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CEPHEID MODELING

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The proceedings of a conference and workshop held at Goddard Space Flight Center, Greenbelt, Maryland, July 29, 1974

Edited by
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PREFACE

The primary motivation for organizing this Cepheid Modeling Conference was to attempt to compare what different researchers obtained using similar techniques and to resolve any differences. We felt that such a conference could be extremely fruitful, if we could get most of the active investigators together. Of course, it followed that such a forum would be useful in elucidating the advantages and differences between newer techniques and the older ones. Inevitably, the vehicle for such discussions revolves around current research, and that leads to discussing future research based upon the available numerical tools and past experience. So the form of the meeting evolved from our first idea of an informal discussion into a two-day meeting, with sessions on some new observations, new techniques and current results, and a session devoted to an intercomparison of different computer codes. In order to compare codes, we selected a model known to be well imbedded in the classical Cepheid instability strip, which we called the "Standard Model." The parameters of the model were 4 $M_\odot$ and 1.234×10^{37} erg/s with the composition appropriate to D. King's opacity table 4a. Eight independent codes from 11 participants provided 13 models, 10 of which are reviewed in this volume.

The Conference, held on July 29, 1974, was composed of 10 papers. Each speaker was allowed approximately 30 minutes, which included a discussion period. The atmosphere was very informal, and the speakers did not always adhere closely to their prepared manuscripts, which are published as submitted in the first part of this volume. Some of the discussion which occurred during paper presentations have been moved to the end of the talk and are so noted. This was necessitated by the common departure of the speakers from the formal text.

A transcript of the discussions was prepared from a tape recording. Unfortunately, the tape was inadvertently turned off during the discussion of Davis' paper and ran off the end of the reel once. Consequently, we are missing the end of the discussion of Davis' paper and all of the discussion of Christy's and Karp's papers.

The Workshop, held on July 30, 1974, was composed solely of conversation after short extemporaneous presentations by the model makers. The second part of this volume was prepared totally from the transcripts of the tapes.

We have attempted to preserve the informal conversational flavor of the Workshop, and have done only minor editing. The sheer volume of material and a desire to go to press as soon as possible precluded the dispersal of the edited transcripts to the participants for further editing. Since the bulk of transcription pertained to the second day, we applied the same rules to the first day's discussion out of simplicity.

A number of editorial techniques have been employed. We have occasionally inserted a phrase or word for clarification, since the tone of voice or the facing of the speaker to another participant is unreproducible in text. These editorial insertions are usually enclosed in square brackets ([ ]). We have used an ellipsis (. . .) when a speaker became inaudible, did not finish his sentence, or was interrupted and continued a few lines later in the text. Pauses, parenthetical expressions, and asides are characterized by a dash (–). Purposely deleted discussion is represented by five asterisks (*****). Examples are the discussion of which way to move overlay graphs, what units to use in tables, notation, and so forth. Finally, we have added a few editors' notes, enclosed in square brackets, in order to elucidate the discussion.

Now, in reading the discussions of both days, please remember that you are reading what the participants said in a very informal atmosphere among friends and colleagues. If we have misquoted or misidentified anyone, we offer our sincere apologies. The tapes have been reviewed five times; some passages, dozens of times. Simultaneous discussions, soft voices, or talking away from the microphones frequently made accurate transcription impossible, and there are probably a goodly number of errors. We regret missing those words, but there is little more we could do in the time available. We preferred not to rewrite the occasional German construction used by Dr. von Sengbusch and, possibly, misquote him.

ACKNOWLEDGMENTS

The Goddard Cepheid Conference and Workshop, in our opinion, was a great success. For this, we would like to extend our appreciative thanks to each of the participants, since they made it the success it was. In addition, we would like to especially thank Drs. Robert F. Christy and Arthur N. Cox for chairing the morning and afternoon sessions of the Conference, respectively. Somehow, they encouraged considerable discussion and yet ended on time. The most difficult job of all was handled magnificently by Dr. Norman H. Baker, who moderated the Workshop. We cannot thank him enough.

The logistical problem of getting slides to and from reproduction facilities, keeping notes on who spoke from the floor, and other menial tasks were handled by a levy of students who held Summer Research Assistantships in the Laboratory for Optical Astronomy. They were John J. Cowen, William S. Gilmore, Denise A. Langel-Frey, and Paul A. Marionni, all from the University of Maryland, and Deborah J. Hamill of the American University Summer Program. We wish to thank them for their invaluable aid.

Last, but not least, we would like to thank the Goddard Space Flight Center for sponsoring the Cepheid Modeling Conference and Workshop.

David Fischel
Warren M. Sparks

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CONFERENCE

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WELCOMING REMARKS

Albert Boggess
Goddard Space Flight Center

I am here, in part, as a surrogate for Dr. John Clark, who is Director of Goddard Space Flight Center. He always makes a point of coming and saying a few welcoming words to each of the many scientific conferences that go on at Goddard and whenever his busy schedule permits, he stays and frequently participates in the discussions. I think he does this in part because he gets a vicarious pleasure out of watching and listening to people who are doing active research.

In any case, not only he, but all of us are delighted to have you here. I think a very small, informal group like this is one of the most satisfying forms of scientific exchange. It gives an opportunity for active workers in the field to exchange ideas without the distraction of a larger meeting, where one frequently must explain or defer to those with less extensive knowledge and more casual interest. We hope you find your two days here productive ones, and certainly we will do what we can to make it enjoyable.

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ULTRAVIOLET OBSERVATIONS OF CLASSICAL CEPHEIDS BY OAO-2

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ABSTRACT

OAO-2 observations of eight bright classical Cepheids in the wavelength region 1910 Å to 4250 Å are presented. The data for RT Aurigae, α Ursa Minoris, δ Cephei, and Y Ophiuchi show excellent agreement with ground-based photometry in the wavelength region of overlap and are consistent with a simple extrapolation of light-curve properties from the visible region. However, in the ultraviolet, the light curve of β Doradus shows two small flux bumps, at phases of 0.75 and 0.85, in addition to the well-known bump at phase 0.0. All three bumps should probably be associated with the arrival of shocks at the stellar surface.

INTRODUCTION

With the advent of astronomical observatories in space, new studies at previously inaccessible wavelengths became possible. Today, I will report on ultraviolet photometry of Cepheids obtained by the Wisconsin Experiment Package (WEP) aboard OAO-2 at (equal energy) effective wavelengths of 1910, 2460, 2980, 3320, and 4250 Å. Before the failure of its high-voltage power supplies, the WEP observed eight bright classical Cepheids. Three of these stars, η Aquilae, FF Aquilae, and ζ Geminorum, were observed only a few times each; the remainder, including RT Aurigae, α Ursa Minoris, δ Cephei, β Doradus, and Y Ophiuchi, were observed at intervals of 0.05 phase or better. A space observatory is ideal for observing variable stars since the length of the day is short (~100 minutes) and weather is not a problem. As a result, the Cepheids were generally observed during a single light cycle; RT Aurigae needed to be followed for three cycles because of its short period. The photometric accuracy, estimated from the scatter between observations close in phase but from successive cycles, is better than $0\overset{m}{.}01$ for the long wavelength filters and about $0\overset{m}{.}1$ for the short wavelengths. Some spectrophotometric measurements at ~20-Å resolution were obtained in the 2600- to 3600-Å region. These scans generally agree with the photometry but are of poorer quality because of the shorter integration time used. Only the broad-band filter photometry with $\Delta\lambda/\lambda \cong 0.15$ will be discussed here.

The instrumentation of the WEP has been described by Code et al. (1970) and the data reduction procedure by Doherty (1972). The OAO-2 magnitude system, in which a magnitude of 0.0 corresponds to a flux of 1.065×10^{-11} erg/s^{-1} cm^{-2} Å^{-1}, has been used throughout this paper. The reduction of ground-based observations to the OAO-2 system relies on the absolute calibrations of Code* for the Stebbins and Whitford (1938) six-color system, Johnson (1965) for the UBVRIJKL system, and Gillett, Merrill, and Stein (1971) for infrared filter photometry.

The data have been corrected for the time-dependent dark count and instrumental effects. Observations of an area near the Cepheids and relatively free of bright stars provided a sky background correction. Since the sky behaves like a 10*th* magnitude F-star in the ultraviolet (Doherty, 1972), the size of the sky correction is significant for Cepheids at the shortest wavelengths observed. Generally, five sky measurements, separated in time, were available for each Cepheid, and the average trend was used to represent the sky contribution. A correction for interstellar extinction was applied according to the average extinction curve of Bless and Savage (1972). Color excesses have been drawn from a number of sources (e.g., Fernie, 1967); the adopted values are given in Table 1 along with other properties of the observed stars. The extinction corrections are only approximate because no attempt was made to account for possible spatial variations in the reddening law (although any reasonable variation should not introduce a large error since the total extinction is, in general, small). Furthermore, for β Doradus at least, significant disagreement exists between several determinations of the E_{B-V} color excess. As I will suggest later, this disagreement may be due in part to a component of circumstellar extinction. Corrections for secondary stars in the 10-arc-minute field-of-view were necessary only for δ Cephei and RT Aurigae. Numerous faint A- and F-type stars are visible on the Palomar *Sky Survey* prints surrounding RT Aurigae, and a model successfully removes most of the excess short wavelength flux. For δ Cephei, removal of flux approximately equivalent to a lightly reddened B7 IV companion, δ Cephei C (Fernie, 1966), and a faint A-type star, was necessary. After correction, both δ Cephei and RT Aurigae show an additional UV excess at 1910 Å (with respect to α Ursa Minoris, for example) of about 1 magnitude. Known deviations among B7 stars at 1910 Å could account for the δ Cephei A excess. The excess could also be attributed to $V = 10^m$ and $V = 14^m$ unreddened O-stars, respectively, but the existence of such stars as contaminants to both Cepheids is unlikely. The cause of the excess for RT Aurigae is not known. Finally, α Ursa Minoris is so bright that the 4250-Å photometer saturated; the data for that filter are useless. Conversely, Y Ophiuchi is so faint and so heavily reddened that detection was not possible with the 1910-Å filter.

No Cepheid was successfully detected shortward of 1910 Å despite the existence of a useful filter centered on 1550 Å. An approximate lower limit on the detectable flux at 1550 Å can be set at about 5.5×10^{-12} erg cm^{-2} s^{-1} Å^{-1} or +0.12 OAO-2 magnitudes. This limit is not sufficiently sensitive for Cepheid detection, except perhaps for α Ursa Minoris.

OAO-2 CEPHEID OBSERVATIONS

OAO-2 ultraviolet light curves of the five well-observed Cepheids are plotted in Figures 1a through 1e. The symbol size represents the approximate expected error except whe--

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Table 1
Classical Cepheids Observed by OAO-2

	Period	Observed Maximum (λ 4250 Å)	E_{B-V}	Distance (parsec)	Number of Observations
RT Aur	$3^d.728$	2440866.129	0.12	443	68
α UMi	3. 970	2440702.200	0.05	90	39
FF Aql	4. 471	–	–	–	3
δ Cep	5. 336	2441452.445	0.11	282	57
η Aql	7. 177	–	0.20	–	5
β Dor	9. 842	2440796.886	0.15	311	63
ζ Gem	10. 151	–	–	–	1
Y Oph	17. 124	2441377.381	0.72	660	42

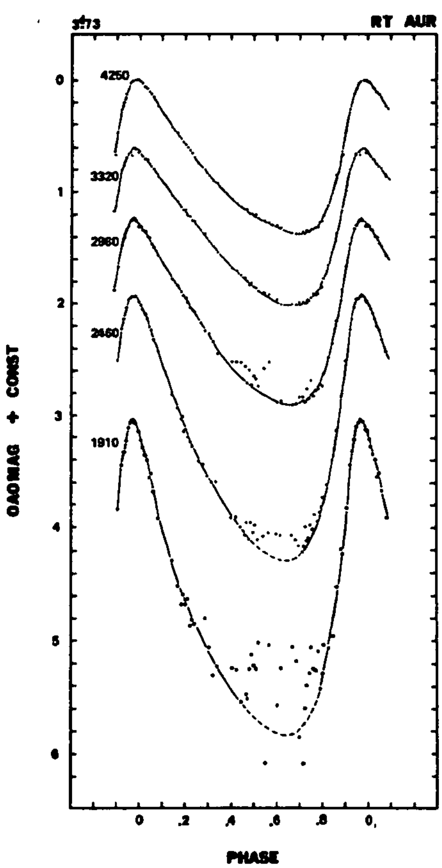

Figure 1a. OAO-2 light curves: RT Aurigae. Symbol sizes give an approximate indication of the measurement errors.

