Part III.

Spherically-Symmetric Motions in Stellar Atmospheres.

A. - Pulsating Variable Stars.

Discussion.

Chairman: E. Schatzman and R. N. Thomas

— E. Böhm-Vitense:

If one considers a static model stellar atmosphere, characterized by $T_{\text{eff}} \sim 5\,000°$ and $g \sim 30$ cm/s², which are the values just discussed for some of the cepheids, he finds that the radiative acceleration – $g_r = -\varkappa \varrho F/c$ – can at some depths exceed the gravitational acceleration – $g = GM/R^2$. $\varkappa$ is the continuous absorption coefficient, and F is the radiative flux. This situation occurs at a depth where $T \sim 10\,000°$. Here, the net acceleration will be outward, and the gas pressure will decrease inward; in deeper layers, $\varkappa$ decreases and the effect reverses. Can this effect be of importance when considering cepheids?

— C. A. Whitney:

If you consider the fact that convective transport may reduce the radiative flux, will the radiative acceleration still predominate?

— E. Böhm-Vitense:

Yes. These models are just in the temperature-gravity region where the instability cannot cause convection because of radiative exchange between convective cells.

— W. B. Thompson:

Whitney must have taken this effect into account in discussing the non-static case. For if you have motions, then the effect of radiation will be to transform the hydrodynamic shock into a radiative shock, in which there is a transition from opaque to transparent regions occurring across the shock front.

— C. A. Whitney:

Mrs. Böhm-Vitense considers the $T = 10^4$ level; while my calculations apply to a much higher level, where the hydrostatic model predicts essentially neutral hydrogen.

— R. LÜST:

Two other questions. 1) What would be the effect of shock waves on the light-curve; 2) How does this picture of shock waves compare with Lautman's calculations?

— C. A. WHITNEY:

LAUTMAN used the non-dimensional equations, but assumed small amplitudes, so that in a sense the integrations were linear. He used the same lower boundary condition as I did, a piston. However, he used a frequency considerably lower than I did, and therefore more appropriate to cepheids.

At the top he used as boundary condition the relation between velocity and velocity gradient which would hold in the linearized case in the absence of downward-running waves. I am not sure that his conditions are physically significant because the situation is actually non-linear.

Another point to notice is that he also did integrations assuming perfect reflection at some particle, and found that the character of the solutions was significantly altered by this change of boundary condition.

Now to the other point, how does the velocity curve with a shock affect the light curve? Since the light curves resemble closely the velocity curves, the feeling has been that the motions in the atmosphere produce the light variations.

However, if there is only a weak shock or a progressive wave, the rate of production of thermal energy by compression is a very small fraction of the stellar flux. Therefore the atmosphere acts only as a filter to the radiation. passing through it.

On the other hand, the rate of production of thermal energy can be significant in the presence of strong shocks, and could produce a modification of the light curve over a small phase interval.

— A. UNDERHILL:

I would draw your attention to the *β Cephei* stars, which have pulsation periods near 6 hours, and atmospheric temperature $((20 \div 25)10^3)^\circ$. The light curves have small amplitude ($\lesssim 0^m.05$), while the range in velocity exceeds 100 km/s. One observes that the velocity increases rapidly, then there occurs a discontinuity as the velocity decreases. On spectra of the highest dispersion, you may see double, and occasionally triple, sets of lines during the very short period when the velocity decreases. The period of one star, *BW Vulpeculae*, has been increasing over the $(25 \div 30)$ years it has been observed. ODGERS and KUSHWAHA have discussed the observations of this star in terms of an isothermal shock.

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— A. J. DEUTSCH:

The discontinuity in the velocity curve is really an astonishing thing. In a 5 minute period, one sees the velocity indicated by the hydrogen Balmer lines change, apparently discontinuously, by nearly 150 km/s. It is the most striking phenomenon encountered in any of the pulsating variables.

— A. UNDERHILL:

Depending upon your temporal and spectral resolution you also get two and sometimes three sets of Balmer lines. You get the feeling that you see different volumes of gas at the same time. The line that is moving at +100 km/s gets weaker, and you do not see much of it—but while you can still see it, you find another one quite strong, but moving outward, — 50 km/s. Also note that H_α goes into emission for a very short time, during the cycle.

