SOUNDING SOLAR AND STELLAR INTERIORS: GENERAL INTRODUCTION

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1. An introduction to this Introduction - Helioseismology is great

in the many senses of the word. *It is a tremendous amount of fun* with a really large number of interesting problems, vigorous dialectic between observations and theory, rapid progress, fantastic observational opportunities, and extraordinarily stimulating colleagues. *It is grand*, in its phenomenally rapid development, the challenges that it presents, and the importance of the problems that it addresses, as well as its promise for the future. *It is doing very, very well* as seen in the vitality of the community of researchers pursuing it, in the major investments made in acquiring beautiful new data to fuel its continuing progress, and in the many new problems that open before us as the current ones achieve a measure of "understanding".

This Symposium marks a significant milestone in the development of helioseismology – the beginning of observations from *GONG* and *SoHO* – and it may be difficult to recall that, in many senses, the discipline began but a scant twenty years ago, here in Nice at a conference entitled "Physique des Mouvements dans les Atmosphères Stellaires" at which Franz-Ludwig Deubner first presented his remarkable observational demonstration that the "five-minute oscillations" were indeed normal modes of oscillation of the solar interior [Figure 1]. Contrast that landmark observation with the state of the art today [Figure 2] to see how dramatic has been our progress.

This meeting also marks the passing of time and the loss of two of the truly great personalities of the field of the "physique des mouvements dans les atmosphères stellaires" who participated very actively in that meeting: Philippe Delache and Dick Thomas. They were truly great figures in their contribution of new ways of thinking about the Universe, in their efforts – and their successes – in encouraging those around them to pursue these

J. Provost and F.-X. Schmider (eds.), Sounding Solar and Stellar Interiors, 1-12.

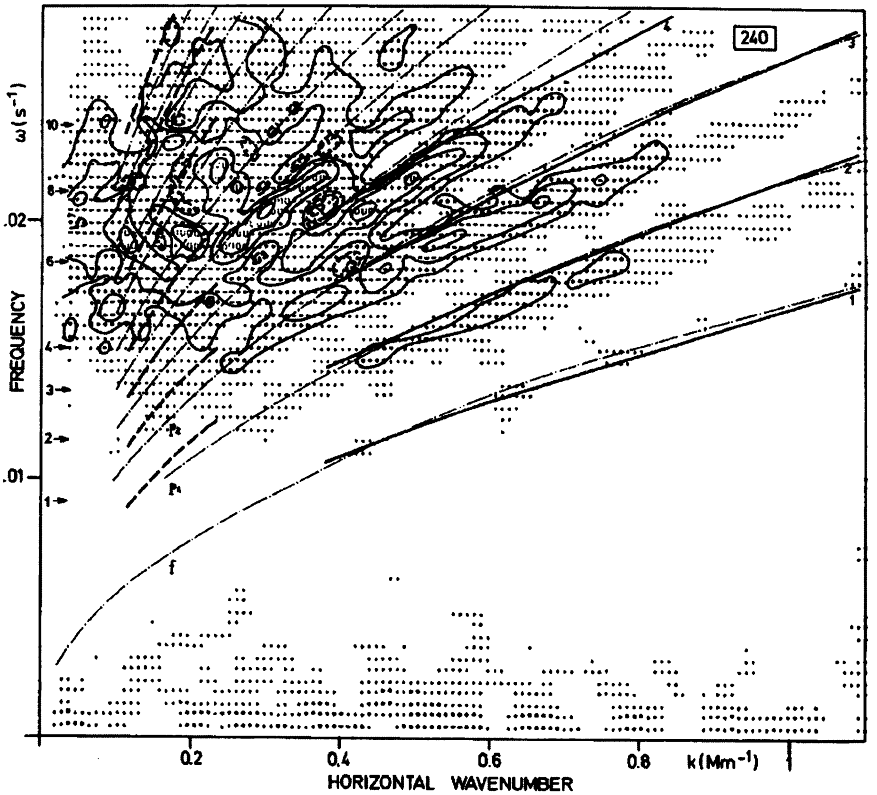

Figure 1. Franz-Ludwig Deubner's landmark $k - \omega$ diagram (Deubner, 1976) first presented at Nice.

new avenues of exploration, and in their personal generosity. They were my mentors and friends, and they represent why helioseismology is great.