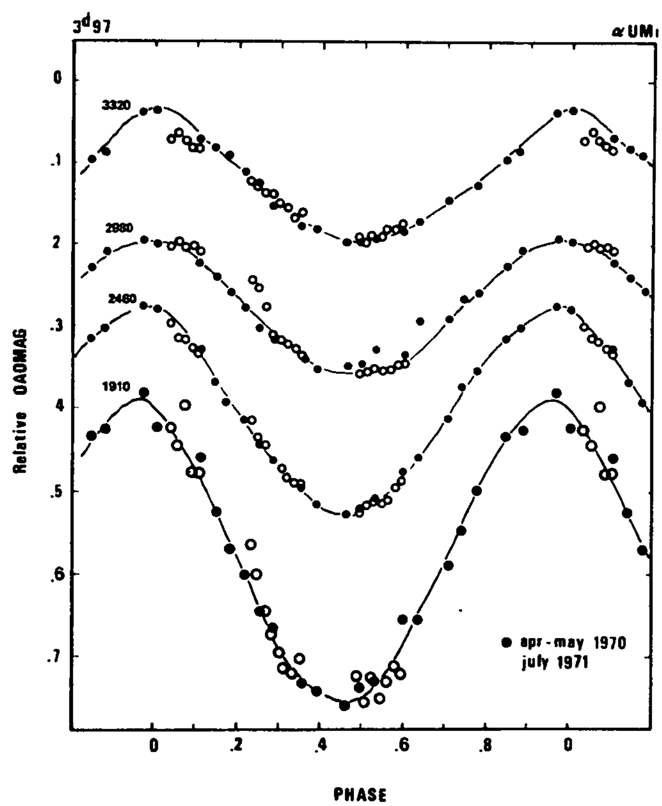

Figure 1b. OAO-2 light curves: α Ursa Minoris. Symbol sizes give an approximate indication of the measurement errors. The open symbols distinguish data obtained during a second observation run.

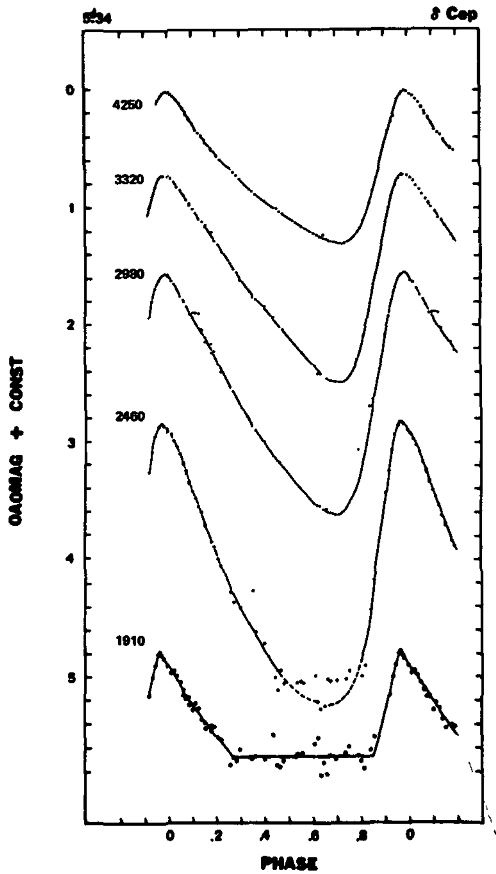

Figure 1c. OAO-2 light curves: δ Cephei. Symbol sizes give an approximate indication of the measurement errors.

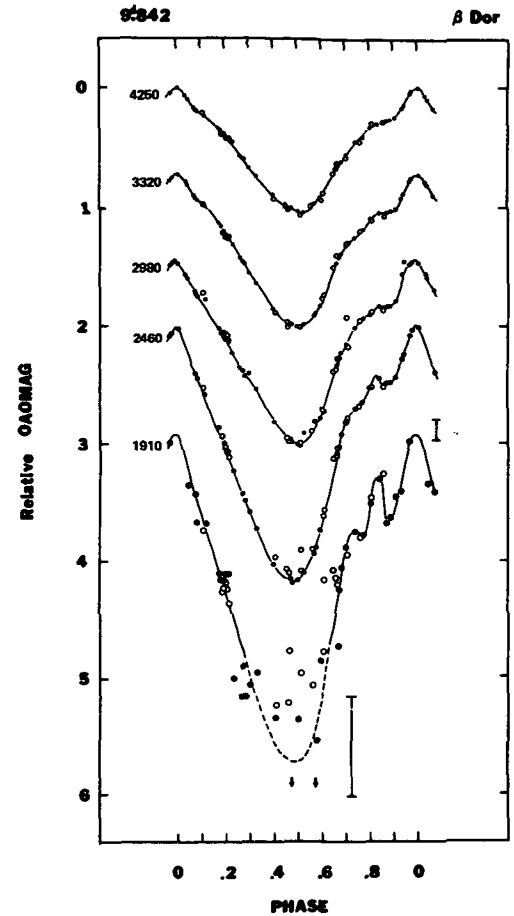

Figure 1d. OAO-2 light curves: β Doradus. Symbol sizes give an approximate indication of the measurement errors. The open symbols distinguish data obtained during a second observation run.

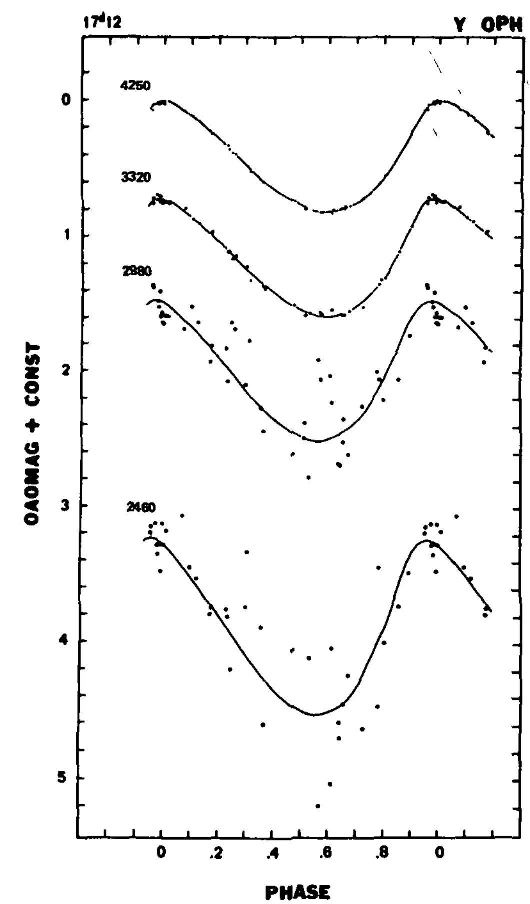

Figure 1e. OAO-2 light curves: Y Ophiuchi. Symbol sizes give an approximate indication of the measurement errors.

error bars are also given. Two observing runs, separated by about a year, were possible for both α Ursa Minoris and β Doradus.

Each run is distinguished by different symbols. The actual data for all eight Cepheids are tabulated in Tables 2a through 2e and Tables 3a through 3c. At the shortest wavelengths, the inherent scatter in the RT Aurigae and δ Cephei data is enhanced by the removal of unwanted contributions by secondary stars. This effect is particularly evident in the δ Cephei 1910-Å light curve. Since Y Ophiuchi was just barely detectable over the sky signal at 2980 Å and 2460 Å, the scatter in those light curves is also large. The smooth curves drawn through the Y Ophiuchi data are based on the 4250-Å light curve shape, scaled according to an extrapolated amplitude-wavelength relation.

Except for β Doradus, which will be discussed separately below, the ultraviolet behavior of this sample of Cepheids is a relatively smooth extrapolation of their visible behavior. Toward shorter wavelengths, amplitudes of light variation increase and maximum light arrives earlier. The relatively small size of these phase shifts precludes any major flux redistribution such as that observed in the Ap and nova variables.

The wavelength-dependent amplitude shows a good linear correlation well into the ultraviolet as shown in Figure 2 for δ Cephei. The filled symbols lacking crossbars are from Mitchell et al. (1964). Deviations from the indicated straight line are perhaps due to variations in the line blanketing with wavelength. Model atmospheres (Parsons, 1969) fitted to the energy distributions at maximum and minimum light show good agreement with observational data in Figure 2 over the region in which the line blanketing is carefully treated. Hack (1956) was the first to explore the linear correlation. Assuming blackbody variation in the Wien approximation, the flux difference in magnitudes for wavelength λ is

$$A = 5 \log \frac{R}{r} - 1.56 \frac{1}{\lambda} \left(\frac{1}{T} - \frac{1}{t} \right)$$

where R is the stellar radius and T the temperature; capital letters represent conditions at maximum light, lower case letters, minimum.

Since $r \cong R$,

$$A = 1.56 \frac{1}{\lambda} \left(\frac{1}{t} - \frac{1}{T} \right)$$

Thus, the amplitude of the light variation is linear in λ^{-1} with the slope dependent upon the stellar temperatures. For typical Cepheid temperatures, this relation is in excellent agreement with observed amplitudes throughout the ultraviolet.

An example of the fit with computed model atmospheres is given in Figure 3 for δ Cephei. The filled symbols indicate long wavelength photometry from Mitchell et al. (1964); the open symbols represent OAO-2 observations. The models have gravities of log g = 1.2 and effective temperatures of 6450 K at maximum and 5400 K at minimum. Both models are

Table 2
OAO-2 Magnitude Data

a. OAO-2 Magnitudes for α UMi

Phase	λ 3320	λ 2980	λ 2460	λ 1910
0.967	−3.343	−2.819	−1.381	−0.250
0.002	−3.348	−2.814	−1.377	−0.207
0.107	−3.311	−2.790	−1.328	−0.170
0.142	−3.301	−2.772	−1.289	−0.104
0.178	−3.292	−2.756	−1.265	−0.058
0.212	−3.271	−2.737	−1.243	−0.026
0.248	−3.259	−2.711	−1.212	+0.017
0.283	−3.229	−2.696	−1.194	+0.036
0.353	−3.207	−2.674	−1.157	+0.103
0.388	−3.202	−2.662	−1.140	+0.113
0.458	−3.185	−2.665	−1.126	+0.132
0.493	−3.187	−2.666	−1.134	+0.109
0.528	−3.190	−2.685	−1.146	+0.101
0.599	−3.198	−2.678	−1.180	+0.028
0.633	−3.211	−2.720	−1.197	+0.027
0.704	−3.236	−2.722	−1.245	−0.039
0.739	–	−2.746	−1.282	−0.083
0.774	−3.256	−2.755	−1.303	−0.130
0.844	−3.286	−2.787	−1.342	−0.197
0.879	−3.295	−2.807	−1.355	−0.204
Filter Degradation:	$-0\overset{m}{.}091$	$-0\overset{m}{.}099$	$-0\overset{m}{.}092$	$-0\overset{m}{.}042$
⟨SKY⟩	+3.85	+4.32	+5.68	+5.35
Extinction Correction:	−0.264	+0.281	−0.345	−0.487
α UMi (b)				
0.036	−3.311	−2.798	−1.359	−0.294
0.054	−3.321	−2.796	−1.352	−0.275
0.071	−3.309	−2.789	−1.340	−0.317
0.088	−3.301	−2.791	−1.330	−0.245
0.106	−3.298	−2.785	−1.324	−0.245
0.229	−3.259	−2.749	−1.241	−0.168
0.246	−3.253	−2.739	−1.222	−0.134
0.264	−3.246	−2.741	−1.211	−0.097

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 3320	λ 2980	λ 2460	λ 1910
0.281	−3.242	−2.707	−1.194	−0.071
0.298	−3.232	−2.703	−1.183	−0.053
0.316	−3.225	−2.695	−1.172	−0.038
0.334	−3.214	−2.689	−1.166	−0.030
0.351	−3.219	−2.681	−1.164	−0.045
0.491	−3.191	−2.660	−1.131	−0.027
0.509	−3.186	−2.661	−1.138	−0.001
0.526	−3.189	−2.666	−1.146	−0.026
0.544	−3.192	−2.664	−1.140	−0.004
0.561	−3.199	−2.665	−1.144	−0.024
0.579	−3.199	−2.669	−1.160	−0.040
0.596	−3.207	−2.672	−1.167	−0.034
Filter Degradation:	$-0^{m}_{.}111$	$-0^{m}_{.}109$	$-0^{m}_{.}115$	$-0^{m}_{.}070$
⟨SKY⟩	+3.85	+4.33	+5.5:	+5.35
Extinction Correction:	−0.264	−0.281	−0.345	−0.487

b. OAO-2 Magnitudes for β Doradus Corrected for
Sky, Extinction, and Filter Degradation

