Part 7. Meeting Summary

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Radial and Nonradial Pulsations as Probes of Stellar Physics
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Meeting Summary and Outlook for the Future

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1. Introduction

There is a theatre company in London that presents all 37 of Shakespeare's plays (abridged) in 97 minutes. With about 70 talks and 140 poster papers presented at this meeting, I must attempt a similar performance here. Obviously, I will not succeed in this, so I will concentrate on some of my favourite subjects and on particular talks and posters that particularly caught my attention during the meeting. This was the largest, best attended and most successful pulsation meeting that I have attended.

My experience with stellar pulsation conferences and meeting sessions goes back to 1973 when I attended my first professional meeting, an American Astronomical Society meeting in Tucson, Arizona. I was a graduate student at the University of Texas at the time, and a group of us camped out for that winter meeting (I doubt that any of the students at this meeting are camping). I remember the name of the campground well: "The Crazy-Horse Outside-Inn". The temperature at night was below freezing and we grad students squeezed into a small camping tent late at night, got into our sleeping bags to try to keep warm and practiced our talks for the next day by gas lantern light.

My own talk was a 10-minute presentation on pulsation and abundances in evolved Am stars, then known as δ Del stars, now more properly called ρ Pup stars. (Sylvaine Turcotte referred to these very stars in his review of chemical peculiarities and pulsation.) Several students during this meeting have said to me that they were disappointed not to get questions at the ends of their talks, and that was clearly going to be my fate at the end of my first talk, too. But the chairman of the session was Martin Schwarzschild, and he, like Tim Bedding at this meeting, had a habit of asking a question of the speaker if no one else did. I later met Martin again on several occasions and came to know what a kind and gentle man he was, but he had a formidable presence and a strong accent, in spite of all his years in the US. He looked at me and said very loudly, "*What* are these stars reason to *pulsate* ?"

I thought I knew the answer to that question, but I also knew that if Martin Schwarzschild didn't know, then I was in trouble with my answer. The session had had half-a-dozen papers on proposed driving mechanisms for pulsation in β Cep stars, so I quickly said, "I'll defer that question to one of the theoreticians". Someone gently said, "They are in the instability strip, Martin", to which he said innocently, "Oh! I missed that." I was deflated and sheepishly crept off to take my seat for the next talk, ears burning.

Still, Martin's question is one of the defining questions of our field: What are these stars reason to pulsate ? This question was addressed during this

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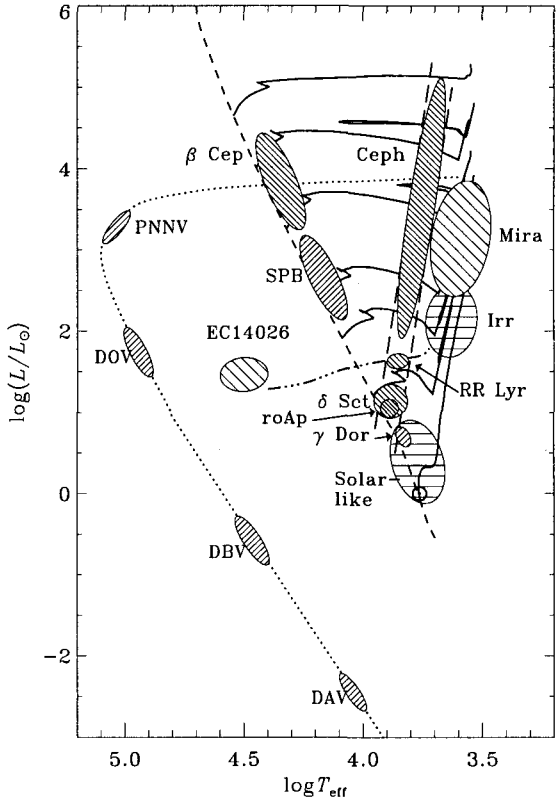


Figure 1. A pulsation HR Diagram showing the positions of many classes of pulsating stars. Courtesy Jørgen Christensen-Dalsgaard.

meeting for several classes of pulsating stars. As I was asked during the week to present an HR diagram and point out where the various classes of pulsating stars lie, I include here a pulsation HR Diagram of our SOC chairman, Jørgen Christensen-Dalsgaard, in Fig. 1.

2. Some research in Leuven

Along the upper main sequence the regions of the β Cep and Slowly Pulsating B (SPB) stars are sketched in Fig. 1. It was the problem of the driving mechanism in the β Cep stars that was being so vigorously debated at that long-ago meeting in Tucson. That problem was nicely solved by Wojtek Dziembowski, Aloisha Pamyatnykh and Pawel Moskalik (Dziembowski et al., 1993; Dziembowski & Pamyatnykh, 1993) when they showed that with the new OP opacities pulsation is driven in those classes by a metal opacity bump at 2×10^5 K. However, at

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the time they produced their first instability strips (actually instability boxes) for the SPB stars, there were very few SPB stars known. It was largely the observational work of our hosts here in Leuven, Christoffel Waelkens, Conny Aerts (the hardworking chairwoman of the LOC) and their colleagues that satisfyingly showed that the positions of over a hundred SPB stars so neatly filled the predicted instability box (see Fig. 1 in Waelkens et al., 1998).

