Inside the Stars, IAU Colloquium 137
ASP Conference Series, Vol. 40, 1993
Werner W. Weiss and Annie Baglin (eds.)

IMPACT OF OBSERVATIONS ON PREJUDICE AND INPUT PHYSICS

D.O.GOUGH
Institute of Astronomy, Madingley Road, Cambridge CB3 0HA, England
and Department of Applied Mathematics and Theoretical Physics,
University of Cambridge

ABSTRACT My brief from the principal organizers of this colloquium is to draw your attention to some of the prejudices upon which the observations that have been discussed here have had or should have had some impact, and to remark on how that might revise our views about physics: it should not be a balanced summary of the meeting, but a prejudiced review.

It appears that the most outstanding prejudice that has come across throughout this meeting is that we, the astrophysicists (I use the term quite literally), believe that we know where we are heading and how to get there. It is very encouraging that this is more than a prejudice; it is also the judgement of our principal guest, M S Longair, who is here representing the rest of astronomy. The reason for our confidence, I believe, is that our understanding of the insides of stars is based on the solid foundations laid down by great men, Eddington and Chandrasekhar, upon whose work a more elaborate theory was firmly built by Hoyle, Schwarzschild, Kippenhahn and his collaborators, and by Iben, once high-speed electronic computers became available. However, the subject then appeared to become moribund: so successful were the pioneers that it appeared to the outsider that there was nothing more to do, except merely to clear up a few minor details. But the astute realized that those details were possibly important clues to more profound understanding, and laboured to unravel the evidence that has led to new discoveries, many of which have been under discussion this week.

As is always the case in man's endeavour to understand the physical world, the way was led by the observers, whose great advances particularly in photometry and spectroscopy have pinned down the surface properties of stars much more precisely; moreover, A Gomez has given us an inkling of what further information will soon become available, both photometric and astronometric, when the Hipparcos data are released. But more exciting yet is the possibility of seismic information, which truly probes the stellar interiors. Professor R A Lyttleton once remarked: 'If a modern astronomer were to meet a nineteenth-century chimney sweep, he would deduce that the sweep were made entirely of carbon.' Helioseismology has, and asteroseismology soon will render that remark outdated.

Seismology is the newest of the powerful observational tools to become available to astrophysicists. A great deal has already been learnt about the inside of the sun from seismic observation, and corresponding advances in our perscrutation of other stars will, in the future, take place. Seismic calibration of stellar models will be the first technique to bear fruit, provided it is carried out intelligently with insight such as that afforded by J Christensen-Dalsgaard's penetrative review. Inversions that answer specific questions about internal structure will subsequently become possible, once suitable data are available. Initial inversions of artificial stellar data indicate that a high degree of precision is required, and I am sure that such precision is attainable by campaigns such as PRISMA, notwithstanding any dubiety expressed by the sceptics. I recall, after a lecture such as this about the sun delivered at a Joint Discussion of the IAU General Assembly in Grenoble only fifteen years ago, that there was expressed not only scepticism but even outright disbelief that helioseismology would ever bear fruit. The principal objection, which stemmed from one of this week's lecturers (who now appears to have been at least partially converted), was that only high-overtone oscillations have been and were likely to be observed, and that since they form an harmonic sequence, only one new piece of information can ever be obtained from seismology (I paraphrase). My reply, of course, was that with adequate accuracy the one-per-cent deviations of the low-degree overtone frequencies from the harmonic sequence, and the similar small deviations of higher-degree modes from the predictions of some theoretical solar model, would reveal the true solar interior. The prejudice of the sceptics was that such accuracy could never be achieved. The reality, however, demonstrated by the outstanding progress made by the observers, is now that K G Libbrecht and M F Woodard illustrate data with error bars to ± 1000 standard deviations in order that the uncertainty can be seen, and that G R Isaak offers us the promise of low-degree frequencies accurate to a part in 10^5 in the not-too-distant future. Even after stripping the almost useless first two digits from each datum, one still has plenty in hand to make inferences that teach us some real physics.