The articles that follow provide the new knowledge that is in the process of emerging, and which is the meat of these proceedings – why we are here. Let me just preface these contributions with my own perspective on where we have come from, what we are doing, and what the future may hold. And, of course, Douglas will have the last word!

2. Where have we come from? Helioseismology is an accident

but an inspired one, a beautiful example of scientific discovery, serendipity at its best. As I view it, in the late 1950's a great deal of interest was being devoted in all of the sciences to characterizing turbulence as it manifested itself in the phenomena which they were dedicated to describing, and there was a general awakening to the importance of non-thermal processes in all

https://doi.org/10.1017/S0074180900060988 Published online by Cambridge University Press

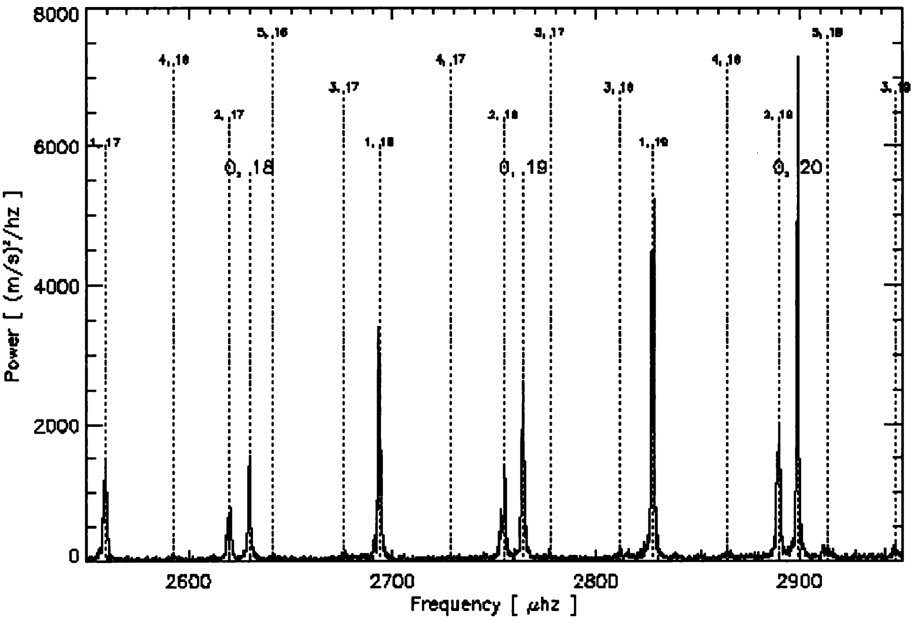

Figure 2. A snippet from a modern $\ell - \nu$ diagram, with the resonant modes identified by their spherical harmonic degree(ℓ) and their radial order (n).

of astrophysics. While many investigations had been devoted to the spatial distribution of "turbulent" motions on the Sun, which as a result of the assumption of ergodicity should have completely characterized the motions, Bob Leighton, a physicist who "didn't know any better" came up with a new scheme for looking at the "turbulent" velocity field of the solar surface that captured both its spatial *and* its temporal variations, and immediately noticed that the "turbulent" motions were, in fact, quite periodic, with a period close to five minutes.

As we now know, the "five-minute oscillations" of the solar surface are an extremely robust phenomenon. They are readily visible in virtually every measured parameter of the Sun's surface, and very simple instrumentation is capable of exhibiting it. It seems *so* obvious, even trivial, now that it is difficult to imagine how many very skilled observers of nature passed so close to this fundamental discovery without stumbling on to it. It is a bit reminiscent of the (apocryphal?) story of Columbus' challenging the detractors of his discovery of the new world to balance an egg on its end. It *is* trivial once the insight has been achieved, but how to stumble upon it??? Without – hopefully – belaboring the point, or romanticizing the mythology of the foundations of our discipline too much (nor trivializing it), let me emphasize that this major discovery was not "dumb luck"; it was quite the contrary in my view. When Leighton was looking for one thing and discovered something quite different, that initiated a whole new area of science, he was first of all looking directly at nature, without any intermediary, and doing so with an innovative technique that – incidentally and virtually simultaneously – made another fundamental discovery ("supergranulation"). He was doing so without extensive conceptual baggage, but with an obvious flair for sensing novelty, for expecting the unexpected. The original paper (Leighton, Noyes, and Simon, 1962) stands as a model for characterizing a new phenomenon. While helioseismology's origin lies in an "accident waiting to happen", it was an inspired accident.

