnitudes, of radius, of parallax, and from the interpolated statistical values of BC (which is relatively small, by convention, in this ST region, near A0–A1).

The values we have used are collected in Table I, where is also included an interpolation formula for the *BC*.

Then we can write the two Equations (5) and (9)+(8)+(6), as follows:

$$\log \mathscr{L}_1 = -3.147 + 4 \log T_{\mathrm{eff}} + 2 \log R \tag{10}$$

$$\log \mathscr{L}_2 = -0.4 m_v + 0.4 A - 2(1 + \log \varpi) + + 4 \log T_{\mathrm{eff}} + 11.600/T_{\mathrm{eff}} + 18.435\,, \tag{11}$$

where A represents the interstellar extinction expressed in magnitudes. Assuming no interstellar absorption (B-V is very small indeed, see Table I), we get for each star a set of two curves $\mathscr{L}(T_{\mathrm{eff}})$. Their intersection gives the values of T_{eff} we need – without intervention of the model atmosphereexcept for the evaluation of the *BC* – obviously a one of first approximation... (see Figure 3). The estimation of the errors on the measured data allows to delineate the margin of the determinations.

The first surprise is to find for Vega a relatively low temperature – lower indeed than most of the determinations made from the detailed use of models (Table II;

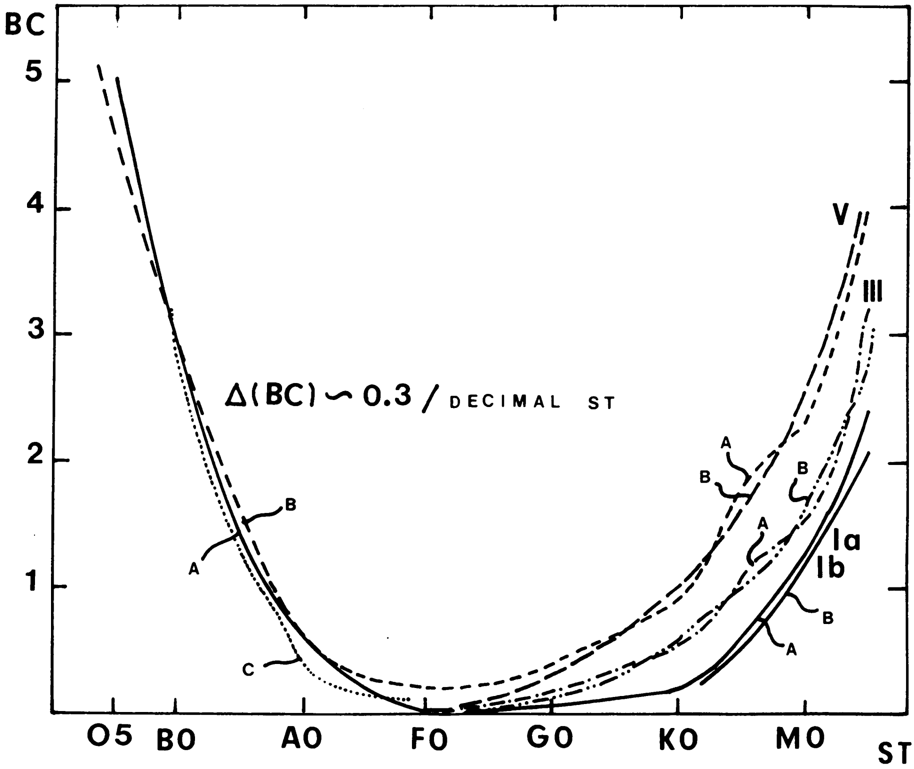

Fig. 3. The bolometric correction generally in use. Full line: LC I; mixed line; LC III; interrupted line: LC V (on the right part of the diagram). On the left part of the diagram, all stars are falling on the same curves. A: according Schmidt-Kaler (1965); B: according Allen (1963); C: according Harris (1963). One has estimated the error made on the BC for one tenth of spectral type to be of the order of 0.3 mag., in the B star area, where this error is maximum.

TABLE II

Some determinations of effective temperatures, either directly, or from model atmospheres

(a) Determinations of T_{eff} of an A0 star (after Keenan, 1963)

T_{eff}	method	authors
9500	Model Atmosphere	Hunger (1955)
10700	Eclipsing binary	Kopal (1955)
10500	Eclipsing binary	Popper (1959)
10900	Model Atmosphere	Melbourne (1960)
9400	Model Atmosphere	Bless (1960)

(b) Fundamental determinations of T_{eff} (after Harris, 1963)

(stars with observed angular diameters)

μ Sco	B1.5 V	27500K	Harris (1963)
α CMa	A0 V	9350K	Popper (1959)
β Aur	A2 V	10500K	Popper (1959)
Sun	G2 V	5784K	Allen (1950)
$\gamma\gamma$ Gem	M1 V	3650K	Popper (1959)
α Boo	K2 IIIp	4090K	Kuiper (1938)
α Tau	K5 III	3780K	Kuiper (1938)
α Sco	M1 Ib	3230K	Kuiper (1938)
α Ori	M2 Iab	3460K	Kuiper (1938)
β Peg	M2 II-III	3080K	Kuiper (1938)
o Cet	M6 (var)	2360(±) K	Kuiper (1938)

This list is based on angular diameters from Pease, 1931; more and better data should now be computed from the lists of measurements by Brown and Twiss. In the present publication, we obtain from these new measurements (see Figure 4):

α CMa	A1 V	10100K	(this publication)
α Lyr	A0 V	8200K	(this publication)

(c) Effective temperatures from model atmospheres (after Harris, 1963)

O5	44600°	Underhill (1957)
O9	36800°	Underhill (1957)
B2 V	27800°	Underhill (1957)
A0 V (α Lyr)	9500°	Hunger (1955)
A3 V	8900°	Osawa (1956)
A9 V	7560°	Osawa (1956)
F2 V (σ Boo)	6800°	Code (1954)

These ancient determinations are indicated on Figure 4 and are obviously superseded by many more models since published.

(d) Various values used in literature for a Sirius model atmosphere

$T_{\mathrm{eff}} = 9700$	$\log g = 4.3$	Boyartchuk (1962)
9700	4.3	Kohl (1964)
9700	4.3	Gehlich (1969)
10000	4.0	Strom *et al.* (1966)
10080	4.44	Warner (1966)
10200	4.35	Schild *et al.* (1971)
10290	4.3	Latham (1971)
10500	4.0	Gros (1972)

Figure 4). According to the usual ST $-$ T_{eff} scale, it corresponds to A0–A1 (Sirius), but to A4–A5 (Vega) instead of A0 ...Vega appears as colder than Sirius!...

Can we discuss the errors in any reasonable way? The Table I gives the values, from the original sources, of the errors that affect each measurement. Therefore, we can estimate:

$$\begin{aligned} \Delta\mathscr{L}_1/\mathscr{L}_1 &= 2\Delta R/R \cong 0.03 \quad \text{(Sirius)} \\ &\cong 0.27 \quad \text{(Vega)} \end{aligned} \tag{12}$$

or

$$\begin{aligned} \Delta \log \mathscr{L}_1 &\cong 0.012 \quad \text{(Sirius)} \\ &\cong 0.12 \quad \text{(Vega)} \end{aligned}$$

$$\begin{aligned} \Delta\mathscr{L}_2/\mathscr{L}_2 &= 0.4\Delta m + 0.4\Delta A + 2\Delta\varpi/\varpi + 0.4\Delta\,(BC), \\ &\cong 0.056 \quad \text{(Sirius)} \\ &\cong 0.154 \quad \text{(Vega)} \end{aligned} \tag{13}$$

or

$$\begin{aligned} \Delta \log \mathscr{L}_2 &\cong 0.02 \quad \text{(Sirius)} \\ &\cong 0.07 \quad \text{(Vega)}. \end{aligned}$$

We have estimated the error on *BC* to be of 0.04 (see Figure 4), which *seems* to be sheer pessimism, in the present state of *usual* model atmospheres. From (12) and (13) – see Figure 5 –, one obtains the following values:

Sirius: $9500 < T_{\text{eff}} < 10700$
Vega: $7700 < T_{\text{eff}} < 9900$.

A source of error has been noted by Popper (1959): the interferometric measurements

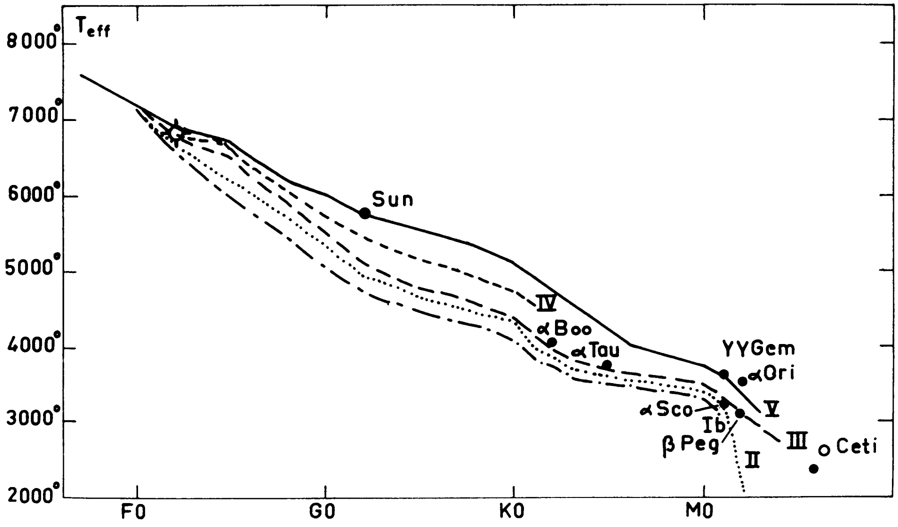

Fig. 4a.

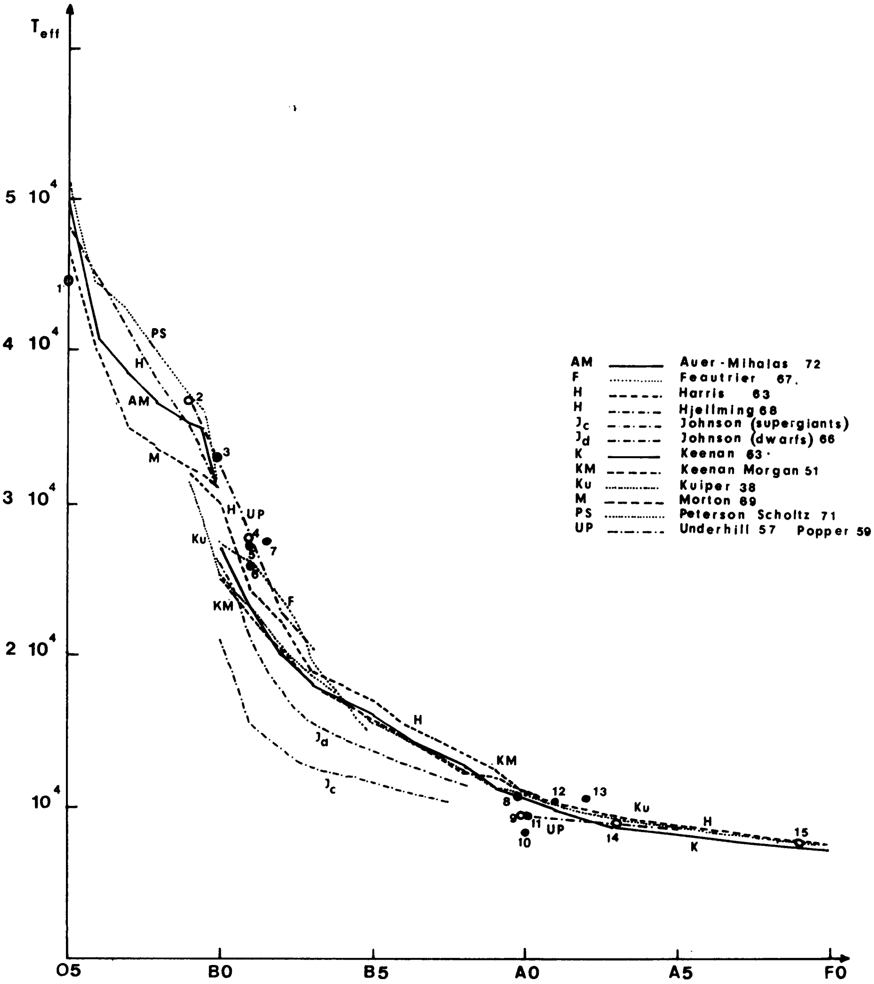

Fig. 4b. The T_{eff} – ST calibration. (a) from F0 to M stars, according Keenan (1963). The different curves refer to different LC. (b) from O5 to F0, according various authors. Open circles denote model atmospheres used by Harris (1963) (see Table 3). Black circles denote a few direct determinations: (3) MD 164402, Dufton (1972); (5) ζ Per, Cayrel (1958); (6) HD 96248, Dufton (1972); (7) μ Sco, Harris (1963); (8) η Leo, Wolff (1971); (10) α Lyr (this investigation); (11) Sirius, Popper (1959); (12) Sirius, this investigation; (13) β Aur, Popper (1959).

of the diameter are affected by the limb darkening. By assuming a limb-darkening coefficient of 0.45, Popper finds a diameter of 0.0072 instead of the 0.0068 measured at that time by Brown and Twiss (1956, 1964). This increases $\log \mathscr{L}_1$ by 0.05 and decreases the temperature of Sirius, by about 100°.