But pulsation driving in other classes remains a problem. Art Cox, in his introductory talk at this meeting, mentioned the problem of driving in the Luminous Blue Variables (not plotted in Fig. 1, but further up the main sequence), GW Vir stars (also called PG 1159 stars, and labelled in Fig. 1 as DOV – meaning variable DO white dwarf – stars), and in EC 14026 (sdBV) stars. Stéphane Charpinet explained the latter as driving from a metal opacity bump in a layer of enhanced Fe abundance caused by radiative levitation, but it would seem that Art retains some doubts about that. The big discussion about driving mechanisms at this meeting concerned the γ Dor stars, and I will return to that subject later.

3. The very small and the very large

Johannes Ruoff gave an intriguing discussion about the remote possibility of detecting oscillations in gravitational waves from non-radial pulsations in neutron stars, particularly from r-modes and w-modes, which are oscillations in space-time itself from frame-dragging. YanQin Wu in her poster paper predicted, from considerations of the crust, that the r-modes would not be detectable in gravitational waves. This certainly represented the furthest look into the future of any observations discussed at the meeting! For the rest of us, and for all the other stars, I was amused by Johannes's description of all the classes in Fig. 1 as "ordinary Newtonian stars".

The biggest of those "ordinary" stars, the Mira and luminous AGB stars, were reviewed by Erwin Sedlmayer who showed intriguingly good 2D theoretical simulation cross-sections of the circumstellar shells. These interact in a complex way through dust absorption with the variable luminosity of the underlying pulsating star. One of the 2D theoretical cross-sections bore a striking resemblance to the face-on (actually 2D) disk in the HST picture of the Eskimo Nebula. The presentations of Jacco van Loon, Susanne Höffner, Thomas Lebzelter and Anja Andersen showed how fascinating work on these stars is, and Bernhard Aringer presented some beautiful animated infra-red spectra over the pulsation cycle of some Mira variables. The latter, however, were frighteningly complex, with over a million H_2O lines alone, not to mention the hundreds of thousands of lines from OH, SiO, TiO and others.

4. Pulsation driving in the γ Dor stars

The γ Dor stars are a recently-discovered class of pulsating variables that pulsate in g-modes and lie either just to the red of the δ Sct instability strip, or overlap with it. Gerald Handler discussed HD 209295, which he suggested is the first star to be a member of two separate classes of pulsating stars: it is both a δ Sct star and a γ Dor star. If that is actually true, it has important repercussions for

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any theory of driving in γ Dor stars, since no other δ Sct star which lies within Handler's observed instability strip for γ Dor stars (see Fig. 1 of Handler, 1999) shows g-mode pulsation frequencies. However, HD 209295 is in a spectroscopic binary with an orbital period near 3 d, and Handler suggested that the g-modes may be tidally driven, so this may be a special case.

Driving in the γ Dor stars was discussed by Joyce Guzik and was the subject of poster papers by YanQin Wu and Wolfgang Löffler. The principal suggested driving mechanism is convective blocking of radiation in stars where the convective zone reaches down to $200,000 - 400,000$ K. This mechanism seems plausible for the early F stars, but does not work for the late A stars which are γ Dor stars, so an important observational test is to determine if there really are late-A γ Dor stars. Joyce Guzik predicted that, if her theory is correct, the hotter γ Dor stars will not be confirmed as members of the class. Gerald Handler, when asked about the certainty of these hotter γ Dor stars, said he was "not too sure". Here is a case where the question, "what are these stars reasons to pulsate ?", is alive and well.

Intriguingly, the presentation on diffusion theory by Sylvaine Turcotte and the poster papers by George Michaud et al. and Olivier Richard et al. suggest that there is a convection zone in late A stars at a temperature of 200,000 K from Fe opacity, and George even finds that there may be some driving at periods of around 1 d from this opacity. The excellent review at this meeting of overshooting and penetration by Jean-Paul Zahn showed how the outer envelope of A stars can be completely mixed from the Fe convection zone at 200,000 K right up through the He II and H I/He I ionisation zones which are linked by overshooting. This is not the same as a full convection zone to a depth of 200,000 K, as in Joyce's models, but it does raise some questions. Could there be two driving mechanisms in the γ Dor strip, once for the hotter stars (Fe opacity) and one for the cooler stars (convective blocking) ? Joyce said during her talk that the hotter γ Dor stars may mean that there "could be more than one mechanism". Are there γ Dor stars that show Am characteristics ? Are there F-type γ Dor stars that show enhanced Sr at λ4077 and λ4215, as in the F Sr stars ? Or, as George Michaud asked me, "Do any Am stars vary with periods near 1 d ?"

No one knows the answer to these questions, and they have considerable bearing on tests of proposed driving mechanisms for the γ Dor stars. The last question about g-mode pulsation in Am stars is essentially untested. No one has ever looked for it. Observing runs of a few hours on Am stars searching for δ Sct pulsation have been done, but those cannot say much about time-scales of 1 – 3 d.

5. How do we know what we don't know ?

One of the biggest problems in the theoretical understanding of the observed frequencies in many classes of pulsating stars is that of rotation, particularly rapid rotation. This subject was reviewed by Hideyuki Saio, and was a thread that ran throughout the meeting.