— L. DAVIS:

Can one assume that the two or three velocities seen simultaneously refer to patches at different places on the star, or must he assume that they represent spherically-symmetric velocities?

— A. UNDERHILL:

I don't know whether one sees patches or several spherically-symmetric shocks at once, but the spectral variations one sees for shell-stars and or supergiants make one ask this question. It is a question of the relative life-times of shocks in these atmospheres, and the path-lengths through which one can observe at any moment. I suspect that one observes several shocks at once; otherwise, he would not observe some of the sudden doublings and widenings.

— P. LEDOUX:

I would like to make a general comment on these *β Cephei* stars. In many of these stars, the line profiles change in the course of the cycle. HUANG has noted that this effect does not affect the equivalent width but consists simply in a widening of the line. This favors the idea that we are looking at different parts of the star surface and not at superposed layers.

On the other hand, many of these stars have two extremely close periods. Despite the fact that these periods may vary in the long run, they are very stable in the sense that the difference between them remains the same for many cycles, giving rise to a very regular beat phenomenon so that the amplitude is modulated with a period which may be 30 to 200 times the short period. This again makes it very difficult to interpret these stars in terms of purely radial oscillations. As far as I am concerned, I fail to see how you can get two very close periodicities on this basis without very artificial assumptions

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leading to near-commensurability of the periods of two radial modes and invoking coupling.

The easiest way out of this is to apply non-radial oscillations in the presence of rotation or a magnetic field. For instance, let us consider the mode represented by a spherical harmonic of order two. In absence of rotation (or magnetic field), we get only one period. But if rotation is present the degeneracy of the frequencies disappears and we can get two periods that are very close to each other. On that picture, part of the star would be moving out while the rest is moving in and this could at least qualitatively account for the changes in the line-profiles, which was the only effect known when I first discussed this problem.

I don't know how the strong discontinuity discussed above in the case of *BW Vul* could be explained on this basis. However, I would like to note that the velocity curve which has been shown suggests a very strong non-linearity, although, if interpreted in terms of radial pulsations, the relative amplitude in these stars, $\delta R/R$, is very small, of the order of 0.01; *i.e.* appreciably smaller than in the classical cepheids, even those that exhibit the smoothest behavior.

— H. Petschek:

It was mentioned that the period had changes in the last 25 years. On the basis of a simple radial mode of an acoustic oscillation how do you explain a change in the period?

— E. Schatzman:

The theories which have been developed on the pulsation show that the period is an extremum property of the whole star, and if the period changes it means that something in the whole star is changing. In the case of the cepheids, for example, some of them are known for (150 ÷ 180) years—with not quite one second change in period in that length of time, which shows that during the last 180 years the star has not changed its structure by any appreciable amount.

— P. Ledoux:

The main difference between the ordinary cepheids and the *β Cephei* stars is that the latter are B stars, which we believe evolve very rapidly to the right of the main sequence, with increasing radii and decreasing densities. Struve has shown that the decrease in density necessary to account for the increase in period (cf. formula (19) in the text) is compatible with the normal theory of stellar evolution.

— E. M. Burbidge:

What would you say the period should be for stars of this type?

— P. LEDOUX:

The observed Q-value is about 0.025 and it is difficult to account for it on the basis of purely radial oscillations unless we adopt a model with an extremely high mean value of the effective polytropic index through the star. This does not seem likely according to the usual views on stellar structure in the relevant part of the Hertzsprung-Russell diagram.

The first p-mode of a non-radial oscillation corresponding to a spherical harmonic of degree two would certainly be more favorable in that respect.

However, there may still be doubts on the correct models and masses for these stars so that it is difficult at this stage to reach definite conclusions.

— S. S. HUANG:

The most striking observational result is that the periods of the *Canis Majoris* stars either remain constant or increase with time—no single example of a decrease has been found. Therefore, we attribute the increase to stellar evolution. As LEDOUX pointed out, a massive star evolves rapidly. When it departs from the main sequence after exhausting the hydrogen in its core, its radius increases, hence its density decreases. Therefore, its period will increase according to the $P\sqrt{\varrho} =$ constant relation.

— E. SPIEGEL:

The so-called van Hoof effect consists of the existence of phase differences in plots of velocity *vs.* time, for lines from different elements. Are there any differential velocity effects observed between elements for these β *Cephei* stars?