But, in and of themselves, the "five-minute oscillations" of the Sun's surface were just a curious phenomenon, worthy of an explanation, but not particularly dramatic. It was far from obvious that they would "lead anywhere". The paradigm of those days – before the term became so hackneyed – suggested that the acoustic spectrum generated by convective "turbulence" should be quite broad, and there was no expectation whatsoever that a particular frequency, or frequencies, should be singled out. There was no already-known mechanism that should produce such an effect, so there was nothing to look for! While there was absolutely no suggestion, or even a hint, that the phenomenon of "five-minute oscillations" might exist prior to their discovery, a plethora of distinct theoretical models sprang into existence quite rapidly, all capable of "explaining" the unanticipated oscillations in very simple, straightforward fashions. [I will stop putting "explain" in quotation marks; my point being simply that we really do abuse the term in conversational science as we *describe* phenomena, hopefully with ever increasing validity, but it is rare that we can categorically *explain* them.] It is truly remarkable that all of these good ideas about how a stellar atmosphere might behave had not been pursued prior to the observational discovery; that they had not been advanced on the basis of pure thought. It rather gives the lie to the assertion attributed to Eddington that an astronomer on a cloud-covered planet should be capable of deriving a complete description of the Universe. Inductive logic would appear to be no match for a novel technique and an open mind. The converse of our lack of prior consideration of all of these mechanisms, in the absence of a good – that is observation-driven – reason to do so, is our possibly hasty discarding of all of the mechanisms that appeared as providing plausible descriptions of the "five-minute oscillations" but that were not supported by the observed modal structure of the spatial-temporal power spectrum. If they were reasonable things to be happening, they probably are! Some place in the Universe, maybe even in the Sun, they should be operating and we should be looking for their signatures, but I digress.

There were many, many more "constructive accidents", or incidents of "falling forward" in the early history of helioseismology. Bob Stein and I set out to demonstrate that the five-minute oscillations could be trapped in the photosphere-chromosphere temperature minimum, but the calculations were plagued by oscillations trapped below the visible surface, which we tried vainly to get rid of – since we just "knew" that they were artifacts – for the longest time before realizing that they were the answer, knocking us between the eyes. I recall Roger Ulrich (1970) acknowledging that he too was surprised initially when his calculations unexpectedly grew in time, before convincing himself that this was in fact a reasonable result of the κ-mechanism and a plausible description of the observed phenomenon. It is also worth recalling that there were very good reasons for believing that sounds waves trapped below the surface could not possibly maintain phase coherence and thus form resonant, normal modes.

Deubner's beautiful demonstration [Figure 1] that the oscillations could indeed be described as normal modes of the solar interior was possibly the first "correct" move in answering "where have we come from?", but I hazard to guess that if we pushed him a bit, there were a few "accidents" along the way to his major contribution to the development of our discipline which – in some sense – closed out a nearly textbook case of the idealized "scientific method": 1) an unanticipated phenomenon was discovered, quite "by chance", 2) a variety of models were advanced to describe the observation, with testable predictions, 3) the predictions of one of the models were, rather surprisingly, confirmed by subsequent observations.

