DISCUSSION

Di Benedetto (p. 25)

PIERRE MAXTED: In distance estimates to Cepheids based on period-luminosity relations, the metalicity contributes to the uncertainty via the correction for differential reddening. How does diffferential reddening affect the Baade-Wesselink method?

PAOLO DI BENEDETTO: The reddening corrections affecting the actual BW distances to extragalactic Cepheids are found to be quite negligible, according to the overall absorption term. For instance, the correction for BW distance to M 100 Galaxy is few hundredths of magnitude and such a small contibution causes the significant reduction of a factor three in the final quoted error with respect to that affecting the distance dcerived by period-luminosity relations.

Kruk (p. 67)

ANDREAS KELZ: Do you have any problems or discrepancies connecting your UV flux distribution with the observed ones in the visible?

JEFFREY KRUK: There are no discrepancies when the calibration is derived by normalizing the white dwarf models by the V magnitude and extrapolating to the FUV. IUE fluxes are 6% low when compared with ground-based spectrophotometry at the atmospheric cutoff, which is what would be expected from their choice of normalization.

Bless & Percival (p. 73)

ROBERT KURUCZ: Why are you willing to settle for 4% in the visible? It is a disgrace to have such low standards.

ROBERT BLESS: I must not have made myself clear. I am not happy with 4%. I'm simply saying that it is hard to do better. When you start with the uncertainty in the calibration standards, transfer these to your own program stars, and then compare your results with those of another observer, it is very difficult to do better than 3-4%. Obviously, however, we should always try to improve.

Bell (p. 159)

MARTIN COHEN: I am not convinced that the few mountain-top near-IR measurements of Vega fluxes that lie above the Kurucz Vega model are indicative of a problem. None lies more than 2 sigma from the model spectrum and Donald Blackwell (priv. comm.) has suggested that the great difficulty associated with comparison of a star with a standard lamp on a distant site means that formal published uncertainties on the measurements are probably underestimated, perhaps by a factor of two.

We have two other methods to check the Kurucz spectra of Vega and Sirius, however. One is a parallel programme that's been running for about five years, in which we try to compare Kuiper Airborne Observatory spectra of bright cool stars like α Tau and α Boo with a specially commissioned absolute blackbody furnace made by NPL, UK. This method eliminates dependence on any adopted models but, because we have already published absolute spectra of these cool giants calibrated through Sirius and Vega, we can indirectly test these hot stellar spectra. We are just completing a year of diffraction calculations necessary to make the direct comparisons of KAO 3-30 μm spectra of K-M giants with the pre- and post-flight ground-based blackbody calibrations. It may be very difficult to recover the absolute *level* of stellar spectra but we should get the *shape*.

The second technique comes from the MSX satellite, currently in orbit, which carries a series of IR radiometers between 4 and 20 μm. This satellite also carried a series of small 'emissive spheres' that were ejected and observed repeatedly by the radiometers. These spheres have been modeled very precisely by their creators (MIT Lincoln Lab.) so that their thermal behaviour in space is highly predictable. MSX then made observations of some of our cool giant calibrators that can be compared directly with measurements of the emissive spheres. Again, all our K-M giant spectra have been assembled from pieces calibrated against Sirius and Vega, so a test of these cool stellar spectra is implicitly a test of these hot stellar models.

Nissen (p. 171)

MONIQUE SPITE: In the cool stars of the Small Magellanic Cloud an unexplained deficiency of nickel has been also found (V. Hill 1997).

POUL ERIK NISSEN: This is interesting and supports our suggestion that the halo stars, which are deficient in the alpha-elements, Na and Ni, have been accreted from a dwarf galaxy.

ROBERT KURUCZ: There is a systematic error that you did not point out in presenting the solar abundances. Abundances are usually from a minor stage of ionization, so the number density is proportional to electron number. Most of the determinations are independent. But the electron density varies with Si or Fe abundance, so changing Si or Fe changes all the abundances. Rigorously, everything should be determined simultaneously.

POUL ERIK NISSEN: I agree. The Holweger-Müller model was computed for $A_{\rm Fe} = 7.67$, which is inconsistent with the meteoritic value of 7.51.