— A. J. DEUTSCH:

Such velocity differences do exist in this star, measured by these phase differences.

— P. LEDOUX:

There is some confusion here. The van Hoof effect consists in the fact that the doubling of the lines does not occur at the same time for different lines. The doubling occurs progressively in those lines whose origin is higher and higher in the atmosphere, exactly as if a discontinuity were indeed moving outward.

— *Ed. Note*:

At this point in the discussion, there arose questions on the source of the pulsational instability. The discussion for the balance of the morning session was long and confused. For this reason, LEDOUX has revised and very con-

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siderably amplified the treatment of pulsational instability in the text of the introductory summary, bearing in mind the source of difficulties encountered during the discussion.

During the afternoon recess, a smaller group held informal discussion, and an unsuccessful attempt was made by CLAUSER and others to present some consensus of opinion on an « aerodynamic » look at the problem of the instability mechanism maintaining the oscillations. Various simplified thermodynamic systems were outlined in an attempt to construct a system schematizing a star. The aim was to clarify the origin of cepheid instability in the simplest physical terms. Again, the result of the discussion was essentially confusion. Consequently, WHITNEY has prepared the following outline of a model, essentially due to EDDINGTON, as a preferable substitute for any attempt to provide an edited coherent account of the actual discussions of this topic.

— *Model presented by Whitney*:

Consider a plane-parallel homogeneous slab of gas whose lower boundary is stationary. Let the slab be confined above by a transparent piston whose height varies sinusoidally with an amplitude small relative to the slab thickness. If the frequency of the piston is kept very low relative to the resonant frequencies of the slab, hydrostatic equilibrium will be maintained. Let $P(t) = \bar{P} + \partial P(t)$ be the pressure within the slab and let $V(t) = \bar{V} + \partial V(t)$ be the volume of a unit column within the slab; bars denote mean values. (Note: Put $V(t)$ as just the piston height.) Write

$$\partial V(t) = A \sin \omega t \,. \tag{1}$$

Let there be an energy flux $F_i(t)$ upward through the lower surface of the slab with

$$F_i(t) = F_i[1 + \varepsilon_i \sin (\omega t + \varphi)] \,. \tag{2}$$

By analogy with the nature of the radiative transfer process at great depths within a star, we should consider this flux to be carried through the slab by thermal conduction, with a temperature-dependent coefficient of conduction. The variations of $F_i(t)$ produce a thermal wave which propagates up through the slab. The amplitude and phase of the flux at the top of the slab are determined essentially by the heat capacity of the slab and the law of temperature-dependence of the coefficient of conduction.

However, at this stage we allow our analogy with the stellar case to be weakened in order to simplify the algebra. We neglect conduction and assume energy transfer to be purely radiative. Further, we require that the gas within the slab be optically thin so that the absorption takes place uniformly throughout the slab. The net rate of absorption per gram will then be determined by F_i and the opacity of the gas. We express this absorbed energy as $F(t) = F \sin(\omega t + \varphi)$.

If $F(t) = 0$, the gas pressure $P(t)$ will be related to the volume of the box, $V(t)$, through the adiabatic law and we may write for the small variations,

$$\delta P(t) = -\gamma \delta V(t) \frac{\bar{P}}{\bar{V}},$$

where $\delta V(t)$ is given by equation (1).

The total work done on the piston during one cycle, W, is then

$$W = \oint \delta P \frac{\mathrm{d}}{\mathrm{d}t} \delta V \,\mathrm{d}t = -\gamma \frac{\bar{P}}{\bar{V}} \omega A^2 \int_0^{2\pi} \sin \omega t \cos \omega t \,\mathrm{d}t = 0,$$

and vanishes.

