

# Conference Summary

## Some Remarks on Stellar Pulsation

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**Abstract.** I wondered at first why I had been asked to perform the task of commenting on the scientific discussion of this meeting, until a member of the Scientific Organising Committee pointed out to me that I have not published a serious paper on the subject of the colloquium in his memory (I am not sure whether this is more a statement of the seriousness of my publications, of my publication frequency or of the duration of Jørgen's memory); however, I am presumably considered to be unbiased by recent advances. Nevertheless, the time allotted (for the oral address) and the corresponding space (in these published proceedings) are inadequate for a complete and balanced review – in any case, that is hardly either desirable or necessary, since the discussions are all still fresh in our minds – and therefore I have been freed to comment personally on some selected issues that have captured my interest.

### 1. Introduction

I have been goaded into pointing out, before embarking on my discussion, why we (or perhaps I should say you) are working in this subject. The reasons are: (i) to understand the physics of stellar pulsation, (ii) to learn how to use the observed properties of stellar pulsation to inform us about the internal structures of the stars that pulsate – one can learn much more about a system from its dynamics than from its hydrostatics, and (iii) to use the results of (ii) to learn about the chemical evolution and the distance scale of the Universe, a very big issue indeed which deeply concerns the grandest branch of astronomy, namely Cosmology.

We are all physicists first and foremost, trying to understand the real world. Consequently, we depend primarily on observational data, the lifeblood of our subject, the acquisition of which is the principal topic of this meeting. It is very impressive how rapidly the data acquisition rate has risen of late, both from purpose-designed observing programmes and as by-products of observations designed originally for other purposes. Since, as Tim Bedding has reminded us, we all (at least, the seriously minded among us) sleep with Volume 51 of the *Handbuch der Physik* under our pillows, we must therefore surely be surprised by the new trend. Comprehensive and authoritative as Ledoux and Walraven's erudite exposition of the state of the subject in 1958 is, the new rapid surge of information demonstrates that the article is, superficially at least, deficient in its clairvoyance.

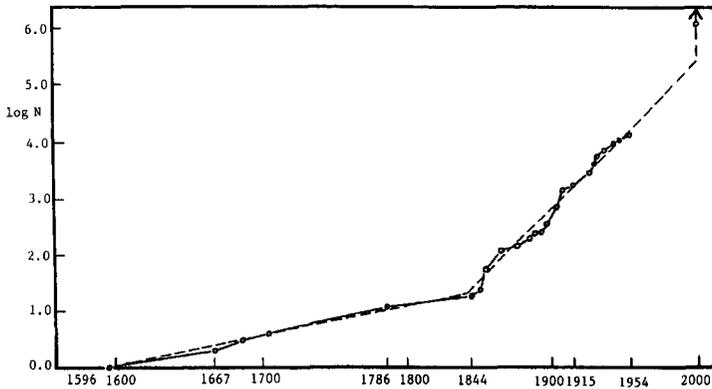


Figure 1. Number of variable stars discovered as a function of time.

Figure 1 of that article, if I recall correctly, is a logarithmic plot against time of the number of stars that are known to be variable. The relation is a combination of two linear functions, and Ledoux and Walraven, apparently naïvely, extrapolate a little into the future, almost as if to predict the date at which all stars will be declared to be variable. However, when I learned in the opening days of the colloquium of the enormous amount of new data that is now becoming available, I imagined that Ledoux and Walraven's figure should be modified as in Fig. 1 here. But then Don Fernie, heeding the prophecy that ubiquitous variability is nigh, made an important point which I had overlooked: we must maintain a limit to the amplitude of variability above which a star is admitted to be variable – not to do so would hasten the epoch at which all stars will be deemed to be variable, and thereby bring nearer the time when we shall all have lost our special identities amongst stellar astronomers. To enable these colloquia to continue for a while longer, we should therefore ensure that we adhere to Ledoux and Walraven's more sober and evidently wiser prediction.

## 2. On Complicated Theories

Attempting to digest the intricate detail with which the theoretical properties of stellar pulsation are matched with observation has impressed on me how much the subject has advanced since the time at which my organising friend remembers (or not) when I last worked seriously on the subject. In those days we were occupied with merely explaining the very existence of the red edge of the instability strip (I'm not sure that we understand it completely today), and with resolving the Cepheid mass problem. At least the latter seems to have gone away, thanks to the work of Moskalik, Buchler, & Marom (1992). For me, the beauty of their explanation is enhanced by the fact that an error in the opacity which created the problem in the first place was subsequently revealed as a result of analysing the frequencies of pulsation of a star, namely the Sun (Christensen-Dalsgaard et al. 1985). That triggered a closer, extremely well directed look at

the opacity calculations, which uncovered, amongst other skeletons, an error in the manner in which spin-orbit coupling in radiatively induced atomic transitions had been taken into account (e.g. Iglesias & Rogers 1996); it illustrates how our subject uses stars as laboratories for studying physics.