Monica Maintz discussed modelling line profile variability in non-radially pulsating, rapidly rotating B stars. She generated 20,000 models and used

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χ^2 tests to select the "best" models to get stellar and pulsational parameters. Stéphane Charpinet raised the stakes and generated 45,084 models in 4D for sdBV (EC 14026) stars. And Travis Metcalfe topped them all with over 10^5 models of the DBV white dwarf GD 358 to determine the central O abundance; he selected the best models "objectively" using a genetic algorithm. Ian Roxburgh asked the question of Stéphane, "How do you judge if the best fit is correct ?", and I would like to discuss that problem further here.

To start with I'd like to look at the problem in only two dimensions, *i.e.* for a Discrete Fourier Transform (DFT). There are many studies in the literature, and even some at this meeting, where there is insufficient understanding of how we assess the uncertainty in a frequency determination. Steve Kawaler in his look to the future of asteroseismology reminded us to "watch our spectral windows". The rule is actually easy: the alias pattern in a DFT of unequally spaced data, the window function, tells you what you do not know. If the difference between the highest peak of a window pattern and its next highest aliases is less than the noise level, then the frequency is uncertain. *Noise peaks have window patterns.* Noise and real signal are convolved in complex space, *i.e.* both have amplitude and phase. Hence noise may add to a real peak, subtract from it, add to an alias, subtract from it, or do nothing. So if the aliases do not differ from the central peak by more than the height of the highest noise peaks, then don't start developing theories to explain the frequencies – they are still uncertain. Also, for marginally detected frequencies, the presence of a window pattern says nothing about their reality, since noise peaks have window patterns (I intentionally repeat myself, as this message is widely ignored).

There is no getting rid of aliases without more data. There are techniques that hide the aliases, but those must be used with extreme caution. And that reminds me of a story: Once, when I was working at the University of Cape Town (UCT), a visitor came to morning tea and introduced himself as Jan Hogboom. He said he was a Swedish astronomer who was retired, but had worked in radio astronomy in the Netherlands, "messing around with Fourier transforms". His wife was a geologist and was working at UCT for some months in the geology department with my colleague and neighbour, Martin de Wit. I organised a tour to Sutherland for Jan, and then didn't see him again. But a few weeks later, my wife and I were invited to dinner at Mike and Conny Feast's house, along with Miller Goss, the director of the VLA, and Radhakrishnan, well-known for his work on neutron stars and pulsars. Both Miller and Rad were visiting on their way to a radio astronomy meeting in Mauritius. During dinner Rad said that he had done his first postdoc in the late 1940s in Sweden, so I asked him, " Do you know a Swedish astronomer named Jan Hogboom ?" He said, "*Do I know Jan Hogboom ? Do I know Jan Hogboom ?* He is my oldest, dearest friend!" I said, "Well, he is staying with my neighbour about 500 m from here." Rad expressed more than a little incredulity, so I went to the phone, called Martin de Wit's house and got Jan on the phone with Rad. With great delight, they made plans to tour the Cape Peninsula together the next day. When Rad returned to the dinner table, he asked, " Do you know *who* Jan Hogboom is ? You should! He wrote CLEAN."

Of course, CLEAN was written to clean up the spatial window pattern for radio interferometers. It was later adapted to clean up the spectral window

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patterns for frequency analysis, and many people seem to have forgotten that CLEAN does not make the window pattern go away. It simply hides it. For strong signals this is no problem. For complex signals, *e.g.* the solar frequency spectrum with a vast number of frequencies, it is a necessity. But you mustn't be fooled into thinking that the aliases have gone away. Only more data in the gaps can make them do that. If a noise peak has added perversely to a signal, or its aliases, in such a way that one of the aliases is the highest peak in the pattern, CLEAN will present the alias to you as the "best model". So it is imperative, wherever possible, to keep the window pattern in the DFT and look at it carefully. The heights of the aliases and the noise peaks tells you how much you do not know.

There was a suggestion at this meeting that autocorrelation analysis avoids alias problems. That is not true. Autocorrelation of short data strings has such a low frequency resolution that the autocorrelation diagram has dips that encompass the entire window pattern of a longer data set, hence simply cannot resolve it. As Geza Kovaćs pointed out, the complete autocorrelation diagram can be cosine transformed into the DFT and the aliases recovered. They are still there.

Filippo Zerbi gave us a good review of the γ Dor observations and the potential for space observations of those stars. He showed many phase diagrams, as did many of the poster papers and other talks. He also showed a full-colour picture of the latest high-frequency, ultra-high amplitude oscillator he has discovered only days before: his new baby! I simply remind you here that phase diagrams do not confirm the reality of frequencies. They are useful for looking at the phase distribution of the data, since if that is bad, the frequency analysis is suspect. But all peaks in a DFT (or any other frequency analysis technique) will give you phase diagrams in which you can see variation in the phased light curve. That is as true for a noise peak as a real signal at a given amplitude – it is just another way of looking at the peak, real or not.

Of course, this section is about Ian Roxburgh's question: How do we know with model fitting, perhaps with many parameters, hence in many dimensions, if the "best fit" is a true representation of the star, and the other, not-quite-as-good fits are significantly ruled out ? One valid method which helps is to do as Stéphane Charpinet, in answering Ian's question, said he does: use other information about the star to rule out all but one model (if you are so lucky as to be able to do that). But, in general, picture in your mind a hairy, 4D field of peaks and valleys in which you have to determine which of the peaks (or valleys, depending on technique) is right, and rule out the others as wrong. This is the 4D equivalent of looking at the window pattern and noise levels.

