The A-Star Puzzle
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Panel discussion section C

CHAIR: **John D. Landstreet**

SECTION ORGANIZER & KEY-NOTE SPEAKER: Friedrich Kupka
INVITED SPEAKERS: B. Smalley, B. Freytag
CONTRIBUTION SPEAKERS: S. Vauclair, M.K. Browning, R. Trampedach

Discussion

KUBÁT: What are the differences between both codes for convection simulations without magnetic fields which we have just seen?

FREYTAG: Both codes solve the same set of basic equations and rely on very similar fundamental assumptions. However, the algorithms used to solve the hydrodynamics or the radiation transport equations differ. The codes have no routines in common. There is an ongoing project to perform a simulation of solar granulation with both codes, relying on the very same settings (grid, model extension, equation of state, opacities, ray system, etc.). The remaining differences are tiny, for example, much smaller than the difference between the 2D and the 3D models. Both codes have some extensions (for instance dust or magnetic field) not (yet) found in the other.

TRAMPEDACH: I can only agree with Bernd Freytag. I am actually not taking part in the detailed comparison between the results of the two codes, but everything looks very similar. I think most of it has to do with concerns about stability. I believe they have higher inherent stability in their code than we have for example. That makes it easier to run. But the results are quite similar.

PISKUNOV: How deep is the temperature inversion below the photosphere?

TRAMPEDACH: It is not very deep, about a megameter or something below the optical depth one. The temperature is about 8000 K to 10000 K.

CUNHA: To Smalley: Have you any idea how the prescription for the efficiency of convection as a function of effective temperature changes for the magnetic stars? If not what is preventing one from determining this using similar techniques?

SMALLEY: The prescription has not been tested for magnetic stars, but in principle one could be developed. However, the effects of a magnetic field on the atmospheric structure and the surface inhomogeneities may make this difficult. Nevertheless, given sufficient fundamental parameters for these stars one should be able to investigate this.

VAUCLAIR: Could you comment a little more about the possible constraint on convection we may have from asteroseismology?

KUPKA: Showing two figures skipped during his talk: For the Sun, helioseismology has already taught us a lot about the convection zone of the Sun. Inversion techniques tell us about the lower boundary of the solar convection zone and its location. We obtained information about the transport of angular momentum and differential rotation in its interior. The numerical simulations by Miesch *et al.* (2000) already reproduce a lot of the inferred properties at equatorial to mid latitudes. It would be marvellous if asteroseismology could help us to gain such kind of insight into the stellar interior of other stars.

VAUCLAIR: As asteroseismology can only detect low order modes, I do not think the resolution will be sufficient to get such a detailed picture, but it may tell us about the deep interior.

KUPKA: It may be also useful to study the interaction between convection and pulsation, as it may be expected for δ Scuti stars.

COWLEY: Can anyone comment on the possible relevance of solar umbral granules? It can be seen that we can have convection or something causing these granules even in the presence of kilogauss fields.

LANDSTREET: In the Sun the filling factor for most of the surface is very small. The gas motions easily dominate the average field and the result is that the gas motions force the field in some way into small flux tubes. The situation in the magnetic A stars is very different in that the filling factor of the field as far as we can tell from observations is 1.0. The field at least has a comparable energy density to the gas that in many cases dominates in the photosphere. So the situation is very different from the Sun.

TRAMPEDACH: If you just look at pictures of sunspots, close-ups, you can see how the granules are distorted and heavily affected by the field. No question about that. I do not know whether there have been hydrodynamic simulations of that yet. Because as you increase the field strength you really shorten the time scale and the problem becomes very hard to handle and simulate. Just with the solar field strength, for the normal fairly quiet Sun, you get a smaller granular effect. So, the field definitely changes the dynamics. We have also tried to increase the overall field strength and actually saw convection pretty much shut off. So magnetic fields have a profound effect on convection and have to be included.

MICHAUD: To Freytag: In your simulations you showed that the zone between the He II and H I convection zones is efficiently mixed in similar stars. Can you say something on what to expect for the mixing above the Fe convection zone at $T \approx 200000\,\mathrm{K}$ up to the hydrogen convection zone. If we try to extrapolate in it a little further, to slightly higher temperature stars, where there is no H convection zone, is it then compatible that the difference between the Am/Fm stars and the HgMn stars will be that about an Fe convection zone? There will be complete mixing when there is also a hydrogen convective zone, but not when the H convective zone disappears completely, at 10000 K.

FREYTAG: The layers between the He II and H convetion zones are easily mixed by overshoot from below and above. The Fe and H I convection zones are much further apart. The layers in between are much harder to mix completely. Overshoot from the H I convection zone would reach down some pressure scale heights. The rest of the mixing has to come from the Fe I convection zone below. That may perhaps be possible if this convection zone has large velocities and spatial scales AND the relative layers above are a "soft" boundary. A rough estimate could be derived from an envelope constructed relying on the classical mixing length theory.

KUPKA: As far as I have seen from the paper by Richard *et al.* (2005), these Fe convection zones in the early A stars are just border-line to being unstable. In the paper it was said that if there is an Fe overabundance by a factor of 3 the zone is not yet convective, only for a factor of 4 it is. So I would expect that this is sort of not very efficient convection and when we estimate the velocities to see what happens, we can be very far off using a simple model such as classical mixing length theory. Also, if you include both the H zone and the Fe zone in a big simulation box, then the stratification is nearly radiative throughout most of the box and to obtain results not affected by the initial conditions, you may have to run simulations for that case for long timescales, which is probably unaffordable at the moment. Non-local models of the type I have discussed in my talk might be useful in that case: at least for a set-up containing the H and He II convection zones we found a qualitative agreement and a quantitative one within a factor of 2 when comparing to simulations and they always underestimated the extent of convective mixing, so their results might be considered a lower limit.