Notwithstanding the detailed investigations that led to that success, I marvel at the multiparameter fitting procedures that are now being used to match pulsation theory to observation. Coming freshly to many of the analyses that were presented at this meeting, however, I have found it difficult to assess the meaningfulness of the results. For example, I cannot judge the import of the disparities in the advertised period–luminosity relations for Cepheids and RR Lyrae stars; these force us to recognise the uncertainty in the distance to the LMC, and consequently the uncertainty in our estimates of the value of  $H_0$ . It reminds me of the time, some three decades ago, when I embarked on a particular very complicated project: to make a solar model and compute from it the neutrino flux. Historically, in days well before my time, presumably unbiased predictions of about 20 snu had been proffered, if I recall correctly (the precise meaning of the flux unit snu is not needed here, so I refrain from defining it); but after Ray Davis's early failure to detect anything significant (his observations were well-designed to detect 20 snu with ease, and the early failure implied a flux well below 10 snu) the uncertain parameters defining the models were reassessed (no doubt by studying more carefully the physics determining them), and the theoretical value dropped to about 7 or 8 snu (by which time the observational upper limit was yet lower). What interested me, however, was that even with hindsight my first models also yielded 20 snu, although further hindsight did subsequently enable me to rectify my computations, making them not inconsistent with others of that era. I tell this story not to insinuate doubt on the analyses to which we have been exposed at this meeting – after all, the solar calculations have been confirmed (subject to relatively minor adjustment) by essentially the entire community working in the field, and should thereby have dispelled any early doubt that I might have harboured. Nevertheless, I do sometimes wonder how the subject would have developed had Davis found the solar neutrino flux to be 50 snu.

It seems to me that, when a subject becomes as complicated as this, one has three options: (i) to acquire one's own informed opinion, which, as I have intimated, involves really getting one's hands dirty and working seriously in the field oneself, (ii) trusting the experts, which requires at the very least establishing who the reliable experts are, particularly when there is dissent (or perhaps, a sceptic might say, particularly when there is not), or (iii) having no opinion, which perhaps doesn't augment one's reputation (yet obviates the risk of augmenting a bad one), or, putting it another way, preserves an open mind. Not having the time to adopt option (i), I am resigned at present to option (iii), in view of the opinion expressed at this colloquium by at least one discordant expert on the influence of metallicity on RR Lyrae pulsation that it is not necessary to determine and assess the origin of the disparity between his and others' conclusions. It is interesting to observe how, when the wider implications of an investigation are so important, reason bows to emotion. It seems to me, however, that there is scope, and necessity, for much more work to be done in this field.

Whilst on the subject of the distance scale of the Universe, I should mention how encouraging it was to hear Tyler Nordgren's determination of Cepheid angular diameters by optical interferometry. By combining the angular-diameter variations with Doppler observations one can derive absolute diameters, and hence obtain the distances to the stars, at present about as accurately (yet more cheaply) as Hipparcos. Further development of the method offers the exciting prospect for calibrating period–luminosity relations.

### 3. On Nomenclature

In any scientific enquiry it is necessary to communicate well. To this end we must not only have an agreed nomenclature, but our nomenclature must both be appropriate (this is important because a name influences the way in which we think about something) and, if possible, be consistent with its uses in other branches of learning. I was therefore gratified to notice that the old custom of using the word 'harmonic' to mean overtone, and moreover, to assign the wrong integer to that harmonic (thereby ensuring discord with music, even had the overtone been a harmonic) appears to have ceased. However, I was dismayed to see Robert Buchler refer to the fundamental radial mode as  $p_0$ , particularly after his emphasising that the trajectories of solutions to (suitably regular) differential problems (with elliptic properties) in the space spanned by parameters occurring in the governing differential equations or the boundary conditions cannot simply end, although they may change character (e.g. stability). Pulsation modes are no different.