When $F(t)$ is non-zero, but its cyclic integral vanishes, *i.e.*

$$\int_t^{t+(2\pi/\omega)} F(t)\,\mathrm{d}t = 0,$$

the energy equation in linearized form is

$$\frac{\gamma}{\gamma-1} \bar{P} \frac{\mathrm{d}}{\mathrm{d}t} \delta V + \frac{\bar{V}}{\gamma-1} \frac{\mathrm{d}}{\mathrm{d}t} \delta P = F(t).$$

This equation has the following integral

$$\delta P(t) = \frac{\gamma-1}{\bar{V}} Q(t) - \gamma \delta V(t) \frac{\bar{P}}{\bar{V}},$$

where

$$Q(t) \equiv \int_0^t F(t)\,\mathrm{d}t = \int_{2\pi n/\omega}^t F(t)\,\mathrm{d}t.$$

The pressure variation now contains a term proportional to $Q(t)$, the heat absorbed since the commencement of the currect cycle, *i.e.* since $t = 2\pi n/\omega$.

Inserting $F(t) = F \sin(\omega t + \varphi)$ leads to

$$\delta P(t) = -\frac{\gamma-1}{\bar{V}} \frac{F}{\omega} \cos(\omega t + \varphi) - \gamma \frac{\bar{P}}{\bar{V}} A \sin \omega t.$$

Inserting this into the work integral gives

$$W = -\frac{\gamma - 1}{V} FA \int_0^{2\pi} \cos(\omega t + \varphi) \cos \omega t \, dt = -\pi FA \frac{\gamma - 1}{\omega V} \cos \varphi .$$

Noting that φ is the angle by which the absorption rate leads the volume change, we see that the implications of this relation are the following: If W, the net work done on the piston is positive, the system is unstable in the sense that there is a net transfer of energy from the radiation field into the mechanical system driving the piston.

For various values of φ we have

$$W = 0 \quad \text{when} \quad \varphi = \pi/2,\ 3\pi/2 ,$$

$$W > 0 \quad \text{when} \quad \pi/2 < \varphi < 3\pi/2 ,$$

$$W < 0 \quad \text{when} \quad 3\pi/2 < \varphi < \pi/2 .$$

The major weakness of the analogy between the system discussed above and the cepheid envelope is in the assumption that the slab is optically thin. A further weakness lies in the assumed boundary conditions of a stationary lower boundary and a driven piston above.

In reviewing the discussion of this subject during the Symposium, it is clear that the major source of confusion was the weakness of the analogy between this simple system and the cepheid envelope.

(*Ed. Note*: Several questions were raised which are appropriate to the model presented; we reproduce these, and their answers; then Pecker's question on asymmetry of line profiles marks the turn of the discussion from this topic of instability source.)

— H. PETSCHEK:

In the model described, the flux is absorbed instantaneously. But in the stellar case, the radiation takes a time of the order of a million years to get from the center to the region where you want to absorb it.

— P. LEDOUX:

Energy generated at the center will take a long time to reach the surface. But we consider a star that has reached a steady state in which the flux at any point is determined by the local temperature gradient. In the same way, the disturbance of flux at a given point at a given instant is determined entirely by the local perturbation of the opacity, of the radiating power per unit surface, and by the local change of the temperature gradient. Effectively, the

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problem can be described in terms of a heat conduction coefficient. We may leave out of consideration the excess heat generated close to the center of the star. This simply provides another local contribution to the instability, which is automatically taken into account in the integral expressing the coefficient of vibrational instability (cf. the text). This excess energy generation does not determine the excess flux. The point is that in a star, the energy generation per second—or the total rate of energy radiation—is only a very small fraction of the internal energy (here 10^{-12} to 10^{-14}). So we have an enormous energy reserve, and the flux will adapt itself at each instant to the local conditions.

— H. PETSCHEK:

Yes, but the effect you are describing is an effect of varying heat conduction coefficient. Can you show why more energy is stopped where the temperature is high due to the wave. I am willing to let you vary the opacity any way you like—I still do not understand the heat flux mechanism.

— P. LEDOUX:

What you need in the piston analogy is that, while you compress the gas, its opacity should increase, subtracting from the flux some energy which is transformed into thermal motion and an excess pressure.

— A. J. DEUTSCH:

Am I to infer now that the question of the phase lag is now understood, and that it no longer constitutes the problem it once did?

— P. LEDOUX:

I would like to emphasize that what I have done this morning is to try to summarize the present state of two of the fundamental problems associated with the interpretation of the cepheids: 1) what is the origin of the instability? 2) if this instability is due to the ionization of an abundant element in the external layers, does it, at the same time, produce a phase lag of the order of that observed?