What physics have we learnt so far? Perhaps the most broadly relevant to the immediate subject-matter of this meeting is the importance of the contribution to stellar opacity of *LS* coupling in radiatively induced atomic transitions. It was the discovery in 1985 that the sound speed throughout much of the radiative interior of the sun is up to one per cent greater than it was in the solar models of the day that led to the realization that the opacity immediately beneath the convection zone had probably been underestimated by up to some 20 per cent. Encouraged by several helioseismologists, principally W Däppen, the opacity in this region was reassessed at Livermore by C A Iglesias and F J Rogers. And indeed, at temperatures and densities representative of the outer layers of the radiative region of the solar interior, the seismological prediction was confirmed. But in addition, as we heard in M J Seaton's review, the more careful new computations have also yielded values of the opacity at temperatures of several hundred thousand Kelvin that are up to three times those obtained previously at Los Alamos. Although not relevant to the sun, this result has far-reaching implications for a variety of other stars.

There has been an almost universal prejudice amongst stellar modellers that the tables of opacity with which they have been supplied are not wholly reliable. That is not to belittle the work of the Los Alamos group; their achievement was truly a major advance of the time. However, in trying to understand certain quite specific properties of stellar pulsation, some workers, such as R F Stellingwerf, have been moved to suggest artificial modifications to the tables.

The ramifications of the new computations by Iglesias and Rogers, however, are more diverse: we have heard already from Christensen-Dalsgaard, W A Dziembowski, G Michaud and their colleagues that with the new opacities there is no longer a problem in explaining simultaneously the period ratios of double-mode Cepheids and δ Scuti stars, that the twenty-five-year-old problem of finding a viable mechanism to excite pulsations in β Cephei stars has been resolved, and that the observed lithium gap is more closely reproduced by calculations of element segregation against diffusion. New tables from the ambitious Opacity Project will soon be available; as Seaton has described, they are somewhat similar to the Livermore values where the latter are available. Of course, no good scientist will believe them unquestioningly to the last digit. However, much of the prejudice developed against the earlier tables has evidently already been dissolved.

Another arena in which seismic observation is teaching us physics is the study of the thermodynamic properties of plasmas. That has come about from an investigation of the adiabatic compressibility of the solar plasma under conditions inducing the second ionization of helium. The initial motivation was to determine the primordial solar helium abundance, a quantity of considerable cosmological importance. But, of course, the transition from adiabatic compressibility to helium abundance can be made only via the equation of state, which, as Däppen has explained, is inadequately known for modelling the sun with helioseismic accuracy. Gone now is the prejudice, held previously by many a stellar modeller, that fine details contributing only a few tenths per cent to the free energy of a gas are astrophysically irrelevant. Indeed, a recent yet unpublished collaboration involving V A Baturin and S V Vorontsov has produced a calibration of a grid of solar envelope models against the seismic data of Libbrecht and Woodard. The equation of state includes a free parameter representing the finite size of bound charged species, and with plausible choices of heavy-element abundance, the calibration yields an effective radius of several Bohr radii. I am not claiming that that radius has been definitively measured, for there may be other aspects of the microscopic properties of the plasma that have been inadequately modelled. What is exciting about the investigation, however, is that the effect has been detected. And it is often the case that once a phenomenon such as this has been detected, a clear measurement of it follows soon afterwards. That demonstrates the existence of yet another field in which astronomy can provide a delicate testbed for more fundamental physics.

Although the full implications of the uncertainties in the equation of state have yet to be investigated, it is appearing to be the case that the helium abundance in the solar convection zone is substantially lower than the value that properly calibrated standard evolutionary models require. Michaud has explained why that should be. But the settling he has calculated with C R Proffitt is rather less than is required to explain the difference. Therefore there still remains some discrepancy to be removed. We must not forget, however, that there are also other inconsistencies to resolve. So long as the solar neutrino problem, in particular, remains unexplained, one should regard the theoretical models with a due measure of suspicion.