Interesting though this phenomenon and its description might be, our discipline would not exist had the story concluded there. But Deubner's seminal work already showed very clearly that while the oscillatory power did exhibit resonant ridges of power in the "diagnostic diagram" or "$k - \omega$ diagram" as the two-dimensional power spectra were called in those antediluvian days, there were very significant differences between the observed frequencies and those calculated by Charlie Wolff (1972), and Hiroyasu Ando and Yoji Osaki (1975) utilizing the then-accepted models of the solar interior. It was this fourth step – the appreciation that not only was the description of the observed five-minute oscillations as sound waves trapped below the solar surface a good model of the phenomenon, but that the remaining differences between the predictions and observations could be used as an effective tool to test, and thus modify, our relatively poorly constrained models of solar, and stellar, structure that helioseismology really came into existence. It is frightfully dangerous, and terribly simplistic – if not simple-minded! – to try to distill the work of dozens of researchers to a few steps like this. This is a cartoon sketched to provide some context for the Symposium, not a history! The cartoon would, however, be dramatically incomplete without a caption, and that would be the term

https://doi.org/10.1017/S0074180900060988 Published online by Cambridge University Press

"helioseismology" itself, which Douglas Gough bludgeoned us into accepting, and which has served so nicely to describe our discipline's content as both a tool and a science. At the same time that we are studying the physics of the oscillations as phenomena in their own right, we are using our working understanding of them as a tool to explore solar structure.

3. Where are we now? Helioseismology is Nice

of course! It is particularly appropriate to be holding this Symposium in Nice, and most fitting – though tragic – to be honoring Philippe, who himself had started the planning for the meeting. If the 1975 conference on the "Physique des Mouvements dans les Atmosphères Stellaires" was a landmark for helioseismology, the real potential was demonstrated the next year – again here in Nice – at IAU Colloquium Number 36 entitled "The Energy Balance and Hydrodynamics of the Solar Chromosphere and Corona" organized by Philippe and Roger Bonnet, where Douglas Gough (1977) pointed out that the discrepancy between the observed and predicted frequencies could be accounted for by a substantial ($\approx 50\%$) increase in the depth of the convection zone; the first major contribution of helioseismology. The pace of new results has not slackened since.

Helioseismology is very nice indeed when one reflects on what it has achieved in less than a twenty two year Hale cycle what assets have been brought to bear to advance the science further, and the community of interest that it has engendered.

After discovering the discrepancy in the depth in the convection zone, helioseismology went on – in very short order – to demonstrate that the deep interior could not be rotating sufficiently rapidly to give rise to a gravitational quadrupole moment sufficiently large to invalidate the support given to general relativity by the precession of Mercury's orbit (as had been ardently proposed), that the interior could not possibly be as cool as some *ad hoc* theories – proposed to address the observed deficit of neutrinos – would have required, and that the descriptions of the opacity and the equation of state of matter at the temperatures and densities representative of stellar interiors were substantially incomplete; *big* issues for all of astrophysics, catapulting helioseismology to a position of great visibility in astronomy, physics, and before the general public.

These "big physics" issues continue to occupy a very prominent position in helioseismology, of course, as we obtain better data, develop better analysis techniques, and advance our understanding of the problems themselves. However, a nice indication of the overall vitality of the discipline has been the continuing addition of new areas of investigation, *e.g.* sunspot seismology and more generally "local helioseismology", time-distance methodologies, temporal variations of the amplitudes, lifetimes, and frequencies of the normal modes on scales from hours to the eleven-year activity cycle, and the physics of the modes themselves as seen, for example, in the asymmetries of the normal mode resonances and in the differences between the atmospheric responses at different heights, or in different diagnostics. In large measure, helioseismology has been defined by its promise to address some of the most major issues in contemporary astrophysics, to which it has beautifully responded, but it continues to grow in importance because of its ability to open new dimensions to the existing questions, and – *most importantly* – by continuing to pose exciting, new questions of broad interest.