The work of ZHAVAKIN and COX confirms that the second ionization of He has a large destabilizing influence; but whether it can make the whole star vibrationally unstable has to be checked by detailed computations. A phase-shift also arises, but its value is certainly not as critical for instability as was once suggested by EDDINGTON. Again, in the case of the cepheids, only detailed computations on realistic models could show whether this phase-shift is similar to the one observed. My own feeling is that the problem is still far from being definitely settled but the present line of approach is promising.

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Are there other lines of approach? As far as the instability is concerned, we cannot quite be sure until we possess a reasonable model for the internal structure of these stars; and the possibility of hard self excited oscillations should not be discarded completely. As far as the phase-shift is concerned, the anharmonicity may contribute to it; and furthermore, the exact interaction between the oscillations of the interior and those of the atmosphere—which may be considered, in part, as driven by the variable flux issuing from the interior—has not received a lot of attention.

— A. UNDERHILL:

I just want to remark that the move toward He II as the driving force of cepheid variation makes me very happy; because in the early type stars, hot O's, and B's, there is ample evidence that small fluctuations of light and radial velocity take place, and in some ways can be qualitatively compared to cepheid variations. But you know perfectly well that in these atmospheres there is no hope of hydrogen convection arising—but there is of helium. You would like to have the same thing work for both types of stars—so qualitatively I am very pleased to hear this result on He II.

— R. LÜST:

I would like just to add to the remarks of LEDOUX that also BAKER and KIPPENHAHN are making similar calculations in Munich. But it is too early to say something definite about the results. They try to fit an adiabatic interior to a non-adiabatic shell.

— E. BÖHM-VITENSE:

I should like to make a remark which may confuse the matter again. But I would like to point out that cepheids in the H-R diagram appear just at the line where the stars change from having a hydrogen convection zone to where they do not. Since this transition is rather abrupt, it seems possible that during the course of pulsation the star may change from a state with active convection to a state where there is no convective energy transport. And so perhaps the driving mechanism may also be correlated to switching on the convection and turning it off again.

— J.-C. PECKER:

I would just come back on the empirical determinations of velocity field. There are essentially three ways of getting information on differential velocities within the atmosphere. Let us consider (Fig. 1)—the different layers of the atmosphere. They could pulsate or as in *a* (standing wave) or as in *b* (progressive wave)—the outer layers being above.

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1) Ledoux has described how we can obtain radii—first from consideration of the radial velocity curve—and then from the consideration of the photometric curves—coming from the combination of data concerning luminosity and temperature. Now if Oke's results (which seem very conclusive) are taken into consideration, it means essentially that the radii determined from luminosity curve and from velocity are in agreement—in other words the radial velocity curve is the same from different ways to get to it. This conclusion is not necessarily in favor of the type of description of Fig. 1-*a*, because it can be very well the case that during all the processes of the pulsation we see the same material layer of Fig. 1-*b*—the interpretation of the difference between the two radii could have been that we see at different moments different layers, as schematically indicated by the crosses and dotted lines on Fig. 1-*b*. And thus, the new agreement obtained between the two radii does not particularly favor the standing wave more than the progressive wave.

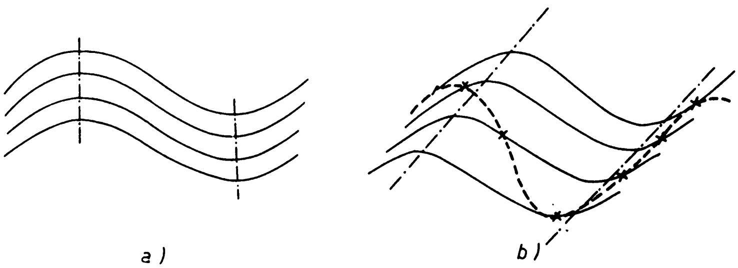

Fig. 1.

2) The second evidence in favor of rate of variation of velocity with depth has been given by Whitney when he presented the time variation of the different radial velocities from line to line curves around which the points were quite scattered. I do not want to comment on this: it is only obvious that experimentalists should look at the question with a greater accuracy than previously done.