Despite that by virtue of its relation to the rest of physics the solar neutrino problem is potentially the most important issue we face at present, I shall not discuss it extensively, because R K Ulrich has already explained part of the controversy in more detail than I have space here. (One might recall in passing, however, D R O Morrison's controversial claim that the controversy does not exist.) Instead I shall address a recent protest against a prejudice which Ulrich did not discuss because, as he pointed out in response to a question, it is not controversial and was therefore inappropriate material for his lecture. It concerns an issue raised recently in a preprint by R L Kurucz questioning the rates of reactions such as $p(p,e^+\nu)D$. Together with the other deuterium-producing reaction, $p(pe^-,\nu)D$, it is the bottleneck of the p-p chain, and thus controls the rate of evolution of all late-type main-sequence stars (though the control is not as strong as one might naively think). Thus, it can hardly be irrelevant to our measuring of the ages of the globular clusters, which is accomplished by comparing the position of the main-sequence turnoff on the H-R diagram with theory, and which, as Longair stressed in his introduction, plays a major role in the story of the value of Hubble's constant H_0. Moreover, according to N Langer, these two are the only important reactions of the chain whose cross-sections have not been measured experimentally at any energy: the rates are purely the product of theory. That a serious misgiving should have been voiced is therefore reason enough to discuss the issue here. In principle, so far as I can understand it, the computation of the mean reaction rate should contain first the evaluation $\mathcal{B}$ of the probability of a single reaction, which involves a two-body or a three-body barrier penetration calculation (in the presence of the rest of the plasma), followed by a statistical-mechanical average $\mathcal{A}$ over the ensemble of all possible states. If the plasma were dilute, its presence would produce but a small perturbation to what $\mathcal{B}$ would have been were there to have been only the two reacting particles, p+p, in the Universe (or p+p+e in the case of the pep reaction), and the operations $\mathcal{B}$ and, in particular, the component $\mathcal{A}_e$ of $\mathcal{A}$ over electron states would commute, enabling one to perform the simpler calculation $\mathcal{B}\mathcal{A}_e$ in place of $\mathcal{A}_e\mathcal{B}$. The same would be true also of all the other nuclear reactions in the pp chain and the CNO cycle, the assumed commutation of $\mathcal{A}_e$ and $\mathcal{B}$ providing the basis for the Salpeter electron-screening corrections. What Kurucz has questioned is the validity of the commutation assumption. He points out that the admittedly unlikely event of the electron, considered in a semi-classical sense, being close to the two protons near the instant of maximum repulsion provides so great a perturbation in a dense plasma that it must be computed prior to the averaging, and that when that is done the outcome for the deuterium-producing reactions differs from the usual calculation $\mathcal{B}\mathcal{A}_e$ by so much that the neutrino flux predicted for Kamiokande and the ^{37}Cl detector is reduced to the observed value. Although Kurucz is undoubtedly formally correct about the order in which the calculations should be carried out, I find it difficult to believe that the process he discusses could have so great an effect, because only a relatively minor contribution to the barrier-penetration probability integral comes from small proton-proton separations. Kurucz does not quote explicitly the factors by which the reactions are augmented, though one can deduce from the information he provides that the rate of the combined deuterium-producing reactions is increased by a factor in excess of 5. As Kurucz points out, the main outcome is to decrease the temperature in the core of the sun, leading to a reduction in the ^{7}Be and ^{8}B neutrino fluxes (assuming, implicitly, that the process does not also substantially augment the ^{7}Be- and ^{8}B-producing reactions). This is partially offset by a decrease in the initial hydrogen abundance required to produce the observed photon luminosity at the appropriate age, which contributes a slight enhancement of the neutrino flux, but this is rather small compared with the influence of the modification to the temperature. [J Christensen-Dalsgaard commented immediately after this lecture that he disbelieved that Kurucz's argument could reduce the neutrino flux. He reported that he had carried out a computation in which he had augmented all the nuclear reaction rates by multiplying the screening correction in Salpeter's formula (which is computed as $\mathcal{B}\mathcal{A}_e$) by the same constant factor, and that in that case the resulting neutrino flux had *increased*. The formal result is quite easy to understand: by balancing the reactions in the chain it is straightforward to show that, because of the charge dependence of the screening, the ppII and ppIII branches are enhanced relative to ppI by more than the reduction due to the decline in temperature. However, enhancement of the $\mathcal{B}\mathcal{A}_e$ screening correction by identical factors to mimic $\mathcal{A}_e\mathcal{B}$ is hardly what Kurucz had in mind. One might add, in passing, that Salpeter's screening of the target does not take account of the incident proton, whose effect is to decrease, not increase, the screening factor, and therefore to *reduce* the neutrino flux.]