β Dor(a)					
Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910
0.491	−2.32	−0.94	−0.24	+1.69	+2.69
0.542	−2.29	−0.95	−0.22	+1.50	+2.91
0.634	−2.40	−1.18	−0.50	+1.22	+2.70
0.677	−2.63	−1.45	−0.84	+0.72	+2.01
0.733	−2.76	−1.63	−1.29	+0.41	+1.82
0.789	−2.88	−1.74	−1.27	+0.29	+1.74
0.839	−3.04	−1.83	−1.33	+0.12	+1.40
0.888	−3.06	−1.86	−1.35	+0.12	+1.19
0.137	−3.13	−1.97	−1.51	+0.12	+1.66
0.215	−2.97	−1.73	−1.17	+0.54	+2.22
0.222	−2.96	−1.73	−1.17	+0.58	+2.16

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910
0.229	−2.92	−1.69	−1.16	+0.62	+2.10
0.236	−2.93	−1.70	−1.15	+0.66	+2.16
0.243	−2.92	−1.68	−1.10	+0.71	+2.29
0.434	−2.41	−1.06	−0.36	+1.55	+3.15
0.484	−2.35	−0.98	−0.27	+1.66	+3.13
0.540	−2.28	−0.93	−0.21	+1.68	+2.89
0.590	−2.35	−1.02	−0.32	+1.50	+2.99
0.639	−2.47	−1.21	−0.50	+1.15	+2.09
0.689	−2.66	−1.49	−0.85	+0.70	+2.07
0.696	−2.71	−1.54	−0.94	+0.63	+2.13
0.738	−2.80	−1.63	−1.04	+0.39	+1.88
β Dor(b)					
0.000	−3.28	−2.20	−1.74	−0.32	+0.59:
0.007	−3.30	−2.22	−1.76	−0.37	–
0.036	−3.33	−2.25	−1.76	−0.39	–
0.065	−3.26	−2.19	−1.66	–	–
0.072	−3.23	−2.16	−1.63	–	+0.96:
0.099	−3.16	−2.07	−1.53	–	–
0.106	−3.14	−2.05	−1.50	+0.00	+1.03
0.113	−3.13	−2.03	−1.48	+0.04	+1.27
0.145	−3.09	−1.98	−1.46	+0.18	+1.28
0.201	−3.00	−1.85	−1.21	+0.46	+1.71
0.208	−2.98	−1.81	−1.17	–	+1.76
0.222	−2.95	−1.77	−1.14	+0.59	+1.76
0.229	−2.92	−1.75	−1.11	+0.65	+1.71
0.237	−2.91	−1.73	−1.10	+0.66	+1.71
0.259	−2.85	−1.66	−0.99	+0.82	+2.60
0.293	−2.77	−1.55	−0.87	–	+2.76
0.300	−2.75	−1.53	−0.85	+1.03	+2.50
0.308	−2.74	−1.50	−0.80	+1.08	+2.75
0.329	−2.67	−1.43	−0.83	+1.18	+2.66
0.358	−2.60	−1.34	−0.68	+1.33	+2.55
0.427	−2.44	−1.12	−0.41	+1.63	+2.94
0.498	−2.32	−0.95	−0.25	+1.75	+3.99
0.505	−2.33	−0.99	−0.23	+1.78	+4.56

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910
0.527	−2.30	−0.98	−0.21	+1.76	+2.96
0.555	−2.29	−0.99	−0.31	+1.70	–
0.597	−2.35	−1.06	−0.41	+1.54	+4.51
0.604	−2.36	−1.08	−0.40	+1.49	+3.14
0.626	−2.41	−1.15	−0.44	+1.34	+2.46
0.696	−2.67	−1.51	−0.90	+0.69	+2.33
0.703	−2.69	−1.55	−0.95	+0.62	+1.85
0.711	−2.71	−1.58	−1.04	+0.52	+1.66
0.732	−2.77	−1.65	−1.07	+0.41	+1.48
0.768	−2.83	−1.72	−1.21	+0.31	+1.35
0.802	−2.92	−1.80	−1.29	+0.25	+1.38
0.831	−2.98	−1.88	−1.33	+0.13	+1.12
0.867	−3.04	−1.93	−1.40	+0.06	+0.91:
0.901	−3.06	−1.93	−1.39	+0.09	+1.29:
0.916	−3.06	−1.94	−1.39	+0.09	+1.24:
0.937	−3.08	−1.96	−1.44	+0.04	+1.06:
0.966	−3.16	−2.05	−1.66	−0.11	+1.01:
0.973	−3.18	−2.08	−1.77	−0.15	–

c. OAO-2 Magnitudes for RT Aur

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910
0.697	−0.48	+0.66	+1.20	+2.69	+3.40
0.716	−0.49	+0.65	+1.20	+2.75	+3.63
0.734	−0.50	+0.64	+1.21	+2.65	+4.68
0.884	−1.21	−0.17	+0.24	+1.34	+1.78
0.902	−1.40	−0.34	+0.03	+1.04	+1.38
0.921	−1.59	−0.53	−0.17	+0.76	+0.99
0.940	−1.73	−0.64	−0.31	+0.56	+0.76
0.958	−1.83	−0.69	−0.38	+0.48	+0.61
0.977	−1.85	−0.71	−0.39	+0.46	+0.61
0.996	−1.85	−0.69	−0.34	+0.51	+0.69
0.141	−1.43	−0.31	+0.14	+1.34	+1.84
0.164	−1.38	−0.25	+0.19	+1.46	+2.06
0.182	−1.33	−0.20	+0.25	+1.56	+2.23
0.201	−1.28	−0.17	+0.31	+1.67	+2.22

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910
0.219	−1.22	−0.11	+0.39	+1.71	+2.42
0.238	−1.16	−0.07	+0.43	+1.79	+2.40
0.406	−0.79	+0.35	+0.88	+2.43	+2.79
0.425	−0.75	+0.36	+0.88	+2.43	+2.80
0.444	−0.72	+0.42	+0.89	+2.52	+3.09
0.462	−0.70	+0.44	+0.92	+2.60	+3.02
0.481	−0.66	+0.50	+0.95	+2.63	+2.80
0.500	−0.65	+0.52	+1.01	+2.58	+2.76
0.518	−0.61	+0.56	+1.10	+2.64	+2.56
0.668	−0.49	+0.67	+1.27	+2.60	+2.61
0.686	−0.48	+0.68	+1.26	+2.65	+2.73
0.723	−0.48	+0.67	+1.24	+2.70	+3.15
0.742	−0.50	+0.65	+1.21	+2.62	+2.84
0.761	−0.52	+0.58	+1.19	+2.55	+2.80
0.780	−0.54	+0.59	+1.12	+2.48	+2.64
0.798	−0.59	+0.52	+1.10	+2.35	+2.85
0.929	−1.65	−0.57	+0.23	+0.66	+0.87
0.948	−1.77	−0.67	−0.34	+0.49	+0.65
0.966	−1.84	−0.66	−0.39	+0.46	+0.58
0.004	−1.83	−0.66	−0.33	+0.54	+0.76
0.022	−1.79	−0.63	−0.29	+0.62	+0.90
0.041	−1.74	−0.58	−0.23	+0.74	+1.10
0.060	−1.70	−0.54	−0.15	+0.85	+1.22
0.078	−1.62	−0.47	−0.07	+0.95	+1.46
0.190	−1.30	−0.17	+0.29	+1.54	+2.14
0.209	−1.25	−0.12	+0.35	+1.65	+2.17
0.228	−1.20	−0.08	+0.41	+1.76	+2.36
0.265	−1.11	+0.01	+0.53	+1.93	+2.66
0.284	−1.06	+0.04	+0.61	+1.96	+2.35
0.302	−1.00	+0.11	+0.63	+2.07	+2.61
0.321	−0.96	+0.15	+0.70	+2.12	+2.85
0.340	−0.93	+0.21	+0.81	+2.17	+2.78
0.470	−0.70	+0.46	+1.07	+2.48	+3.06
0.489	−0.66	+0.48	+1.09	+2.48	+2.67
0.508	−0.65	+0.53	+1.03	+2.51	+2.80
0.545	−0.60	+0.57	+0.94	+2.61	+3.63
0.564	−0.58	+0.59	+0.88	+2.58	+2.59

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910
0.582	−0.56	–	–	–	–
0.601	−0.56	+0.63	+1.24	+2.59	+3.12
0.620	−0.54	+0.68	+1.23	+2.60	+2.79
0.732	−0.51	+0.66	+1.13	+2.53	+2.95
0.750	−0.51	+0.62	+1.05	+2.51	+2.61
0.769	−0.54	+0.60	+1.14	+2.42	+2.81
0.788	−0.59	+0.53	+1.08	+2.41	+2.98
0.806	−0.64	+0.45	+0.99	+2.27	+2.59
0.825	−0.73	+0.33	+0.84	+2.16	+2.62
0.844	−0.84	+0.21	+0.72	+1.93	+2.52
0.862	−1.00	+0.04	+0.52	+1.66	+2.08
0.881	−1.18	−0.14	+0.34	+1.36	+1.74
0.900	−1.38	–	–	–	–
0.993	−1.85	−0.68	−0.33	+0.51	+0.68
0.012	−1.82	−0.65	−0.30	+0.58	+0.82
0.031	−1.78	−0.62	−0.24	+0.66	+0.94
0.049	−1.73	−0.56	−0.18	+0.76	+1.06
Filter Degradation:	$-0^m.094$	$-0^m.108$	$-0^m.114$	$-0^m.098$	$-0^m.054$
⟨SKY⟩	+2.03	+2.85	+3.24	+4.53	+5.0
⟨Secondary Contaminant⟩	+2.03	+2.89	+3.10	+3.28	+2.75
Extinction Correction:	$-0^m.444$	−0.527	−0.561	−0.691	−0.974

d. OAO-2 Magnitudes for δ Cep A

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910	1550*
0.007	−3.33	−2.01	−1.62	−0.56	−0.90	−0.05
0.020	−3.32	−1.97	−1.60	−0.51	−0.92	+0.44
0.033	−3.28	−1.93	−1.54	−0.46	−0.87	−0.21
0.046	−3.26	−1.91	−1.48	−0.39	−0.81	−0.18
0.059	−3.21	−1.86	−1.45	−0.31	−0.75	−0.13
0.060	−3.21	−1.87	−1.44	−0.30	−0.71	−0.42

*Residual energy after subtraction of secondary model.

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910	1550*
0.073	−3.16	−1.83	−1.39	−0.23	−0.68	−0.42
0.086	−3.12	−1.79	−1.33	−0.14	−0.63	+0.23
0.087	−3.13	−1.78	−1.32	−0.15	–	–
0.100	−3.09	−1.75	−1.34	−0.07	−0.55	−0.06
0.112	−3.03	−1.71	−1.34	−0.01	−0.64	−0.23
0.125	−3.00	−1.66	−1.33	+0.07	−0.59	−0.29
0.138	−2.98	−1.64	−1.20	+0.11	−0.50	−0.18
0.151	−2.95	−1.59	−1.13	+0.22	−0.42	−0.11
0.153	−2.92	−1.61	−1.10	+0.24	−0.47	−0.22
0.177	−2.87	−1.51	−1.08	+0.36	−0.45	−0.06
0.179	−2.86	−1.55	−0.99	+0.36	−0.40	−0.25
0.191	−2.85	−1.48	−1.01	+0.41	−0.44	−0.11
0.204	−2.81	−1.45	−0.90	+0.51	−0.35	−0.13
0.217	−2.78	−1.38	−0.84	+0.57	−0.33	−0.10
0.255	−2.71	−1.28	−0.70	+0.79	−0.12	+0.01
0.268	−2.67	−1.24	−0.67	+0.88	−0.18	−0.14
0.281	−2.64	−1.21	−0.62	–	−0.15	−0.09
0.294	−2.62	−1.14	−0.56	+0.92	−0.25	−0.18
0.347	−2.51	−0.99	−0.37	+0.78	−0.17	+0.01
0.360	−2.48	−0.97	−0.34	+1.14	−0.19	+0.04
0.398	−2.43	−0.91	−0.24	+1.23	−0.16	−0.34
0.436	−2.37	−0.81	−0.11	+1.52	−0.37	−0.08
0.449	−2.36	–	–	+1.63	−0.11	−0.11
0.462	−2.34	−0.73	−0.06	+1.46	−0.10	−0.50
0.475	−2.32	−0.71	−0.19	+1.57	−0.15	+0.41
0.528	−2.24	–	–	+1.57	−0.19	−0.13
0.534	−2.22	–	–	+1.55	−0.21	−0.05
0.554	−2.21	−0.51	+0.11	+1.57	−0.22	+0.12
0.606	−2.15	−0.40	+0.30	+1.51	−0.29	−0.12
0.618	−2.14	−0.34	+0.31	+1.73	−0.34	−0.09
0.631	−2.12	−0.33	+0.32	+1.79	−0.03	−0.11
0.644	−2.14	−0.31	+0.35	+1.77	−0.13	+0.25
0.657	−2.10	−0.29	+0.34	+1.53	−0.04	−0.40
0.670	−2.09	–	–	+1.55	−0.19	−0.24
0.696	−2.07	−0.26	+0.39	+1.54	−0.16	−0.12
0.736	−2.07	–	–	+1.53	−0.21	−0.12

*Residual energy after subtraction of secondary model.