However, Popper, using the same analysis as we made, finds for Sirius 9350° ± 340°. His value for *BC* is different from ours by about 0.07: this would increase our $\log \mathscr{L}_2$ by 0.03 and would not be sufficient to bring our values to an agreement. We shall come back on the *BC*, – but at this stage we do not understand the difference between our calculations and Popper's. The m_v is -1.43 for us, -1.47 in his computation. But the diameter value is definitely smaller in the more recent measurements by Brown *et al.* (1967) than the measurements used by Popper.

Can we understand why we obtain temperatures that seem too high for Sirius, too low for Vega? If, instead of having put $A=0$ in the equations, we would have taken some positive value for the absorption, we would have obtained higher temperatures, more compatible for Vega with the usual calibrations: A value of $A \cong 0.7$ mag. (Vega) is indeed fitting the data. But it seems really high, and difficult to reconcile with the very low value of the color index $B-C$, unless we assume a practically grey circumstellar additional absorption of that order of magnitude.

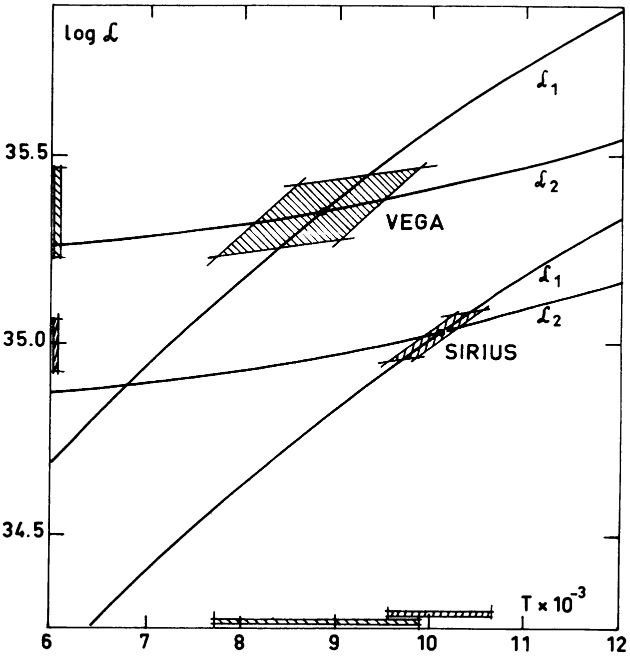

Fig. 5. Determination of T_{eff} and $\mathcal{L}$ for Sirius and Vega. The curves represent the functions described in the text (Equations (10) and (11)).

We have so far not determined the value of g; we can do it for Sirius, without reference to the model atmosphere, as Sirius is a well-known double system. We then obtain:

$$\log g = 4.309$$

The error is then

$$\Delta g/g = \Delta\mathcal{M}/\mathcal{M} + 2\Delta R/R \cong 0.13$$

$$\text{hence} \quad \Delta \log g \cong 0.06 .$$

Let us discuss the two cases separately, using the various available data.

1.3.1. *Case of Sirius*

The small measured values of R do not fit either the statistical relation for the statistical value of T_{eff} deduced from the spectral type A1 V (or the above value of T_{eff} (Figure 6). It means that we have to reduce the radius R, thus push Sirius under the MS; g has to be accordingly increased, and $\mathcal{M}$ decreased. Can we accept such an error in the analysis of a binary system so well known? It is doubtful; we are temptated to class Sirius as an A1 VI, or to class it as a A4–A5 V with an abnormally high T_{eff}... but why? And anyway, is this linked with the fact that Sirius seems to have properties similar to metallic-line stars?

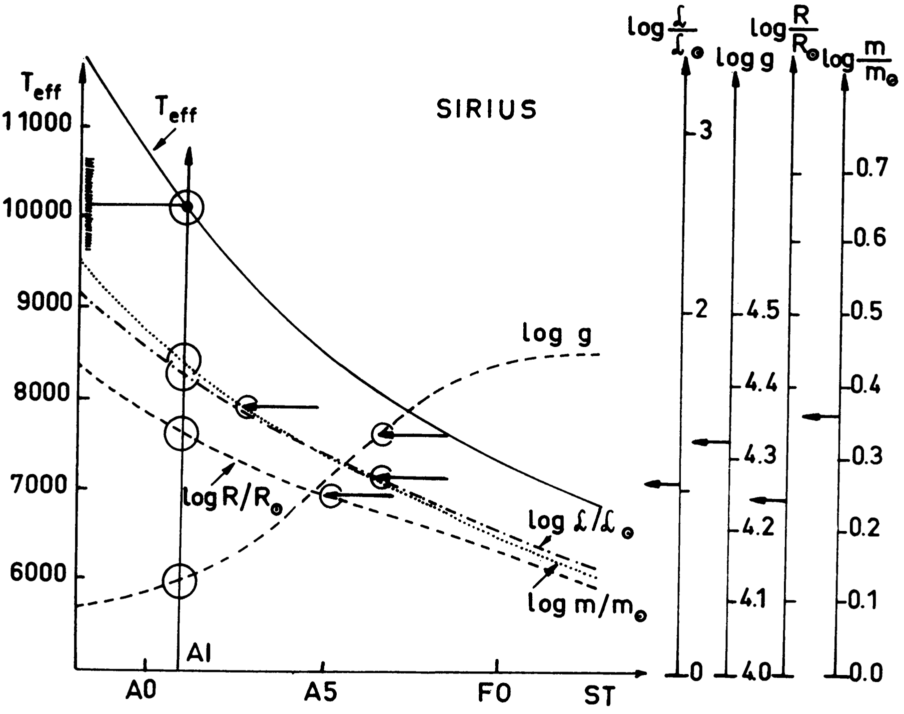

Fig. 6. The case of Sirius. The arrows on the right indicate direct determinations. The heavy line on the left, parallel to the T_{eff} axis, is taken from Figure 5. The curves correspond to the usual calibration for class V stars.

1.3.2. *Case of Vega*

The measured value of R is too high for the generally admitted statistical value of T_{eff} – and much too high for the new one. A factor of 1.5 to 3 has to be applied. We think therefore that whatever is the reason for the T_{eff} anomaly, the star Vega is probably well above the main sequence and can be considered as of LC IV, (as suggested also by BCD classification). The luminosity, according the usual calibration, is allright. But according the new measurements of T_{eff}, the star is twice of three times overluminous confirming that the star might be colder and bigger than its usual classification, – let us say A 4–5 IV (Figure 7).

A high value for the extinction may be the cause of the trouble; but it would still keep Vega above the MS. An error in the other measurements is unlikely larger than

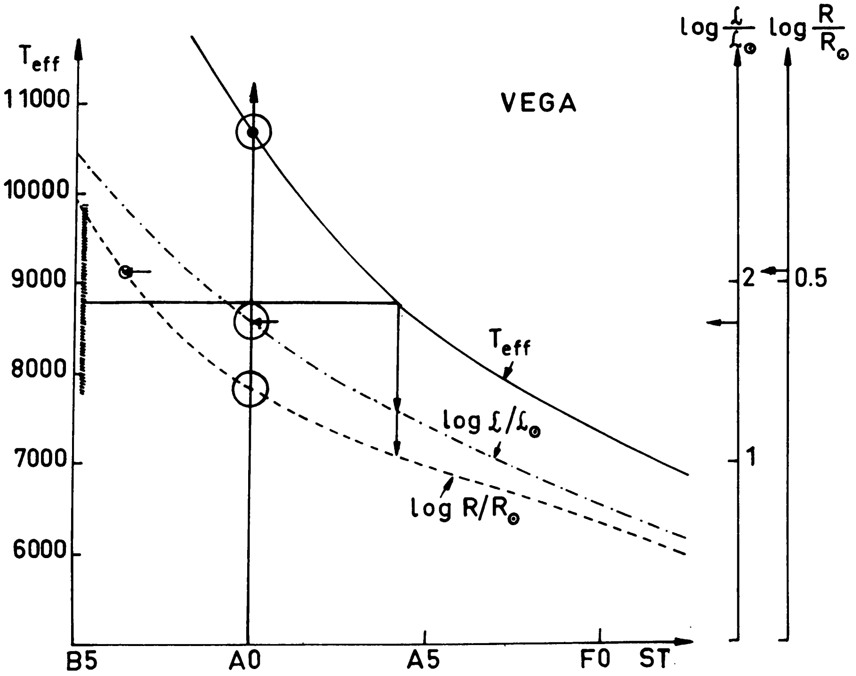

Fig. 7. The case of Vega. Conventions as in Figure 6.

the margins we have adopted. Therefore, one is forced to admit that, at least in the case of Vega, possibly in the case of Sirius too, something else is badly wrong... It brings thus the question: '*how valid indeed is the usual* ST – T_{eff} *calibration*?' As this relation depends very little upon the LC, the possible changes in LC do not affect this basic question.

Of course, we know that none of the two stars is a 'bona fide' star; even for Sirius, which seems the less abnormal, we know that the use of D_B to fix the ST gives A0–A4; but the gradients φ_b, φ_{uv} of the Paris classification give B8–B9. Whenever we happen to know better a star, it seems indeed an universal rule that we put it progressively out of power as a standard, ... isn't it true? After an extensive use of stellar demography, we found now that the more classical stars, as Sirius, Vega, or S Monocerotis are indeed psychotic individuals! – But they are not... We just happen to know them better now. And we should not forget that, according Freud's views on psychosis, the only thing wrong about such individuals is the way we tackle them, ... i.e. the theory of model atmospheres.

In the particular case of Sirius or Vega, obviously no model, in the long series of available tables, replies in any satisfactorily way to the observed difficulties. In the case of Sirius, recent attempts by M. Gros (1972) to introduce a chromosphere, as suggested by the UV observations of Stecher (1969) and Carruthers (1968, 1969), do not reach any completely satisfying solution. Her model is represented in Figure 8; the remaining discrepancies are clear on her computed UV spectrum (Figure 9).

We may note on this figure that the UV spectrum is generally much smaller than that predicted by the models – due to line absorption, most probably.

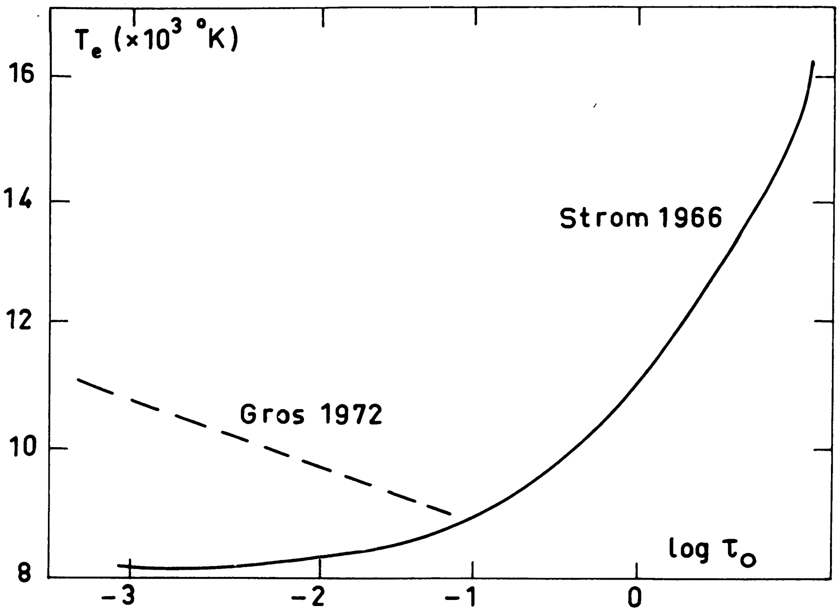

Fig. 8. The models of Sirius, according Strom *et al.* (1966) and Gros (1972).

What kind of models should be used to reconcile computed spectra with observed spectra, in both cases of Vega and Sirius, – admitting that they cannot be the statistically valid models representing roughly well stars of the main sequence – if any such model does exist?...

(a) In the case of Sirius, the radius is probably smaller than in the main sequence; the temperature is probably correct. Therefore, the model is sufficiently well defined by the usual values of g and T_{eff}. However, the calibration obviously does not work too well.... Can we think the BC is in error? If so, the model is in error itself, the energy is distributed in the spectrum in a different way than in the case of a normal 'classical' model. Much flux is missing in the UV. Therefore, the BC is probably smaller, and $\mathscr{L}_2$ also. We shall then have to lower the temperature; which fits a number of things (g, R, $\mathscr{M}$), as well as the measurements of D_B.... The distribution of the radiation in the spectrum, strongly affected by an abnormal blanketing effect, might lead to an abnormal temperature gradient, and then to an excess of color, as

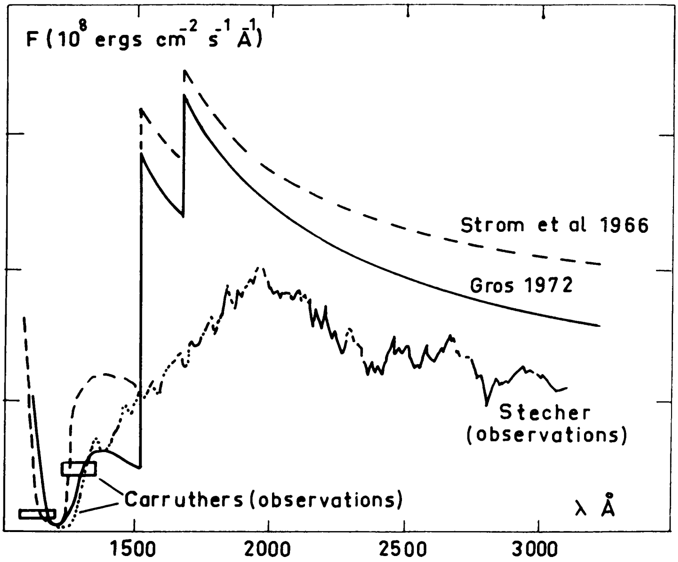

Fig. 9. The UV spectrum of Sirius. According models by Strom *et al.* (1966); Gros (1972), and measurements by Stecher (1969, 1970), and Carruthers (1968, 1969).

shown by the measurements of the φ_b and φ_{uv}.... In that case, the use of series of 'homologous' models derived from two-dimensional grids would be wrong and misleading. Let us note that the various authors who have computed T_{eff} for Sirius – independently of other differences in the data – have used different values for the *BC*. Our value, with the adopted temperature, was 0.40; Popper (who has been using larger radii, coming from previous series of measurements by Hanbury Brown and Twiss, 1956, 1964) used 0.27; Pease has used the value 0.72. However, this does not satisfy us: a change in the *BC* changes $\mathscr{L}_2$: but, according the fact that the representative curve for $\mathscr{L}_2$ is almost flat, the total obtained luminosity would be decreased, which pushes the representative point in diagram 6 still further away from where it should be.

