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Discussion on solar-like oscillators and γ Doradus stars

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Gough: Perhaps we should go through what we've heard today, and see what issues were raised. Just one point (before I forget): the kind of precision that Günter succeeded so marvellously in obtaining for things like the ionization of helium in the Sun is much greater than the precision we can expect to get from other stars. The high precision enabled him, in particular, to distinguish (barely) between the subtleties of the fits of various formulae to the data. The issue especially for other stars is just how to model a seismic signature with an appropriate functional form, particularly when several different formulae appear to fit the less precise data equally well. If one wants to calibrate other stars and extract quantities like the helium abundance, Y, it is essential that Y is contained in the signature in the right way. One could simply fit stellar models using Y as one of the several parameters that have been chosen (often for computational convenience) to define them, but how does one know whether the outcome is biased by other, hidden, variables? I don't think Günter stressed in his talk the importance of relating in a robust way the seismic signature directly to the physical quantities one wants to determine.

Perhaps we should refresh our minds and get back to what we've learnt from γ Doradus stars. They are very useful because they are g-mode pulsators. One of the important issues is what excites them. This appears to be at least partially understood now, which I thought was great, but there remain some physical principles that aren't understood. To be sure, there was modulation of the heat flux, and there were important contributions from convection; and one thing we heard today was that convection is very important, and those who ignore it, at their peril, get results that are at least suspect. But how could theorists improve the situation?

We have theories - not very many - that attempt to address variations of the heat flux and the diagonal components of the Reynolds stress, but for g modes the non-diagonal components are also important, as Marc-Antoine said in his talk. As far as I am aware, to estimate those components in terms of the others in the theory requires the introduction of more parameters, a procedure which is always worrying. For then one must question whether the formalism is predictive? I don't know how to answer this, but I am asking for comments on the theories, and how one might then carry out even a half-way meaningful calculation. Fritz has been absolutely silent on this issue; I wonder if you have anything to say?

Kupka: The existing simulations (star-in-a-box) describe how a star evolves, which is not very useful in this case because convection occurs at a very small scale. We can't resolve this in such simulations. The other case is a box-in-a-star where we resolve the up- and downflows. The problem with the g modes is that the size of the waves is much larger than the boxes. You will have to set artificial boundaries and take care to connect them properly in the case of these stars. So I don't think it would be easy to make simulations that are useful for testing this idea.

Kaye: The dynamical models, which are numerical models, and which are more than a sketch on a piece of paper, have not been around for that long. The reason for this is that we started with the frozen-in convection, simply because it was the easiest thing and in fact the only one that was available. As we progressed further and put in physics that must be included we thought that qualitatively that description is OK. I think this is what Marc-Antoine tried to say. He did not say that we know everything and we understand all the frequencies and amplitudes etc. But what he did say, at a very qualitative level, is that we know what the basic driving mechanism is. And if you go back, say, five years, we couldn't say that.

Dupret: The time-dependent convection treatment I included in my non-adiabatic models is a perturbation of the mixing-length treatment, so of course there are approximations in this treatment. But what we saw is that if you include the effects of convective flux variations and turbulent pressure variations, this does not change significantly the driving. The basic mechanism remains the same: a periodic flux blocking at the base of the convective envelope driving the gravity modes. There is still the problem of how to model the non-diagonal components of the Reynolds stress variations. We have begun such work but the predictions depend strongly on unknown parameters, and it is not yet possible to conclude. It would be fine if we could get information on the coherent interaction between convection and pulsation from numerical simulations, but I am not sure how far this would be possible at this time.

Montgomery: Here, the relevant time scales, the pulsational time scale and the convective turnover time scale, are of the same order. There are some stars where things should be somewhat easier. If you look at pulsating white dwarfs, the convective turnover times are of the order of a second and the pulsation periods are a few hundred seconds. Therefore you can treat the convection zone as instantly adjusting. Then all we need to care about is what is the thermal re-adjustment time in the convection zone. You can show that the dissipation due to the turbulent viscous pressure is small because the mixing is so rapid that the eigenfunctions are flat. So anyway there are regimes where you can test things and in which you don't need a time-dependent model of convection in order to do convection in a pulsating star necessarily. So this is the opposite of frozen-in convection, this is the infinitely-easily-able-to-adjust-to-the-pulsation-conditions convection.

Gough: Mike came to Cambridge as a man who was keen on using observations to understand the world. I seem to have converted him into a theorist, because his answer was: this problem is too difficult, don't think about those stars; let's go for something easy!

Dupret: For γ Doradus stars, near the bottom of the convection zone the time scale of convection is close to the period of the pulsations and it is smaller in the upper layers. Therefore we cannot use just one approximation; between the two extreme cases is a complex region where we have no choice but using time-dependent convection models.

Roxburgh: Two comments. First, qualitative agreement is not enough, we need quantitative agreement. We have quantitative frequencies and the properties of the oscillations in order to make inferences about the star. Qualitative is not enough. The other point is that if you want to test a theory, or a model, such as a time-dependent theory of convection, which can't be tested in the the parameter domain of the real star, you can still test the concept against the numerical simulations in different parameter regions.

Kaye: I agree that qualitative is not enough, but to be fair we need to give the theorists more than a few dozen stars that are uniquely identified and that have enough frequencies. Since they are all hovering at period of about 0.8 days (which is the mean), it's extremely difficult and you simply must have spectroscopy to back it up. I never thought that, as I heard from Jaymie, we now have three stars that are both γ Dor and δ Scutis, but what are the chances that they are all Am and they are all single? It's not fair to put higher burdens onto the theorists before we can provide them with sufficient data to work with.