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 4250	λ 3320	λ 2980	λ 2460	λ 1910	1550*
0.748	−2.09	–	–	+1.46	−0.27	−0.03
0.787	−2.16	−0.45	−0.17	+1.43	−0.20	−0.22
0.799	−2.20	−0.55	–	+1.51	−0.15	+0.05
0.812	−2.26	−0.65	–	+1.42	−0.09	+0.11
0.838	−2.41	−0.93	−0.54	+0.94	−0.24	–
0.851	−2.52	−1.10	−0.59	+0.70	−0.35	−0.13
0.904	−2.92	−1.63	−1.15	−0.05	−0.62	−0.19
0.917	−3.02	−1.75	−1.30	−0.24	−0.71	−0.09
0.929	−3.09	−1.85	−1.44	−0.42	−0.86	−0.19
0.943	−3.22	−1.93	−1.56	−0.55	−0.96	−0.15
0.956	−3.29	−2.01	−1.63	−0.63	−1.06	−0.03
0.969	−3.33	−2.03	−1.67	−0.66	−1.05	−0.08
0.976	−3.35	−2.03	–	−0.64	−1.02	+0.24
0.994	−3.35	−2.02	–	−0.59	−0.96	−0.08
Filter Degradation:	$-0^m_.081$	$-0^m_.114$	$-0^m_.073$	$-0^m_.089$	$-0^m_.495$	$-1^m_.975$
Average SKY	+3.02	+2.13	+3.33	+4.11	+4.33	~5.5
δ Cep C Extinction Correction: $E_{B-V} = 0^m_.105$	−0.466	−0.553	−0.589	−0.726	−1.023	Not incl.

*Residual energy after subtraction of secondary model.

e. OAO-2 Magnitudes of Y Oph

Phase	λ 4250	λ 3320	λ 2980	λ 2460
0.569	−1.94	−0.45	−0.37	+2.70
0.614	−1.93	−0.46	−0.25	+2.55
0.618	−1.95	−0.52	−0.05	+1.68
0.646	−1.96	−0.36	+0.40	+2.09
0.650	−1.96	−0.43	+0.42	+2.20
0.659	−1.97	−0.43	+0.24	+1.95
0.679	−2.01	−0.46	+0.33	+1.74
0.728	−2.05	−0.49	−0.03	+2.13

Table 2 (continued)
OAO-2 Magnitude Data

Phase	λ 4250	λ 3320	λ 2980	λ 2460
0.785	−2.14	−0.66	−0.29	+1.97
0.793	−2.16	−0.70	−0.23	+0.95
0.809	−2.21	−0.71	−0.08	+1.51
0.862	−2.37	−0.90	−0.23	+1.23
0.902	−2.53	−1.09	−0.56	+0.98
0.959	−2.69	−1.25	−0.93	+0.69
0.963	−2.73	−1.29	−0.92	+0.64
0.980	−2.74	−1.32	−0.81	+0.62
0.984	−2.73	−1.27	−0.77	+0.78
0.988	−2.75	−1.31	−0.88	+0.85
0.992	−2.75	−1.26	−0.70	+0.77
0.996	−2.76	−1.28	−0.73	+0.78
0.000	−2.76	−1.28	−0.65	+0.98
0.004	−2.73	−1.27	−0.64	+0.62
0.008	−2.75	−1.27	−0.70	+0.78
0.020	−2.75	−1.26	−0.70	+0.68
0.077	−2.69	−1.22	−0.62	+0.57
0.106	−2.64	−1.15	−0.77	+0.94
0.130	−2.61	−1.12	−0.65	+1.03
0.175	−2.52	−1.04	−0.36	+1.29
0.179	−2.51	−1.05	−0.48	+1.24
0.236	−2.42	−0.89	−0.46	+1.26
0.240	−2.40	−0.91	−0.21	+1.31
0.252	−2.38	−0.85	−0.65	+1.70
0.264	−2.37	−0.87	−0.60	+0.98
0.305	−2.32	−0.79	−0.11	+1.24
0.317	−2.26	−0.69	−0.51	+0.84
0.362	−2.17	−0.65	−0.01	+1.39
0.370	−2.16	−0.63	+0.16	+2.11
0.475	−2.02	−0.50	+0.33	+1.55
0.536	−1.97	−0.44	+0.51	+1.61
Filter Degradation:	$-0\overset{m}{.}113$	$-0\overset{m}{.}092$	$-0\overset{m}{.}082$	$-0\overset{m}{.}092$
⟨SKY⟩	+2.15	+3.0	+3.5	+4.7
Extinction Correction:	−3.202	−3.797	−4.041	−4.973

Table 3
Filter Photometry Data

a. Filter Photometry of η Aql

DSD	Date/Time	Phase	Ecl. Long.	S1F3	S1F1	S1F4	S3F2	S3F1
20513R	11/02/72 1710	0.899	-79°.4	-2.008	-0.398	-0.234	1.316	2.613
20515R	11/02/72 1849	0.909	-79°.3	-2.044	-0.476	-0.272	1.259	2.554
20522S	11/03/72 0811	0.987	-78°.8	-2.482	-1.277	-0.766	0.664	1.834
20522S	11/03/72 0951	0.996	-78°.7	-2.490	-1.269	-0.789	0.670	1.849
20524Q	11/03/72 1312	0.016	-78°.6	-2.484	-1.263	-0.798	0.691	1.898
20527R	11/03/72 1448	SKY	-78°.5	2.142	2.871	3.372	4.655	5.115

b. Filter Photometry of FF Aql

14838M	10/05/71 0511	0.433	-96°.3	-0.267	1.166	1.782	3.706	5.72
14846R	10/05/71 1655	0.543	-95°.8	-0.281	1.143	1.963	3.673	6.11
14847O	10/05/71 2018	0.574	-95°.7	-0.269	1.133	1.778	3.611	–
14847O	10/05/71 1839	SKY	-95°.4	2.511	3.428	3.775	4.802	4.74:

c. Filter Photometry of ζ Gem

9593R	10/05/70 1014	0.249	87°.39	-1.876	-0.527	0.216	2.191	4.082
		SKY		2.3:	3.4:	3.5:	4.7:	4.9:

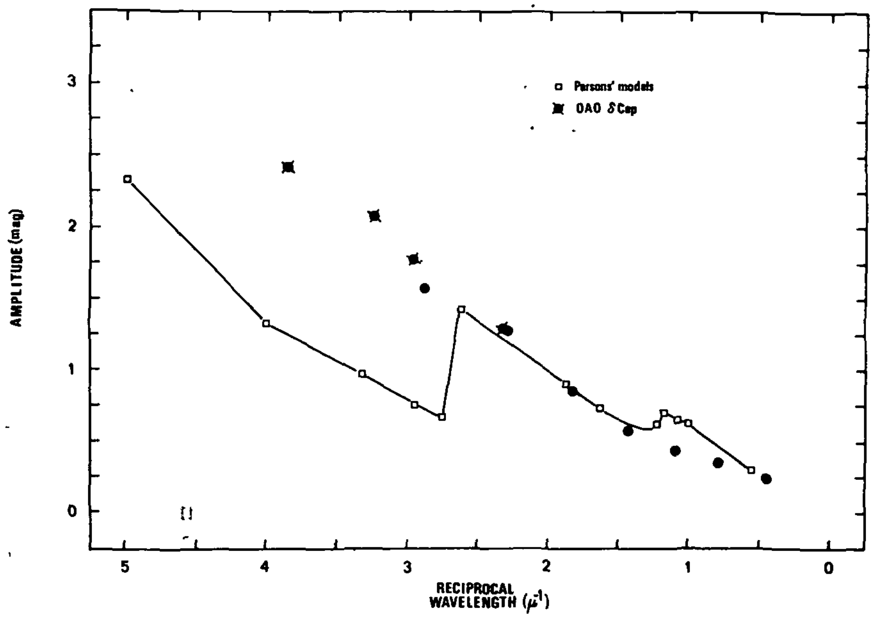

Figure 2. Amplitude versus 1/λ for δ Cephei. The straight line has a slope of $0^m_.6/\mu m^{-1}$. Dashed line is A(λ) relation obtained for model atmospheres of Parsons (1969). T = 6450 K and 5450 K log g = 1.2 with line blanketing very carefully treated between 3400 Å ≤ λ ≤ 8200 Å.

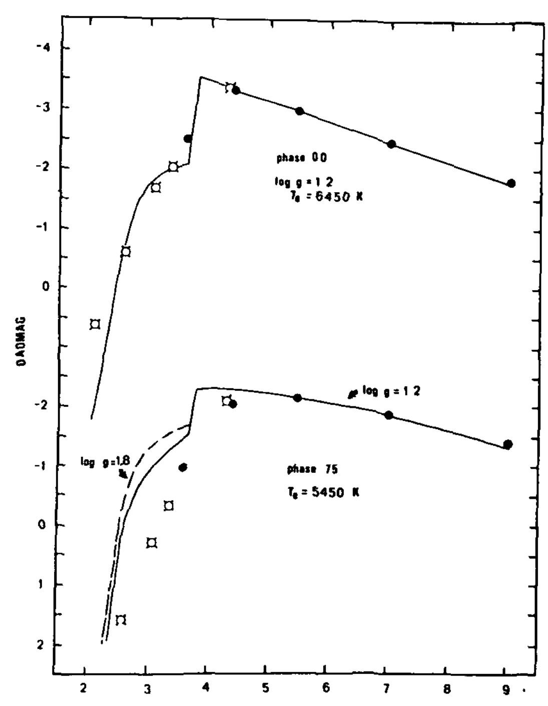

Figure 3. Fit between model atmospheres of Parsons (1969) and photometry in OAO-2 magnitudes.

interpolations from the grid of Parsons (1969). Observations of maximum light appear to be well described by the model down to 2000 Å, although the 1^m excess at 1910 Å pointed out earlier remains. However, minimum light, which employs Parsons' lowest temperature and gravity model, is not well described at all. Both a lower temperature and gravity seem to be in order, but then the temperature variation over the cycle becomes prohibitively large. Again, the discrepancy here may be due to inadequate treatment of the ultraviolet line blanketing.

In the ultraviolet, Cepheid energy distributions have now been observed down to about 1 percent of the peak at ~4500 Å. In addition, Gehrz (1972) observed a seven Cepheid sample out to 11.3 μm. While only δ Cephei is common to both data sets, the infrared region is Planckian, so we can extrapolate redward. By integrating under the photometrically defined energy distributions, we can obtain an estimate of the luminosity of the star with an accuracy of about 20 percent. Columns 2 and 3 of Table 4 give the results of these calculations. The luminosities, L, are based on distances from Sandage and Tammann (1971). These luminosities are in good agreement with values computed from $L = 4\pi R^2 \sigma Te^4$, using Wesselink radius determinations and temperatures on Schmidt's (1972) temperature scale (see Table 4, last column).

BETA DORADUS

One interesting exception to the smooth transition of photometric behavior from the visible to the ultraviolet is the *4th* magnitude Cepheid, β Doradus. It is the only OAO-2 observed Cepheid possessing a secondary flux bump, and it is the bump which shows changes with wavelength. The observations are discussed in detail elsewhere (Hutchinson*). Those results which are of potential value to constructors of pulsational models are summarized here.