DAVID ARNETT: Why should we believe that the overshoot region is spherically symmetric? The assumption should affect both the models of evolution and of spectra, shouldn't it?

POUL ERIK NISSEN: Yes, but that is what the 3D hydrodynamic models of the solar photosphere try to take into account.

BENGT GUSTAFSSON: The ad-hoc recipe to handle convective overshoot in the Kurucz ATLAS9 models leads to two effects which both increase the estimated abundances: (1) the effective temperature is increased and (2) the temperature gradient is decreased in the line-forming region. There is no clear proof that real convective overshoot would lead to these effects. The only way to clarify this is to carry out model simulations of Nordlund's type for stars with different metallicity and calculate lines for these models. This could and should be done.

Spite (p. 185)

JOHN NORRIS: You report very small scatter on the Spite Plateau. Could you comment on the result of Deliyannis, Boesgaard and King that a real lithium abundance scatter exists near the main sequence turn off of the metal poor cluster M92?

MONIQUE SPITE: Deliyannis et al. found different lithium abundances for stars having the same $B-V$. First, the S/N ratio of the spectra is not very good and thus the accuracy of the equivalent widths is not excellent. Moreover, the $(B-V)$ colors in a crowded globular cluster are not very accurate, and it is possible that the stars have not exactly the same temperature - I will add that the stars observed by Deliyannis et al. are not exactly at the turn off; they are subgiants and dilution could explain the 'lithium-poor' stars. One of the stars is, on the contrary, very 'lithium-rich,' but it has been found also very 'magnesium-rich.' The star is, as a consequence, very peculiar and a detailed analysis would be useful.

BENGT GUSTAFSSON: How would you explain a decrease of [O/Fe] from +0.65 to +0.40 within the halo? As a result of different O/Fe yields for SNe II of different masses or of different metal abundances, or is it too early to speculate about that?

MONIQUE SPITE: Up to now, with the ATLAS9 (without overshooting) or the NMARCS models and Teff/colour relations of Carney or Nissen, the oxygen abundance has been determined or estimated in the halo for four stars, one with a metallicity of about -1 and three with a metallicity of about -2 dex. For HD 103095 ([Fe/H] about -1), computations give a good agreement between the different systems of the oxygen lines and the ratio [O/Fe] has been found to be about 0.3 dex.

For the three other stars with [Fe/H] about -2, I could only estimate the change of the [O/Fe] ratio due to the change of the temperature. Thus it is really too early to speculate about a possible tilt of the ratio [O/Fe] versus metallicity in the halo. At least, before doing it, it is necessary to redo the computations of the OH molecular lines in the stars of the Nissen et al. (1994) paper with the new temperatures.

ROGER BELL: The high O abundances are disturbing in terms of results from globular cluster giants. Would you expect to see CO bands in these sub-dwarfs?

MONIQUE SPITE: These stars are hotter than the globular cluster giants and I am not sure that the CO band can be measured.

Sasselov (p. 253)

JOHANNES ANDERSEN: Your results seem to demolish most of the assumptions underlying the simple-minded applications of the Baade-Wesselink method, in particular the determination of radial-velocity curves from spectra averaging lots of lines of different origins. Is there any hope of getting useful results from these existing observations, or will they all have to be thrown out and replaced by new data from selected sets of spectral lines?

DIMITAR SASSELOV: The Baade-Wesselink method is attractive because it has the potential to provide accurate one-step distances to nearby galaxies. The simple-minded application of the method suffers from systematic errors which puts its reliability in question and makes it less competitive with other methods. One way to improve the quality of BW solutions is with the use high-resolution spectra. Unfortunately, the necessary information on line profiles and strengths cannot be recovered from existing observations.

MICHAEL SCHOLZ: Perhaps, the situation is different in Cepheids, but in case of the Miras the structure of the deep atmospheric layers (= upper interior 'envelope') changes little as you replace the simple grey atmosphere by a more sophisticated non-grey atmosphere.

DIMITAR SASSELOV: Yes, the atmospheres of Miras are so extended that regions in them may be completely decoupled and behave locally with the corresponding molecular opacitites.