My following remarks suggest that formal modal nomenclature could be reasonably well defined in principle, although to my knowledge there is yet no proof that my attempt at justifying it is correct. For a sufficiently slowly varying stellar model (e.g. a polytrope of low index) the nonradial (adiabatic) modes fall into two clear sequences: the p modes (acoustic modes), whose frequencies  $\omega$  are determined principally by the sound speed, and, if the polytrope is sub-adiabatically stratified, the g modes, whose frequencies are determined by the buoyancy frequency (Brunt–Väisälä frequency)  $N$ , and which, for a given value of the degree  $\ell$ , all lie below the frequencies of all the p modes. As  $\omega$  increases (for p modes) or decreases (for g modes) at fixed  $\ell$ , the number  $n$  of nodes in the vertical displacement eigenfunction increases; all values of  $n$ , starting from unity, are represented, and there are two different eigenfunctions, one in each sequence, with the same value of  $n$ . Therefore one can label the mode with  $n$  (at least when  $\ell > 1$ ), which we call the order; thus we may talk of mode  $p_n$  or mode  $g_n$ . In addition there is a mode with no node, provided  $\ell > 1$ , whose frequency at a given value of  $\ell$  lies between those of  $g_1$  and  $p_1$ . It is called the f (fundamental) mode. It can be shown that as  $\ell \rightarrow \infty$  its frequency becomes independent of sound speed (because the motion is uncompressed, even though the fluid is compressible), so it cannot be an acoustic mode and is rightly regarded as the fundamental gravity mode. All the gravity modes are fundamentally spherically asymmetrical (the word 'nonradial' is used in this subject to describe that property even though there is a non-zero radial component to the flow; the term is presumably a contraction of 'non-solely-radial'), and they do not exist (physically) for  $\ell = 0$ . However, by analogy with the g modes of a plane parallel

atmosphere under constant gravitational acceleration, all of whose frequencies tend continuously to zero as the horizontal wavenumber tends to zero, one can imagine a formal set of  $g$  modes with  $\ell = 0$ , all with frequency zero.

The  $p$  modes with  $\ell = 0$  are the radial modes which provoked this discussion. The issue regarding the assignment of  $n$  to these modes concerns whether or not to count the node at the centre of the star. The obvious way to find out in a mathematically acceptable manner, as I'm sure Robert Buchler would agree, is to free  $\ell$  from being an integer and see what happens as  $\ell$  is permitted to tend continuously to zero. That calculation has been carried out by Vandakurov (1967).

Before I remind you of the outcome, it is of some intellectual interest to ponder first over the possibilities. As an aid to thought, it might be useful first to point out that adiabatic modes never cross in the  $\ell - \omega$  plane. If the fundamental  $p$  mode is  $p_0$ , then it must therefore be met by the  $f$  mode as  $\ell \rightarrow 0$ , which should cause us to worry about why the  $f$  mode changed its character (to become acoustic) and why its frequency does not tend to zero, as does its plane-parallel counterpart. If, however, the  $f$ -mode frequency does tend to zero, and the fundamental  $p$  mode is met by  $p_1$  as  $\ell \rightarrow 0$ , there is no immediately obvious cause for concern. It may now not be surprising that Vandakurov found the latter actually to be the case. But before leaving the matter, permit me to make a further observation: the  $f$ -mode frequency is thought to vanish at  $\ell = 1$ ; the dipole  $f$  mode is normally considered to be simply a uniform translation of the star. I leave it as an exercise to the reader to sort out what happens to the  $f$  mode as  $\ell$  varies continuously from 1 to 0, and how one should really classify the gravity modes with  $\ell = 1$ . (It is my opinion that the classification should be corrected without further delay, before the present erroneous one is set in stone).

Finally, the identity of a nonadiabatic mode can be determined by continuously reducing to zero the nonadiabatic coupling to the thermal field, thereby ascertaining to which adiabatic mode the nonadiabatic mode corresponds; if magnetic fields and stellar rotation are ignored, any nonadiabatic mode that does not connect with an adiabatic mode is a thermal mode (such modes have also been called 'secular', but I do not like to use this term because the modes have exponential time dependence, which is not in accord with the classical use of the term secular in stability theory to denote algebraic time variation), some of which have recently been called 'strange' – I shall return briefly to strange modes later.

Another complaint I have about nomenclature (Gough 1990) – which I make again because of the conceptual error it engenders – is that on several occasions at this meeting it has been said that the condition for reflection of acoustic waves near the surface of the star depends on the value of  $N$ . Although, strictly speaking, the value of  $N$  is not numerically irrelevant in practice, it is really the value of Lamb's (1909) critical cutoff frequency  $\omega_c$ , not  $N$ , which determines whether or not a radial acoustic wave can propagate. Unlike  $N$ , the critical frequency  $\omega_c$  depends only on the sound speed and the actual density scale height of the background state, and not on the difference between an actual scale height and a scale height evaluated at constant entropy, as does  $N$ . The distinction is conceptually important. To be sure, in a plane-parallel isothermal atmosphere of perfect gas with  $\gamma = 5/3$  in a constant gravitational field, the values of  $N$  and

$\omega_c$  are very similar –  $N^2 = 0.96\omega_c^2$  – but they measure quite different properties; acoustic reflection is basically not concerned with buoyancy, but with whether or not the effective wavelength of the acoustic wave is sufficiently smaller than a scale height of the background state. Moreover, acoustic reflection can occur in a convection zone, in which  $N$  is imaginary.