Our only solution is indeed to accept a smaller radius, a larger g – and to hope that such models will fit better the visible spectra as do the usual models; of course the *BC* will have to be revised; but this will not be sufficient!... Undoubtedly, the building of models richer in their physical value as the ones now in use, will greatly help; but it has still to be done!

(b) The case of Vega is entirely different, as already seen. What kind of model could put the picture more coherent? Here some circumstellar *grey* absorption might do the trick. What would do some change in the *BC*? In order to improve the temperature fit, we should not, again, change the luminosity too much; therefore, a change in the radius would fit the classical T_{eff} value, provided we diminish the 1956 Hanbury Brown and Twiss value, by a factor 2.7. It is highly unlikely that we can do that. Taking a A4 ST value for T_{eff} would not fit the line spectrum, unless some strong abundance anomaly exists. Here, we feel that assuming a circumstellar absorption is a better choice – but a small value for g will still be necessary. And we stay unhappy... Indeed similar discouraging conclusions are reached in a very thorough analysis by Hardorp and Scholz (1968), as well as by Heintze (1968) who feels also that a small value of g is necessary.

Coming back to a sentence already expressed earlier ... the wrong thing is the theory of stellar atmospheres; and we must not use blindly the classical theory. Drastic changes have to be done in it.

(a) The UV spectrum (and, as we shall see for other stars, the IR spectrum) will force us to modify the calculations of the *BC*. Models are unadequate.

(b) They are unadequate because, possibly, of abnormal abundances, of extended atmospheres, of circumstellar phenomena (see Section 3).

(c) The contribution of chromospheric layers to the T_{eff} may affect the T_{eff} direct measurements without affecting the layers where the continuum spectrum is formed – or affecting them very little.

(d) The LTE classical analysis of the spectrum has to be put under strong suspicion.

(e) The treatment of blanketing in the usually available models is primitive.

What is this study of the difficulties encountered in the case of Sirius and Vega teaching to us? After all, both stars are amongst the standards that have been used to establish the average calibration of spectral sequences. So, the fit SHOULD be excellent ... And it is not!

Even if the models were systematically wrong, one could say they should be wrong in the same way. We have just shown it is not true. Therefore, the use of DIFFERENTIAL spectrum analysis (essentially based on the fact that if models are wrong they are wrong in the same way for the two stars to be compared) should be abandoned. Incidently, we shall see later that, for similar reasons, we should abandon the differential curve of growth analysis as well, an opinion we have expressed several times without being enough convincing so far – at least, we feel so...!

1.4. CALIBRATION OF ST – T_{eff} RELATION

To calibrate the relation used in our preceeding discussion of the cases of Vega and Sirius, use has been done of well-known (let us say 'well-known' instead, with quotation marks!...) stars. The Table II taken from Harris and Keenan, gives the basic data used in such calibrations. The curves of Figure 4 show the relation, as finally established, according the same sources. The internal dispersion reveals the same kind of effect as discussed for Vega and Sirius and can be considered as real and physical, not as spurious.

Clearly, the two-dimensional models are only a historical step, but we may foresee that, for obvious reasons of convenience, it will still be applied, in practice, for many years.

At least, when doing so, an important question will have to be answered (and, whenever feasible, i.e. rarely, replied): let us assume (Figure 10) that the use of two criteria k_1 and k_2 gives, using a set of iso-k_1 and iso-k_2 curves, in the $T_{\text{eff}} - \log g$ plane, allows the determination of one point in this diagram. The use of k_3 and k_4 gives another

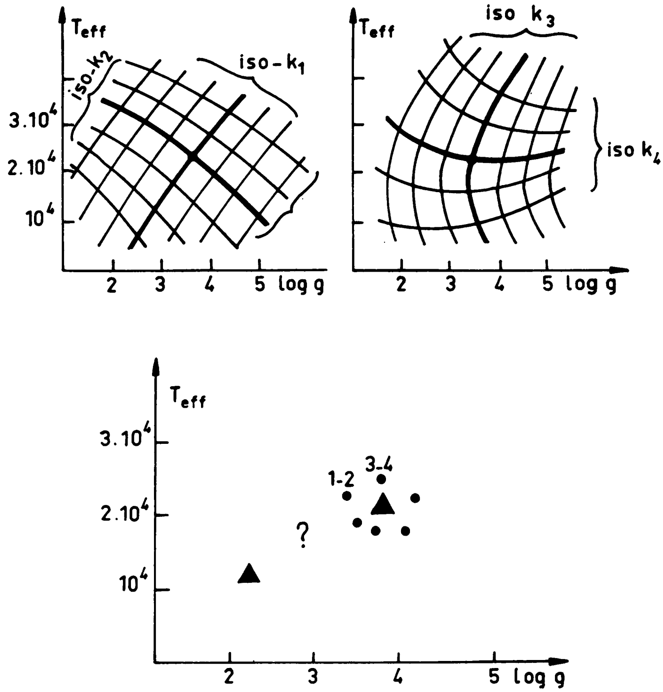

Fig. 10. The basic difficulty in using two-dimensional determinations of T_{eff} and g. The problem is: by using two different sets of two criteria, one find different solutions. The heavy triangle is the average of such determinations, represented by dots. With better models, the dispersion may be reduced... But will the average triangle be strongly displaced or not, in the twodimensional diagram?

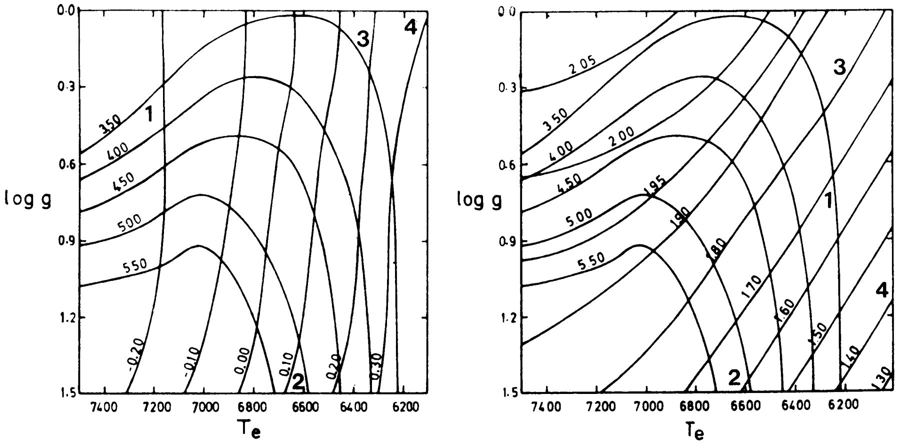

Fig. 11. (according Osmer) – Two-dimensional analysis; an actual case. The two-criteria method applied to two sets of criteria (left: Hγ, and C I; right, Hγ and D_B) and to some stars: 1: φ Cas; 2: α Per; 3: HD 10 494: 4: + 60° 2532. One can see how abnormal is especially the case of stars 1 and 4.

point. Any set of two criteria will allow us the determination of a new set of values for T_{eff} and g. The dispersion of these points is due, in part, to errors in measurements, in the essential part however to the need for additional parameters, and to the unadequacy of the models.

The question now is: 'assuming that we introduce properly the third, fourth, etc. ... parameters, and that we use excellent models for the star in consideration, we shall find, instead of a few points, a single one, within the margin of errors. Will this point fall near the center of gravity of the first group of separate points determinate with the first approximation theory? Or will it be strongly displaced with respect to this group?

In the latter case, the two-parameters classical approach is not even giving a correct order of magnitude, even when all possible sets of two criteria are used, and, although, this discussion is rarely done in a systematical way, we have a hint that this is indeed very often the case, and that an extremely strong warning, once more, should be made, against the classical two-parameters model approach. A recent example is clearly given by Figure 11, drawn according Osmer (1972).

2. Failures of the Two-Parameters Model Atmospheres

2.1. THE OBSERVATIONAL NEED FOR MORE-THAN-TWO-PARAMETERS CLASSIFICATIONS

We have seen clearly that even the best stars available for calibrating the classification, whenever they could be approached both by direct methods and by model atmospheres methods, gave results not so easy to interpret. They do not fit very well the statistical relations established precisely by their use – which proves not so much an unadequacy of the samples which have been used, than the basic unadequacy of the very idea of the feasibility of a two-parameters sampling.

Actually, independently of that kind of considerations, if became soon apparent, whenever any two-dimensional system of spectral classification was used, that it was inconsistent with the others, and therefore unsufficient. This was shown through several types of approaches.

(a) In the Paris BCD classification, for example, some stars fall out of the 'surface' where all normal stars are falling; in other terms, the surface has a real non-zero width, which does not correspond only to the internal dispersion of measurements, but to the real need for a third classification parameter. In the BCD classification, in addition to λ_1 and D_B (which give in principle T_{eff} and g), the use of the gradient φ_b of the continuous spectrum seems necessary. The 'surface' mentioned above is indeed constructed in the space D_B, λ_1, φ_b.

It is interesting to note that the effect seems to be especially noticeable for the 'metallic-line stars' and for the subdwarfs – indicating more or less that the effect is through their continuous spectrum, an effect of metallic abundance. As the continuous spectrum is directly linked to the model, this only comment makes us believe that the influence of metallic lines and metallic continuum, not only on the spectrum, but also, and essentially, on the model itself, is of a noticeable importance, as we shall see later.

(b) Several stars are denoted in the usual classifications, by some additional symbol, such as the letters *e* (for emission), *p* (for peculiar) or still *f*, or others, to indicate abnormal spectral features, often appearing in emission. This is generally thought to imply envelopes, or extension of the atmosphere, which the models rarely predict when used in the classical simple-minded way.

(c) Several stars display 'symbiotic' spectra, where are juxtaposed, simultaneously, features corresponding to very different types of stars. They often could be interpreted not in terms of binary spectrum, but by using detached envelopes and the like, where physical conditions strongly differ from those of the classical photosphere.

The 'shell' type spectra belong more or less to this class; often observers, although they agree on the 'type' of peculiarity displayed by the spectrum, disagree on the interpretation, and naturally, as a way of consequence, on how various strange stars are related, physically, each to the other.

Accordingly, theoreticians should indeed certainly consider the symbiotic character of a spectrum as very general, in all stellar atmospheres.

(d) Several stars, classed in a certain way, according one type of classification, are classed in another way according another type of classification. Typical of this case are the metallic line stars. But we should note here that 'metals' (indeed, other elements than H and He, according some authors) cannot be considered as a whole. The ratio of abundances of two given elements seem to vary very much, from a star to another one, and this variation differs from the two elements selected to a group of two other elements. There are 'metallic-line stars'; but there are also 'baryum stars', 'europium stars', 'helium-poor stars', 'helium-rich stars', etc....

A particularly difficult region of the HR diagram is that of the cold stars. Three additional parameters (at least?) are necessary. The figure in Schmidt-Kaler (1965) shows this quite clearly. So does Figure 6.1 and Table 6.3 in Pagel (1971), who quotes Fawell and Greene.

Several cases illustrating that type of difficulties have been quoted in the literature. The following list of stars is only a partial list of examples of the non-suitability of a two-dimensional classification, according to Schmidt-Kaler.

(i) Emission line stars of early types: Oe, Be, Ae
    such as φ Per, $\varkappa$ Dra, HD 45677, etc....

(ii) Stars with strongly broadened lines (Bnn, Ann, Fnn)
    such as η UMa.

(iii) A-stars with peculiar spectra: Ap, Fp
    such as α And, β CrB.

Generally, the abundances of ions Mn II, Si II, Eu II, Cr II, Sr II seem higher than normal.

(iv) Metallic-line stars: Am, Fm
    Type: α Gem B.

The ratio Ca II (K-line) to metallic lines is abnormally weak.

(v) Wolf-Rayet stars WN5 to 8, WC5 to 8
    Types: HD 192163, 192103.

These stars have broad emission lines of ions of either C or N.

(vi) Population II stars

Types: F to M.

Such as δ Lep, HD 140283.

Their spectra are characterized by abnormal intensities of CH and/or CN bands (possibly due to abnormal abundance ratios C/metals).