Mathematicians can solve this for well-formed problems, but in astronomy, with non-white noise, non-equally-spaced data, and uncertain external errors, there is no sure-fire analytical solution to the problem. The helioseismologists are well-aware of this, and long ago they conducted a "hare-and-hounds" exercise in which theoretician-hares constructed artificial, but plausible, frequency sets with realistic noise, and observer-hounds tried to reconstruct the input frequencies. The frightening answer for a spectrum as complex as the sun's is that good results couldn't be obtained for duty cycles of less than about 92%! Of course, that is the reason for the helioseismic telescope networks – GONG, BiSON,

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IRIS, TON – and a good part of the justification for the upcoming asteroseismic satellites.

It does suggest, though, that Monica, Stéphane, Travis and others who are producing large numbers of models, then trying to select the *right* one (not just the best one) should be running some hare-and-hound exercises to show the rest of us that they are succeeding.

6. Variable star names

Dave Kilkenny gave us an excellent introduction to the fascination of EC 14026 stars, after which Art Cox asked him how variable stars are named. This is a terrible problem, one that I grappled with for years, and now, I am happy to say, is the problem of Jørgen Christensen-Dalsgaard (the current president of Commission 27) and then Conny Aerts (the next president of Commission 27).

In 1603 Johann Bayer introduced the Greek letter naming scheme for stars that is so familiar to all of us. Bayer also used capital Roman letters for names up to the letter Q. Two and a half centuries later, in 1859, F.W.A. Argelander began his monumental "Bonner Durchmusterung" (Bonn Survey) using a 7.6-cm refractor. He also invented a naming scheme for variable stars. He called the first variable star in a constellation R Con (where Con is the generic abbreviation of the Latin genitive of the constellation name). He started with R, since Bayer had used the letters up to Q. From R he went to Z, then he used RR to RZ, SS to SZ, TT to TZ, up to ZZ. Those letters ran out, so next came AA to AZ, BB to BZ, CC to CZ, up to QQ to QZ. Along the way the Js were omitted because of possible confusion with the Is. That gave 334 names for variable stars in each constellation, but, inevitably, that was not enough, so the next variable star in Con discovered after QZ Con is V335 Con, then V336 Con. We now have passed V4000 Con and, in the near future, are heading for V∞ Con!

Prototypes of classes of variable stars, like ZZ Ceti, gave their names to the class, in this example the DAV stars of Fig. 1. The HIPPARCOS project was diligent and obtained official variable star names for all the variables in their catalogue, work that was largely the responsibility of Laurent Eyer and Michel Grenon. But other names are appearing, like the EC 14026 stars, named after EC (Edinburgh-Cape Survey) 14026-2647 (RA and DEC). Simon O'Toole talked to us about the EC 14026 (sdBV) star PG (Palomar-Green Survey, Green being Richard Green) 1605+072, and he said, "you get used to the telephone numbers". Nowadays, most variable stars have been discovered by the Large Scale Surveys; this was the main subject of the previous pulsation meeting (see the papers in Szabados & Kurtz, 2000). The MACHO project alone has discovered tens of thousands of variable stars, and they have chosen to use their own naming convention, and even changed conventions part way through the project giving two naming schemes. Future space missions like GAIA and FAME are going to detect variable stars by their hundreds of millions, perhaps billions. How will we name them ?

One suggestion has been to use the telephone numbers. We could have a catalogue, perhaps called the Encyclopaedia Stellarum, or ES (after Douglas Gough's invented star name during his after-dinner talk at the banquet), so each star might be something like ES220418.23+190754.62. Of course, we will

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the Balmer line profiles and in the amplitudes and phases as a function of colour in the photometric data. The latter was the first of these effects observed and explained (see Medupe & Kurtz, 1998) and was discussed by Thebe Medupe at this meeting. Oleg highlighted the need for observations with 8-m telescopes to get the desired s/n for high dispersion, $R \geq 100,000$, high-time-resolution (typically 1-min) observations of roAp stars. It is now possible to do this; *e.g.* Gautier Mathys and I recently obtained such data with the VLT on the roAp star HD 166473.

A potential problem that I see for Oleg's model for γ Equ is that it requires a pulsational radial velocity of 7 to 12 km s^{-1} for an $\ell = 2$ or 3 mode. Is this theoretically plausible ? If velocities this high are possible for $\ell = 2$ modes, then why not $\ell = 1$ modes ? Wouldn't the latter then show much higher photometric variations than has been observed ? How do photometric amplitude and radial velocity amplitude relate in roAp stars ? Is a radial velocity of 7 to 12 km s^{-1} consistent with theoretical considerations of diffusion ? Is it consistent with theoretical considerations of the magnetic field ?