Before moving away from nomenclature, I remark that I was interested to learn at this meeting about strange modes. I had always regarded strange modes to be a manifestation of thermal modes whose heat transfer is so fast that they can oscillate on a dynamical time and thereby couple strongly with acoustics. Such modes evidently have no counterpart in adiabatic theory. But, strangely, it seems from the discussion at this meeting that it is acceptable to refer to another class of modes that do have adiabatic counterparts as being strange; I have not discovered how they are distinguished from ordinary p modes, perhaps because they are ordinary p modes, although it seems to be necessary that they be trapped between two reflecting layers that are closer together than somebody's expectation. Presumably there is a property (called strangeness?) that is common to both the thermal and the acoustic strange modes that differentiates them from the other modes in their respective categories, but unfortunately that property has eluded me.

I was startled at first to learn from Ernst Dorfi that the nonlinear response of the mean state of a stellar model to strange-mode pulsation is to increase the mean radius. Surely, one might think, the natural response of a star to a spontaneously unstable motion would be to increase the rate of loss of entropy, thereby, at least for homologous response, inducing contraction. But, as Ernst explained, strange modes are concentrated near the surface, and their localised dissipation leads to a nonhomologous expansion of the atmosphere. I would not be surprised, however, if the response deep inside the star, if it can be detected, is a contraction. Indeed, at the end of his presentation Ernst pointed out that he has investigated a model whose mean photospheric radius actually does decrease.

#### 4. The Blazhko Phenomenon

Coming now to the physics of pulsation, the biggest surprise of the meeting for me was to learn that the Blazhko phenomenon remains unexplained. I first heard about the phenomenon three decades ago, and immediately came up with a prototheory, which was a modification of a 'theory' I had already invented to explain pulsars in the weeks immediately following their discovery. The theory was aimed at explaining both the pulses and the march of the subpulses (of pulsars), and predicted that the pulses are only roughly periodic, as seemed not unlikely to be the case in those early days. Of course, it soon transpired how accurately pulsars keep time, so I dropped the idea, and by default I neglected Blazhko.

Interestingly, the two models of the Blazhko phenomenon presented at this meeting have a property in common with mine: both are derived from theories developed originally to explain something else. Hiromoto Shibahashi, who has studied the oblique pulsator model of Ap stars, maintains that the Blazhko phenomenon is a manifestation of the pulsation of a rotating star that is obliquely distorted by a magnetic field; the variation is strictly periodic, with a frequency

directly related to the angular velocity of the star. On the other hand, Wojtek Dziembowski, who for many years has studied resonant mode interactions in stars, joins Tim Van Hoolst to suggest that the Blazhko variation is a result of energy exchange between resonating modes. Which, if either, is correct?

The beauty of Hiromoto's theory is that in principle it is easily refutable by observation, for it ties the frequency of the amplitude variation to the star's angular velocity, which should be observable, and it demands a magnetic field of such a strength that should also be observable. If the Blazhko variation is found not to be periodic, the theory must fail. The resonant-interaction model could yield either periodic or aperiodic variation, and therefore cannot be so tested. However, it does require the existence of nonradial modes. In principle, that is observable. However, it is not easy to explain with the resonant-mode theory how one can get phase variation without amplitude variation. (It is not clear whether or not that is so also of the oblique-pulsator model).

One might ask why the Blazhko phenomenon is not exhibited in Cepheid pulsations. Hiromoto responds that Cepheids do not have (strong enough) magnetic fields, Wojtek that the pulsations are too nonadiabatic (the nonadiabatic theory has not been worked out). Although the beautiful results reported by Katrien Kolenberg do not at present resolve the issue, the method she explained promises greater sensitivity and the possibility of the use of more extensive analysis, such as Doppler imaging, thereby providing hope for testing these models in the future.