In an extensively circulated response to Kurucz's argument, J N Bahcall and E E Salpeter have pointed out that because electrons are not well localized they cannot influence the barrier penetration probability by as much as Kurucz claims. From the semiclassical viewpoint the electron is too rarely in the vicinity of the protons near the instant of maximum repulsion. Bahcall and Salpeter did not address explicitly the extent to which $\mathcal{A}_e$ and $\mathcal{B}$ fail to commute. However, if one takes into account the spread of the electron wave function as Bahcall and Salpeter require, it appears that even if the barrier penetration probability were to be increased to unity when the electron, considered as a particle, were near the colliding protons, after averaging over phase space the influence on the mean reaction rate should not be very great. However, it would nonetheless certainly be worthwhile investigating the extent to which fluctuations in the electron screening (other than those induced by the relatively slow movement of the protons, which has already been considered) do modify the Salpeter formula and its recent extensions.

There have been many other prejudices expressed at this meeting, none of which I have space to address in any detail. One that I cannot refrain from just mentioning, however, is the presumed universality of the so-called mixing-length parameter α. I find that prejudice difficult to understand, particularly because different workers use different formulae to relate heat flux to temperature gradient, often without even stating which one they have used, yet they are not unhappy then to compare the values of α they prefer. The desire for a parameterless formula has been expressed several times at this colloquium. That is quite understandable. Nonetheless, I regard it to be a more pressing goal to find a more faithful representation of the physics, even if that does require calibration against observation. That is, of course, only my prejudice. I even go so far as to consider it essential that for a theory of convection (and not only convection, for that matter) to be trustworthy, it should work also in the laboratory. That opinion is almost heretical, most stellar modellers claiming that conditions in stars are so different from those on Earth that no meaningful comparison can be made. Yet, aside from the final assumption that the mixing length is proportional to the pressure (or density) scale height, the astrophysical formulation of the mixing-length theory does not explicitly demand that the convection be in a star, and in any case the approximations to the equations of fluid motion that are used are actually more suited to terrestrial than to stellar conditions. My plea is that because we do not have an adequate theory of convection we calibrate α across the H-R diagram by comparing stellar models with observation, finding how it depends on luminosity, effective temperature and chemical composition. Then at least we shall have some useful foundation with which to compare future theories. We might then be using astronomy as a useful 'experimental' tool to test macroscopic physics. Needless to say, the results of the calibration should be accompanied by an explicit statement of precisely which formulae for the heat flux and the Reynolds stress have been used.

The importance of convective overshoot and semiconvection to stellar evolution has been stressed several times, notably by J P Zahn and A Noels, but also by others. After many years of disagreement, Zahn now appears to agree with Roxburgh's formulation of overshoot. However, as Roxburgh emphasized earlier (admittedly in a different context) we must not be misled by that: 'When people agree it does not necessarily mean that they are right.' With overshooting from cores seeming to take on a lesser role than was previously believed, the problems of semiconvection are again coming under scrutiny. The prejudices concerning the final state that is attained when gradients of temperature and molecular weight are in opposition are, as is astrophysically popular, based on neutrality with respect to some stability condition. As I understand it, preferences range from the so-called 'Ledoux criterion', which is a restatement of the *linear* convective stability criterion introduced into the astronomical literature by K Schwarzschild, and the 'Schwarzschild criterion', which is what that convective stability criterion would have been were gradients in chemical composition to have been ignored, and which, before (and even after) Ledoux's paper to point it out was published, had been mistaken for the criterion introduced by K Schwarzschild, and furthermore which M Schwarzschild and R Härm considered to be the approximate outcome of the *nonlinear* development of the instability. It might be of interest at this point just to throw in the prejudice of the fluid dynamicists: that the mean stratification adjusts itself into thin horizontal layers of essentially homogeneous isentropic convection separated by diffusive interfaces across which the jumps in temperature and chemical inhomogeneity are such as to render the ratio of their fluxes equal to the square root of the ratio of the corresponding microscopic diffusion coefficients, irrespective of any other conditions controlling the flow. That prejudice is well confirmed by laboratory experiment. And it would provide a powerful tool for predicting the stratification of chemical abundances in stars if indeed it really could be trusted under all conditions. Unfortunately, however, the only dynamical analysis of the phenomenon of which I am aware, and which admittedly is far from being satisfactory, predicts that under the very different conditions encountered in stars the behaviour of this phenomenon is actually quite different from laboratory experience.