3) There is a possible third way of inference of the variation of radial velocity with depth—a way which is a very difficult one indeed and which requires a detailed theory of the atmosphere, but which could anyway be used:—In a cepheid—in a variable star—the lines are asymmetrical and the asymmetry can arise in two ways. The main one is that we integrate over the disk of the star. In addition to this, a gradient of velocity in the atmosphere can influence the asymmetry. The study of the asymmetry of the lines, I think, should be taken, as a very difficult way but as a possible one, to get to those differential velocity effects. I do not know of any recent work on interpretations of asymmetries in any completely satisfactory way.

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— C. A. Whitney:

The observational data that you suggested are very difficult to use because the purity of the spectrum is not what we would like. Teske, from Harvard, has been looking in a theoretical sense at constructing profiles in an atmosphere with a velocity gradient. The work is not completed yet, but it does give some hope if we can just get better spectra. The work so far has been done with the LTE approximation, since the mathematics even in this case is fairly involved.

— W. H. McCrea:

I wish to put forward the suggestion that cepheid pulsation may be essentially a *resonance phenomenon*. This would mean that every star in some general category would have two characteristic times associated with it, but that a star in this category would be a pulsating star only if these two times are equal or commensurate. If this suggestion is valid, it is natural to expect that one of the characteristic times would be associated with the main part of the interior; while the other time would be associated with its outer part, or envelope. In this context, it should be remembered that pulsating stars do apparently posses extended atmospheres that might have larger characteristic times for associated phenomena than the atmospheres of, say, main-sequence stars.

The following properties may support the suggestion:

a) We have the fact that the pulsation phenomenon is restricted to very narrow regions in the H-R diagram. Schatzman has reminded us that no known peculiarity in nuclear processes of energy generation accounts for instability in these regions. In that case, the occurence of instabilities is indeed characteristic of a resonance phenomenon.

b) We have also been reminded by Ledoux that there is in fact in the observations some evidence for the presence of two periods with the occurence of beats, and not just one simple period, in some cepheid phenomena. This seems to be rather direct support of the suggestion.

c) Again, it has been pointed out that there are at least three chief types of pulsating star. A possibility on the basis of the present suggestion might be that there are stars in which the two characteristic times are equal, stars in which one is twice the other, and stars in which some other simple commensurability occurs.

d) If cepheid pulsation is a simple periodic phenomenon then, as has often been said, if is very hard to see why the oscillations are excited in some stars and not in others. According to the present suggestion, simple periodic oscillations may occur for any star. But normally they would be an exceed-

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ingly feeble phenomenon. I believe, however, that there is some observational evidence for such weak periodic phenomena in some stars other then cepheids. While any star may show these weak periodic effects, my suggestion is that it will show strong periodic effects only when a resonance occurs.

e) I think the suggestion is not out of accord with much of today's discussion. From various points of view, we have had the concept of an oscillation of the interior of a star being linked with phenomena in the other regions. My suggestion would change nothing in all this except to require that these latter phenomena should have a characteristic time, and that the oscillation would lead to a « pulsation » only if this time is commensurate with this oscillation period. Further, my suggestion does not alter the need for a way of « driving » the pulsation as has been discussed.

f) A very tentative quantitative test may be noted. The characteristic time t_1 for an oscillation of the main part of a star is of the order R/a_1, where R is the radius and a_1 is sound-speed in the interior. A characteristic time t_0 associated with an envelope could be H/a_0, when H is the depth of the envelope and a_0 is sound-speed in this region. Thus, roughly,

$$t_1/t_0 = (R/H)(a_0/a_1) = (R/H)(T_0/T_1)^{\frac{1}{2}} ,$$

where T_0, T_1 are typical temperatures for the envelope and for the interior. Now suppose very provisionally that the depth of the envelope is fixed by the level at which helium becomes ionized, in conformity with some of the indications of the discussion. This would require T_0 to be of the order of 10^5 degrees, and we know that T_1 is of the order of 10^7 degrees. Thus $(T_0/T_1)^{\frac{1}{2}}$ would be of the order of 0.1. For resonance to be possible, t_1/t_0 would have then a value about unity. Then we should have $H/R \sim 0.1$. This would require the critical level for the ionization of helium to be at about 0.1 R, which is not unreasonable. It seems, therefore, that the suggestion is worth pursuing.