Gough: Perhaps we should move on, although still sticking with the γ Doradus stars. We've been talking about the frequencies, but we need to know what the modes are. The frequency ratio method (which should really be a period ratio method because it's the periods that follow the simple asymptotic relations) is used by some people to fit the data. It is based on taking just the leading term in the asymptotic eigenperiod formula, and is analogous to what we did in helioseismology with p modes. What we found immediately is that in practice the use of such a crude approximation doesn't work. To get anything right, or at least anything that looks like a correct result, one must take at least the next term in the asymptotic formula into account. Now for the g modes that comes from two integrals. The first depends on the sound speed and the acoustical cutoff frequency (which depends on the density scaleheight, not to mention the density itself via its influence on the gravitational potential); the second comes from the nature of the transition beween the radiative interior and the convection. So there are (at least) two important uncertainties in the extended formula, and these need to be inferred from the data, in preference to trying to fit the over-simple formula that is in current use.

Kaye: The problem has been, really, the lack of concentrated, detailed spectroscopy. It's easier (not easy) to combine photometric campaigns than to combine spectroscopic campaigns. The number of spectroscopic campaigns of γ Doradus stars that I am aware of and where mode identification was later tried used the moment method etc. But there was only one case where it was possible to model in detail the line profile variations and to go through various steps to do mode identification, and that was Conny's work.

Gough: Well, you are talking about a star that only has one, unidentified, period, and I am not sure how much we can learn from that. Conny has identified the surface structure of the mode, which is just what we need for learning about the structure of the star; of course, the so-called frequency ratio method is aiming at obtaining just that too, but more indirectly. The issue I am adressing now is whether there are some stars with several frequencies that are determined accurately enough for us to say something about their rightful positions in the spectrum.

Weiss: Not yet. We know from MOST that the data are coming and there are several candidates for γ Dor/δ Scuti hybrids and for bona fide γ Dor stars. So maybe in two years from now we should have a γ Doradus conference.

Gough: I look forward to that. Referring to the Sun again, we learnt an awful lot in the early days when we were struggling with understanding the broad Lorentz-like spectrum of global-scale observations. However, one of the principal observers came along with his interpretation that what was being observed was a spectrum of only $\ell = 0$ modes, because to him that was the simplest thing to say, and in science simplicity rules. To some of us, that was evidently wrong even at the time, and indeed now we all agree on that. But I noticed that basically we heard exactly that this afternoon: 'It's probably a white-dwarf spectrum because all these stars must be the same'.

Bedding: For the K giants, I'd like to take the opposite view that none of them are multiperiodic, but all that we see is a broad Lorentzian envelope. For every one of them, if you look at the power spectrum, they're all the same.

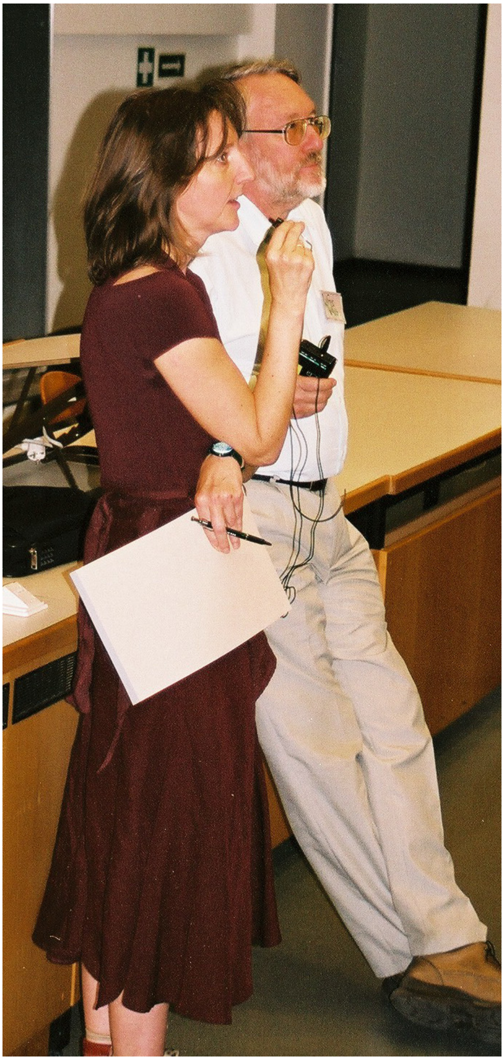
Gough: Just like the Sun!

Bedding: Well, no. Like one mode in the Sun that you haven't observed long enough. I think Artie was arguing that there is no K giant with a resolved frequency spectrum, unless we get many, many months of observations, which we will get from COROT and probably Kepler. I don't think we have enough information to say that these stars do have multiple modes.

Gough: I have one simple request: the single mode you are observing: please identify it!

Matthews: On behalf of Thomas (Kallinger) and his poster on MOST observations of K giants, we tried to demonstrate that the multimode identification of the star HD 20884 is inconsistent with damped oscillations because the peaks aren't following a Lorentzian profile. Conny and her students like Saskia Hekker have argued from line-profile variations that there is evidence for nonradial oscillations, so I think there is growing evidence for that.

Beta Cephei and Slowly Pulsating B stars



Conny Aerts and Werner Weiss.