The problem of pulsation in the presence of a strong magnetic field is now being studied theoretically by many people, primarily Lionel Bigot and Margarida Cunha. Lionel presented his study with Wojtek Dziembowski of the interaction of the magnetic field and rotation with pulsation modes in roAp stars. He finds that the pulsation modes have a mixed-mode character with contributions from $\ell = 1, m = -1, 0, +1$ modes in a plane that is tilted at nearly 90° to the magnetic axis. This is a very different model to the standard oblique pulsator model. In the specific case of HR 3831, Lionel's model results in what appears to be an $\ell = 1, m = 1$ mode with an axis tilted at about 90° to the magnetic axis. The pulsation period is then the period of this essentially sectoral mode with the whole pattern rotating with the star's rotation period. The theory is able to reproduce the pulsation photometric amplitudes and phases, but requires a rotational inclination and magnetic obliquity that are not consistent with those needed to explain the Broad Band Linear Polarisation (BBLP) studies of HR 3831. However, the oblique pulsator model also cannot reconcile the BBLP results and the photometric amplitude of HR 3831. It is now important to test the Bigot-Dziembowski theory against other observations: Can they reproduce the radial velocity modulation in HR 3831 (Baldry et al., 1998) ? Can they say anything about the pulsation phases as a function of depth in the atmosphere ? Baldry & Bedding (2000) had such extreme difficulty understanding the pulsation phases for the Balmer line radial velocities in HR 3831 that they suggested the oblique pulsator model may not be correct. The new theory needs to be tested against those observations. What does it say about the line profile variations in roAp stars that may soon be measured using 8-m telescope data ?

The session on roAp stars showed how lively the research on these stars is – both theoretical and observational. Oleg Kochukhov said he is beginning to model roAp stars in 3D, but he commented on how difficult it is to separate the effects of pulsation and rotation. This theme was reflected in many of the talks for many kinds of stars: for the Red Supergiants, for the SPB stars and for the λ Eri Be stars. For the latter Thomas Rivinius concluded with confidence that there is "no need for any mechanism other than nonradial pulsation" to explain

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them, whereas Luis Balona has for years argued that nonradial pulsation cannot be the explanation for these stars, that the solution lies in rotation (Balona, 2001). For the SPB stars Maryline Briquet showed that some chemically peculiar (CP) stars have been mistaken for SPB stars. Peter de Cat found similar cases. It makes me wonder: Would we recognise a CP SPB star with a rotation period in the same range as the pulsation periods ? Can we separate these two if they coexist ? *Can* CP B stars pulsate ? If so, what implications does this have for diffusion theory, and vice versa ? An example of a CP SPB star would be interesting to find and study.

8. δ Sct stars

MarieJo Goupil bravely gave an excellent review of the δ Sct stars (I say "bravely" because the interpretation of the pulsations is so difficult). Before the very recent discovery of solar-type oscillations in β Hyi and α Cen, the δ Sct stars were the only non-degenerate stars with many pulsation frequencies offering a lot of scope for asteroseismology. Some people think they have excellent prospects for asteroseismology, while others are totally pessimistic. Wojtek Dziembowski was quoted as saying we would "never be able to use these stars for seismic probing", although he indicated during discussion that he was willing to withdraw that remark if Michel Breger withdrew his claim for the existence of low-frequency g-modes in some δ Sct stars. Mike withdrew, so Wojtek withdrew, but others remain pessimistic. Hans Kjeldsen said to me that he thought rotation, and especially differential rotation, made the problem of understanding the δ Sct star frequencies intractable. YanQin Wu was even more pessimistic; she asked me, "why do you want to spend all that money to put up asteroseismic satellites to observe δ Sct stars when we are nowhere near to understanding them theoretically ?" My immediate response was to remind her of Tycho, Kepler and Newton; Tycho did not know that Newton would be born 60 years later when he embarked on high precision observations with an extravagant government grant.

The upcoming asteroseismic missions were well-reviewed and justified on the last morning of the meeting, making it clear how much exciting science will result from them. Hans Kjeldsen even gave us a good, simple demonstration on the overhead projector of the problem of scintillation, a problem that MONS, MOST, COROT, EDDINGTON (if it is funded) and other missions will eliminate, giving us an order of magnitude higher precision photometry. But MarieJo had to show us the asteroseismic state of the δ Sct stars in the presence of all the doubters, and she did it well. There is no getting around the problem of rotation (it was one of Art Cox's "unsolved problems" in his introductory review): it generates a vast number of possible pulsation frequencies in the models. There is also the problem of mixed modes – modes that behave as g-modes in the interior but as p-modes in the envelope. Theoretical models have modes up to $\ell = 1000$ excited. Michel Breger pointed out that a model for 4 CVn has 554 unstable modes, but the observations have, so far, only detected several dozen modes, leaving the old problem: how do we judge goodness-of-fit ? Eventually, the models will have to deal with rotation (even rapid rotation), differential rotation, convection and diffusion. Hideyuki Saio warned us of the further complication that rotational deformation reduces the effect of the κ-mechanism, so

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tends to stabilise p-modes, but cannot stabilise all g-modes. MarieJo finished with "the reality" of seismology for δ Sct stars: simultaneous spectroscopic and photometric observations are mandatory. Observers should heed that call. A big problem is, of course, mode identification. It was a subject in several talks, and we even were told a "fairy tale" by Michel Breger of the foundation of the photometric and spectroscopic "churches" of mode identification. Notwithstanding that another Viennese astronomer once anointed a "Cepheid Pope" in an A&A paper, Michel refrained from naming photometric and spectroscopic mode id popes; there was no need, as during one of the discussions (anonymity will be preserved here) two of the participants declared their adherence to opposing mode-id churches.