Since neither model yet explains the Blazhko phenomenon adequately, I shall throw my hat in the ring by mentioning my prototheory. I was studying convection at the time I thought of it, so naturally it involves a modulation of the vigour of convection by the imposed oscillation. The modulation occurs via the thermal stratification, and, because the convection is turbulent, is not periodic. One could hazard a guess at what the characteristic time scale of the variation might be: it is likely to be closely related to the (essentially thermally controlled) natural growth time of the pulsation, which, for an RR Lyrae model in which convective interactions with the pulsation are taken into account, is about 30 d (Baker & Gough 1979); this is not very different from the 41-d time scale observed. Géza Kovács has (privately) objected to the idea on the ground that if one derives amplitude equations one cannot couple radial modes with convective modes. That may be the case to the order at which Géza normally truncates his expansion, but it is certainly not true of all orders. Perhaps the high-order coupling is too weak to be effective; it would take more serious consideration of the matter to know.

How can one choose now between the models? Wojtek Dziembowski advocates (perhaps not seriously) use of Ockham's razor to eliminate Hiromoto's unnecessary magnetic field. If one accepts that argument, then surely one should also eliminate the unnecessary  $g$  mode, leaving just the radial pulsation and the convection which are bound to be present anyway! But nobody would seriously advocate that argument, because the prototheory that would then remain has not been worked out.

## 5. Convection and Nonlinear Pulsation

Åke Noordlund's very impressive calculations have now reached the stage where, by suitable matching onto an underlying adiabatically stratified layer, a model of the entire convection zone of a star can be constructed. In principle, there are no adjustable parameters; the microphysics is incorporated realistically, and the macroscopic motion appears to be resolved well enough to render many of the global properties of the model independent of the computational mesh. A critical test is the predicted jump in entropy across the upper superadiabatic boundary layer in a model of the Sun, for that determines the depth of the convection zone; the outcome agrees with the helioseismological determination remarkably well.

We should now anticipate substantial improvements in stellar modelling. I would not propose immediately incorporating his computer programme into a programme for computing stellar evolution, as Åke has suggested, but rather suggest constructing a grid of model convective envelopes which can be grafted onto evolving stellar interiors, as was done in the days when computers were not adequate to evolve entire stellar models with mixing-length theory. One could certainly then calibrate the mixing-length parameter, and learn how it varies over the HR diagram (and how it depends on other properties of the star, such as chemical composition). It will be interesting to discover to what extent that variation corresponds with proposals that have been made as a result of comparing stellar models with observations. Having grown up with relatively simple models of convection, I would wish to compare detailed predictions of those models with the more reliable direct simulations. But I sometimes wonder whether this view is too old-fashioned, and that in the long run one should abandon thinking in terms of simple phenomenological models of convection. Nevertheless, in the immediate future the demands on computers will remain too severe to incorporate the simulations in real time for all astrophysical purposes, and in many cases the simulations may best be used as a laboratory for calibrating less sophisticated prescriptions.

How sophisticated should those prescriptions be? The answer surely depends on the purposes for which they are to be used. I have questioned at this meeting the validity of the theory with seven parameters described by Robert Buchler, for example, suggesting that an eighth might be needed to explain both metal-rich and metal-poor Cepheids. That is an even greater number of parameters than Pauli needs to make his elephant walk. But it is not just the number of parameters that matters, but what those parameters represent; changing the mathematical structure of a prescription can have a much greater impact than modifying the balance of two similar terms. Indeed, with an apparently much simpler prescription Robert Buchler and Zoltán Kolláth appear to have successfully explained the dominant physics of mode selection; their predictions of fundamental, overtone or double-mode pulsation with hysteresis resembles to some degree what is observed. It would now be very interesting if one could isolate some simple property of the equilibrium stellar model that indicates in what manner the model wishes to pulsate: fundamental or overtone, singly or multiply periodic, radial or nonradial?

I have often pondered on a related matter: is there a simple property of the equilibrium structure of a stellar model that indicates at what amplitude

the star should pulsate? So far as I am aware, theoretical amplitudes are determined only by carrying out nonlinear initial-value pulsation calculations until a limit cycle is reached. What is it about Cepheids that makes them pulsate at much greater (surface) amplitudes than roAp stars? Why do Cepheids (and also roAp stars, we now ask) pulsate at much greater amplitudes than the Sun? The standard answer to the second question is that all p modes of the Sun are intrinsically stable, and are weakly excited by their interaction with the turbulence in the upper boundary layer of the convection zone, whereas Cepheids are intrinsically unstable. We know that the solar modes are stable, it seems, not because calculations show them to be (for not all calculations do, and nobody would maintain that those that do are reliable), but because if any of the p modes were unstable they would naturally grow to much larger amplitudes than observed. However, in the absence of an informed opinion of what those amplitudes should be, how do we know that? In the early days of helioseismology the assumption was simply that the amplitude of any unstable p mode of any star, and therefore of the Sun, would saturate at a value comparable with those of Cepheids and RR Lyrae stars; but then Don Kurtz discovered roAp stars.

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