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Round table discussion of session B: observational probes of convection

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Abstract. A summary of the round table discussion following Session B on *Observational Probes of Convection.*

The round table discussion panel for Session B on *Observational Probes of Convection* comprised the following speakers: J.D. Landstreet, A.G. Kosovichev, M. Asplund, H. Kjeldsen and G. Cauzzi and was chaired by B. Smalley.

As well as the high-quality observations discussed during the oral presentations in this session, there were several interesting posters presenting various observational tests of convection theories.

The discussion started with an observer's view of convection which traditionally manifests itself as microturbulence, macroturbulence, line asymmetries, etc. Most observers do not care about convection when performing their analyses and still use 1-d mixing-length models, with their free parameters (α, ξ_{turb}). As such, they are free to choose unphysical values ($\alpha < 0$, $\xi_{turb}^2 < 0$), but in practice they use whatever values were used in the original model atmosphere calculations. While 3-d models give wonderfully self-consistent abundances without the need for these free parameters, this type of analysis is impractical for analyses of large numbers of stars. Observers need a prescription for the 1-d parameters, based on 3-d analyses, including what spectral lines and regions to use or avoid for 1-d versus 3-d effects.

J. Landstreet pointed out that microturbulence and macroturbulence actually have recipes: microturbulence is determined so as to make abundances concordant among weak and strong lines of a particular element, while macroturbulence is obtained from fitting many line profiles. They are adjustable parameters, rather than totally free parameters. Like abundance, these are parameters that you fit with the data and can be tested by the internal consistency of the results. That doesn't answer the question as to what connection these parameters have to real 3-d convection that occurs in real stars. That is a different step. But, the experimental definition of them is fairly clear.

M. Asplund objected to putting microturbulence and abundance on the same par. Microturbulence is a simplistic approximation to the reality that assumes you can describe all the 3-d geometry and velocity structure and how it varies with depth, with one value. Then you assume that this value is going to be the same for all spectral lines and all different elements. It is very unrealistic. You may get something that appears reasonable, but you can never really know that you get exactly the right value and the right result when you derive elemental abundances. That is the basic problem. Abundances are something that is real, but microturbulence is not.

J. Landstreet replied that there are exceptions. Diffusion occurs even within stellar atmospheres, e.g. HgMn stars, and in these cases the abundance is not a single number, but a complicated model.

I. Roxburgh commented that the mixing-length (parameter), if you want to use it, also needs to vary with height. And indeed does when you use a Kurucz model relative to an interior model. B. Smalley thought that the mixing-length is the hardest parameter to fix, because it cannot be done observationally. M. Asplund stated that mixing-length does not occur in 3-d models.

J. Toomre then asked about the weaknesses of the current generation of 3-d models. M. Asplund replied that one weak point is the radiative transfer, since you have to use fairly crude assumptions in order to do it computationally. Millions of frequency points have been binned together in terms of wavelengths that behave similarly in their opacities. The radiative transfer in the simulations use around 4 or 5 different opacity bins. That is a very crude assumption. It is, however, not that bad. We check it on regular intervals by doing the full monochromatic radiative transfer and it agrees well to within 1% when it comes to the radiative heating, which is the important thing. When it comes to the abundance analysis, another weak point is the NLTE calculations. In particular, the atomic data needed are actually not that well known. The radiative data are fairly well known for the most important elements, but the collisional data are very poorly known. Many, though not for the case of oxygen, rely on very classical recipes developed in the 1960s, not full quantum mechanical calculations. Fortunately, there are a few people doing these full quantum mechanical calculations, Paul Barklem in Uppsala for example. This is something that we would like to improve, but we need to convince atomic physicists that this is an important problem.

J. Christensen-Dalsgaard recalled that simple parameters have been measured for lots of different stars. Therefore, if one could somehow make the relationship between the simple parameters and the complicated models, one would have some diagnostics of the properties of convection in these different stars. We cannot do detailed simulation of each and every star. This would be somewhat similar to the calibration of mixing-length in terms of simple models by comparing with selected simulations.

While V. Canuto thought that the use of mixing-length was like flogging a dead horse, the α parameter does have a physical meaning. The mixing-length has two problems; one is it is local and the other is that it only takes one eddy into account. The Canuto & Mazzitelli model gives some improvement to the latter. Non-locality is much harder to compute. This is what α represents, even if it is a fudge factor. His feeling was that it is not a multiplicative factor, but something to add to mixing length that is required in order to take account of non-locality.

As an observer who uses mixing length as simply to decide which model to believe, T. Bedding remarked that astroseismology is starting to constrain it. He cited his recent work on the metal-poor star ν Indi as an example. We are now beginning to put some constraints on which value of the fudge factor is the real one.

The next discussion was started by the chair who commented that in some respects Solar observations are too detailed when compared to the best stellar observations. He suggested the thought experiment of move the Sun to 1pc away. What would we still be able to determine from observations and would they be consistent?