Can any of the prejudices be tested? Christensen-Dalsgaard indicated how one might differentiate seismologically between two ideal stellar models, one with and one without overshoot, using the small separation $d_{n,l} = \nu_{n,l} - \nu_{n-1,l+2}$ between the frequencies $\nu_{n,l}$ of acoustic modes of high order n and low degree l. However, some care must be taken to take into account asphericity, which also contributes to $d_{n,l}$ and which is great enough in some stars to be quite substantial. It is also necessary to seek more subtle signatures which one might hope to use for distinguishing between regions of overshoot and of semiconvection. It is worth remarking also that there are two posters addressing the possibility of detecting overshoot beneath convective envelopes. The conclusions appear to differ. It is interesting that those authors to whose hearts overshooting is the more dear appear to have invested enough effort to find evidence in favour of it, whereas the others did not. This is yet another example of how prejudice has an impact on physics.

Any session on stellar dynamos is always so steeped in prejudice that one runs the risk of forgetting what a dynamo actually is. If one is to try selling one to a national electricity generating organization, one obvious prerequisite is that, after subtracting internal ohmic decay, the electrical output from the mechanical work invested exceeds zero. Most of the dynamo theorists I know who are not also astrophysicists accept that prerequisite too. But stellar physicists, in general, do not. Even the existence of a dynamo so defined can hardly be demonstrated in stars, since the decay time of a large-scale magnetic field in the radiative zone of a star is either greater than or comparable with the lifetime of that star. F Krause argued that the reversal of the exterior field on any timescale much shorter is proof of the presence of a dynamo, and C Schrijver seemed implicitly to have taken that for granted. Implicit also in the discussions seemed to be that the dynamo exists in the body of the convection zone. Yet dynamical modellers of the solar convection zone have failed to reproduce simultaneously the field reversals, the butterfly diagram and the so-called torsional oscillations of the photospheric layers. Moreover, they finally seem to have capitulated in the face of the suggestion that the angular velocity throughout the convection zone inferred from seismology appears to be a function only of latitude, whereas the models require it to be approximately a function of only distance from the rotation axis, at least at distances greater than the radius of the base of the convection zone. Consequently, they have retreated just beneath the base of the convection zone, where it was suggested by E A Spiegel and N O Weiss that the dynamo must reside. If that were the case, then one would expect such dynamos to be absent from fully convective stars, for in those stars convection zones have no base. That is why P Podsiadlowski asked Schrijver whether what he had called dynamo activity was absent in fully convective stars. What he was really asking, of course, was whether there is any evidence for the dynamo being located immediately beneath the convection zone. Schrijver's answer was: 'No.' So where does that leave us?[1] Something one might reexamine is the interpretation of the seismological evidence. At present the solar rotational splitting data are expressed in terms of a severely truncated expansion which certainly does not preclude the possibility that in the equatorial regions the angular velocity in the convection zone is a function of only distance from the rotation axis. If one were to adopt that functional form as a constraint, one would find that the rotation in

[1] After the meeting I learned from Podsiadlowski that F. d'Antona had told him later that ROSAT data showed much lesser X-ray emission from late-type main-sequence stars that are expected to be fully convective, suggesting that such stars do have much weaker magnetic fields.

the polar regions would be inferred to be slower than was previously expected, and not so smoothly varying with position. Whether one finds that to be less or more credible than the alternative I mentioned previously depends solely on one's prejudice. So perhaps Krause is right. Indeed, in the poster by Libbrecht *et al* the rather closely spaced plot of the contours of constant angular velocity deduced seismically is actually not very unlike that wanted by the dynamical modellers of the convection zone.

