## DISCUSSION (Želwanowa and Schöneich)

**KHOKHLOVA**: Have any of these stars been investigated spectroscopically, and is there any information about the variations of lines of any elements?
**ŽELWANOWA**: Yes. For example, HD 175362 has well known variations of He I 4026 Å similar to those in a Cen. At the moment, I have no other information.
**NORTH**: I have a question about HD 74196. The period is really very short, and there are only six points, if I understood you correctly. To what extent can you guarantee the period?
**ŽELWANOWA**: [ consults translator ] We can't state it [the period] definitely for the moment, because we lack the observational data and we need to confirm them.

## DISCUSSION (Mégessier)

**STĘPIEŃ**: Did you take into account centrifugal forces when calculating the motion of Si ions in the atmospheres of these stars?
**MÉGESSIER**: No, the model should be completed by taking into account some additional effects, such as the microturbulence, on the meridional circulation. As the rotational velocities of Ap stars are relatively small, the centrifugal forces may be considered to be second-order effects. A more precise model should take these into account.
**STĘPIEŃ**: In your diagram of Hγ equivalent width vs. Si overabundance, is the variation of Hγ with effective temperature taken into account?
**MÉGESSIER**: To study the evolutionary effects, I grouped the stars with the same effective temperature together, so that the only effect on the Balmer line is the gravity.
**KHOKHLOVA**: Could you please show the position of the Si stars HD 124224 and HD 34452, with large overabundances and weak magnetic fields, on your diagram?
**MÉGESSIER**: HD 124224 (CU Vir) is here [indicates]. I did not consider the other star because I did not have its Balmer line equivalent width. It seems to be in a consistent position on the [Si/H] vs. $T_{eff}$ plot.
**DOLGINOV**: Magnetic traps are imperfect. It is well known from laboratory experiments with devices such as 'Tokamak', that the time for particles to escape from the trap is not governed by the normal diffusion in the field, but is due in some cases to Bohm diffusion which is faster, and is due to the various instabilities. The same is true for the trap in the terrestrial megnetosphere. Thus the Si particles can escape from the trap in a much shorter time than that obtained from the diffusion velocity. Have you taken into account these possibilities? Also, how long are the Si particles kept in the trap, and is this sufficient to get the observed overabundances?
**MÉGESSIER**: In the laboratory the conditions are not the same as in the stars. Here, the forces which maintain the Si ions trapped in the magnetic field lines are vertical, that is to say, normal to the field lines, which is not the case in the laboratory. Also, the equilibrium here is not static. There are permanent ascending and descending vertical Si flows in the atmosphere. In this way the descending atoms



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are continuously replaced by the ascending ones.

**MICHAUD:** Mégessier's model is the simplest model that can be constructed using diffusion. In so far as it is confirmed by observations, no additional amplifications are needed. We realize that additional instabilities may well exist. However, they may not be important for the evolution of the abundances.

**COWLEY:** The lower effective temperatures for the Si stars which have been found as a result of the blanketing re-open the old debate between Leckrone and Preston concerning the He abundance. If the effective temperatures are really lower than was thought 10 - 15 years ago, then surely the underabundance factor of He must be smaller, that is to say, the He must be more nearly normal than we thought it was. Has this question been re-examined in the light of these new lower effective temperatures, and if so, what is the result?

**MÉGESSIER:** If the effective temperatures are in fact lower than those determined from visible wavelength colour indices, then perhaps He is not as underabundant as it was thought to be. However, one has to check the exact influence of those new temperatures on the stellar atmospheric structure and on the models.

**COWLEY:** The point is that the phenomenological spectral type would then be closer to the true type according to $T_{eff}$, because with low dispersion spectra we have classified these stars as later in type because the He lines are weaker. But, the He lines may be weaker because the $T_{eff}$'s really are lower than the colour temperatures. This would bring the spectral type more into line with the new effective temperatures, and they would be lower than the older colour temperatures.

**MÉGESSIER:** Yes, yes.

**COWLEY:** The plot of $W_\lambda$[Fe II 4233 Å] vs. v sin i is very interesting, but because this line is on the flat part of the curve-of-growth its strength can depend on factors other than the abundance of iron, such as microturbulence, etc.

**MÉGESSIER:** I think you are right. I mentioned this because it is a clearly observed effect, and Didelot, who made the statistical study, proposed that it is an abundance effect. However, he does not exclude any other effects; it's just a hypothesis.

**ADELMAN:** Heasley, Wolff & Timothy [Ap. J., 262, p. 663, 1982] have shown that one can obtain He/H values with errors of 10%, provided that one uses data with S/N ≥ 50. Such observations are needed for Si stars. Determinations of He/H with equivalent width values are notoriously unreliable.

I wish to make an additional brief comment about spectrophotometry. This is, to a large extent, a 'black art'. Most observers use mean extinction coefficients instead of properly determining it each night. A good check on the quality of the data is to synthesize intermediate band colours such as Strömgren or Geneva colours. Only if the agreement of the synthesized and directly observed values is good, should the spectrophotometry be trusted. Observers who do not take an adequate number of standard measurements (6 or more) can easily produce bad data. As referee, I recently rejected a paper based on only one or two standard star measurements per night.