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BENGT GUSTAFSSON: Concerning the effects of blanketing - are they, in addition to being significant for colour calculations, etc., also significant for the structure and dynamics of the upper layers?

DIMITAR SASSELOV: Yes, I suspect they may be significant. We will not know how significant until a consistent calculation is made.

Kjeldsen & Bedding (p. 279)

ROBERT KURUCZ: I have a philosophical comment; do not take it personally. We do not know the spectrum of a single star. We would learn a lot more if you would spend some of your observing time taking a high-resolution, high signal-to-noise spectrum.

HANS KJELDSEN: I agree that we would learn a lot from such a spectrum. Firstly, however, it would not take very much time to do this for Procyon. In fact, at $R \approx 200\,000$ you can reach S/N of 1000 in 10 s and S/N of 10 000 in 15 minutes on a 4-m telescope. Secondly, I disagree that such a spectrum would provide more information than would detections of p modes. The solar oscillations provide a huge amount of information and tell us almost all that we know about the Sun. This will never be the case for a high S/N spectrum.

JØRGEN CHRISTENSEN-DALSGAARD (also responding to Kurucz): Observations of the stellar radiative spectrum, however precise, would not give information about the structure of the deep stellar interior. One might perhaps be forgiven for regarding such information as being more fundamental than fine details of the surface properties and composition.

Christensen-Dalsgaard (p. 285)

GIUSEPPE BONO: What is the dependency of nonradial polar modes on the assumption of the adiabatic approximation?

JØRGEN CHRISTENSEN-DALSGAARD: The error made in assuming adiabatic oscillations is part of a larger set of errors coming from an uncertain understanding of the near-surface regions, including effects of turbulent pressure, etc.

However, since in this region the modes propagate almost vertically, the effects are independent of degree, when properly scaled. This allows the errors to be eliminated in the analysis of the data, when inverting for the internal structure.

JOHANNES ANDERSEN: How does your solar model perform as regards neutrinos?

JØRGEN CHRISTENSEN-DALSGAARD: The computed neutrino fluxes for the model essentially agree with other standard solar models, such as that of Bahcall & Pinsonneault. Hence they are much higher than measurements. However, it must be emphasised that helioseismology does not directly constrain the computed neutrino flux. What is measured is squared sound speed $c^2 \propto T/\mu$ (T is temperature and μ is mean molecular weight). Thus T and μ are not constained individually. In principle it is possible to make models with low neutrino fluxes that agree with helioseismology, through carefully tuned mixing and, e.g., changes in opacity and nuclear parameters. In practice, such a solution seems unlikely.

DON VANDENBERG: Can you say something about the internal rotation in the Sun and how well models that allow for rotation do in matching the observed oscillations?

JØRGEN CHRISTENSEN-DALSGAARD: Solar internal rotation, as a function of radius and latitude, can be inferred from splitting of the frequencies according to m. The results show the surface differential rotation to persist through the convection zone, with a sharp transistion to nearly constant rotation in the radiative interior; there is somewhat controversial evidence for slow core rotation. This profile is quite inconsistent with the Yale rotation model, which predicts rather rapid rotations in the interior.

Chiosi (p. 323)

JØRGEN CHRISTENSEN-DALSGAARD: Could you elaborate on the difference between the normal overshoot model and the diffusive scheme?

CESARE CHIOSI: In a normal overshoot scheme, the extension of the overshoot region is customarily derived from the velocity law of convective elements as a function of the radial distance (cf. Bressan et al. 1981). Within this region, the temperature gradient is either adiabatic or radiative, but what is more relevant, mixing is assumed to be instantaneous and complete. In the diffusive-overshoot scheme, the same picture holds with the only difference that in the overshoot region mixing is thought to take place over a finite time scale (this may be a function of local properties) and a suitable prescription is sought for the diffusion coefficient (or equivalently time scale of mixing). See for instance the algorithm by Deng et al. (1996). In an ideal model both the overshoot distance and mixing time scale should be derived from a complete physical description of this phenomenon (see for instance the formulation by Grossman et al. 1993). The problem is with the complexity of these models which has so far hampered the practical usage of these (more physically sounded) theories in stellar model calculations.