The ultraviolet data, especially at 1910 Å, indicate the presence of three flux bumps: the well-known main bump at phase 0.0 (also called maximum light, but referred to here as B3) and two smaller bumps at about phase 0.75 (B1) and 0.85 (B2). B2 rises out of the shoulder previously observed on the rising branch of the light curve. I identified a bump at maximum light because, for a wide wavelength range (0.2 μm to 1 μm), B3 appears to be constant in phase (within 0P005) in contrast to the phase shifts with wavelength noted for maximum light in other Cepheids. If produced in the atmosphere, the disturbance represented by B3 must be traveling faster than ~6 km/s, based on considerations of the wavelength-dependent depth of formation of the continuum, in a region where the sound speed is about 7 km/s. On the other hand, the smaller bumps do show phase shifts, but in an opposite sense to those normally shown by other Cepheids; that is, the bumps arrive latest at the shortest wavelengths. Calculations of the effective velocities of the B1 and B2 disturbances yield 4.5 km/s and 3.0 km/s, respectively, and possibly values even closer to the sound speed. In addition, on a color-color plot, Figure 4, β Doradus shows two very thin, blue arms at the phases of the small bumps, indicating moments of very rapid heating in the atmosphere.

*Hutchinson, J. L., 1974 submitted to *Astrophys. J.*

Table 4
Bolometric Calculations

	M_{bol} (OAO-2 Mags)	F_{bol} (10^{-7} erg/s^{-1} cm^{-2})	L (10^{36} erg/s^{-1})	
			Observed (bolometric)	$\langle 4\pi R^2 \sigma Te^4 \rangle$
RT Aur	-11.33	3.6	8.5	5.6
	-10.64	1.9	4.5	
α UMi	-13.54	27.8	2.7	–
	-13.46	25.7	2.5	
δ Cep	-12.43	10.0	9.5	9.7
	-11.86	5.9	5.6	
β Dor	-12.81	14.1	16.4	17.0
	-12.06	7.1	8.2	
Y Oph	-12.43	9.9	51.8	26.3
	-12.08	7.3	37.7	
"Standard Model"	–	–	12.43	

Although B3 appears to remain constant in phase with wavelength, there is evidence for a phase-shifting component underlying the main bump. This is probably more easily seen in the six-color observations of Breckinridge and Kron (1963), especially for the near-infrared R and I filters. The underlying component should thus be identified with "true" maximum light (produced by a combination of temperature and radius variations); B3 itself should be identified with a traveling shock, releasing its energy primarily in the ultraviolet. The small bumps are also probably produced by shocks.

Because of these effects, I decomposed the light curve of β Doradus into a smoothly varying, "normal" Cepheid part and the flux bump part attributed to the shocks. The light curve of ζ Geminorum with a period of $10^{d}15$ was used as a model for the "normal" part even though the two Cepheids appear quite different in both light and radial velocity. Such a decomposition is a convenient device to permit study of the flux bumps alone. I found that the total energy emitted by B3 is about 10 times that of either B1 or B2; the first small bump is about two-thirds the size of the second. Longward of about 4000 Å for B3 and about 2500 Å for the small bumps, the bump spectra were essentially flat at phases of maximum bump emission. Below the cutoff wavelengths, the spectra fall off rapidly. Similar continuous spectra are observed in the laboratory from shocked gases (Petshek et al., 1955).

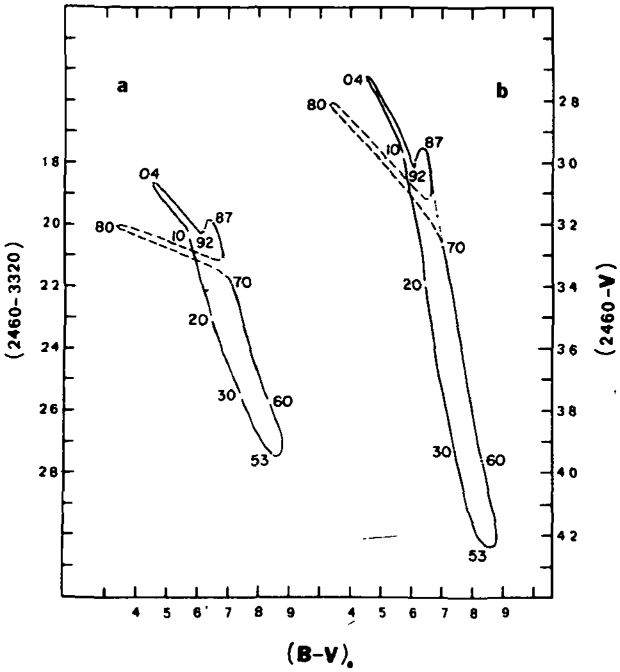

Figure 4. Color-color behavior of β Doradus in two ultraviolet colors versus (B-V). At phases of the bumps β Doradus becomes very blue indicating atmospheric heating of short duration.

While the production of the shocks will be addressed by others at this Workshop, I would like to take this opportunity to point out some interesting features concerning the long-term behavior of β Doradus. Figure 5 summarizes essentially all of the published photometry of β Doradus since its discovery, reduced according to the ephemeris:

$$\text{JD (max light)} = \text{JD}2435206.44 + 9.84200 \times \text{Epoch}$$

Visual observations (Kukarkin et al., 1969) and the Harvard-Patrol photographic measurements of the 1910s are given low weight. The upper curve, (a), indicates that the arrival time of maximum light does not significantly deviate from the ephemeris; hence, the period of pulsation has been remarkably constant. However, the bottom curves, (b), show that the amplitude of the light variation has perhaps undergone some change during the same time interval, especially in the blue. (Note that comparison of amplitudes can only be made between observations at the same effective wavelength.) Because Rodgers and Bell (1967) also note significant long-term changes in the radial velocity of β Doradus (but only between phases 0.8 and 1.0), the indication is that the amplitude of the shock bump alone is varying, and not the whole light curve.

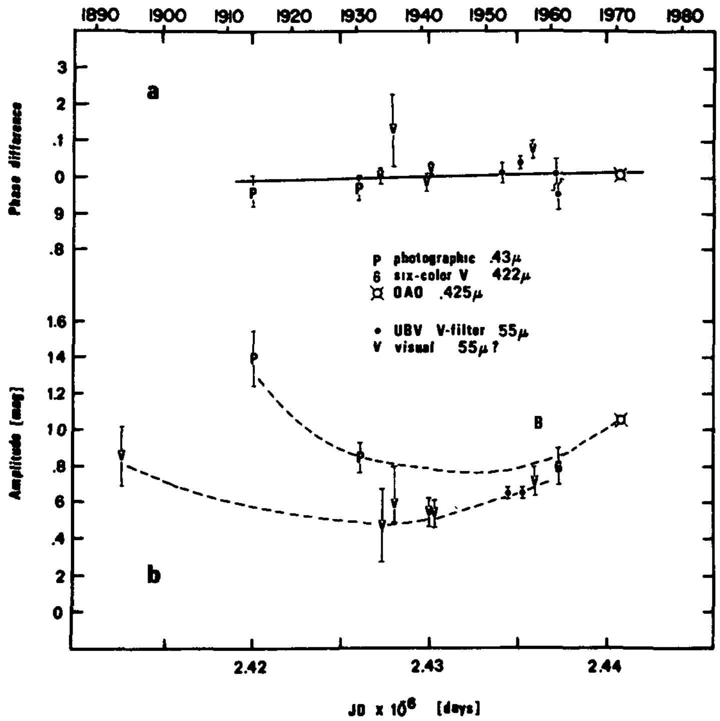

Figure 5. Long-term behavior of β Doradus. (a) Difference in fractions of a period, between observed time of maximum light and time calculated by JD (max) = JD2435206.44 + 9.84200E. (b) Variation in the light amplitude at effective wavelengths of ~4250 Å and ~5500 Å.

SUMMARY

The ultraviolet behavior of most of the Cepheids observed by OAO-2 is essentially an extension of their visible behavior and can be described reasonably well by existing model atmospheres. Amplitudes of light variation show a strong linear correlation with inverse wavelength down to 1910 Å, and the phase of maximum light is earlier at shorter wavelengths.

On the other hand, the only secondary-bump Cepheid fully observed by OAO-2, β Doradus, reveals additional information about possible shocks. It is unfortunate that the OAO-2 observations of η Aquilae, a $7^{d}_{.}2$ secondary-bump Cepheid, are incomplete; perhaps future observations with the Astronomical Netherlands Satellite (ANS) can remedy this situation.

This work has been supported in part by NASA grant NGR-06-003-079.

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DISCUSSION

COX:

I want to ask about these dips that you have been analyzing so much. Are they real; are they being displayed every period?

HUTCHINSON:

Beta Doradus and the other Cepheids were only observed for one cycle. However, in the case of β Doradus, observations were obtained a year apart in two runs, each time for only a single cycle. Unfortunately, the observations for the second run did not include the region of bumps. However, they are statistically significant. The error bar I showed was the maximum error I would expect, including whatever systematic errors I should expect from scatter and the sky.

COX:

I would like to strike a blow about the inaccuracy of photometry 70 years ago. You know, the size and age of the universe has changed dramatically from what I remember from when I was a lad. The change was sudden and by a factor of 2 due to the inaccuracies of photometry. Seventy years ago, photometry was very uncertain. I'm not sure you have any case at all for a change in the amplitude of β Doradus.

HUTCHINSON:

I agree that that is a distinct possibility. I just point out that also it could be due to very poorly done photometry. For instance, if someone didn't include the maximum peak, one could obtain a light curve which would yield a lower amplitude than actually one should measure, I agree. I just wanted to point out that it might be better used as a call for better photometry.

DAVIS:

Did you do a spectrum analysis of minimum and maximum light for β Doradus and did they differ?

HUTCHINSON:

In the ultraviolet, the spectra do not exist at those phases. They were obtained mostly at phase 0.4, and, at that phase, β Doradus essentially looks like a normal super giant.

STELLINGWERF:

Is it possible to obtain, more or less, continuous observation of the light variations?

HUTCHINSON:

The satellite had an orbital period of about 100 minutes, and you can observe during 20 minutes of those 100 minutes. During that time, the satellite passes into the earth's shadow, and for that interval you could obtain continuous monitoring of the star.

KARP:

I am wondering about subtracting off the light curve of ζ Geminorum, because that has a very symmetric low amplitude light curve, whereas β Doradus has a large amplitude and very asymmetric light curve.

HUTCHINSON:

Yes, I scaled the light curve of the ζ Geminorum to match in the minimum part (of the light curve) where I expected no flux from the bump. I was trying to find the asymmetries. That is why I subtracted the symmetric light curve.

PARSONS:

If the amplitude really does go through a minimum, does anyone here have any idea how this can be explained in terms of the evolution? Evolutionwise, one might expect the opposite.

COX:

It depends on how the star evolves. The amplitude varies across the strip. It depends on where it is and which way it's going.

PARSONS:

Why would it go through a minimum?

COX:

I've already said, I don't believe it.

CHRISTY:

There are some other amplitude changes of rather (faster) character, as in RR Lyrae itself, which are not clearly understood. I think it seems clear there is more than just simple spherical pulsation going on on a number of these stars. The spherical pulsation is perhaps the dominant phenomenon.

ROSENDHAL:

I would just like to comment that it seems to me interesting that there is no evidence for a period change over 70 years in this star and so many of the other Cepheids do seem to show period changes. This is one of the ones that has a secondary bump. I'm wondering if anyone knows anything about the statistics of the period changes and if period changes are related in any way to Cepheids showing secondary bumps.

COX:

I'm not sure that we have ever seen this in Cepheids. We've seen a wealth of data for RR Lyrae stars . . .

CHRISTY:

Period changes?

COX:

Period changes, yes.

CHRISTY:

I think that there is also quite well established data for Cepheids.

COX:

Which star? Do you know, Bob?

HOLLARS:

Zeta Geminorum is one of the stars, and I recently found period changes in RT Aurigae; at least period changes is one of the explanations.

COX:

Well, I just asked whether or not the period changes correlate with the bumps.

HOLLARS:

I think not.

HUTCHINSON:

Eta Aquilae also shows a period change.

VOICE:

It has a bump and ζ Geminorum does not.

CHRISTY:

I believe the Kippenhahn and Baker group did compare the period changes with what might be expected from evolution across the instability strip and found that they were at least compatible.

BAKER:

They certainly are of the right order of magnitude. I think this is going on the basis of the period changes in the ones that I think are in M3 and ω Centauri.

COX:

But aren't those RR Lyrae stars?

BAKER:

I think even in the case of the RR Lyrae stars, where you have a lot more data, that you have to be rather careful. First of all, it's a tricky business to make sure you don't have to miss a cycle, and beyond that there is a selection effect that I think has never been properly investigated. The stars that have large period changes, of course, are studied, and the ones that don't seem to have any period changes are not mentioned. So I think it's rather poor data to try to base any evolutionary importance on.