At the same time that we congratulate ourselves on the very rapid strides forward that we have made and the ease with which we have succeeded, it may be prudent to reflect on the rigor that we may have put aside "for just the moment" as we rushed forward. Our rough analyses, quite often using techniques developed for other problems and cobbled together to apply to our new data and scientific questions, have been remarkably useful and successful. However, just to give an example or two, the decomposition of our images into spherical harmonics assuming that the oscillatory velocities are purely radial or that the state variables do not display any center-to-limb variations is certainly not correct, and may well give rise to systematic errors of potentially significant magnitude. Ignoring the frequency asymmetries of the resonance peaks in our estimation of eigenfrequencies is almost certainly guaranteed to change our structural inferences systematically, and the apparent difference in the asymmetries seen in velocity and intensity spectra may by signaling even more serious difficulties. Our use of simple fourier transforms and rudimentary time series analyses has our colleagues in statistics grinding their teeth, and the list of "thin ice" goes on and on. In addition to the simple work-arounds of these first-generation problems – that, I should hasten to point out, have served us remarkably well – there are many areas in our analyses where we have simply not had the time to develop more sophisticated approaches to reap the maximum benefit from the data. I am thinking of mode leakage in the spherical harmonic decomposition, or optimal use of all of the individual frequencies in our inversions. Once again, I am not suggesting that we have been sloppy, but rather that the "quick and dirty" techniques worked so well, and gave such interesting results, that we were justified in rushing forward, but now we should reflect upon the assumptions made and short cuts taken, and critically evaluate our methodologies.

The extraordinary scientific potential, and support for fulfilling it, has engendered a correspondingly impressive array of observational assets summarized in Table 1.

TABLE 1. Some Current Helioseismology Projects

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IRIS
TON
SoHO - GOLF
SoHO - SOI/MDI
SoHO - VIRGO
and single site programs
Crimea
El Teide
Haleakala
Kitt Peak
Mauna Loa
Mount Wilson
Napoli
Roma

Just for the record – it is unlikely to be lost on the participants at this Symposium – this armada of telescopes is required by the challenges of the current observational requirements and particularly by the subtlety of the measurements and the identification and removal of observational, or diagnostic-related, artifacts. While the basic oscillation signal is extremely robust, the current state of the art depends on the combining and differencing of many, many individual measurements; identifying and overcoming the tiny, tiny systematic errors is of paramount importance. We desperately need the independent, and complementary, observations to believe any of this!

The final aspect of where we are – our current assets – is the vigorous community as witnessed by the number and demographics of the participants at this meeting. For a discipline that literally did not exist twenty years ago, it is I believe unique in the annals of solar physics, and I hope the participants can sense the momentum of the discipline of which they are a part, and exploit the opportunity that this confluence of exciting and important scientific questions, extraordinary observational capabilities, and brilliant talent represents.

The community has developed in a rather harmonious way, I believe. Data exchange is very free and open to all, and this is a tremendous strength of course. While there remain many problems where a single, isolated investigator can work effectively and contribute significantly, I believe that the nature of many of the problems is such, and we have grown together in such a way, that the most effective way to attack many of the most "important" problems is through substantial collaborations, and this has been very nicely demonstrated in the several "hare and hounds" activities that have tested various inversions techniques, as well as the team publications of first results – where sundry analysis techniques were very nicely compared and a syntheses emerged that was – in may senses – a significantly more useful advancement of the subject than the sum of the individual works.

The large quantity of excellent and widely available data, the complexity and subtlety of the analysis, and the wide range of physical problems being addressed rather inevitably leads to some interesting changes in the way that the community works. Glancing at my bookshelf, I count seventeen major, international conferences devoted to helioseismology in the last twenty years, and I may well have overlooked one or two. Helioseismology has become something of a separate discipline within solar physics and astrophysics, and while the size and productivity of our activity is wonderful, we really must guard against becoming isolated from our colleagues in closely related fields. We are scientists, problem setters, not just problem solvers – although that is a lot of fun too!

4. In the guise of a conclusion – Helioseismology is just beginning

to become an established science, entering its second generation. Let me use the conclusion of this Introduction to put forward a few questions that hopefully will all be well answered within far less than the twenty years that separate us from the beginnings of helioseismology. And, even then, I think that we are justified in the well-founded assumption that they will be replaced with more, exciting, new questions derived from their answers.

- What improvements in our techniques of analysis of the data should be pursued?
- How can we render more certain our inferences from the data?
- What remains unknown in our description and modelling of the oscillations?
- What remains to be learned about the microphysics of stellar structure?
- What major uncertainties about the macrophysics of stellar structure are becoming amenable to attack?
- How well can we describe the phenomena of the Sun?