— E. Spiegel:

I would like to suggest a physical reason for the possibility of a two-period situation as McCrea has suggested. Suppose that this outer zone that he mentioned—it need not be the entire outer zone—were convectively unstable and there were some rotation. We expect that at the low Prandtl numbers characteristic of the stellar atmospheres (the Prandtl number is the kinematic viscosity divided by thermal conductivity), the instability would arise as over-stability; *i.e.* a periodic oscillation. This has been studied in the incompressible situation by Chandrasekhar. This over-stable layer has a natural frequency, and this provides a possibility for mechanical driving of pulsations

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of the star. This would be, of course, a weak input, but if there were a resonance of the sort mentioned by McCrea, you might expect it to be potentially a mechanism for driving pulsations. I really think of this in connection with the *β Cepheid* stars—for various reasons I will not go into. The difficulty is that it is hard to evaluate what the frequency of the over-stability is because the calculation has been done only for a plane-parallel case in an incompressible medium, and moreover, it has only been done in the stability situation. In the stars, we have clearly a highly unstable situation in which the stability period may not be relevant, but the qualitative calculations have shown that it is not really impossible to expect this kind of thing.

— P. Ledoux:

Although very interesting in itself, the idea of a resonance in a continuous hydrodynamical system between two parts of the system seems difficult to apply unless one has good reasons to treat these two parts as practically independent. Eigenperiods are only defined by boundary conditions and, if the boundary condition at the common boundary between the two regions contemplated is the continuity of the displacement and the pressure, the two regions cease to be independent units. Furthermore, resonance does not free us from finding a source of mechanical work capable of amplifying the small motion with which we start.

— W. B. Thompson:

The question has been raised as to the influence of radiation when it is included in the discussion of the shock. There are laboratory situations where you try to produce a hot shock; and when you do, you find a precursor, which has been associated with radiation running ahead of the shock. The precursor seems to play a vital role in the whole role in the laboratory. Whether there is an analogy in the stellar atmosphere I do not know.

— R. N. Thomas:

This question, indeed the whole question of transient affects, is extremely interesting, if one starts talking about hydrogen and helium lines going into emission. Sone of us are reasonably convinced that we can now do a good job on the non-LTE calculation of calcium and hydrogen lines in the time-steady-state situation. As far I know, this approach has not been applied to discuss the problem of emission lines in the cepheid atmospheres, but I am confident that it will have to be, in time. However, if it should turn out that transient effects occur so rapidly that the steady-state computations become invalid, we have a more nasty species of non-LTE calculations to make. I am surprised that this question of relaxation times has not been of wider concern here.

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— P. LEDOUX:

There is a question on which the comments of the aerodynamicists would be very welcome, namely: how do we pass exactly from the simple picture of a linear standing oscillation of the whole star to that of a finite oscillation with running waves or shock-waves at least in the external layers?

Let us suppose first that we have a perfectly reflecting boundary and that the oscillation remains adiabatic right through to the surface but that, by some means, we increase progressively the amplitude. Will the oscillation remain a purely standing wave even when the velocity associated with the periodic displacement becomes larger than the local velocity of sound of some region of the star (usually this happens first close to the surface)? For instance, we can find such solutions for the adiabatic oscillations of the homogeneous compressible model. Are they meaningful?

On the other hand, if we have no sharp boundary (surrounding medium) or if we have some dissipation in the external layers, the oscillation in the external part of the star, even in the linear approximation, will acquire a more or less important progressive part. It is this part which gives rise to shock conditions as the amplitude increases? Where and when will this happen?

— H. PETSCHEK:

I would think that one could answer this in terms of the time it takes a pressure pulse to steepen to form a shock-wave. If one makes the piston assumption WHITNEY has used, one can compare the distance it takes the pulse to steepen with the scale-height of the atmosphere. If the distance is less, you certainly get shock-waves.

— *Ed. Note*:

From this point on, the discussion turned to what one could say about the effect of an atmospheric density gradient on the steepening process—the steepening in an homogeneous atmosphere following the usual Riemann arguments. MINNAERT emphasized that in the astronomical case, the wave-length was very large compared to atmospheric extent, so that probably the question of the effect of atmospheric density gradient was all-important. Since the problem of this density-gradient formed the subject for a future session, further discussion was deferred. Because this was the turn of the discussion, these records have been altered from their chronological order to make that session the next reported.

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