(vii) Carbon stars

Types C0–C9 (R and N)

Type: UX Dra

The bands of C_2 and CN are abnormally enhanced.

(viii) S-Stars

Type S

such as: R And

The oxide bands (ZrO, YO, LaO, TiO) and the technetium lines are over-intense.

These are very clearcut cases. But let us remember the case of Sirius and of Vega, which, although often used as standards for the classifications, cannot be labelled unambiguously! ... We can therefore consider that the success of the two-dimensional classification has been indeed hiding its basic unadequacy ....

2.2. THE ABUNDANCES OF ELEMENTS, THE LINE FORMATION, AND THE MODEL ATMOSPHERES

Some of the difficulties described hereabove are obviously due to the dispersion in the various abundance-ratios.

Of course the first check to do, which would be quite convincing (could it be done without ambiguity), is the direct determination of abundances.

In a series of papers, qualified by some authors, in a regrettable way, as 'regrettable' (no quotation is necessary), several authors, including myself, have shown that, in the solar case, using a model deduced from the empirical analysis of the continuum data, the abundances of several metals are difficult to determine, a priori, from the classical LTE analysis of the curve of growth, or of individual lines. Important errors were the consequence of the LTE analysis. Similar results have been obtain since for the Sun, by De Jager and Neven (1967, 1968), and by Wijbenga and Zwaan (1972), for B stars, and helium, by Hearn (1970, 1971), for O–B stars and Mg, by Mihalas (1972).

We shall come back on some aspects of this discussion. Clearly, whatever is the interpretation of line intensities, abundances differences affect primarily the lines, and through the lines, spectral broad- or narrow-band analysis. Indices of metallicity can be deduced from different types of classification, used for different degrees of stellar brightness. The φ_b of the BCD classification is an index of metallicity; so is the m_1 index of the Strömgren classification; or again the index of the Barbier's method, recently developed by Morguleff and Gerbaldi, which is a combination of the GB (blue gradient 4010–4070 Å), D_B, Hβ photometric measures (Barbier, 1960; Barbier, Morguleff, 1964; Morguleff and Véron, 1970; Gerbaldi, 1972). It is not useful here to list the indices of metallicity that exist in the literature; but they are many....

Some authors consider that absolute abundances are not necessary in classification purposes, so long as we can determine (through such methods as differential curve of growth analysis) relative abundances from star to star. The comment we made about the BCD classification shows that differential analysis, as we have already said, can, at the most, be qualitative, as structures of models are affected by abundance determination. Moreover, other things can affect the models, and give place to spurious abundance differences, that are deduced only from some idea a priori about the similitude of the convenient models....

Therefore, we can as well consider the effects we have just listed as indicators of the fact that models of the usual way are not universal, and that we should worry about the two-dimensional classical grids of models. And of course, the first thing is to look for the effect of the abundances not only on the spectra, but also on the models themselves.

How the existence of a variety of abundances do influence the model atmospheres and, hence, the T_{eff}, g calibrations?

The elements other than hydrogen (and helium, in early-type stars) are more or less 'impurities'. They can influence the models through:

(i) the continuum opacity;

(ii) the line opacity ('blanketing effect').

2.2.1. *The Continuum of Elements Other than Hydrogen*

The discussion of the continuum opacity is possible in various parts of the HR diagram, where different studies have looked for the influence of the most significant continua, particularly in the UV part of the spectrum. As precisely most of the metallic continua affect only the UV spectrum, observational tests are difficult; the only possible tests are the somewhat doubtful line intensities analyses.

The following elements have been considered in some detail: He, He^+, C, Ne, Ne^+,

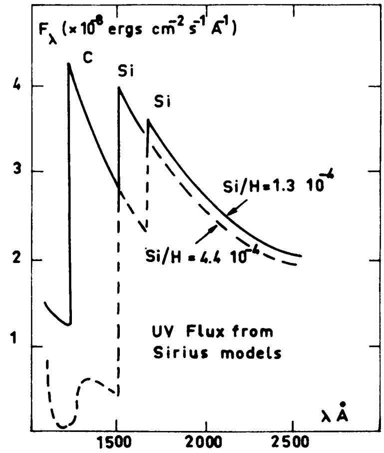

Fig. 12. (after Gros, 1972): The influence of Silicon abundance on the computed spectrum of Sirius in the UV.

Na, Mg, Al, Ca, Si. Other elements have been also introduced in the computations, with less accurate quantum-mechanical data. The Figure 12 shows a particular case where abundance differences has led to differences of spectrum in the wave-lengths where the responsible ions are not absorbing, according Gros.

We should note here that the models are to be criticized for many reasons (possibly different in each case), but, as they are more or less homogeneous series of models, the influence of metal continua on the spectral criteria is of significance.

Obviously, nothing more realistic can be said at this stage – except a strong warning: only a few elements have been properly considered, in many respects in a quite primitive way, and the only result we get is an order of magnitude of the effects that have to be added to effects of another nature (NLTE, for example), which may be much larger….

Another point to make at this point is the importance of space observations, in order to clear up the opacity sources that affect the UV, and the structure on the outer layers of the atmosphere which also affect the UV strongly.

2.2.2. *The Effect of Spectral Lines (Blanketing) on Model Atmospheres*

Irrespective of the abundance problem, the problem of blanketing is in itself a formidable problem.

Studies of the blanketing effect have been done several times in the case of the solar atmosphere, using the measured intensities of the lines in the observed spectrum as boundary conditions to the problem.

This question has been often obscured by a confused terminology. Actually, the effect of lines of all sorts is manifold. An historical semantical note is not, at this point, completely out of place. We shall at least outline such a note.

The first studies made about the blanketing effect considered only the 'pure absorption' lines, and in a still more restricted way, only those, amongst such lines, which were not linked with the adjacent continuum by some atomic transitions (lines which we shall call 'impurity lines' were thus the only ones to be considered really).

They conclude that the effect of introducing such lines in the spectrum has several effects (Figure 13):

(a) to *lower* the 'surface temperature' – (*the superficial cooling*), – and, correlatively (because the radiative flux has to be kept constant), to increase the temperature in the deeper layers, around the depths where the continuum is formed (*backwarming* effect).

(b) to *modify* the relation between any mean opacity coefficient $\bar{\varkappa}$ and $\varkappa_0$ (at $\lambda = 5000$ Å), in the sense that $\bar{\varkappa}$ is increased, and correlatively $\bar{\tau}$ also, at a given depth τ_0, or at a given geometrical height h.

(c) to *block* the energy in the lines (*blocking effect*): this is a consequence of the effect (b): the necessary, observed, decrease of the intensity in the lines has to be compensated, the total flux being well defined for a given model, by an increase somewhere in the continuum; this effect has to be expressed with much caution; the only possible base of reference for this increase being the continuum of a model atmosphere of the same T_{eff} as the model with lines, but this one computed without the lines….

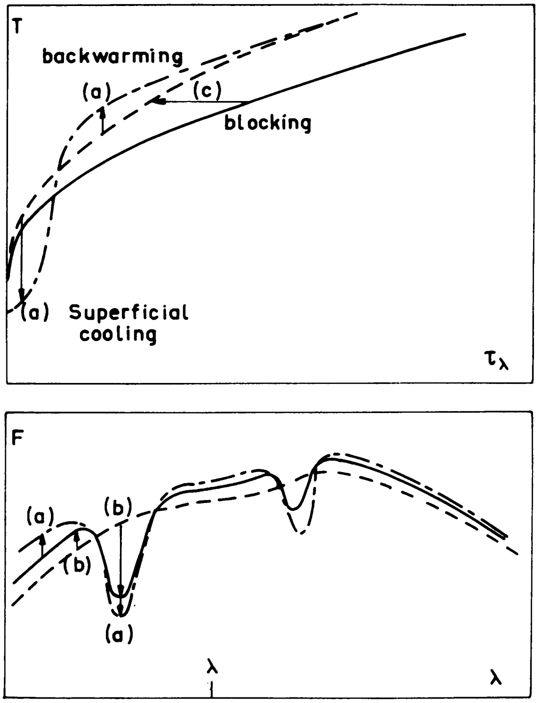

Fig. 13. The various aspects of the blanketing effects (schematic). Top full line: unblanketed model; dotted line: model blanketed by 'scattering' lines; mixed line: effect of 'pure absorption' lines. – Bottom: dotted line: spectrum computed without lines from unblanketed model; full line: actual spectrum, computed from unblanketed model; mixed line: flux computed from blanketed model.

The 'pure scattering' processes, in line formation (we continue here to use the classical terminology, how obsolete as it may appear now), do not take any energy from the radiation field; they just modify its angular distribution. Accordingly, the effect (a) is absent; no modification of the value of T_0, no backwarming either, is present. But the effect (b) (and as well the blocking effect) is present. It means that the relation $T(\bar{\tau})$ is not modified, but the relation $T(\tau_0)$ is modified, and therefore any expression $T(\tau_\lambda)$ of the model is indeed also modified; so is the spectrum $F(\lambda)$.

This has been discussed several times. Let us refer to the discussion by the author of one of the first exhaustive discussions along these lines. But neither has it been the first, nor the only one. Many authors have computed blanketed models of all sorts. Not only the discussion of the source-function in use is of importance; but also the way to schematize by a few typical-lines, the dozens of thousands lines that are to be taken into account: some authors use representations such as the symbolic 'picket-fence'; some use instead more realistic typical lines.

We shall not attempt here to review all these works. Let us quote only the early and classical result of Münch (1946) essentially identical to our schematical description, and the fine recent examples of blocking effect by Mihalas. Let us quote also the Strom-Kurucz results for Procyon where 30000 lines have been taken into account.

In the modern way of looking at the blanketing effect, we must clearly distinguish: (a) the impurity-lines cooling; (b) the continuum-coupled lines (or population cooling, according Thomas's terminology).

2.2.2.1. *Impurity lines.* We forget about the now obsolete use of 'absorption' vs 'scattering' lines. The general source-function is

$$S_\nu = \frac{\int_0^\infty I_\nu \varphi_\nu \,\mathrm{d}\nu + \varepsilon B_\nu + \eta B_\nu^*}{1 + \varepsilon + \eta} \tag{14}$$

as often discussed (Thomas and Athay, 1961; Thomas, 1965; Jefferies, 1968; Mihalas, 1970, to quote some of the more important reference-books).

Using such a formula, the treatment of the blanketing effect, insuring the conservation of the radiative flux, is possible and can be performed by modern methods of the transfer solution, such as Feautriers's – used by Dumont (1972) –, or Gebbie's and Thomas's (1971) – the TCB method, now in the process of being applied by its authors and F. Praderie. It gives, quite naturally, results that are somewhat intermediary between the two classical extreme cases.

The problems which faced the earlier workers in the field face now the present ones. It seems to us that the method of 'typical lines' (such as developed, in the earlier context, by Labs, 1951, or Pecker, 1951) is the only possible one to achieve realistic results. But now, it will be necessary to assign to each line of the actual spectrum a certain range of the parameters ε, η of the source-function, and of the intensity parameter $(gfAb_L\,\lambda)$. Clearly, it is far from easy!...

2.2.2.2. *The continuum-coupled lines (or population effects).* More important still is the importance (heavily emphasized by Thomas, in the principles, and used in computations by Auer and Mihalas (1972)) of the effect of the lines associated with the neighbouring continuum. We can understand easily that ionizations (by radiation or collision) are indeed coupling the electron temperature and the radiation field.

To describe the effect, let us schematize the opacity by the two levels + continuum atom. Then the spectrum has two continua and one line. One can compute a model M_1 by forgetting the opacity in the line. One can, at the contrary, take it into account: the effect of the line will be commanded by N_1, number of atoms in the level l, and is strongly coupled with both continua. The statistical equilibrium equations can be written, in the case with the line:

$$\begin{aligned} \frac{\mathrm{d}N_1}{\mathrm{d}t} = 0 = - N_1 \int_0^{\nu_1} J_\nu \alpha_\nu \,\mathrm{d}\nu - \underline{N_1 N_e C_{12}} - N_1 N_e C_{1K} + \\ + N^+ N_e C_{K1} + \underline{N_2 A_{21}} + \underline{N_2 N_e C_{21}} \end{aligned} \tag{15}$$

$$\frac{\mathrm{d}N_2}{\mathrm{d}t} = 0 = - N_2 \int_0^{\nu_2} J_\nu \alpha_\nu \,\mathrm{d}\nu - N_2 N_e C_{2K} - \underline{N_2 A_{21}} - \underline{N_2 N_e C_{21}} + \tag{16}$$

$$+ N^+ N_e C_{K2} + \underline{N_1 N_e C_{12}}$$

$$N^+ = N_e . \tag{17}$$

In the simplified case (without the line), the underlined terms, corresponding to collision – and radiation – induced transitions between levels 1 and 2 are vanishing, the line being absent. Without entering in the details, the three equations above give the ratios N_1/N_e, N_2/N_e and the value of N_e; they give very different results in both cases: clearly, in the case without-line, the level 2 is strongly overpopulated, the dominant term $A_{21}N_2$ being uneffective. Then the $L\alpha$ line, when taken into account, has for direct effect to decrease the opacity in the adjacent continuum (Balmer continuum), and therefore to increase the flux in the same Balmer continuum, for a given value of T_{eff}, as well as the temperature in the layers of formation of this continuum.