- Can g-modes by unambiguously identified in the Sun?
- How can we best identify p-mode analogues unambiguously in the Sun?

Our techniques for inferring parameters – transforming measurements of light emitted by the surface of the Sun into numbers that can be directly compared to theoretical predictions, or in some cases directly input to adjust the theory – have been spectacularly successful, while at the same time they have been rather crude. I am thinking here of better ways of fitting the observations to our model (that they arise from a superposition of independently excited normal modes of oscillation of a strongly inhomogeneous, differentially rotating fluid), and *vice versa.*

Whatever the techniques for relating observations to models, there will remain uncertainties ("noise") and outright lack of knowledge (temporal and spatial incompleteness) in our observations and a major challenge for the second generation of helioseismology will be to render these inferences less biased by our techniques and optimally representing constraints on the observational model.

It is only in the last couple of years that a good model for the excitation of the modes has emerged, and it is quite remarkable in hindsight to see what great strides were made using the frequencies of the modes with extremely little idea of how they were actually driven – thank goodness for linear phenomena! But, our knowledge of the physics of their excitation – and damping – as well as their behaviour through the visible atmosphere still leaves a great deal to be desired, and I anticipate considerable progress in this area in the immediate future; *e.g.* how does the energy in a mode vary with time, to what extent do excitation "events" couple various modes together or imprint similar signatures on different modes.

With the advent of helioseismic probing of the internal conditions of the Sun, deficiencies in our description of matter in the interesting temperature and density regimes became quickly apparent. As the precision of the measurements and the robustness of the inversions improves, how much further can we tighten these constraints and how much further can we extend the ranges of temperatures and densities over which they obtain? People really want to know!

A number of very significant astrophysical processes (*e.g.* rotational shear induced mixing, penetrative convection, diffusion, circulation currents) have become fairly directly accessible to our sounding using helioseismology. I think that it is fair to assume that our first attempts in these areas will not be uniformly successful and that we have a lot of changes in store. The elucidation of these processes may be one of helioseismology's most important demonstrations of the Sun's role as an astrophysical laboratory.

Even were the macrophysics of stellar internal structure to be fairly well understood, it is a considerable leap forward to be able to characterize the major phenomena of the solar interior such as differential rotation, the generation of magnetic fields and the twenty two year cycle of magnetic activity. For generations to come, the Sun will continue to provide the one place in the Universe where we can study these intriguing phenomena.

While helioseismology was at its outset driven by observational discoveries for which we were entirely unprepared, we have found ourselves for some time now with two major theoretically predicted phenomenon that have not been established observationally: internal gravity modes, and p-mode analogues in other, solar-type, stars. The search for these "holy grails" has provided a sort of frontier spirit to helioseismology. While all of the advances back in the better understood territory of p-mode helioseismology have been moving forward, the lure of these uncharted wildernesses continues to be enormously seductive. There is very little question that both must surely exist – the physics is quite straight-forward – and there have been numerous suggestions of their identification over the years, but their wileyness has become really quite frustrating on one hand, and enervating on the other. The potential new knowledge represented by each of them is staggering, as the number of contributions devoted to them at this Symposium bears witness. Gravity mode frequencies promise dramatically improved diagnostics of the deep solar interior, as well as all of the new insights that we are confidant that the existence of a new wave mode will offer.

In many way, we are better prepared – in theory at least – to deal with the implications of p-mode measurements on other stars, given their close analogy to the physics of the modes within the Sun; however the potential for surprises may be even greater. In some sense, we are calibrating the values of the parameters and processes that control stellar structure for one set of conditions with our helioseismic sounding. Asteroseismology offers the possibility of measuring the gradients of the parameters and processes!

It is simple enough to look back and see how helioseismology has developed from nothing over the past two decades, and we can all marvel at the sorts of questions that we can pose today and the precision with which we can offer answers. Yet the promise of the physics that we have at hand and the data that we are now in the process of acquiring is astonishing by comparison, while the potential of g-modes and of stellar p-modes is staggering. And, it may not take another twenty years to fulfill their promise, and open the next chapter!

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