VON SENGBUSCH:

. . . They are not in contradiction to evolutionary models.

COX:

Whereas for RR Lyrae stars there are some presumably well observed period changes that are too fast for evolution, and that poses a problem.

BAKER:

Well we still have RU Cassiopeiae . . .

COX:

That's an amplitude problem, mostly . . .

BAKER:

Didn't it change its period as well?

HILL:

I think that in the case of RR Lyrae itself, the major effect of the period changes is really an amplitude change, because we see that the "stillstand" comes and goes. The stillstand seems to indicate a shock wave origin, so if you take the shock wave away as the velocity amplitude would indicate, that moves your period around. So I don't know about any other RR Lyrae stars, but at least that one I did a model on. That would seem to explain that period change. But then that doesn't help, because it doesn't explain why you have an amplitude change.

HOLLARS:

In RT Aurigae, if you take the Johnson and Wisniewski data in 1968 and do a best fit, you get a six-hour change. You might want to treat that as a period change or an error in the epoch. I don't know which, but six hours is fairly large.

BAKER:

But unless it has been monitored almost continuously, how can you tell?

PARSONS:

I'd just like to make a comment on the fit or the nonfit of my model at 5400 K. I'm sure I didn't do blanketing properly in the ultraviolet and that could affect the κ ratio.

HUTCHINSON:

Also, in the amplitude with wavelength relation, I'm sure that where the fit is good it's because the blanketing is treated correctly. Maybe the dips and wiggles in that relation are due to blanketing.

COX:

I'd like to ask about that amplitude versus wavelength. How do you do that? I didn't think you had done any nonlinear calculations. I gather somehow you fit various Parsons' model atmospheres at various times . . .

HUTCHINSON:

I fit one to the maximum light and I fit one to the minimum.

COX:

And then you looked at the output of those static atmospheres?

HUTCHINSON:

Right.

SHOCK WAVES IN A BETA DORADUS MODEL

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INTRODUCTION

The accumulation of observational evidence and interpretation supports the conclusion that β Doradus has running shock waves in its atmosphere. Early observations include those by Gratton (1953) of Ca II in emission at certain phases and the measurements of the Hα radial velocities by Bell and Rodgers (1967). Later investigation of the variation of the surface gravity with phase has suggested to Parsons (1971) that running waves are consistent with his model atmosphere calculations. The conclusion that the running waves develop into shock waves is further supported by the Orbiting Astronomical Observatory (OAO-2) ultraviolet observations and analysis by Hutchinson (1974). It is the work by Hutchinson with its predictions for the phases of the shock waves that prompted this author to construct a numerical hydrodynamical model for the atmosphere of β Doradus.

A short description of the procedure of the calculation follows. The next section presents the results for the β Doradus model and contains a comparison with the observations. It is of particular interest to verify that the atmosphere contains multiple shock waves as suggested by Hutchinson for the ultraviolet observations.

THE NUMERICAL METHOD

The numerical model is based upon the usual mass and momentum equations approximated by their finite difference form with the inclusion of a quadratic artificial-viscosity pressure term. The model is spherically symmetric. The finite difference formulation is described by Richtmyer and Morton (1967). This model is a sample of a large grid of atmosphere models. Because of the large total number of cases desired, the need to obtain solutions without residual transients, and the convenience of using the "no-charge" observatory minicomputer, the algorithm must be small and fast in its execution. For this reason, an isothermal approximation is used. This is justified by the rapid postshock cooling as found by Whitney and Skalafuris (1963). It is also justified by the numerical hydrodynamical calculations for an RR Lyrae atmosphere and calculations of the resulting shock waves (Hill, 1972). These calculations explicitly included the effects of radiative losses.

The atmosphere is driven by a spherical piston. Both the velocity and location of the piston are specified as a function of time. Parsons' (1971) summary table of observed velocities is used. It is assumed that these are the velocities at a fixed mass depth even though, in truth, they are weak line velocities originating at a nearly constant optical depth. The inability to know the precise location of the optical depth is necessitated by the isothermal approximation. It is believed that the use of the "wrong" radial velocities for the fixed-mass piston is the greatest source of possible error in this analysis. This difficulty will again be mentioned in the section on the comparison with the observations. The mass depth for the placement of the piston was determined from the β Doradus type model atmospheres of Parsons (1969).

RESULTS AND COMPARISON WITH THE OBSERVATIONS

The characteristics of the β Doradus model are listed in Table 1. Figure 1 illustrates the location of the shock waves and compression waves in the atmosphere. For reference, Figure 2 illustrates the piston velocity which is used for this model. In the model there is one main shock wave (S1) which is enhanced by strong phases of compression (C1 and C2) and by a total of three more "feeder" shock waves (S2, S3, and S4). The main shock wave is produced at phase 0.5 as a result of the deceleration of the piston. The main shock is

Table 1
Characteristics of Beta Doradus Model

Period	8.5035×10^{5} s
Average radius	4.32×10^{12} cm
Outer mass depth	1×10^{-7} g/cm^2
Inner mass depth	3×10^{1} g/cm^2
Mass ratio [M(I)/M(I+1)]	1.2
Gravitational acceleration at average radius	4×10^{1}
Temperature of isothermal atmosphere	6000 K

then strengthened by the strong compression events; the first is associated with the phase of zero outward acceleration and the second is associated with the phase of zero velocity. These changes in the acceleration of the piston are in opposition to the lower density, pre-shock material which, because of the low pressure gradients, is still accelerating inward. Figure 3 shows the preshock zones in their accelerated return to the shock front. In Figure 3, the compression due to each shock may be seen. Figure 4 indicates the rapid increase in the velocity discontinuity due to the two compression events. At phase 0.85, the first outward maximum velocity of the piston, a second shock (S2) is produced. It quickly merges with the main shock. The infall of the postshock material behind the main shock (S1), in combination with the outward velocity of the piston, produces the third shock (S3). The

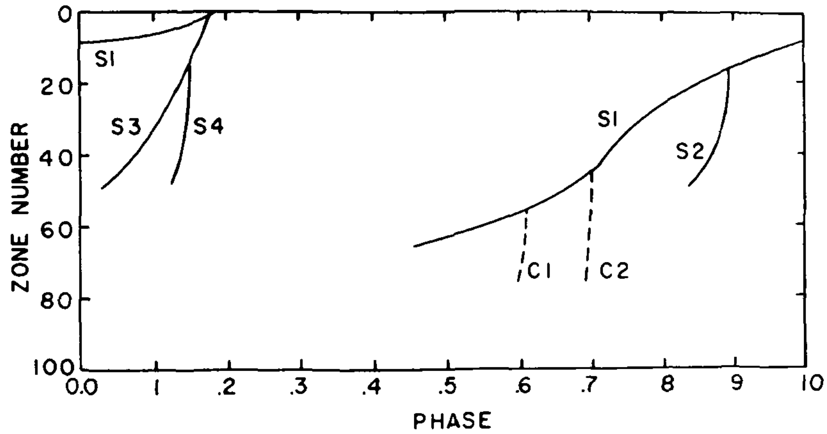

Figure 1. Location of shock front in β Doradus model. The compression events are denoted by the dashed lines.

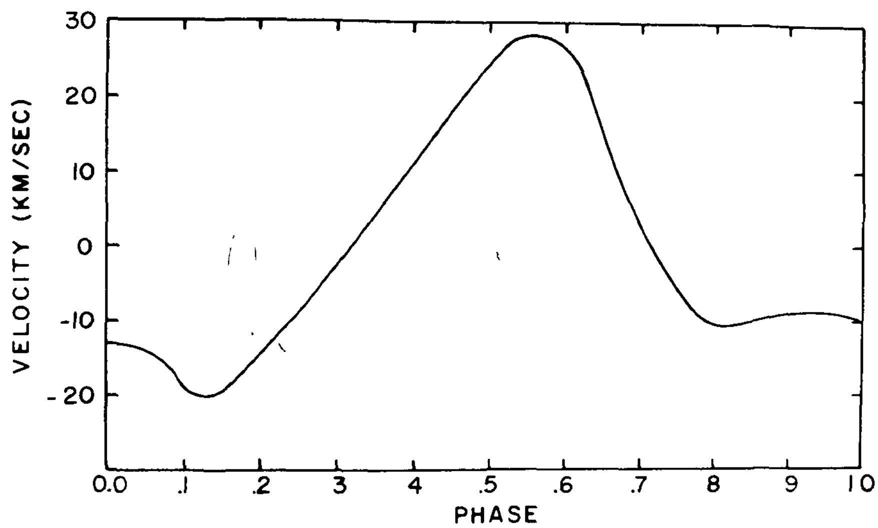

Figure 2. Radial velocity curve used in β Doradus model (from Parsons, 1971).

piston reaches a second maximum velocity at phase 0.1 and creates the fourth shock (S4). Shortly after the shocks combine to strengthen the main shock (S1), the main shock leaves the upper zone of the model.

The OAO-2 ultraviolet observations are used to confirm this picture of the shock structure in the atmosphere. The observations were presented by Hutchinson (1974). On the basis of the fluctuations in the ultraviolet light curves, Hutchinson suggests the presence of three shock waves appearing near phases 0.75, 0.85, and 0.0. Figure 5 illustrates the radiative

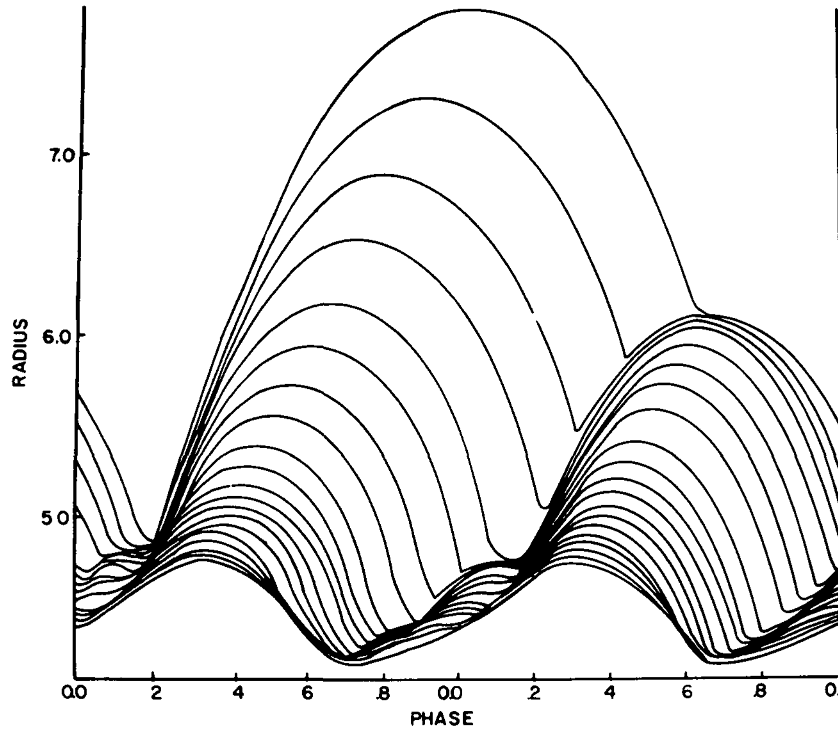

Figure 3. Relative positions of zones of constant mass depth in the β Doradus model.

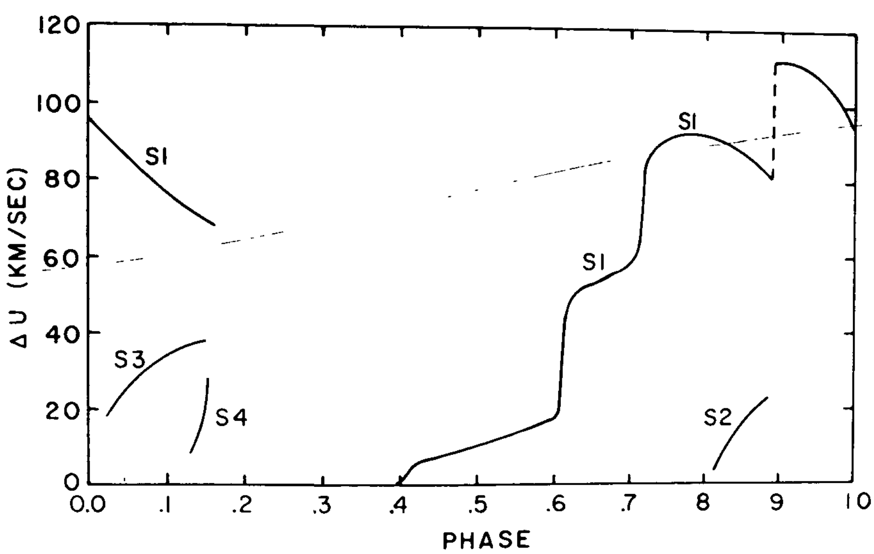

Figure 4. Velocity discontinuity (ΔU) across shock front as obtained from the model.