STEVE KAWALER: Can you include overshooting during the convective phase of a thermal pulse on the AGB ?

CESARE CHIOSI: In principle yes. Marigo is indeed working on it. Our plan is to adopt the same diffusive-overshoot scheme as in Deng et al. (1996). The overshoot distance should result from the velocity profile (layer at which the velocity of convective elements vanishes) and mixing in it should be governed by diffusion at a suitable rate.

BENGT GUSTAFSSON: Does your work with Marigo produce different distribution of C/O ratios for a population of metal-poor carbon stars, such as in the LMC, as compared with galactic ones ? If so, which are the results ?

CESARE CHIOSI: We must distinguish between stars without and

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with envelope burning, i.e., stars lighter and heavier than about $4M_\odot$. In the first group, at given mass of the star and assuming constant the parameters governing the *3rd dredge-up*, owing to the longer AGB lifetime (larger number of pulses) the ratio C/O is higher in low metallicity stars. In stars of the second group the situation is more complicated. Even if envelope burning is more efficient in low metallicity stars, the interplay between number of pulses and nuclear burning may lead to the case in which the ratio C/O is on the average higher in low than in high metallicity stars.

Langer (p. 343)

JOHANNES ANDERSEN: Three comments and a question:

1. Wouldn't a radial velocity survey be a more direct way to exclude (post-mass-exchange) binaries?
2. Comparison of slowly rotating (synchronized) B-type binary stars with models seems to indicate the kind of convective core enlargement commonly labeled by the nickname 'overshooting.'
3. Because of orbital synchronization, the M-L relation derived from binary data will be for slowly rotating stars (mostly).

Q. Could fast interior rotation decoupled from the surface make any of these comparisons inaccurate/invalid?

NORBERT LANGER:

1. That is done and most appear to be single stars. Still, they could have been in a binary system before.
2. Other mixing mechanisms might act in these close binaries, e.g., tidal mixing. I would be careful to conclude anything about mixing processes in single stars from them.
3. It might only apply to components of close binaries (see point 2).

Q. No. It has been shown by J.P. Zahn that the timescale of angular momentum transport in massive main sequence stars is too short to allow a decoupling of core and envelope.

Proffitt (p. 355)

GILLES CHABRIER: In the collision integrals, the potential is screened by the ions and by the electrons. How do you take that into account?

CHARLES PROFFITT: Yes, all charged particles are taken into account when calculating the Debye screening length, which is the relevant screening length for the Sun and the metal-poor turn-off stars.

Arnett (p. 389)

BENGT GUSTAFSSON: Could you speculate concerning how your results would change qualitatively when you will get 3D hydro simulations?

DAVID ARNETT: With 2D, the angular momentum vectors must be perpendicular to the plane, so that vortices are 'pinned.' This tends to exaggerate them. In 3D, they would interact in a more complex way and break apart more easily. The NOVA experiments are really 3D, of course, and we are beginning a study of the 2D/3D differences for the Rayleigh-Taylor/Richtmeyer-Meshkov instability.

ROBERT KURUCZ: Norris quoted you as saying that most supernova calculations are duds. That reminded me that most supernovae in nature might be duds. We see only the successes. Modellers force their calculations to give the answers they expect (I do). If your calculations produce duds, they might be real. For example, wouldn't your asymmetric convection yield asymmetric explosions?

DAVID ARNETT: Our understanding of the event rates of supernovae, supernova remnants and pulsars has shown steady improvement over the years. While your question would have had some force some decades past, it appears now that 'silent supernovae' are not so common. We have long expected that some of the duds became black holes. As for asymmetric explosions - they tend to become spherical as they expand, that is, toward a Hubble law for velocity. For me, the point of the asymmetries caused by convection is that they should give rise to a breaking of symmetry in the explosion.

Leggett et al. (p. 429)

JOHANNES ANDERSEN: You referred to your sample as a 'local' one, but the galactic orbits of 'local' F dwarfs show that they originate from a wide range of galacto-centric distances. Can you do a similar analysis for the white dwarfs?

SANDY LEGGETT: Unfortunately we do not have radial velocities and so we cannot calculate UVW velocities. The tangential velocities only show that they are old disk stars.