9. EC 14026 stars and tidally induced oscillations

The EC 14026-2647 stars, also known as the EC 14026 stars and as sdBV stars, were discovered seven years ago, but the first publication of the existence is only four years ago. Dave Kilkenny was there from the start as part of the discovery team, and he entertainingly reviewed their status for us. There are now 28 EC 14026 stars known. They are extreme horizontal branch stars, essentially He stars with very thin H surface layers. Their pulsation periods are typically $100 - 200$ s, making them great fun to observe since a lot happens in a short time, although periods up to 500 s are known. Their photometric amplitudes are typically only a few percent, although in the case of PG 1605 it is as high as 0.2 mag. At the time that Dave Kilkenny and his collaborators were discovering the EC 14026 stars, Stéphane Charpinet and his colleagues were independently predicting their existence. They had asked themselves the question "do these stars have a reason to pulsate" (perhaps in other words), and concluded yes: they are driven by a metal-line opacity bump in a layer where the Fe abundance is enhanced by radiative diffusion. Stéphane reviewed the theory of EC 14026 stars and showed good agreement between theoretical and observational values for $\log g$ and $T_{\rm eff}$ for PG 1047+003.

Arguably the most exciting of the EC 14026 stars is PG 1336. Dave Kilkenny made that strikingly clear when he put up the light curve of this star (see his Fig. 2 in these proceedings) and was genuinely struck yet again, even though he has seen it many times for some years now, with how remarkable it is. He was so struck that he exclaimed "crikey", and I rather hope that this sticks as PG 1336's nickname. Back before the EC 14026 stars were publicly known, I wandered into Darragh O'Donoghue's office one day and saw that light curve sitting on his desk. I exclaimed "crikey, Darragh, was is *that*!?" (Alright, I didn't say "crikey", but given Dave's astonishment in his talk at this light curve, I should have, since it expresses the same amazement I felt.) Darragh flipped the light curve over to hide it and said "I can't tell you. It's secret!" Well, I forced the story out of him, but only by promising complete secrecy until the data were published (a promise I kept).

In the second chapter of the well-known textbook, *Nonradial Oscillations of Stars*, by Unno, Osaki, Ando, Saio & Shibahashi, the authors discuss the advantages of finding nonradial pulsators in eclipsing binary systems. David Mkrtichian discussed several such stars in his talk, but the most amazing one

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yet has to be PG 1336, and Mike Reed made it abundantly clear why. He showed preliminary results of a recent Whole Earth Telescope Campaign on PG 1336, and he showed animated models of how the amplitudes and phases of various nonradial modes change during an eclipse such as that seen in PG 1336. He made a good case that at least some of the modes in this star are tidally distorted modes with their pulsation axis aligned with the orbital axis.

Pulsating stars in binary systems were the subject of several talks. Nancy Evans discussed setting benchmark masses of Cepheid for evolution calculations using HST and IUE data on Cepheid binary systems. Bart Willems discussed his theoretical work, done with Paul Smeyers and Conny Aerts, on tidally induced oscillations in close binaries, with particular application to B stars, although the general theory (Smeyers & Willems, 1998) has wider application, including to studies of PG 1336. Bart presented examples for orbital periods of 2 to 6 d and eccentricities in the range 0.3 to 0.5; many modes are calculated to be excited with amplitudes of km s^{-1} for a 5-$M_\odot$ star with a 1.4-$M_\odot$ companion. Other parameters could produce m s^{-1} variations, and I wonder whether there is potential for such systems to mimic planet detections. Although the big controversy over 51 Peg a few years ago was resolved in favour of the planet hypothesis, there is still a chance that some of the planet detections may be pulsations. This ambiguity will remain true as long as only radial velocity data are available. Satellite missions such as MOST presented by Jaymie Matthews, will be able to detect the light variations from planets as they go though their phases and clearly distinguish them from pulsational light variations. David Mkrtichian discussed δ Sct stars in eclipsing binary systems; Peter de Cat found binary SPB stars; Gerald Handler suggested that tidally forced oscillations might be the explanation for the first hybrid pulsator, HD 209295, which is both a δ Sct star and a γ Dor star. The study of tidally induced, and tidally distorted pulsation is a healthy, growing field about which we will hear much more in the future.

10. Solar-type variables

For decades we have been listening to talks at pulsation meetings and seismology meetings about the discovery of solar-type oscillations in stars other than the sun. The discovery of such objects has been a major goal of many observing programs, and has driven the development of new instruments for higher precision observations. But, as Tim Bedding pointed out in his talk, it was the high-precision radial velocity instrumentation of the planet searches that finally made the detection possible. The long history of over-enthusiastic claims of detection of solar-like oscillations that didn't survive scrutiny was discussed by Tim, who said, "theorists look at data dispassionately" – clearly meaning that the data and analyses need to be scrutinised by someone not so intimately involved in trying to make the discovery. At least he had the good grace to let a look of surprise at his own statement cross his face briefly after uttering the above quote. One of my own experiences with a theoretician's "dispassionate" look at data was sitting next to Wojtek Dziembowski at a helio- and asteroseismology meeting some years ago when one of the claims for the discovery of solar-like oscillations in a G star was being presented. Wojtek was clearly very

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interested in the talk and I could almost see the wheels grinding in his head as he tried to come to a theoretical understanding of the unexpected large spacing. Near the end of the talk he leaned over to me and whispered, "this is very exciting, Don. What do you think ?" My whispered answer was, "it is nonsense, Wojtek", to which he said, "Oh! OK. If you don't believe it, then neither do I." The problem was, repeatedly, that the drive to be the discoverer of solar-type oscillations was so strong that people were fooling themselves in the noise of Fourier transforms of pieces of Fourier transforms in the attempt to extract the large spacing when the individual frequencies could not be detected. Tim made the case that there are now good detections of solar-type oscillations in β Hyi (Bedding et al., 2001 and these proceedings) and α Cen A. Douglas Gough (2001) has referred to the discovery for β Hyi as "the birth of asteroseismology" (although the white dwarf people dispute that). Günter Houdek gave a good discussion of the current state of models for the new solar-type variables and the stochastic driving of their pulsation.