The various examples given by Auer and Mihalas (1972) or Mihalas (1971) show that the effect of lines is great on a model; it influences the model in rather deep layers: in deeper layers if Hα is considered alone than if $L\alpha$ is considered alone; and in opposite direction (in NLTE) as easily understood when looking for the effect of various levels ($n=2$ is primarily a heating continuum, whenever $n=3$ cools – see Mihalas, 1971). Would all lines be considered, it is likely that even deeper layers would be influenced and spectral criteria calculated from the model would be strongly affected. A detailed study, which is still in the beginning, of the spectral criteria that are the less sensitive to these effects would be very valuable.

2.2.2.3. *The effect of different abundances; abundance determinations.* According the two preceding paragraphs, it appears clearly that, both through the influence of continuum absorption, and through the influence of spectral lines, the chemical composition affects definitely the model atmospheres, and leads to different computed spectra, according the composition. At this point, we should warn again both observers and theoreticians against the common error of using differential curve-of-growth analysis to have a hint on to which kind of chemical composition should be used in the 'modeling' of a given stellar atmosphere.

We should here state that the differential curve of growth method is highly qualitative when we have to compare stars that are considered as normal, to stars that are considered as abnormal, and precisely because of their 'abnormality'. The abnormality may be, or may not be, a matter of chemical composition; when it is the case, the abnormal composition may have distorted completely the model and led to absurd determinations, the temperature gradients coming explicitely in the curve of growth analysis. In addition, NLTE effects, as we have said, may be very important.

A very good example of this is given in the case of the hot stars, by Mihalas, for several lines, H-lines, or Mg II lines ... or for features of the continuum such as those noted by Mihalas or by Maeder (1971). Let us note the fact that a correct NLTE treatment forces the apparent variation of Mg abundance with ST to appear as spurious (Mihalas, 1972)!

Another effect of various abundances is that of the ratio He/H, which affects strongly the continuum. The Figure 14, derived from the tables of Klinglesmith (1971) (quite criticable actually in many ways, but at least sufficiently extensive and homogeneous for tests of that sort) shows that the models with different H/He ratio would give place to different evaluations of T_{eff} and g.

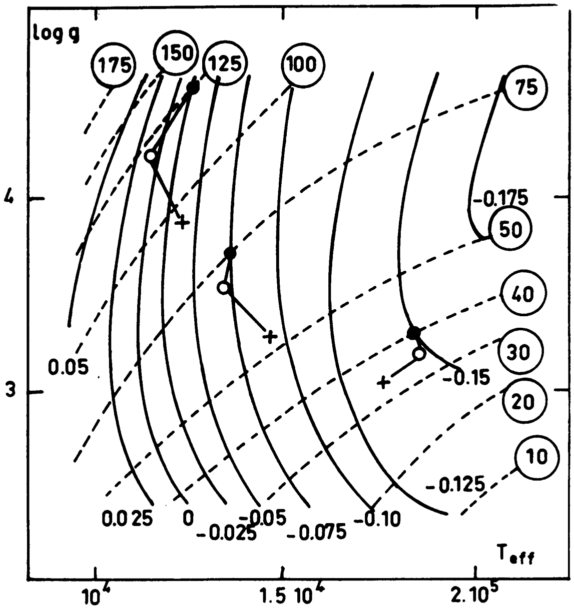

Fig. 14. The two-dimensional analysis as a function of helium content (after the computed spectra of Kinglesmith, 1971). The curves represent, for the abundances $X = 1$, $Y = 0$, the iso-Hγ (dotted lines, labeled in tenths of ångströms) and the iso-$(B - V)$ (full lines) labelled in magnitudes. The open circles correspond to the same determination made with $X = 0.67$; $Y = 0.33$; and the crosses to $X = 0.143$; $Y = 0.857$. One sees the influence of the assumed abundances on the $T_{\text{eff}} - g$ determination (compare with Figures 10 and 11).

But, a long time ago already, Underhill has insisted on the fact that apparent chemical composition differences (such as between WC and WN stars) are spurious and due only, or at least mostly, to structure differences. Similar views have been expressed often in the case of Am and Ap stars, as well as in the case of some late-type stars.

At this stage, we do not need to say more, although the subject is immensely large and still far from being explored in any satisfactory way.

2.3. VARIOUS SOURCES OF UNADEQUACY OF MODELS

Evidence for unadequacy of models has to come, in any particular case, from the fact that a model atmosphere made to fit some, or most, of the observed features, does not fit them all.

Apart from the abundance problem (for which we can at least adjust the abundance parameters so that to influence properly the models and the computed spectrum, lines included, and to fit the observations for a few lines of each significant element) we must mention that many kinds of uncertainties in the line source-function are leading to severe difficulties. Even if we treat properly the various levels of a given element, a Fe abundance (for example) made to fit the Fe I lines, does not fit the Fe II lines, or even does not fit all Fe I lines. A few examples are given by Auer and Mihalas (1972) and Mihalas (1971), and concern He I and He II lines.

Without being able here to do anything else but to list a few additional difficulties related to line formation, we can mention the following facts, more or less well-known, and often, if not always, neglected in the model building.

2.3.1. *Fluorescence Phenomena*

Fortuitous wavelength coincidence in the spectrum of two elements couples strongly their spectrum. When elements of small abundance are involved, it modifies their spectra, but does not affect too much the models. But if the lines in question are used as criteria to adjust a model to an observed spectrum, this may lead to severe errors.

The Figure 15 shows the case of He lines when one does take into account the overlap between $n_1 - n_2$ lines of hydrogen, and $2n_1 - 2n_2$ lines of ionized helium He II. For example λ1640 He II (2–3) weakens because Lα (H, 1–2), pumps He II (2–4) and the cascade 4–3–2 brings extra emission in the He line. The Pickering lines, at the contrary ($n=4$) strengthen, because of the pumping to the 4th level. Of particular interest is the behaviour of the near-IR line 10124.

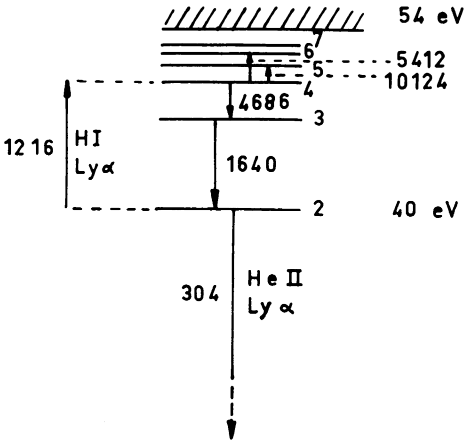

Fig. 15. The pumping of the He II transition 2–4 by the Lα line.

Let us note here that the use of He II 4686 in stellar classification (it is actually used for O and Wolf-Rayet stars) might be misleading, if models do not take into account in a proper way the fluorescent effect we mention here, an effect which, although not enormous, is far from negligible.

2.3.2. *Magnetic Fields*

In lines of high Landé factor, a magnetic field induces a strong separation of the Zeeman components, and therefore reduces the saturation effect, giving place to the enhancement of total intensities of the involved lines.

Once such effect is suspected, it is of course possible to determine the magnetic field, although, even in the solar case, this operation is far from accurate, unless we can use the polarized-profiles analysis. And anyway, this will be quite unsufficient: we must indeed take the line splitting into account when solving the transfer problem – which we hardly know how to do well, except by very elaborate methods. The solution of the problem is made still more difficult by the fact that magnetic fields can vary on the stellar surface.

2.3.3. *Large Scale Doppler Broadening of Lines*

Just as the magnetic broadening enhances some lines, large scale velocity fields (rotation, convective motions, expansion) do affect spectral lines. They not affect in the first order the total intensity of the line: the splitting occurs after the transfer in the atmosphere. But they do affect definitely the profiles – hence the transfer solution, hence the model. So when a model does fit intensities, but cannot fit the profiles unless a large-scale velocity field is assumed, one should start to worry about the unadequacy of the model itself, which does not say indeed anything about how to predict the velocity field, but which depends upon it (this effect has been discussed in Pecker and Thomas, 1961).

2.3.4. *Small Scale Doppler (Non-Thermal) Broadening of the Lines*

There, even the total intensity of the lines is affected; but the profile also, as well known. The errors made on the model itself, by assuming non-thermal velocity field at all, or some unrealistic one, might still be large, and intervene, as in the three preceding case, through the modification of the first term of the numerator of the source-function S_ν (formula 14) through the blanketing effect calculations. It is too easy (in both cases (2.3.3.) and (2.3.4.) to fit observations by 'ad hoc' velocities fields which are just a 'deus ex machina', which no physical theory can predict in a correct way.

2.3.5. *Envelopes and Shells*

This problem seems to me so important as to need a complete paragraph of this section of our report, mostly because its intervention in the thinking about models is recent (not because the effect will be larger than the other sources of uncertainty above quoted in 2.3./1.–4.!).

2.4. ENVELOPES OR SHELL FEATURES; THEIR INFLUENCE ON MODEL BUILDING

Whenever a shell is suspected, through the appearance in the spectrum of emission lines – or, at an higher dispersion, of strangely shaped profiles – the model maker should start to seriously worry.

The first idea is that some of the features of the spectra are, in a way, superimposed to those of a *normal* spectrum. Therefore, one is temptated to correct for the emission features (emission cores of lines) and to give as 'undisturbed spectrum' a substracted spectrum more or less interpolated and supposedly well represented by a classical model atmosphere.

The fact that such an attitude is generally not badly justified has been proved in the case of several Be star analyses, such as those from Burbidge and Burbidge (1953), amongst the pioneers, till, more recently, those from Delplace, Doazan and Briot, without mentioning the many other similar analyses (Figure 16).

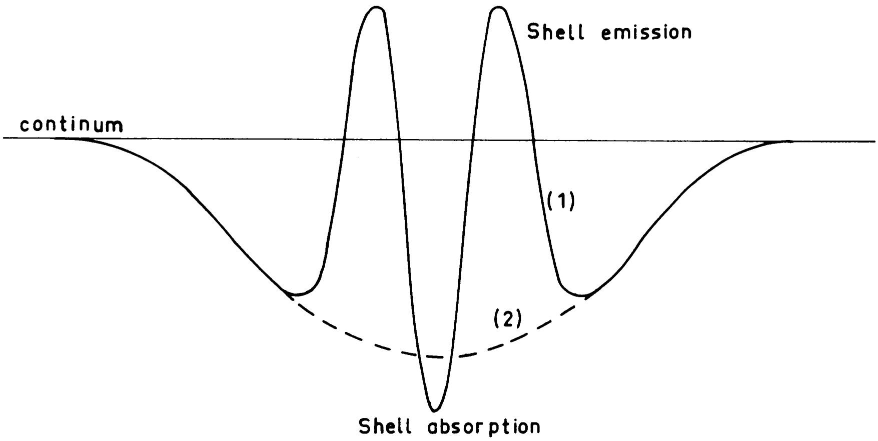

Fig. 16. A typical shell-feature in an early-type star spectrum.

Unfortunately, this happy circumstance is not the happy end.

On one hand, some apparent 'dilution' effects are due only to a bad thermodynamical treatment, and more precisely to NLTE over populating ground levels. We have to keep in mind that the spectrum is an image of the variation of the source-function S with some τ, not with h; the relation $\tau(h)$ comes finaly from theoretical considerations except in very rare cases (some eclipsing binaries, for example).

On the other side, these doubts do not hide the real fact that the stars (the Be stars are a good case) are indeed surrounded by a complex envelope of dust and gas, of which the observable effects are manifold:

(a) in the visible spectrum, not only the emission features are visible, but the star is underluminous, when it is possible to determine its luminosity.

(b) this under luminosity may exist, even though there is no visible emission lines.

The emission features may appear in the UV and this just means that the chromospheric regions may have different behaviours (according the chromosphere thickness, possibly).

(c) in the IR, the envelope may strongly radiate, either through dust black-body radiation, or through free-free emission of the ionized gas of the H I and the H II region surrounding the star (Pecker, 1971; Dyck and Milkey, 1972).

The physical properties of the envelope will be discussed later on. At this stage, we may wonder how the diagnostic of such stars may be affected especially when keeping in mind that, after all, Sirius and Vega, although they are not showing conspicuous emission profiles, *may* have (at least Vega) extended absorbing atmospheres, and the fact that the Sun itself has an extended atmosphere which cannot be readily forgotten, as it includes our Earth itself...!

A very interesting particular case (quite similar, seemingly, to S Monocerotis), is that of HD 45677, which has been observed very extensively, and to which we shall devote the forthcoming paragraph.

2.5. THE CASE OF HD 45677. DIAGNOSTIC OF EARLY-TYPE STARS

The star is well known as a variable Be star, with a strong IR excess, as observed by Low *et al.* (1970), or by Allen and Swings (1971).

We shall assume that the IR excess is due to a thin spherical dust shell; arguments in favor of such an interpretation have been given elsewhere (Pecker, 1971), but we should keep in mind that arguments in favor of the interpretation by the free-free radiation of H^- have been recently given in similar cases (Dyck and Milkey, 1972) – a controversy which shows us immediately the feeling that the diagnostic is far from obvious.

Anyway, the IR observations of HD 45677 can be interpreted as the radiation of a shell of $T_2 = 750°$; $R_2 = 600\ R_*$, $\tau_{2,\mathrm{IR}} = 0.34$; $\tau_{2,\mathrm{vis}} = 4$ (which corresponds roughly to $A = 2.5 \log e^4 = 4.3$ mag. of visible extinction; we have assumed a $1/\lambda$ extinction law).

The bolometric correction, thus reaching several magnitudes, is enormous, and would not have been guessed, knowing only the visible spectrum.