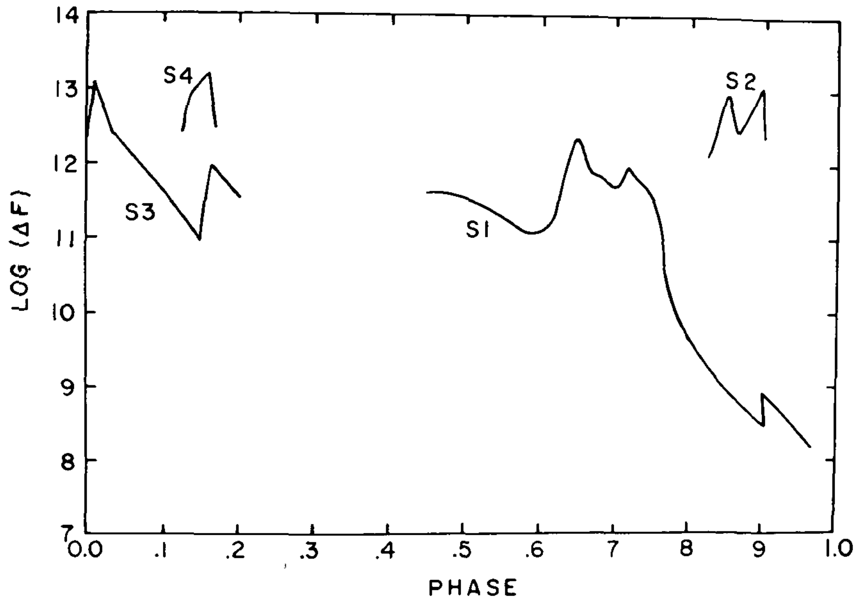

Figure 5. Radiative energy losses (ΔF, in ergs/cm²/s) as obtained from the model.

flux expected from the shock waves in this model. This was calculated from the relationship for the flux,

$$F = \rho_a (\Delta U)^3$$

where ρ_a is the preshock density, and ΔU is the velocity discontinuity of the shock.

In Figure 5 we see the maximum contributions to the radiative flux by the shock wave begin at phases 0.64, 0.72, 0.85, 0.04, and 0.13. However, hydrogen is ionized by velocity discontinuities greater than 50 km/s. Thus, we would not expect ultraviolet radiation from the main shock (S1) before a phase of 0.7, or from the fourth shock (S4) at any phase. Because the most significant portion of the energy deposited by the fourth shock is in the visible region, we might expect an indication of its contribution to be added to the visible light curves without a complementary indication in the velocity curve. An inspection of the photometric visual observations of Mitchell et al. (1964) in Figure 6 shows a very suggestive shoulder at phase 0.15. This could be associated with the fourth shock.

The phases of the production of the initial weak shocks could be very much in error. Particularly susceptible to error is the phase of the production of the main shock at phase 0.4. From the range 0.4 to 0.7, the opacity is increasing, due to the compressional increase in the density and the increase in the effective temperature. Thus, the optical depth scale will move out through mass during those phases. The piston will have a greater inward acceleration during those phases than the observations would indicate. This greater inward acceleration by the piston would retard the formation of the weak shock to a later phase than is indicated in Figure 4. Also, the "hard" nature of the piston, by not allowing the motions in the atmosphere to alter the position of the piston, would tend to produce a shock which is too early in its phase.

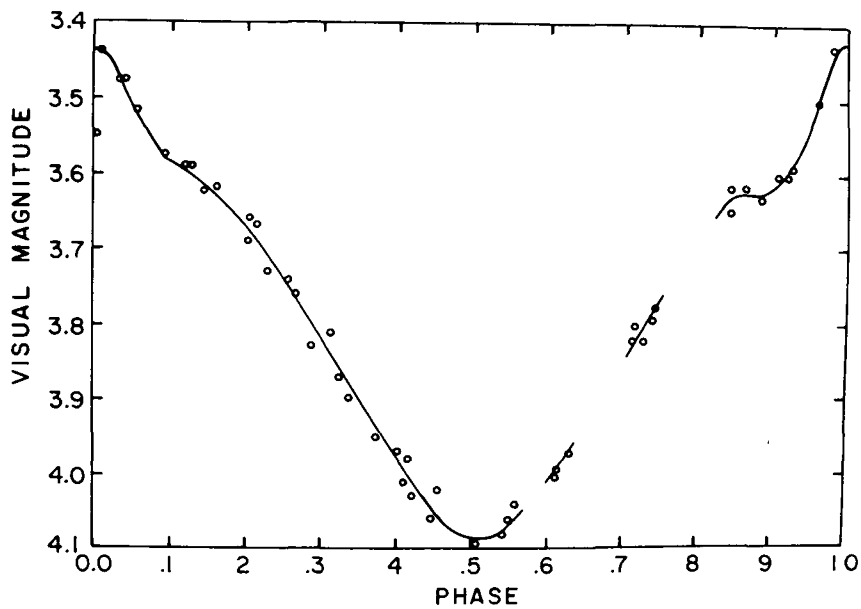

Figure 6. Observed visual magnitude of β Doradus (from Mitchell et al., 1964).

Figure 7 illustrates the observed velocity curve as determined by Bell and Rodgers (1967) for the Hα line. Also shown is a constant mass depth velocity curve. This mass depth for the formation of the Hα line was obtained from Preston (1969). The comparison between the two curves is disappointing. However, it is noted that the integral of the observed Hα velocity is far from zero. This may be due to three possible effects. They are as follows:

- The Hα line is not formed at a constant depth; its depth of formation alters as the atmosphere responds to the shock waves. However, the model of the atmosphere contains no zones which exhibit an inward velocity between phases 0.8 to 1.0. Also, in a previous application (Hill, 1972), it was possible to assume a constant mass depth for the origin of the Balmer lines and to match the theoretical results with the observations.

- The effect of emission of Hα in the immediate postshock region would move the line center to the red. Investigation of the line profiles does not reveal any evidence of emission. Thus, we will assume that this is not a major cause for the discrepancy.

- The cores of the absorption lines are noticeably asymmetrical between phases 0.75 to 0.9. This is the same interval in which there is the greatest disagreement. This asymmetrical shape is that which one would expect in pulsating stars. Figure 8 illustrates the asymmetry of the absorption line. This asymmetry strongly affects the velocity curve by Bell and Rodgers (1967). They determined their velocities from the point of minimum intensity in the core. A similar asymmetry was investigated by van Hoof and Deurinck (1952) for η Aquilae. They

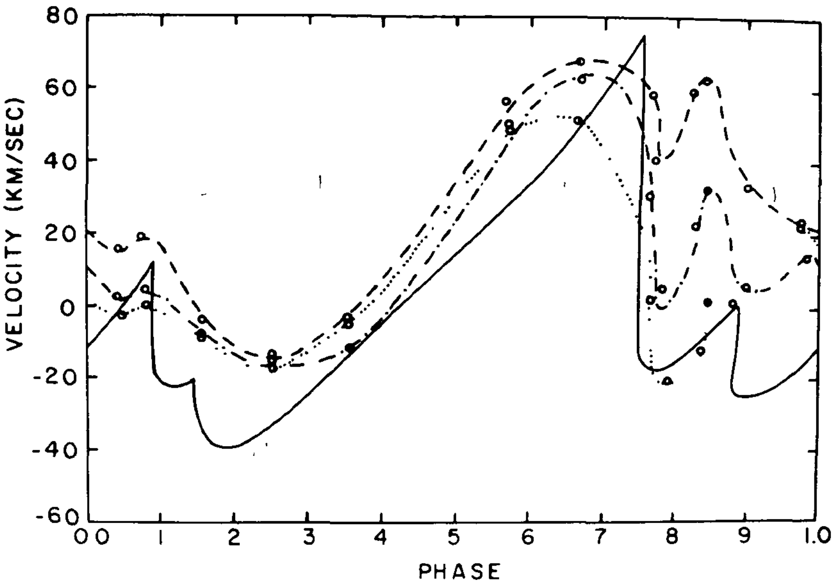

Figure 7. Hα velocity curves for β Doradus. The continuous line is obtained from the model. The dashed line is from Bell and Rodgers (1967), who used the location of minimum intensity. The two other lines are obtained from the Hα line profiles by Bell and Rodgers. The dashed line with dots used the location of the optical center of mass of the lines in the range $\lambda = \pm 4$ Å. The dotted line is derived from the line center of the line as measured at I_ℓ/I_c (intensity in line/continuum intensity) = 0.65.

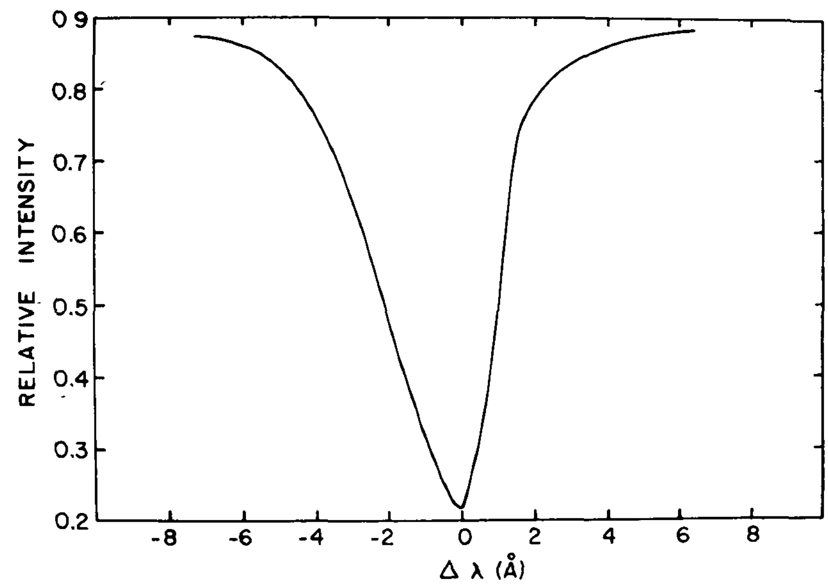

Figure 8. Observed line profile of the line in β Doradus obtained by Bell and Rodgers (1967). This profile is from plate 969 at phase 0.83.

state that the 24/17 correction factor is only applicable when used with the position of the optical center of gravity of the absorption line. Figure 7 illustrates the velocities as obtained by this method. The published (Bell and Rodgers, 1967) line profiles were used to determine the optical center of gravity. Figure 7 also contains the result of using the midpoint of the core as measured at I_ℓ/I_c equal to 0.65. This was chosen as a representative depth in the line which an observer might select in measuring the apparent center of the absorption core. It is very revealing to observe that the velocity discrepancy due to the different methods of measurement indicated in Figure 7 ranges from only 2 km/s to almost 80 km/s about half a period later. This lack of a constant correction factor for the strong lines was emphasized by Teske.*

In the light of the uncertainties in the interpretation of the observations, it is encouraging to note that the phases and the relative shock strengths are not in contradiction with the corrected observations. In fact, the comparison throws doubts on the applicability for the uncorrected measurements made at the bottom of the central core of the strong absorption lines.

CONCLUSIONS

Construction of a hydrodynamical, piston driven model for the atmosphere of β Doradus has indicated the existence of multiple shock waves in one period. These multiple shock waves are the natural result of the nature of the underlying photospheric velocity. The model is in agreement with the phase of the appearance of the excess ultraviolet radiation as noted by Hutchinson (1974) and corroborates his conclusion that it is associated with shock waves. The model is compared with the observations of the Hα lines, and this comparison dramatizes the inaccuracies of velocity determinations using strong lines. However, the observations can be presented in a fashion which is entirely consistent with the observations.

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DISCUSSION DURING PRESENTATION

CHRISTY:

It doesn't take much of a wiggle there [at the bottom of Figure 3] to . . .

HILL:

That's right, the slightest amount of error in the piston motion can cause significant effects in the calculated motions.

COX:

Could these shocks [Figure 3] be due to transients?

HILL:

I ran it for three months to assure myself that there were no transients.

BAKER:

How many periods is that?

HILL:

Twenty thousand. That's one of the advantages of your own minicomputer. You can just let it sit there and run. Explosive starts or slow starts made no differences.

BAKER:

Did you see evidence of reflection from the upper level of the atmosphere?