**DWORETSKY**: You're not very nice, are you? [laughter]
**ADELMAN**: No! But they could not reproduce the standard colours!
**ARTRU**: The gallium overabundance, which was considered to be characteristic mainly of Hg-Mn stars, is also shown to be commonly overabundant in Si stars in the poster paper at this Colloquium presented by Takada, Sadakane & Jugaku. I would like to ask, are there definite sets of elements overabundant in each class of peculiar star?
**MÉGESSIER**: Each group of CP stars is characterized by the most prominent enhanced elements. From time to time, papers appear which claim the exclusion of overabundances of some specific species in the same stars, but so far nothing conclusive can be said. One may find several abundance anomalies in the same spectrum, with relative overabundances being different from star to star.
**KHOKHLOVA**: Two remarks concerning Cowley's question about helium. The first is that the meaning of "effective temperature" is not always explained. Is it the parameter in the Stefan-Boltzmann law, or is it the real temperature in the atmosphere, which determines the colour, line strengths, ionization, excitation, etc? Could you explain which meaning is used in your paper?
**MÉGESSIER**: The value of effective temperature was deduced from the use of the Blackwell-Shallis method, using the angular diameters, bolometric corrections, and infrared fluxes.
**KHOKHLOVA**: So it is the parameter in the Stefan-Boltzmann formula. So it doesn't reflect the real temperature in the atmosphere unless you take blanketing effects into account; this is what I meant.

I would like to mention another problem with He. Perhaps my paper on He NLTE excitation wasn't noticed, but there may be very strong NLTE effects for He excitation because everything depends on only one resonance line which is optically thick, so people assume that there is an equilibrium excitation. But, this resonance line is coupled with many Fe IV, Mn IV and other lines, and this could lead to very strong deviations from LTE.
**MÉGESSIER**: It is true that you have to make a careful study of the bolometric correction to get the right effective temperature, since it is realized that if you deduce it from the visual fluxes only, you get too high a value. If you use only the ultraviolet, you deduce too small a value. It is best to use data from all the wavelength ranges available, whatever the blanketing.
**HUBENÝ**: I would like to emphasize that effective temperature only makes sense as a measure of the total radiation flux coming from below through the boundary of an atmosphere. Its relation to the physical temperatures (e.g., electron temperature) as a function of depth has to be obtained from a model, and its relation to various 'temperatures' (colour temperature, excitation temperature, etc.) has to be obtained in each particular case by appropriate transfer solutions. This calculation can be done even in NLTE.

Concerning the He problem, I think that although the resonance lines could affect the formation of the singlet system, the triplet system should be affected to a much lesser extent, because these systems are not directly coupled, but are only connected by very weak intercombination lines. These are especially weak in A type stars.

Could you tell me if there is any other evidence for changes in effective temperature - in other words, the total flux - in Ap Si stars, besides those you mentioned, such as Muthsam and Stepien for $\alpha^2$ CVn, and the recent work of Iliev?

**MÉGESSIER:** No, those are the first two.

**HUBENÝ:** I think it is very hazardous to speak of the concept of an averaged effective temperature, and from the methodological point of view, it is too easy to introduce new concepts which might explain everything.

**MÉGESSIER:** The point is that at two different phases of the same star, you can not use the same effective temperature model to compare what you see. But it does not mean that the effective temperature of the star is different. It just means that you can not invoke only line intensities to reproduce what you see. It is just a kind of fictitious parameter that you try to fit with some $T_{eff}$ and some metal abundances. [ barely audible, but apparently heated, background discussion ] I think that Dr. Stępień is concerned!

**STĘPIEŃ:** I would like to correct one of your statements! The effective temperature is, of course, connected through the Stefan-Boltzmann formula with the total emission from a give element of surface, but it does not have to be directly connected with the total flux observed from a star. If you have a distribution of effective temperature over the surface, each element has its own value, which is a well-determined physical parameter, but the total flux integrated over the whole surface of the star is not related to any of these particular values of temperature. This means you need the concept of an average effective temperature. In other words, you may have the total flux obtained from the star integrated over all wavelengths. If the star is not spherically symmetric, or has spots, then you may still have the total flux from the star constant over all phases, but what you observe is something which represents the average temperature over the star at different phases. [ interrupted by inaudible voice ] No! The meaning of $T_{eff}$ is that it is connected with emission from a given element, say one square centimetre. [interrupted again ] But not from the whole star! Just from one square centimetre. Well, it is that way!

Is there really any observational evidence that the total integrated flux from a star changes with phase? I have seen a few papers, but I don't know whether the results are statistically above the errors. The paper given at this Colloquium by Polosukhina and Malanushenko on 53 Cam says that the flux may marginally change by 1-2%, but who knows if this is real or not? All I am saying is that some people have reported variations of the total integrated flux with phase.

**ADELMAN:** Much of the residual variability in the total flux in Leckrone's study of HD 215441, which is also seen in its effective temperature, is due to the lack of correction for the variability of the 5200 Å feature, which was unknown at the time the paper was written. Spectrophotometry shows that the strength of this feature varies by a factor of two. Correction for this substantially reduces the total flux variations.