The many consequences of this conclusion are not to be described here; let us only remember that, obviously, absolute magnitude criteria have not much meaning in this context....

But we cannot escape the conclusions that many early-type stars are surrounded by shells, envelopes, and the like. When we have only under hands the visible information, some important data are lacking.

The absolute magnitude of O stars have been calibrated by several authors, and quite recently, by Burnichon, (1972) who has reviewed other work on this question (Figure 17). The results are indeed distressing (see also Blaauw, 1963). The use of different samplings, of different criteria, is obviously the main cause of the dispersion; and no method is really protected against systematic errors.

The method used by Burnichon is certainly amongst the most reliable. She considers only O stars belonging to multiple systems in which stars of other types are present.

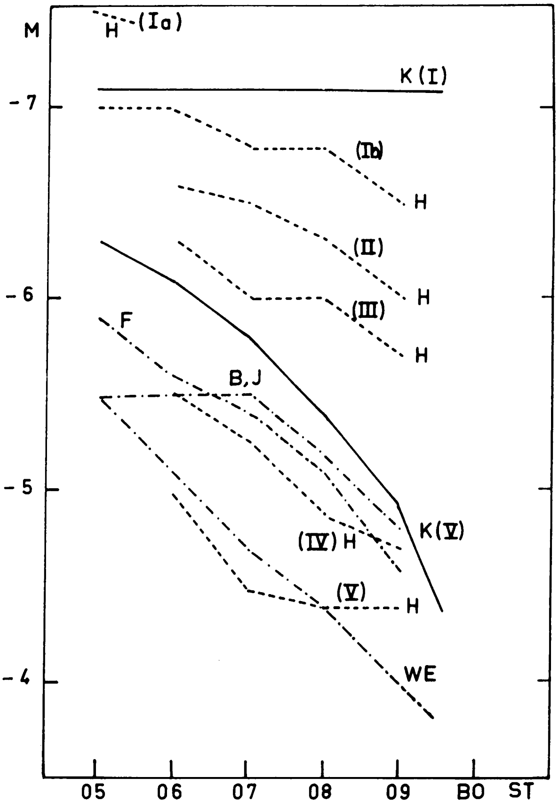

Fig. 17. (after Burnichon, 1972) – (a) The scale of absolute magnitude, according various authors, for O–B stars. The curves refer to: (K) Kopylov (1955); (H) Hack (1963); (F) Fitzgerald (1969); (B) Blaauw (1963); (J) Johnson and Iriarte (1958); (WE) Weaver-Ebert (1964); they are also labeled by the LC, in roman numbers. – (b) Comparison of the absolute magnitudes obtained by Burnichon (1972), with those obtained by other authors: (K) Kopylov (1958); (CA), Conti and Altschuler, (1972); (U) Underhill (1955).

The latter are calibrated, using well established absolute magnitude criteria (from the BCD classification of stars of type AFG). However a strong doubt still exists: are the AFG used in the calibration of the O stars under study really well calibrated themselves? Their belonging to a multiple system (which is rather young, as proved by the existence in it of an O star) might have affected the M_v – ST relation used for their study.

Apart from the M_v – ST relation, another source of undetermination of the luminosities is the bolometric correction *BC*. Clearly, it is always to be strongly put under question in the case of Be and Oe stars, – as shown by the example of HD 45677.

This at least assumed that the relation T_{eff} – ST is well determined; but it is indeed just as bad as the M_v – ST calibration, as shown by the use of several sets of models (Figure 18).

Truly enough, this state of affairs is usually considered as peculiar to either the

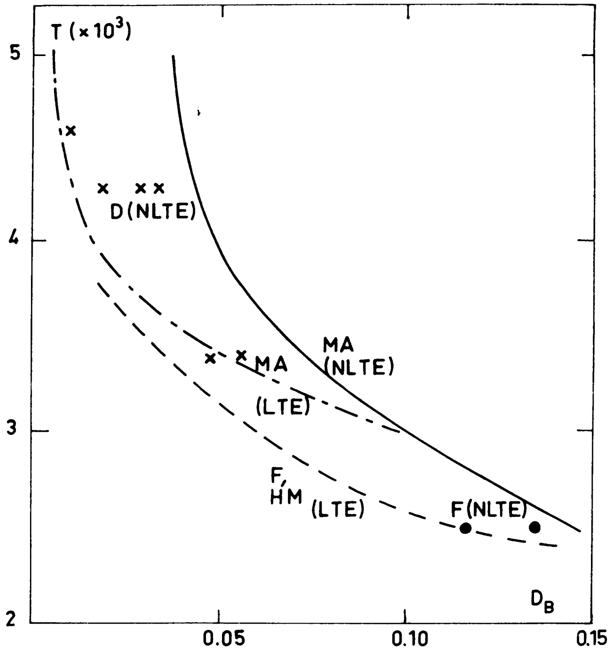

Fig. 18. (after Burnichon) – The $T_{\mathrm{eff}} - D_B$ relation, from model atmospheres, in the O–B region. (MA): Mihalas and Auer (1970); (F) Feautrier (1967 (LTE), 1968 (NLTE)); (HM) Hickok and Morton (1968); (D) Dumont (1972).

early-type stars, or, for similar reasons, to some late-type stars, such as T Tau stars. But the case of Vega, and possibly Sirius, has shown us that indeed we should carry our worries all along the spectral sequence.

2.6. Various unexplained spectral features

We have quoted the IR excesses observed for early-type stars; we have quoted the UV features observed for Sirius.... There are stars with UV excesses, in number... All of these features are generally unexplained, at least quantitatively (and we cannot be satisfied by any qualitative interpretation, which may be quite unconsistent...) The case of the star BD$^+$ 39° 4926, which has a very large $D_B(=0.71)$ as shown by Divan (1963), and confirmed later by several authors, certainly does not fit the usual calibration pattern.

Of a different nature are the discrepancies due to the unidentified absorption features of many spectra. Amongst the most conspicuous of them is the discontinuity at 4800 Å, discovered by Berger *et al.* (1956), which seems to prove the existence of some free-bound absorption increasingly intense for increasingly hot stars. This feature may be the origin of some difficulties encountered in fitting the continuum spectrum by model atmospheres (Krikorian, 1972; Figure 19). Some unsuccessful attempts to reproduce this 4800 Å discontinuity have been made (Van Regemorter, 1959), but there is no need to study them in detail now.

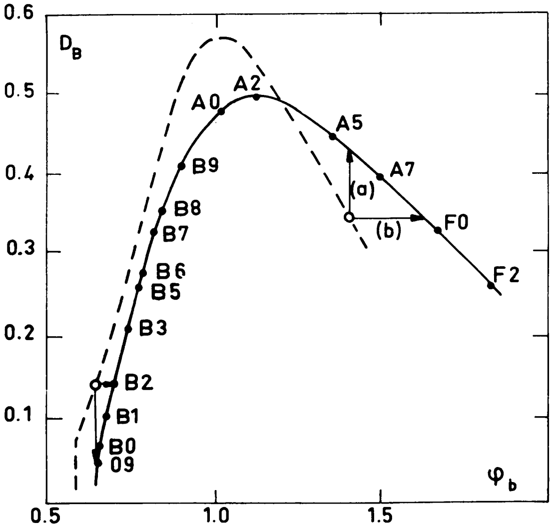

Fig. 19. (after Krikorian, 1972). The $D_B - \varphi b$ relation for LC V stars, according model atmospheres (dotted line) and the BCD calibration (full line). One sees that, whether one used one or the other of the two parameters for the ST determination, errors of a few tenths of spectral type can be made.

All this section is meant to say that models, arranged in two-dimensional grids, how well made they may be, are indeed unadequate. We must now look, in the present state of the use of models for calibration purposes, on the prospective aspects of this question. This will occupy the last section of this report.

3. The Present State of the Model Factory

3.1. THE 'CLASSICAL' MODELS

We shall not list here the various numerical aspects of the model making business. I shall send back the reader to my paper of 1965, in *Annual Reviews*, in which is described the essential of the methodology of non-grey models, and to Mihalas's book: essentially these references concern practically only RE models, which is a severe restriction.

Neither shall I try to review all models that have been built since that time: many have been done; some are excellent... I shall only refer to the papers of Mihalas and associates, mostly for early-type stars, of Vardya for late-type stars, where some references can be found. I would like to attract the attention, at this occasion, to the fact that a complete *tabulation* of models is a somewhat obsolete procedure (may I say 'unfortunately'?), the reasons being (a) the great number of parameters that can be introduced, (b) the fact that computers are available in many places, enabling scientists to use not tabulated models but borrowed programs. Therefore, one refers only to models 'built with the Feautrier's program' or 'with the Mihalas's program', without never seing these models actually published. It does not help the one who is supposed to give a clear account of the use of models and their validity!...

Instead, I shall try to describe, more or less according a paper now under completion by Pecker *et al.* (1973), the necessary evolution of point of view which should dominate, in the years to come, the whole approach to stellar atmospheres (see Thomas, 1970), going probably as far as to eliminate the concept of grid of models, the number of parameters becoming really excessively large for allowing us to apply simple interpolation methods in order to derive a model for a given observed star.

Let us first agree on one semantical definition. We shall call *classical* a model atmosphere which essentially satisfies a few equilibrium equations, and depends upon a few parameters. Needless to say, there is no more question to use black-bodies, or even grey models, although this is still often done by observers who have not paid enough attention to the evolution of the problem in the last thirty years.

Actually, the first non-grey models, very rough indeed, but rather stimulating, still now, in the sense that their methodology is still able to teach us a lot about the atmospheric structures, were due to Rudkjøbing (1947) and Mustel (1940). Their methods could not been applied to any realistic opacity law; but the Strömgren's method was on its way (Pecker, 1950, 1951) and its first applications was indeed the beginning of a new era: grid of grey models are not any more acceptable, since we know how to build non-grey ones. Let us here note that grey models have a tendency to give always a value of T_{eff} too low for a given spectral type, compared to the non-grey models. The new era, however, could not be effective (in view of the numerical complexity of the problem) before the availability of large computers; nowadays, non-grey models incorporate blanketing effect – at least in a simplified way –, and are the basis of the interpretation of stellar spectra, and of the calibration in T_{eff} and g of the ST and LC scales.

The difficulties are still however far from being removed; but, essentially, they are no more of a numerical nature, but much more on a physical, or even conceptual nature.

The models that can be now (September 1972) considered as 'classical' are:

– *In radiative equilibrium (RE).* It implies that T_{eff} is given, and $F=(\sigma/\pi)\,T_{\mathrm{eff}}^{\,4}$ is constant throughout the atmosphere. The chemical composition can be modified, according the wishes of the author; the opacity introduced in the models can be any opacity thought to be suitable; it can take line opacity into account. The only restriction is that we have to treat the opacity in accordance with the other hypotheses of the models.

– *In hydrostatic equilibrium (HE).* It implies a given value of g_{eff}, constant in the atmosphere, and the validity of equation (2). Note that g_{eff} may be different from g, because of turbulence, or other mechanisms able to transfer momentum: we mean only that p varies in the atmosphere according the Equation (2), where g_{eff} is a constant.

– *In plane parallel geometry (PP)*

– *In local thermodynamical equilibrium (LTE)*, where the populations of all atomic levels – and hence opacities – strictly follow the laws of Boltzmann and Saha.

– *Without any magnetic field, or velocity fields of systematic nature* (i.e. macro-velocity fields: expansion, rotation, convection).

Fortunately, the meaning of the word 'classical' is quickly changing.

Nowadays, the hypothesis LTE, at least in the continuous opacity computations, is not any more necessary, since the work of Feautrier (1968), Auer and Mihalas (1972), and others. The blanketing account has even been taken into account in model building – but generally not in a complete way and almost always in LTE: it is either the impurity blanketing (such as the models of Bradley and Morton (1969), or Van Citters and Morton (1970), who take into account UV lines) – or the blanketing by lines coupled to the continuum (such as the models of Auer and Mihalas, 1972), where NLTE are essential. Sometimes, models include both NLTE effects and lines. The papers by Auer and Mihalas (1972) and Mihalas (1971) show the influence of these improvements in the models, and the quality of the new models when compared to the observations. Particularly illustrative is the case of Mg II lines in early-type stars. It is remarkable to note that NLTE, which was still heresy, less than ten years ago, seems now of such a quantitative importance...

The PP hypothesis has been also more or less overcome: Hummer and Rybicki, Cassinelli (1971), Cassinelli and Hummer (1971), Lucy and others, have introduced ways of computing models in spherical symmetry, and even with laws different from HE (laws such as $\varrho \propto r^{-n}$, applicable to some extended atmospheres). Similarly, the transfert in stochastic media is reaching a state where it will soon be possible to use it in atmosphere making. Maeder and Peytremann (1970) have considered rotating models where T_{eff} is a function of the latitude. Etc....

Unfortunately, such improvements bring in so many numerical complexities, that it seems so far impossible to take them into account and, at the same time, to keep realistic opacity laws.

Very little effort has been done to compute models departing from RE. Convective zones of first approximation (mixing-length theory) have been used for intermediate or late-type stars. But a really hydrodynamically consistent solution, although not far, possibly, from completion (Spiegel, Lacour, Zahn, see Souffrin (1971)), has not been performed so as to obtain realistic models. Leibacher's approach can be also taken into serious consideration whenever propagating waves may change significantly the structure of the atmosphere. But again, we are at the beginning of a long road...

Similarly, the introduction of magnetic fields, or velocity fields, has given place to interesting solutions of the transfer problems (let us quote the recent works by Simonneau and by Magnan, amongst many others); but this does not go far enough as to affect the models.