The detection for α Cen A is particularly impressive, as was discussed by Francois Bouchy. The Fourier transform of 1850 radial velocity measurements of α Cen A made over 13 nights with CORALIE on the 1.2-m La Silla telescope shows a strong signal with peaks between 1.8 and 3.0 mHz and maximum amplitudes of 31 cm s^{-1}. Francois gave the large spacing as $\Delta\nu_0 = 105.5$ μHz and the small spacing as $\delta\nu = 6.6$ μHz, although Tim Bedding gave $\delta\nu = 10$ μHz, and, in response to a question by Jaymie Matthews suggested that the difference was due to "bad luck" with the aliases. Luis Balona expressed very pleased surprise during his own talk at how convincing Francois's result is for α Cen A, I think echoing the feeling of many of the participants.

11. Asteroseismology from space

Asteroseismology from space got an unexpectedly early start in 1998 when the WIRE (Wide-Field Infrared Explorer) mission lost all of its coolant and failed. Derek Buzasi discussed his results using the 52-mm star camera photometrically to study pulsation in 28 stars covering a wide variety of types. The noise levels from this small instrument (which was *not* designed for high-precision photometry) are similar to what is obtained from ground-based photometry, but benefit greatly from the ability to get extended data sets. There is an orbital alias pattern to the observations, but it is totally different from the ground-based daily aliases, and, in general, is not a problem. The other imminent space missions, MOST, MONS and COROT were discussed by Jaymie Matthews, Hans Kjeldsen, and Annie Baglin, respectively. Jaymie had the lecture hall roaring with laughter at his Martian solution to the Great Katrien Kolenberg Asteroseismic Pulsation Mode Name Badge Puzzle (also known by the acronym GKKAPMNBP – apparently a pronounceable word for Jaymie's alien). Fundamentally, there was a degree of order to the puzzle, and in most sectors it was all the mode to study name tags for the whole week. Jaymie's alien solution was incorrect; the grand prize was won by Andrzej Pigulski whose solution to GKKAPMNBP was apparently close – I am not too sure how close because I didn't actually fully understand the solution when Katrien explained it all to us! Fortunately, Jaymie, lecturing in his shorts and t-shirt, was wearing far more than on a pre-

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vious night (when seeing dancing in the streets of Leuven with a cohort who can breathe a sigh of relief that I am allowing them to remain anonymous), allowing us to concentrate on his message that MOST is to be launched in 2002. If all goes well, the next pulsation meeting will have a session on MOST results. Hans and Annie gave us the current state of the MONS and COROT missions to be launched in 2004, so two meetings down the line we are going to have many sessions dedicated to space results. All of these satellites promise to revolutionise asteroseismic observations; MOST and COROT also plan to detect planets, even earth-sized planets in the case of COROT (as Annie warned: do *not* pronounce the terminal "T"; the acronym is French.) Farther down the line is the possibility, let us optimistically call it the promise, of EDDINGTON which is planned to test 50,000 stars for asteroseismic variations for two years, then spend a year surveying 700,000 stars for planets. EDDINGTON is much more ambitious than MOST, MONS and COROT; it is planned to have a 1.2-m aperture telescope with a 3° field-of-view!

12. Titbits and tale-end thoughts

I have had a rather longer tale to tell here than I have been given room for, and I have miserably failed to cover all the talks and posters, as I failed during my summary talk at the end of the meeting. Part of the reason for that, though, was the extra 15 minutes for Jaymie's well-received comedy routine, and for Katrien's long-awaited solution to GKKAPMNBP, came out of my allotted time. My only reason for telling you this is to make the editors (who were on the SOC and LOC – two of them were even the *heads* of the SOC and LOC) feel guilty enough to let me have all the extra pages to tell stories, as well as review the science.

The long and short distance scales featured in several talks, as they will at meetings for the foreseeable future. I was particularly interested in Andy Layden's attempt at the distance scale using white dwarfs in M5. There have been many "new ideas" for measuring the distance scale over the last decades, but they, in general, go for the brightest stars for obvious reasons. Andy has tackled some of the faintest stars with completely different astrophysics, so hopefully a different set of constraints on the uncertainties. Amazingly, he was talking about observing down to mag 28 or even 29 with his HST data, and discussing the possibility of seismology on 26^{th}-mag stars, perhaps using 10-m telescopes. It is clear that a successful application of this method is still in the future, but it was stimulating to think about.

Geza Kovaćs and Pawel Moskalik showed us that substantial progress has been made in the two years since the Budapest meeting on the problem of the Blazhko Effect in RR Lyrae stars using MACHO and OGLE data. Although a definitive model has yet to win the day, the photometric observational constraints are now impressive: 149 RRab and 52 RRc Blazhko stars in the LMC MACHO data; 35 RRab and 2 RRc Blazhko stars in the OGLE Bulge data; lots of stars that convincingly show frequency doublets; lots of others that show convincing triplets. As Geza pointed out, spectroscopic observations and tests for magnetic fields are now needed.