H. Kjeldsen recalled the echelle diagrams of the Sun shown during his talk. These showed the Sun observed like a star. In stars, we have a diagram that is sensitive to the model, and can reveal the depth of convection zone, the stellar age, the density and therefore, if you have some other parameters, the mass. You can also see granulation directly. The Soho satellite has an instrument that actually observes the Sun as if the Sun were 10 pc away. You see the whole power spectrum from convection. If you use the models by H.-G. Ludwig you can really learn about the details of convection.

From all the discussions, it was clear to K.L. Chan that mixing length is not useful at all for the surface of stars, but what about deeper inside? In stellar evolution calculations mixing-length is applied all the way into very deep regions. It does change even though it is almost adiabatic – it is not fully adiabatic. Going toward the bottom of the convection zone, there is about 2 or 3% change in the adiabaticity. Maybe some kind of recipe or mixing-length theory in the deeper region of convection is still necessary.

By putting the Sun at 1 pc, D. Darvins stated that one parameter that is still observable is spectral line asymmetries and, in principle, also wavelength shifts. Arguably they are easier to measure than the power spectrum of intensity fluctuations. He continued, stating that we have to find observables that are falsifying theory. The point is not to have observations that fit theory because that will not make any progress. We must find something that can be predicted and cannot be fiddled with. For example, microturbulence and macroturbulence are already useless because they can be fiddled with in the modelling. If one makes a detailed prediction of line asymmetries, and how they vary among lines of different parameters, then that is an observable. This has to be predicted from a model and if a model does not fit one can go back and change the model. This is the sort of thing that has to be identified. That way we can move out well beyond 1 pc, a 1 kpc or maybe even as far as the Magellanic Clouds.

J. Landstreet stated that while it is true that models work, if you are close to the Sun, it is not true across the whole HR Diagram. If you look among the A stars, the numerical models do not fit the observed macroturbulence. There is a tendency to say that we have fitting the Sun so the model works, but there is a more extensive parameter space than that particular $T_{\rm eff}$ and $\log g$.

The final topic discussed was what observations do modellers require to help constrain their theories.

M. Steffen suggested another test that had not been mentioned; taking a small patch of the granulation pattern and recording the time-dependence of the line profile. G. Cauzzi stated that she had an example showing line profile changes with time. It showed very interesting episodes of, what could be described as, extreme acoustic oscillations. That is something that could be compared with the models.

As an observer, G. Cauzzi stated that the surface convection simulations work very well, but they work only for a small fraction of cases. As soon as you move out of the most classical environment, you get magnetic fields that change the whole picture. This is evident in recent observations of chromosphere lines which have different motions and periodicities.

M. Asplund added a quick comment that he had tried to get centre-to-limb observations, but it is very difficult to get data close to the limb due to problems with scattered light and the finite slit length.

Despite decades of solar observations, H.-G. Ludwig wondered why there was no high-quality atlas of the solar spectrum at various limb angles, with a quality level similar to the famous Kurucz *et al.* Solar Flux Atlas.

I. Roxburgh noted that as far as stellar evolution is concerned what matters is convection in the middle of the star. Helioseismology is the extracting of knowledge of the interior of the Sun. We need high precision observations of the oscillations of a wide variety of stars, either from space (e.g. Corot) or ground-based networks (e.g. GONG).

There was a comment about the revised solar abundances. The disagreement is not with the seismology but the solar structure. J. Christensen-Dalsgaard discussed the adjustment of solar structure models to make model sound speed match the well-determined values from seismology. The effect of the changes in abundances are almost all due to changes in the opacity, because it is only opacity that is significantly affected by a change in abundance. We can always solve the problem by changing the opacity back through atomic physics. However, we would need to change some of the opacity calculations by something of the order of 15 or 20%. It would be an amazing coincidence if we could get back to the original models. Convective overshooting can possibly extend the nearly-adiabatic layer somewhat and that would have some effect on the sound speeds. But, it would not extend as deeply as we require to remove the difference between the models and the helioseismic sound speeds. “I guess Dave Arnett is going to tell us the truth, the full truth and nothing but the truth tomorrow. Until then I’m puzzled.”

Next, D. Arnett gave a brief preview of his talk. It’s certainly true, that if one relaxes the constraint that the standard solar model is static, and includes gravity waves and entrainment at the lower convective boundary, you can move the lower convective boundary down to where helioseismology says it is. So far so good. The difficulty is that with increased wave motion you have an increased diffusion, and the standard solar models are already put at 80% rather than 100% of the calculated diffusion, because you end up with too little helium in the solar convection zone. It gets worse with the wave motion, because that enhances the diffusion. So, there is another problem. This might very well be solved if we admit that the Sun is actually complicated and doesn’t fit in all the isolated little boxes we have been talking about – it rotates. The shear at the bottom of the convection layer is about ten times larger from rotation than from convection. and rotation can give stirring which counter acts the diffusive settling. Hence, there is a possibility of developing a global model in the future.

Observations continue to improve in their quality and these continue to challenge our theoretical understanding of solar and stellar convection.