On the other hand, even the 'classical' models are not, within this very limited concept, very satisfactory in many parts of the HR diagram, because of an inadequate treatment of the opacity sources. The molecular lines and continua, the stable ions, etc...., are hardly going out of the shadow put on them by the uncertainties in molecular, atomic, ionic energy transitions physics, or by the formidable numerical work associated with a correct treatment, even in the classical frame (see Tsuji, Vardya, etc. ...).

3.2. New concepts in the description of a stellar atmosphere

In spite, or because, of the large drawbacks and small improvements of the classical theory of model atmospheres, there is nowadays a strong movement towards a new look at a stellar atmosphere. Whenever the classical atmosphere is more or less a boundary (rather sharp) between the star and the vacuum outside, we have now a tendency to consider it, as, *in essence*, a transition region between the internal parts of the star and the interstellar medium, a region which may be as extended indeed as to cover the space between the deepest part in the star from where photons can escape directly to the observer, to the place where atoms (or grains) do not know anymore to which star they belong.

In this sense, the atmosphere is the region which covers not only the classical atmosphere ('photosphere') but also the various kinds of extensions or envelopes.

Many things have to be said about the new concepts. We shall only try to abstract them here, as they will be described in more details in the series of papers to come (Gebbie, Pecker, Praderie, Thomas and others) and in the report of the President of Commission 36 of the IAU (Thomas, 1973).

The interstellar medium is characterized by the existence, locally, of a great many degrees of freedom, a state indeed of complete degeneracy. That is: we cannot describe the state of the medium by only its temperature, or by only the temperature and the radiation field.... Many other parameters (local, or not) are indeed necessary!

At the contrary, the internal parts of a star are completely degenerated; the temperature fixes every other property of the medium, except the density (they are not coupled each to the other): We have a black body in TE.

When going from inside to outside, the degeneracies progressively disappear. Radiation escapes, and is no more in equilibrium with the local properties of the medium, being somewhat diluted; this however is still in TE, as collisions dominate, and as, therefore, the local temperature commands the population of atomic energy levels; the Boltzmann, Saha, Maxwell laws are still valid – not any more the Planck law. We shall call this region the *lower photosphere*. The escape of photons is the major physical phenomenon which creates the model.

When proceeding outside the photosphere, the Boltzmann-Saha laws starts to be no more valid. NLTE effects start to prevail. But, still, the escape of photons is the way the thermodynamical structure of the atmosphere is built out. We shall call this region the '*upper photosphere*' essentially in RE (see Pecker and Thomas (1961) as one of the first papers where is described this behaviour).

When going still outside, the flux of photons is not any more the essential phenomena. Most of the photons truly enough do escape, and are only partly decoupled from the transparent medium (the opacity has decreased, the photons travel with less interaction with the medium). But we start to depart from RE; the mechanical energy plays an important part, but the density is still high enough for the flux of mechanical energy (waves) to interact with the medium. We have the '*chromosphere*'.

And we meet next the '*corona*' where definition of the local conditions is requesting the solution of energy equations taking solar wind into account.

There the magnetic conditions become predominantly important and command the flow of matter and the energy balance. Various regions can be found in the corona, according the variation of temperature; the extension of this region is leading to a decrease of the photon density, according a law in r^{-2}, of mass density in the solar wind, according a law in $r^{-2n} (n < 1)$, but to a still smaller decrease in the magnetic field (in $r^{-2n} (n < 1)$), the difference between n and 1 being essentially due to the geometry of magnetic lines of forces.

We have so far made a distinction of various regions in term of energy balance and mean free path. It affects the distribution of T and ϱ with some optical depth in the deep region, with geometrical depth in the outer regions.

We must also introduce a distinction between regions in term of momentum balance. There the pressure terms come in the picture, and so the HE hypothesis. In the deepest parts of the atmosphere, the momentum transfer is local, and the HE is prevailing, commanding the density structure, whenever the RE commands the temperature structure. When going outside, the momentum transfer changes its character, due to the mean free path of material particles. About where the NLTE or NRE effects starts to prevail, the HE starts to fail (this coincidence being actually fortuitous and certainly not general). According the case, and the physical processes which dominates in the hydrodynamical (and still outside, hydromagnetical) equations, the density structure may have different behaviours. Whereas the spectrum (made out of photons) is an image of the variation of the structure (through various source-functions) with optical depth, no diagnosis (except for objects of non-negligible apparent size, using eventually interferometric Michelson-type devices, or for such objects as eclipsed stars – either by a companion, or by the Moon's limb, or else) gives the variation of density with h, or of any optical depth with h, something that in general, only a purely theoretical (i.e. through a theoretical diagnosis of the observations) treatment can obtain. We have therefore appearances ('symbiotic' spectra, shell spectra, extended envelopes spectra) of which the spectral characteristics are often well described, but do not necessarily fit a conceptual definition of the words used for this description. For commodity purposes, with no certainty of fitting the usual meaning of the words in their purely spectroscopical definition, we shall use the word '*extended atmosphere*', whenever HE does not hold anymore, '*shell*', whenever there is even an increase of the total density outwards, somewhere, '*dust-shell*', or 'H I-*shell*', or else, whenever only one of the various components of the circumstellar matter undergoes a neat increase at some distance of the star; '*envelope*' whenever the density is very near the interstellar density, but the physical conditions are nevertheless still affected by the vicinity of the star (examples: the H II region, the C II region, are essentially envelopes).

The Figure 20 gives a schematical description of our terminology. We send back the reader to Pecker-Thomas (1961), to the Münich Colloquium (papers by Thomas (1969) and Pecker (1969)), to the Menzel jubilee symposium (Thomas), and to the forthcoming series of papers by Gebbie, Praderie, Pecker and Thomas, for further

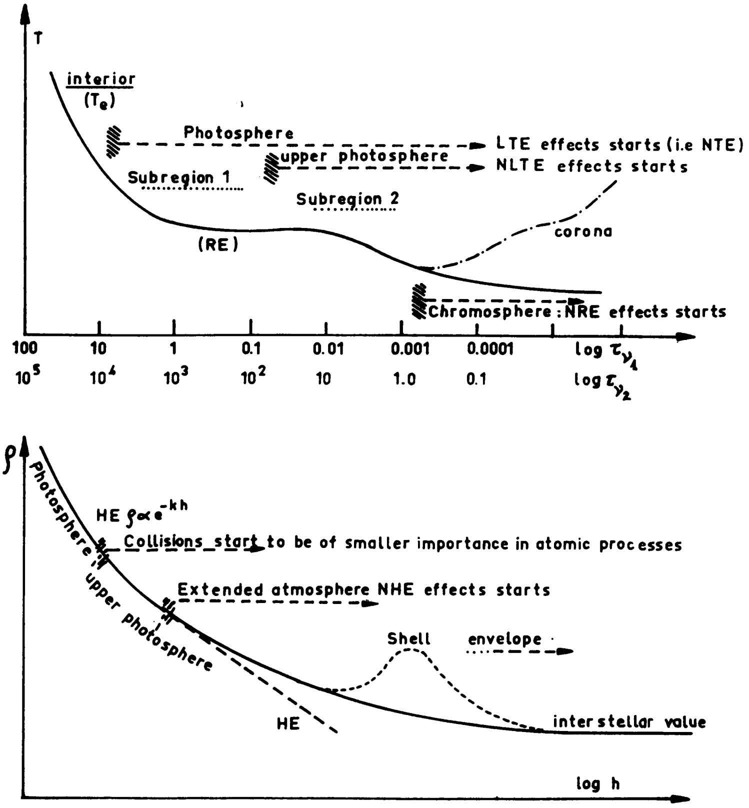

Fig. 20. Terminology of the theory of stellar atmospheres (according concepts developed by Pecker *et al.*, 1973). The frequency ν_1 corresponds to the more transparent part of the continuum spectrum, ν_2 to the more opaque.

details and a better physical insight concerning the phenomena affecting each part of the atmosphere – each 'region' – and even the 'subregions' (defined, within each region, for example the photosphere, by the various sources of opacity which affect predominantly a part – subregion – or another of the photosphere: for example there is a photospheric subregion defined by the region affected by the escape of Lyman continuum photons, another one defined by the escape of the Balmer continuum photons, – and the like).

3.3. NEW APPROACHES TO MODEL MAKING

This new type of concepts obviously influences the methods to apply to build models. And we cannot, at the present time, conceive of a completely self-consistent method of solution; obviously, we can treat some problems such as the transfer of radiation, whenever we are at the infancy for hydrodynamical transfer, and still at the foetal state for the hydromagnetical transfer...

So far as the radiation equilibrium condition is the only one concerned, we need, to start with, a geometry, a chemical composition, a value for the flux of radiative energy, and a law for the momentum transfer.

To express the RE, it is customary to impose to the source-function the classical conservation energy:

$$\int_0^\infty \varkappa_\nu [J_\nu - S_\nu] \, d\nu = 0 \quad \text{at all depths}. \tag{18}$$

As S_ν is related to T_e, we can write this in some explicit way – although this is not quite easy; essentially it can be written as follows:

$$T_e = \phi\,(T_e, \text{etc.} \ldots) \tag{19}$$

an expression which implies some kind of iterative procedure, the first member corresponding to the nth approximation, the second to the $(n-1)$th approximation.

It is essentially what has been done early by Strömgren, in a very slowly converging method, more recently by Gebbie and Thomas, in the very far reaching so-called TCB method, where the various sources of opacities intervene in successive terms, and can be introduced progressively when proceeding from the state of degeneracy of the internal layers to the outer layers; the Gebbie-Thomas procedure has the advantage of giving a physical way to improve any provisional solution taking into account only some components of the opacity, and to, accordingly, reach better solutions. For example, the introduction of the blanketing effect can be more easy in their method than in any other. Essentially, the method groups the terms in isolating in each of them a 'TCB' (temperature controlled bracket).

We do not need here, and do not want, to enter in the details of the physical meaning of this procedure. We want only to note that the failure of the Strömgren's method was partly due to the fact that in the solution of Equation (19), the second member is strongly depending upon the values of T_e and ϱ; whenever, essentially, in the TCB method the interaction works much more quickly, as the second member depends much more slightly on these parameters. This seems essentially due to the fact that the interaction procedure operates on a smaller part of the integrals of Equation (18). It actually subtracts out from Equation (19) large terms, equal in both members, which therefore do not affect the constancy of the flux, concentrating on the small, unequal terms, which, at a given depth, fix T_e.

The method converges so quickly, and is so easy to apply, that it can be handled with a reasonable accuracy by desk computers or even slide rules at least on simple problems.

But this does not yet solve either the HD problems, or the MHD...

At least, we should now comment on the fact that all parts of the atmosphere, all regions and subregions, are indeed intimately coupled each to the other, and therefore one is badly in error when treating a star as isolated from the outer regions, shells or envelopes, or extended regions, which necessarily surrounds it.

This coupling, in a way, has been known for a very long time. Two sub-regions influence each other, and give non-grey models different from grey models; the blanketing effect affects deep layers where line absorption may be very small, etc....

But we must note also that an envelope, or a shell, often acts as a reflecting wall, or semi-reflecting – semi-transparent wall. An H II – H I transition region, for example, is an H I wall; and the diffuse Ly C radiation inside an H II region is similar to that of a black-body of about 10^4 K. This radiation falls back to the star, and the usual boundary condition $I(\cos\theta<0)=0$ cannot be readily applied to a stellar photosphere. The whole thing, photosphere and envelope, has to be treated as a whole, in the same numerical procedure, because of the existence of such couplings.

Similarly, the fact that interstellar medium surrounds a star breaks the HE hypothesis quite quickly. For example, if $n_{\text{interst.}}=10^2$, if we assume the density low in the photosphere is following the HE law, one has:

$$n = n_0 e^{-Kh}, \tag{20}$$

where $n_0=10^{15}$ for $\tau\cong 1$, and $h=0$ (typical values).

Then, $n=10^2$ for $e^{-Kh}=10^{-13}$ or $Kh=30$. This is indeed not so far from the photosphere... And if we assume the Balmer continuum formed near $n=n_0$, the Lyman continuum may well be formed not far from the $n=100$ limit... a fact which changes strongly the transfer solution itself in the Lyman continuum. A typical example of this is the H II – H I transition region, where HE has completely ceased to be valid, and which corresponds roughly to the region where the Lyman photon escapes from.

That kind of effect can change considerably the classical models – as shown by Mihalas in preliminary computations dealing, truly enough, with extreme cases, but nevertheless rather demonstrative (Mihalas, 1972).

A soon as that kind of effects need to be taken into account, curvature has also to come in the picture; and a lot of usual approximation (the classical Eddington approximation $3K=J$ for example) are no more valid (we have to use $K=J$ instead): their use in the computing procedures might be very misleading.

We can therefore conceive of the computation procedure of a model in the following way:

(i) compute a (T_{eff}, g, classical composition) – classical photosphere;

(ii) use this first-approximation model to redetermine (out of LTE) new abundances, improved opacities..., and new T_e values;

(iii) compute the limitations of this model: where is the 'upper photosphere' starting, where is the 'chromosphere' starting... and, accordingly, treat properly, above the appropriate layers, the photon transport, and the mechanical transport.

(iv) at each of these successive points, introduce through the TCB, the correct NLTE opacities, the correct geometries, the correct energy input when feasible, the correct momentum balance (if feasible) – which implies some knowledge about the HD and MHD behaviour of the outer layers.