It is perhaps worth pointing out that having prejudices is not unique to stellar physics. Indeed, Longair, in his wide-ranging introductory lecture, presented us with observational evidence which, when naively interpreted, leads to the following values of the deceleration parameter, Hubble's constant and the density parameter: $q_0 = 3.5, H_0 = 50 - 100\ \mathrm{km\,s^{-1}Mpc^{-1}}, \Omega \simeq \alpha$. (The last value is my paraphrase, α now being the fine structure constant, not to be confused with the mixing-length parameter which is not a constant.) Yet Longair clearly gave us the impression that he believed that $q_0 = 0.5 \pm 0.5$ and $\Omega = 1$, even if he didn't say so explicitly. The latter, so far as I can see, is based purely on theology, or perhaps on aesthetics. Yet perhaps that is more reliable a criterion than some scientists might readily accept. Knowledge comes from a variety of different sources, and one should not reject any evidence without first weighing it carefully. That $\Omega > \alpha$ provides the case for the existence of dark matter. Given that the matter has not been detected directly, it must surely be the case too that whether one believes it to be hot or cold is largely a matter of prejudice. Longair clearly described the case against it being hot, and left one with the impression that he thought it must be cold. This, indeed, is the popular belief. From my outsider's naive understanding of Longair's talk, however, it seems that if it is to account for the properties of large-scale structure, it might be simpler for us if dark matter were to be luke warm (which might actually be a mixture of hot and cold).

Before concluding, at the request of W Weiss I include a provocative remark. I have chosen to draw your attention to the poster by R Vera, which explains how there might be neutron stars in the cores of main-sequence stars, and how, in particular, the existence of one at the centre of the sun would explain, amongst other things, the low neutrino flux. This is reminiscent of an idea by S W Hawking, who many years ago suggested that the neutrino problem is solved by a black hole in the solar core. I had often wondered whether, if there were such a condensed body in the sun, it would remain at the centre. For might not it lead to an overstability, though of a kind rather different from that discussed earlier this week by Y Osaki? Consider the condensed body to receive a small perturbative impulse, leading to motion and a consequent asymmetry in the accretion flow around it. The compression of the reacting gas in the wake would induce a localized increase in the nuclear energy generation rate, leading to an expansion of the heated gases, as in a jet engine. The thrust would be insufficient to counter gravity, so the body would still accelerate towards the centre of the sun, and oscillate much as it would in the absence of the engine. But the direction of the thrust is evidently such as to augment the amplitude of oscillation. The increase cannot continue forever, however. Once the body rises far from the core, energy generation in the wake ceases, and the motion is retarded by accretion of matter having relatively low momentum. Thus a balance between driving in the core and damping in the envelope is reached (as is also the case for the oscillation of many other kinds of star). The period of oscillation depends on the amplitude at which that balance occurs, and if the trajectory were to extend almost to the surface of the sun it would be about 160 minutes. Therefore one might hope to test the theory by looking for an oscillation with this period. Early investigations suggested that indeed it might be present, but today there are only very few who believe the evidence to be convincing. So perhaps this story must be abandoned.

Finally, permit me to state my prejudice about prejudice. I believe that one should never approach a new scientific problem with an unbiassed mind. Without prior knowledge of the answer, how is one to know whether one has obtained the right result? But with prior knowledge, on the other hand, one can usually correct one's observations or one's theory until the outcome is correct. At that point one usually stops looking for more mistakes. One can then be content that one's understanding is confirmed, and receive a boost to one's ego (which is well understood in Vienna). However, there are rare occasions on which, no matter how hard one tries, one cannot arrive at the correct result. Once one has exhausted all possibilities for error, one is finally forced to abandon a prejudice, and redefine what one means by 'correct'. So painful is the experience that one does not forget it. That subsequent replacing of the old prejudice with a new one is what constitutes a gain in real knowledge. And that is what we, as scientists, continually pursue.