The extreme He stars were discussed by Holger Pikall and Pilar Montanes Rodriguez. These stars are probably driven by a metal opacity bump, but their

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most incredible property to me is their radial velocity curves. Have a look at those velocity curves: the driving is spectacularly rapid and short-lived each cycle, then the stars coast. Pilar's models are beginning to look like the observed radial velocity curves.

YanQin Wu tackled the hard problem of why, right across the HR Diagram, similar stars have different amplitudes. She discussed the "flushing toilet bowl" mechanism of parametric instability (I wonder whether the term "ftb-mechanism" will one day be as familiar as the term "κ-mechanism"). Hiromoto Shibahashi discussed his attempts to model the observation that stars for which new planets have been found in the last few years seem to be, as a group, metal-rich. If this observation survives the test of larger samples, then he pointed out it may mean that large metallicity is needed to form planets, or that for stars with planets, the migration inwards of planets (as is needed to understand the closely orbiting giant planets that have been discovered) and then absorption of them onto the stellar surface, may contaminate the surface. The latter is a worrisome idea for the GENESIS mission which is now on the way to L1 to collect 10-20 μg of solar wind; the presumption of that mission is that the solar atmosphere and wind have the unchanged abundances of the primordial solar nebula. The observational lack of planets in 47 Tuc (Gilliland et al., 2000) suggests perhaps that the former hypothesis may be correct – that metallicity is a prerequisite to planet formation. However, there is also the possibility that the proximity of the stars in a globular cluster may strip any planets formed from their stars.

A good look at the future of our field was given in back-to-back lectures by Steve Kawaler and Luis Balona on the last day of the meeting. While Steve was supposed to be wearing his theorist's hat and Luis his observer's hat for these talks, it was not surprising that both lectures integrated discussion of theoretical and observational needs for the future.

Art Cox started the meeting with his review of unsolved problems, and I was delighted to see that most of these were discussed in many of the talks that followed. Arlette Noels, in her surprise talk dedicating the meeting to Paul Smeyers, showed us that Paul's record number of equations in a publication was 247. That reminds me of the story of the comment of the Emperor Joseph II to Mozart following a performance of Mozart's first opera, Abduction from the Seraglio: "Too many notes Mr. Mozart", to which Mozart famously replied, "Yes, but not one more than necessary." The meeting was also dedicated to Paul Ledoux in an invited talk by Douglas Gough where he told us that all that we need to know of the physics of stellar pulsation is contained in the articles by Ledoux & Walraven and by Ledoux in volume LI of the Handbuch der Physik. Douglas pointed out that Ledoux often did not give us enough equations, so he expanded, and expounded on, some of them – the message being that all we need do now is work out the details.

Well, it was abundantly clear from this meeting that the details are complicated, both theoretically and observationally. Some of the observational details are coming in the satellite missions about to be launched, and in the new data now being obtained with 8-m class telescopes. Some theoretical details may come from projects such as Djehuty (Turcotte et al., these proceedings), a massive parallel processing computing project that aims to create a stellar structure code with full 3D hydrodynamics. Given Djehuti's original role as a patron god

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of those who compute, those who work on the project are known as "Djehuti's witnesses". This is a goal far in the theoretical future. Perhaps closer in the observational future are satellite measurements so precise that we may find that there are almost no non-pulsating stars anywhere in the HR Diagram. If that time comes, we may remember Martin Schwarzschild's question and ask of the few constant stars: "What are those stars reasons *not* to pulsate ?".

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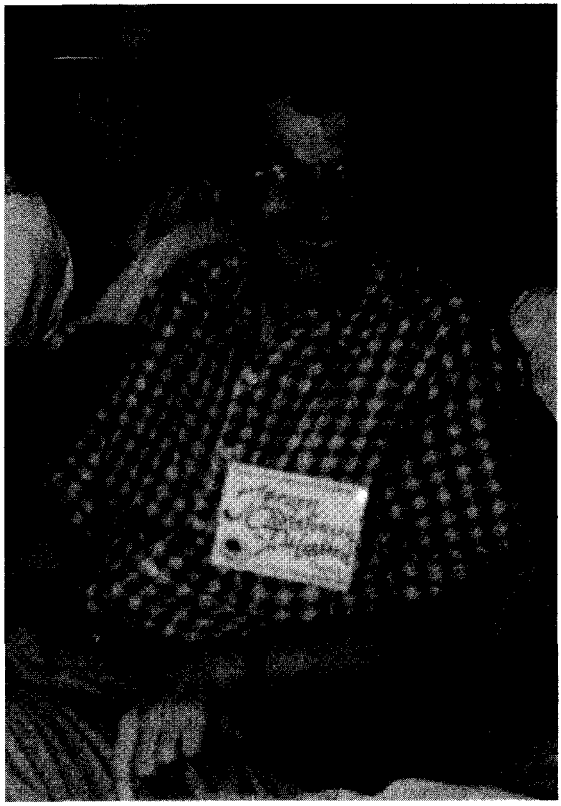
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We all felt a little bit dizzy after the meeting, because of the heavy scientific programme and the high temperature. Despite this, the SOC chairman looks quite satisfied and he received a new nick-name during IAU185: "mister super-badge".

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