Of course, at this stage, the bad treatment of HD and MHD can force us to use the comparison between the observation and the best RE-model to infer some better knowledge of the HD – MHD physics of the model. And then, the limitation is basically strong: if we use models to improve the knowledge of their behaviour, how can we use them to calibrate observations?

3.4. CONCLUSIONS

Having expressed several times already in this report that rather pessimistic view, we should explain why the two-dimensional classical methods have been working so well – an actual fact that many authors are using to justify their reluctance to a change in approach to the 'generalized theory of atmospheres and environments' we have just been outlining.

The main reason of this success is that most of the abnormalities are located in the outer layers, non-photospheric, and therefore, it has been possible to describe with a certain set of T_{eff}, g_{eff}, $A_{i,\text{eff}}$ the photospheric regions. But we must be conscious that the parameters found from the best fit between models and observations might not necessarily be the good ones, the physical ones – as the deeper regions are counter-affected by the unobserved (or little observed) outer regions. They are just 'ad hoc' parameters.

The 'grobanalyse' of Unsöld, which essentially distinguishes between the T_{eff} of the deep photosphere and the T_0 characteristic of the regions where the lines are formed does not modify this conclusion, as the T_0 in question is some kind of excitation – ionization temperature, far from identical to an average electron temperature of these layers. Actually, one should describe an 'ad hoc' model with, at each layer, three kinds of temperature: $T_{\text{exc-ion}}$; $T_{\text{radiation}}$; T_e...

The adequacy of these parameters for a 'representation' of the observations is not in question. The success of the methods rely on the fact that the various layers are strongly coupled – which we certainly do not deny. After all, due to the kind of couplings that exist, the number of significant parameters is not large... What are they? How many?

Certainly, the $\mathscr{L}$, the $\mathscr{M}$, the R, the X, the Y, coupled by the *two* equations of internal structure (and together with a physical approach to the various physical phenomena linked with the escape of energy and momentum from the star's internal regions) are amongst what we need to describe the star, and in particular its atmosphere. These three parameters might be indeed replaced by T_{eff}, g, and the main chemical composition parameter, H/He.

But also the *age*, which may affect the outer layers very strongly and therefore, indirectly, the photosphere itself: envelopes are thicker for young stars, shells probably (as in the case of planetaries) for stars that have undergone unstabilities and matter ejection, and which are therefore rather evolved objects.

But also the *details* of the chemical composition (not only H/He, or H/(H+He++others)), such as the ratios Fe/C, C/O, etc.... do play a part. Such details are a function of the age (through evolutionary processes involving nuclear reactions, diffusion processes, etc....) or of the stellar population (location in the Galaxy of the birth-place of the star, composition of protostellar matter...) – or a function of both.

This is, finally, more than two parameters. But the fact that most of the stars used in the calibrations are members, more or less, of the solar neighbourhood, explain essentially why age effects or stellar population effects have been of very little influence upon the apparent quality of the two-dimensional classifications (being admitted that the ratio H/He does affect little this calibration). This statement brings a 'serious warning': the calibrations valid near the Sun can be completely wrong for stars of the halo, or for stars in the Magellanic Clouds...

And our discussion brings now another 'serious warning': if, by using coherent grids of models, we found a value of T_{eff}, and g, for a given star, we still shall have to recalibrate these values, according everything else we may know about the star (envelopes and ages, populations and chemical composition, magnetic fields, etc....) into another set of values of T_{eff}, and g, that are really, physically, linked to the values of $\mathscr{L}$, $\mathscr{M}$, R, X, Y... characterizing really the star as a whole (as illustrated earlier by Figure 1).

The best conclusion is certainly that, as much as possible, we should treat stars as individuals, not judging a priori from any classification scheme their atmospheric parameters, – or, if forced to do so, doing it with a sufficient degree of self-defidence. By no means can we say that the diagnosis of effective temperatures and gravity is still in its quantitative stage; we can indeed obtain no more than acceptable approximations, and the model atmosphere theory is just at its start!...

Acknowledgements

The author is glad to express here his gratitude to the several colleagues with whom he has been fortunate enough to discuss about these problems, and especially to Dr Katherine Gebbie, Dr Françoise Praderie, Dr Dimitri Mihalas, and above all, to Dr Richard N. Thomas who has been a permanent source of inspiration, for several years.

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DISCUSSION

Thomas: In the case of early-type stars we know that those stars are loosing mass, which means that the atmospheres around them must be expanding. How good are the model atmospheres which take into account expansion velocities?

Pecker: The problem is a very complex one. For example, the transfer in some intense lines (a transfer which we know to affect the model through blanketing effects) is completely different, according to the velocity field, as the absorption coefficient is displaced with respect to the incident profile of the line, at any depth. Actually it has been possible to include that type of effects in relatively simple atmospheres, where other refinements have not been applied (for example, in grey atmospheres), A good example of what can be done is given by the work by Magnan (in the process of publication), who is using the Monte-Carlo method. I expect that this field will progress significantly in the coming years; the situation is not too good, but less 'discouraging' than in the case of convective layers for example. But the main problem will then be: 'what type of velocity field to introduce?'. And if one wants a physically consistent reply to this question, then, I must confess that I think we are still a very long way off.

Demarque: This is probably a discouraging question, but one of importance to stellar interior research. You have said very little about convection in stellar atmospheres.

Pecker: I fully agree with Dr Demarque. Indeed, the ionization of the main components of a star gives place to a convective zone, more or less deep in the photosphere, at least for spectral types later than A0. But at the present time not only do we not know how to compute well the T, p_e distribution in this zone (the mixing-length theory has well known drawbacks; improvements are, I believe, on the way through the work of Spiegel, Souffrin, Zahn and others, but it is still at a very preliminary stage), but we do also not know well how to compute the effects of the convective zone in the outer layers where mechanical energy transport starts to play a role. The important work by Leibacher gives certainly a better basis to this problem, but again we are only starting to understand the physics and still very far from being able to compute ready-to-be-used models! Indeed, the situation is 'discouraging'!!

McCarthy: Do you consider it to be possible that some kind of a scheme for classifying stellar models could be made as an aid to observational astronomy. Such a classification scheme could be analogous to that which has on several occasions proven so helpful in observational stellar spectroscopy.

Pecker: Why not? I think this would indeed be an excellent idea. Apart from a T_{eff}, g_{eff}, abundance classification, this classification would indicate in each case what kind of degeneracy has been introduced to improve the model compared to what I call 'classical' models in my paper. This would essentially make clear at what height (when proceeding from the inside) the model in consideration ceases to be valid.

Jaschek: That means essentially that one can explain stars which in the MK system are called normal, with perhaps the exception of the supergiants.

Pecker: You cannot, I think, say you 'explain' them. At least, you may 'represent' them by a set of parameters, such as T_{eff} or $\log g$ but the physical meaning of that set of parameters is still to be questioned, even for class V stars, in many areas of the HR diagram (early-type stars, late-type stars, Am stars...). With this restriction, I agree with you, and would say that for many stars of class V, no obvious contradiction can be detected between models derived in order to represent any given set of observed criteria.

Eggen: Is it possible that the difficulties you have been discussing are the cause of the observed spread in stellar metal abundance (omitting the extreme halo stars)?

Pecker: Probably partly, but only partly. In each specific case, a careful discussion should be done, to reply to the question: "Is the spectral anomaly we measure in a given group of lines due to metal abundances differences, or is it due to abnormal NLTE effects, to shells, to effects of convection on temperature gradients and so forth...?"

Jaschek: I conclude from what you said that you would not trust abundance differences between stars smaller than a factor of 5. What worries me is that many photometries are detecting small metal abundances differences, which are calibrated on atmospheric analyses which apparently are somewhat shaky.

Pecker: I do certainly share your worry. Small differences in metal content may often be due to some undue simplification (differential methods?) of the determination of abundances in stars that may have different structures. I can refer to the already old discussion of WR stars: Underhill years ago, denied that the difference between WC and WN is an abundance difference; and we should not forget that when applying differential methods, one implies that many things are identical in the two stars in comparison – from NLTE effects to differences in gradients of the temperature (even in LTE), between stars having for example convective zone of different importance.

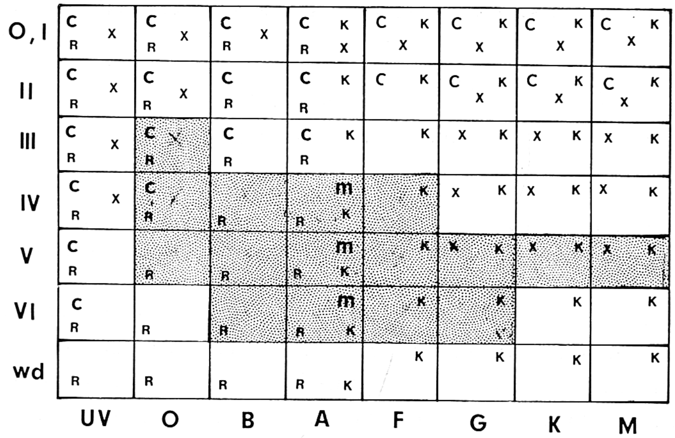
Hack: What do you think about the super-metal rich stars found by the spectrophotometric method of Spinrad? In a paper by Spinrad spectrograms and tracings are reported of a SMR star and of a so-called normal star, which look practically identical.

Pecker: I am puzzled, as you are, by the kind of facts you are stating. I would be tempted to say that the Spinrad classification scheme may be sensitive to metal abundance *and/or* other factors in the atmosphere. But I have not studied this scheme in detail and I do not consider my reply as satisfactory!

Newell: If I may add to the story of α Cen; while there are perplexing cyclic changes in the helium spectrum and in the metal line spectrum, there appears to be no corresponding change in the Balmer jump, Paschen slope, and hydrogen line spectrum. It is hard to admit significant changes in effective temperature and effective surface gravity during the cycle.

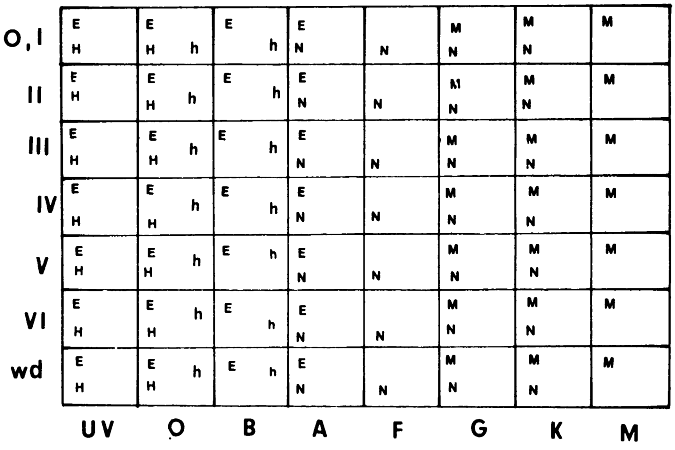
Golay: I should like to ask Pecker to give me some bibliographical information: I suggest that Pecker draws at the blackboard a HR diagram and gives the bibliographic references of the best models for each part of HR.

Pecker: I am not prepared to give list of models, so rich has been the production of 'rather good' models in the recent years. Only as examples of very good models, I could quote Auer-Mihalas models for early-type stars – Hummer-Mihalas models for nuclei of Planetary Nebulae... and on the other side of the diagram, Aumann or Tsuji models for cold stars. But good models produced by authors such as Peytremann, Carbon and associates, and many others, without being 'perfect' (none is perfect!) should obviously be quoted. I shall try to draw a 'conceptual' diagram of the sort you want, if possible for inclusion as an appendix to this discussion, in time for being published with it (Figure 21), I certainly do agree on the fact that such a diagram would be a very interesting guide to have under hand.



	UV	O	B	A	F	G	K	M
O, I	C X R	C X R	C X R	C K R X	C K X	C K X	C K X	C K X
II	C X R	C X R	C R	C K R	C K	C K X	C K X	C K X
III	C X R	C X R	C R	C K R	K	X K	X K	X K
IV	C X R	C X R	R	m R K	K	X K	X K	X K
V	C R	R	R	m R K	K	X K	X K	X K
VI	C R	R	R	m R K	K	K	K	K
wd	R	R	R	R K	K	K	K	K

astrophysical complexities



	UV	O	B	A	F	G	K	M
O, I	E H	E H h	E h	E N	N	M N	M N	M
II	E H	E h H	E h	E N	N	M N	M N	M
III	E H	E h H	E h	E N	N	M N	M N	M
IV	E H	E h H	E h	E N	N	M N	M N	M
V	E H	E h H	E h	E N	N	M N	M N	M
VI	E H	E h H	E h	E N	N	M N	M N	M
wd	E H	E h H	E h	E N	N	M N	M N	M

sources of opacity

Fig. 21. Astrophysical complexities; sources of opacity. *On abscissa*, the spectral type – including the very hot UV stars that can exist (for example in young galaxies); in ordinate, the luminosity class, including class 0, to which belong some stars of the Magellanic Cloud. *In the upper part*, the letters designate the complexities that have to be introduced in models: C is for curvature; R for non-grey radiative equilibrium, m for magnetism, K for convective layers, X for extended atmospheres. The shaded area correspond to those relatively well filled by honest-to-good models. *In the lower part*, E is for electron scattering, H for ionized helium, h for neutral helium, N for negative hydrogen ion, M for molecules; blanketing by impurities and atomic hydrogen have to be put in all cases. Clearly this is highly schematical; it has not been found suitable to put in the diagram any particular selection of models...