DISCUSSION (Dworetsky)

PTITSYN: I'd like to make a remark on the superficially normal late-B star HR 8226 = HD 204754, which according to the WCS analysis of Cowley, Sears, Aikman & Sadakane (1982) was expected to be iron-weak. We have done a detailed chemical analysis of this star using 9 Å/mm spectrograms. It turns out to have a fairly normal chemical composition. The abundance of Fe may be slightly low, but no more than 0.3 dex, which is within the errors of the analysis. We suspect that the reason for the discrepancy between our results and those of Cowley and his co-workers is that they adopted too low a value of v sin i, which we think is in the range 10-12 km/s, but we can not be certain of this.
DWORETSKY: [to Cowley] Do you want to reply?
COWLEY: I find no problem with this result. Our method should have statistical validity, but I would not make a strong claim in any individual case.
DWORETSKY: The possible slight weakness of Fe mentioned (0.3 dex) may have some bearing on this question. I agree with your remark about the statistical nature of the results of Cowley et al. (1982).
DROBYSHEVSKI: Are there any indications of possible duplicity of Vega, not only spectroscopic Doppler shifts, but possibly also in the X-ray emission?
DWORETSKY: I am not aware of any. The X-ray observations of Vega were very difficult to make. The Einstein Image Proportional Counter was, I recall, unable to detect Vega, and the High Resolution Imager had to be used, with a very long integration time. Vega has a very, very weak X-ray emission consistent with a fairly low temperature corona, about $5 \cdot 10^5$ K. This is consistent with the scaling law for single B and A stars which I mentioned in my review.
SEVERNY: I have a short comment about the magnetic field in Vega. If it exists at all, it may not be constant.
DWORETSKY: I thought all results were null so far, such as yours and those of Borra and Landstreet. Are you saying that there may be a real but variable field?
SEVERNY: There might be, but sometimes it can not be detected, within error limits of ±5 gauss.
DWORETSKY: We are at the limits of the statistical accuracy of the data, and perhaps we should not conclude that there is a field, but say only that it is proven that the field is no stronger than some upper limit.
SEVERNY: Yes, that is correct.
DWORETSKY: I would interpret the published results by saying that no field has been detected, within the errors of ±20 gauss or so. The existence of very weak general fields is not yet excluded.
ALECIAN: One must be very careful in interpreting the abundance determinations for Hg-Mn stars. If there are abundance stratifications in the atmosphere, the "classical" methods, which assume homogeneity, may fail. For example, a "cloud" of Mn with 10^5 overabundance located above $\tau_{5000} = 10^{-4}$ gives a curve of growth for Mn III lines which looks like the curve of growth given by uniformly distributed Mn with 10^2 overabundance and $\xi = 3$ km/s. This is an example: other kinds of

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effects are possible.

DWORETSKY: I don't quite know how to reply. The use of the homogeneous model does not strictly imply that we believe every assumption made when the model is used. I believe that you yourself suggested a possible explanation a few years ago in unpublished work. You pointed out that small amounts of microturbulence can co-exist with diffusion. This turbulence can spread the thin cloud produced by diffusion into something resembling a homogeneous distribution. Perhaps this explains why homogeneous models always seem consistent with the data, within the errors of analysis. The stratification effects mimic other things, and are consequently difficult to detect.

ALECIAN: Have you determined the microturbulence from different elements in the same star, and do you get the same value of ξ for the different elements?

DWORETSKY: Yes, the values are the same within the errors, for Fe I, Fe II, and Y II, in the few stars I have investigated recently, using fully line blanketed Kurucz model atmospheres.

I would like to emphasize that great care has to be taken to eliminate the spurious results for microturbulence which result from improper treatment of statistical errors in the observed equivalent widths. Nowadays, one usually plots line strength against derived abundance, and adjusts the value of ξ to produce constant abundance independent of equivalent width. However, the equivalent widths are observed values, and their random errors can produce a systematic increase in the microturbulence derived. I discussed Magain's (1984) paper on this subject in my review, and recommend that all of us adopt his technique of substituting theoretical equivalent widths, for the guessed microturbulence and abundance, for the observed values. This should lead us to still more accurate microturbulence estimates. It is also very important to use fully blanketed model atmospheres, because Kurucz has shown that unblanketed models lead to overestimates of ξ, and I have confirmed that use of fully blanketed models reduces the value considerably, though not usually to zero.

HUBENÝ: I would like to return to the question of the disadvantages of Vega (α Lyr) as a spectrophotometric standard, and a consequent skipping of observations of this star in the Space Telescope programme mentioned by Dr. Adelman. I would like to see this reconsidered, because Vega, besides serving as a standard from the observational point of view, would be an excellent standard star from the point of view of theoretical modelling. For example, much more work has been done for Vega than for Sirius, detailed NLTE studies have already been performed, both for overall model atmospheres and detailed transfer solutions for individual atoms, and the lower metallicity makes NLTE modelling easier (blanketing is not so heavy), and finally the search for chromospheres and coronae could be continued. High resolution observations would help resolve one of the basic questions about A stars, the structure of the most superficial layers.

DWORETSKY: I am not a spokesman for the Space Telescope programme. However, some of us plan to apply to observe Vega if no one else wants to. In other words, although Vega is not a standard star for Space Telescope, it will probably be observed.

HUBENÝ: Concerning the discussion of λ Boo stars and Vega, I would like to show you a plot of the TD-1 observations of Vega, which here [shows a transparency] is compared to theoretical spectra based on NLTE calculations. One may recognize a feature near 1600 Å.

DWORETSKY: There is indeed some sort of depression there. Perhaps we should vote whether Vega is a λ Boo star or not! All in favour? [pause] All against? [pause] All not voting? [pause] Most people are not voting either way!

JOHANSSON: During this conference we have heard of three different broad depressions at 5200, 1400, and now at 1600 Å. Do you know of any similar feature at another wavelength in any star, for which there is a satisfactory explanation?

DWORETSKY: Not really, though there is also a 4200 Å feature, I think. Explanations, so far are very unsatisfactory for all the features. There has been some limited success in identifying part of the 5200 Å feature as Fe I lines, by Maitzen and Muthsam (Astron. Astrophys., 83, p. 334, 1980), but perhaps others can answer this question better than I can. What is astonishing, of course, is that we are seeing a strong depression in a metal-weak star. Very, very interesting.

ADELMAN: The Fe identification only works for 5200 Å in the coolest Ap stars, and you need a lot of iron to produce it.

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