LANDSTREET: Changing a topic somewhat. When you look at the middle A stars, you try to measure the photospheric convection using microturbulence, you find normal A stars having microturbulence of the order of 2 km s^{-1} and Am stars with microturbulence of 4 - 5 km s^{-1}. Has anybody an idea what could produce this kind of rather important velocity difference between these two not that different atmospheres?

KUPKA: When we computed the non-local models, one of the runs was for 3 times the solar metallicity and indeed velocities did change in the right direction [increasing], but not to the extent that we had expected. It would be interesting to repeat these experiments using more realistic opacities [i.e. suitable for Am stars].

SMALLEY: Just a comment on rotation. Am stars have slower rotation compared to "normal" stars. Interestingly, the microturbulence of those [Am] stars is higher. Maybe microturbulence decreases with rotation?

RYABCHIKOVA: I always read in the literature about 4 to 6 km s^{-1} microturbulences in Am stars. However, fitting the profiles of strong lines in extremely sharp-lined Am-stars is impossible with such high microturbulence, it needs 0 to 1 km s^{-1} and no more.

LANDSTREET: I can comment on it. It is not that you can fit with some combination of microturbulence and rotational velocity. You just do not fit with those very strong line profiles very correctly. So when you determine the microturbulence as we do it simply by fitting the equivalent widths, we do not correctly reproduce the actual shapes with any combination of microturbulence or rotational velocity for the very strongest lines in very sharp lined stars.

RYABCHIKOVA: But this means that the real microturbulence is not 4 km s^{-1}.

TRAMPEDACH: Yes, if we are not looking at the line profile, and just fitting the equivalent width. There is not necessarily a relation between the actual velocity fields and the derived equivalent widths and derived microturbulence. I find that a little dangerous, actually. So, please, look at the line profiles when you fit. Fit the spectrum instead of the equivalent widths. We will get a lot more physics out of that.

LANDSTREET: There is hardly any A star which has sharp enough lines to actually see these details in the line profile. So even very slowly rotating ones for 5 - 10 km s^{-1} wash out the shape of the line profiles with the rotation profile.

DWORETSKY: Microturbulence is a fitting parameter, not a direct measure of any actual microturbulence or streaming motion. Many years ago we astronomers found that, for example, including more realistic opacity yielded $T - \tau$ relationships that reduced considerably the microturbulence fitting parameters for curves-of-growth. So it may or may not have anything to do with actual gas motions.

COWLEY: Numerical models explain the need for a microturbulence in a 1D model which does not have velocity fields (by assumption). But if the calculated profile does not fit the observed ones with an assumed $v_{\rm turb} \sim 5$ or 6 km s^{-1} or whatever, then there is clearly something wrong with its determination. Years ago, Myron Smith got $v_{\rm turb} \sim 7$ or 8 km s^{-1} in Am stars and in that case it is clear the problem was with the f-values. The log gf's were OK for strong lines but too small for weak ones. I suggested an error in the way $v_{\rm turb}$ was determined. This does not mean one does not need a $v_{\rm turb}$ in a classical 1D calculation.

LANDSTREET: There are also large scale velocity flows and what is missing for the microturbulence velocity is basically macroturbulence. That produces a kind of asymmetry that we observe in a few of very sharp lines. So it is that what is completely missing from even the model with microturbulence.

COWLEY: Yes, the classical macroturbulence does not affect the curve-of-growth, it only affects profiles. Whereas the classical microturbulence affects the look of the curve-of-growth and the line profile. So if you do not get a correct line profile, you have something wrong. You have not accounted for the velocity correctly.

LANDSTREET: The classical model does not have macroturbulence, the one that I use for the fit is incorrect. It does not have macroturbulence in it. When I put in an empirical macroturbulence, a completely parametrized macroturbulence plus a variation of microturbulence with height, I come much closer to fitting the observed profiles.

COWLEY: I am curious about these sharp lined stars. Are there stars with lines sharper than those of say, 63 Tau and 32 Aqr? Because when I look at those stars I always see a lot of lines with a little square. Just only DAO spectra. Now there are much better spectra than what I had. It is a big impression, but it was a pretty definite one relative to sharp lined HgMn stars. I never saw such a squarish profile. The profiles are wider, although at least strong line profiles are certainly wider than in the HgMn stars, but it is not a rotational velocity in the very sharpest line stars.

LANDSTREET: The numerical models do not yet reproduce the line profiles in the A stars.

TRAMPEDACH: I have not calculated lines yet, so I do not know. We might be missing something essential in the simulations.

PISKUNOV: I was wondering, if we can put some efforts together and bring the magnetic fields generated in the core to the surface, with perhaps the help of David Moss. I will be more than happy, if the residual spatial frequencies of these magnetic fields on the surface will have a maximum in spherical harmonics between 10 and 20.

MOSS: Certainly, the expectation is that for a field to rise from the core to the surface within a Main Sequence lifetime requires a strong field in thin tubes. Then somehow this “spaghetti-like” field has to organize itself into the observed kG+ strength global fields that we observe. This looks like a real difficulty. Remember that some Bp stars are observed to be strongly magnetic at ages $\sim< 10^7$ yrs! Here the problem is accentuated. Other problems are the lack of universality of A star fields, given the core dynamo mechanism should be universal, the lack of correlation of field strength with rotation period (in general, one might expect $|B|$ to increase with Omega for a dynamo), etc. Core dynamos almost certainly operate, but their connection with the observed fields seems unlikely.

PRESTON: One word about damping constants: We are talking about the fitting of strong lines. Were the damping constants included in the calculations?

LANDSTREET: We have got three different independent codes that agree on line profiles to very close tolerance. I think we all included the damping parameters correctly.

References

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