HILL:

The reflected shock from the upper, very massive zone is not significant.

DISCUSSION

DAVIS:

I would like to ask you, Steve, have you compared these shocks with Hillendahl's studies? He has about the same number of shocks.

HILL:

No, I haven't done that for the following reason: I did not expect a close comparison, because my memory tells me that he had his shocks going through the photosphere, therefore he was producing much stronger shocks than I was. It is the case that he is producing a large number of shocks. I find it interesting that independent of what the ionization zones are doing, given the observational velocity curve, you can produce a good number of shocks in the lower part of the atmosphere. But if you go up high enough, they are all going to join together. So, looking at the top of the whole situation, you are going to see them join and produce one shock.

BEDNAREK:

Couldn't you have zones crossing if there were some sort of turbulence?

HILL:

Oh, certainly. Wipe out everything I've said if you are going to . . .

PARSONS:

For the next few months I wonder if you will tune your piston to ζ Geminorum and η Aquilae?

HILL:

Yes, that's what I am going to do. It's an interesting question on some of these stars that have the same periods that look like they don't show that excess ultraviolet emission and try to see whether or not those shocks are formed too high to produce anything, or the densities are too low, or whether or not they are literally defused by an early weak shock which is produced. When it comes time to build up your main shock, it runs through as compression so you never see it. Then, even though you have a large velocity jump up there, the preshock density is so small that you would have no ultraviolet emission. That's just a guess. I am going to do that. That's part of the grant.

PARSONS:

And, also how dependent it is on the shape of the velocity curve? One of the things that I have done with this is to look at different assumptions for the radius and different assumptions for the gravity to see if we could turn around and use it as a tool for the timing of the shock waves—to tell us something about the gravities and the radii of these objects.

KARP:

Have you tried tweaking the observed velocity curve a little bit to see how large an effect you had on your computed models?

HILL:

I have done that, but not with this particular case. For this particular case, as a comparison I ran a sinusoidal model, and there were worlds of difference. In fact, I have those results with me if you would like.

KARP:

But they were extremely different. What about just within the range of the observations?

HILL:

I haven't tweaked the velocity curves. All I've done is tweak the radius and the gravity of the object. So the radius is essentially giving you a second-order effect of gravity in this situation.

STELLINGWERF:

You showed the double period of the outer zone. I wonder what that depends on. Does that depend on the outer zone mass? Is it possible to get triple periodic variations?

HILL:

If I go far enough, yes. Quite true. Because then this "g" that that zone feels is way down. I can trace it through for 18 periods, in fact. I can lay it out on the rug and see what becomes periodic; then for the outermost zones, that entire thing becomes periodic in 18 periods. But there you are talking about the densities being like the interstellar medium. How much effect that really has is–it's an interesting curiosity, but I maintain it's no more than that right now.

CHRISTY:

From the point of view of a mathematician, however, there is a different thing that is probably that the boundary condition, if you really place it out to the very low densities, is not going to be periodic.

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MEAN COLORS OF CEPHEIDS

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This paper concerns the method of taking a mean of the color variations of Cepheids over their pulsational cycle. It is demonstrated that the mean color depends on the type of mean employed. Thus, color observations of Cepheids can be interpreted by a color-effective temperature relation to give different T_e values for each kind of mean color. Here, theoretical colors from numerical integrations of Cepheid pulsations are used to determine the proper method of taking the color mean in order to get, by the color-T_e relation, the correct nonpulsating T_e.

Of great interest are the so-called pulsation masses of Cepheids which depend on accurate T_e values derived from colors and their mean. With the proper color mean, and therefore T_e, an accurate luminosity of a Cepheid in a galactic cluster can be used to give a photospheric radius. Then the period-mean-density relation, with a constant in that relation determined from linear pulsation theory, can be used with this photospheric radius and the observed period to give a pulsation mass. A pulsation mass depends on accurate colors, corrected for reddening, a reliable method of taking their mean, the appropriate color-T_e relation, an accurate luminosity, the proper period-mean-density relation for the pulsation mode, and the precision of the observed period.

Typically, the pulsation mass of a Cepheid is only 70 percent of the mass needed in evolutionary calculations for the observed luminosity. This mass discrepancy can be due to problems in no mass-loss evolution calculations, which are still being discussed. It can be due to errors in the calculational process needed for pulsation masses, such as the method of taking the color mean, the color-T_e relation, or errors in the reddening or luminosity. Finally, the mass discrepancy may be real in spite of the current idea that mass loss of 20 percent or more will not allow evolution of stars to Cepheid conditions.

The discussion here is solely about how to take a mean color of a Cepheid and its impact on pulsation masses. Others such as Bell and Parsons* are discussing the color-T_e relation. Iben and Tuggle (1972a) have discussed the mass-luminosity relation for galactic Cepheids and concluded that the mass discrepancy can be resolved by increasing Cepheid distances and therefore their luminosities by about 0.3 magnitude. Further, evolutionary calculations with mass loss seem appropriate to complete the investigation of the Cepheid mass discrepancy and to study the difference between the mass-luminosity relations

*Bell, R. A. and S. B. Parsons, 1974, MNRAS (in press).

$$L \sim M_{evo}^4$$

and

$$L \sim M_{puls}^{5.6}$$

Given by Iben and Tuggle (1972b).

If the derivation of an effective temperature results in a value too hot by 300 K, or about 5 percent, the true cooler temperature would give a 10 percent larger radius to furnish the assumed accurate luminosity and a 30 percent larger mass to match the observed period. Böhm-Vitense (1972) derives a 500 K cooler Cepheid effective temperature and presumably solves the mass discrepancy. The Bell and Parsons colors, however, do not agree with those of Böhm-Vitense, and the color-T_e relation such as that of Kraft (1961) still remains uncertain. In any case, the proper color mean is necessary, especially for large amplitude Cepheids, and temperatures too cool (not the desired too hot temperatures) of 300 K are possible when using certain means.

The hydrodynamic models used to derive colors and their means have been calculated using the variable Eddington radiation approximation described by Bendt and Davis (1971). This monochromatic radiation treatment is an improvement over the diffusion approximation extensively used previously for even the outer layers of the pulsation model. The structure of the atmospheric layers is now accurate enough for a color mean discussion, though perhaps even better blanketed absorption coefficients are needed to set the structure for determination of absolute colors.

Typically in the calculation there are 5 to 10 zones above optical depth unity, but in test cases this number has been halved. While only 13 photon energy groups are used in the hydrodynamic calculations, the structures have been stored for 30 phases in each of three successive periods and spectra predicted from the T(r), ρ(r) data with 30 photon group absorption coefficients and line blocking data from Bell.* These spectra are convoluted with the B and V filters and corrected for their relative transparency to give B and V colors.

Raw colors B, V are corrected by the relative filter transmissions by the formula

$$(B\text{-}V)_c = B - V + 0.65$$

though the constant should be perhaps 0.69 ± 0.02 to match theoretical colors of Bell and Parsons for various of Parsons' (1969) models at 6000 K, 5700 K, and 5400 K. Allen (1963) recommends a correction of 0.71. There is only a slight change in the B-V, log T_e relation between our colors and those of Bell and Parsons. Our reddest colors should even be redder as will be seen later in comparing with the Kraft relation also. If better blanketed absorptions are used, as in the case when static atmosphere programs produce atmospheres to match onto the hydrodynamic calculation, then more accurate colors would result. Our color means, however, must be more accurate than 0.01 in B-V and by our conversion to log T_e (to be given later) more accurate than one-half percent in T_e.

*Bell, R. A., 1974, private communication.

Table 1 gives the basic data for the four models newly calculated for this study. Three are from the list of King et al. (1973) and represent large amplitude pulsators ($\Delta M_{bol} \geqslant 1_{mag}$). Also included is a model for β Doradus. At these amplitudes there is a larger than usual difference between the linear and nonlinear theory periods, but the linear theory period for E15b may be underestimated because the envelope was not taken too deep.

Table 1
Radiation Transport Models
(Masses, radii, effective temperatures, luminosities, and periods of the four models studied in this work.)

Name	$M/M_\odot$	$R/R_\odot$	T_e(K)	Log T_e	$L/L_\odot$	M_{bol}	Fundamental Period (days) Linear	Fundamental Period (days) Nonlinear (King, Cox, Eilers, Davey)
4a	4	49.9	5984	3 777	2820	-3 90	7.32	7.49
β Dor	4	58.7	5900	3.771	3700	-4 20	9 78	–
E7b	7	68.3	5781	3.762	4603	-4 44	8 64	8.80
E15b	15	287 5	5200	3.716	53460	-7 10	63.00	67.20

The light curve for model E7b is shown for three periods in Figure 1. A notch on the rising part of the light curve at phase 0.5 has been ascribed by Hillendahl* and others as a problem with the necessary artificial viscosity used with the numerical hydrodynamics. The problem of "which are real bumps" and "which are not" has been discussed by Keller and Mutschlecner (1971). It does seem, though, that a reasonable looking light curve has been calculated.

Figure 2 gives the (B-V) color curve for the same three periods. While this curve, as for the light curve, is afflicted with some spurious bumps, the color dips are less than 0.02 magnitude and do not affect any of the mean colors.

Velocity curves at the levels of the Rosseland mean optical depths of 0.1 and 2/3 are shown in Figure 3. The integration of these data to give the radii curves at these two levels is shown as Figure 4. This difference between the photosphere and line-forming regions is always much less than one percent of the stellar radius even for the reddest Cepheids. Use of spectral lines for measuring photosphere radius changes in the Wesselink method seems valid, and only few percent negative corrections seem necessary for Wesselink method radii determinations.

Relations between our colors and effective temperatures are given for the four models in Figures 5, 6, 7, and 8. In all cases the Kraft (1961) relation

$$\log T_e = 3.886 - 0.175\,(B\text{-}V)$$

*Hillendahl, R. W., 1971, paper presented at the Los Alamos Conference on Pulsating Stars.

is shown as the solid line. Investigation of normal versus coarse zoning, and with nonequilibrium gray diffusion (radiation T $\neq$ matter T) versus our full monochromatic treatment in the hydrodynamic program, result in the same (B-V) -log T_e relations. These are Figures 9, 10, and 11 for the model 4b of the King et al. tabulation. We believe our difference from Kraft and also from Bell and Parsons is due only to our lack of blanketed absorption coefficients in the hydrodynamic calculation.

Table 2 gives the results for four different means taken of the colors from the four models. Because of the difference between the Kraft (B-V) -log T_e relation and ours, we have used individual relations given for each model. The slope runs about -0.25 as compared to -0.175 for Kraft and -0.15 for Bell and Parsons. The first mean is a magnitude mean, while the other three involve intensity means, respectively, of B, V, (B-V), and (V-B). Conversion of the color to T_e and comparison with the nonpulsating model T_e gives T_e values between +69 K and -311 K. In almost every case, the $\langle B \rangle_{int} - \langle V \rangle_{int}$ mean, a currently popular one, is the best to represent the nonpulsating star.

No systematic error is seen to give mean colors too blue and the inferred temperatures as too hot. Actually, the opposite is the case for some of the less desirable means. Improved atmosphere structures and color systems will certainly change quantitative numbers, but the conclusions seem unchangeable. These same general results were found with less accurate data for another model by Cox and Wing (1973) last year.

This work was supported by the U.S. Atomic Energy Commission and the National Science Foundation.

Table 2
Mean Colors and Temperatures for Transport Models
(individual (B-V), log T_e relations)

	β Dor 3 947 - 0 258 (B-V)				4a 3.974 - 0.292 (B-V)			
	Color	log T_e	T_e(K)	ΔT_e(K)	Color	log T_e	T_e(K)	ΔT_e(K)
Model	–	3.771	5902	–	–	3.777	5984	–
$\langle B\text{-}V \rangle_{mag}$	0.718	3.762	5781	-121	0 689	3.773	5929	- 55
$\langle B \rangle_{int} - \langle V \rangle_{int}$	0.668	3 775	5957	+ 55	0.658	3.782	6053	+ 69
$\langle B\text{-}V \rangle_{int}$	0.726	3.760	5754	-148	0.694	3.771	5902	- 82
$-\langle V\text{-}B \rangle_{int}$	0.763	3.750	5623	-279	0 717	3.765	5821	-163
	E7b 3 947 - 0.253 (B-V)				E15b 3 936 - 0 228 (B-V)			
Model	–	3.762	5781	–	–	3.716	5200	–
$\langle B\text{-}V \rangle_{mag}$	0 777	3 751	5636	-145	1 000	3.708	5105	- 95
$\langle B \rangle_{int} - \langle V \rangle_{int}$	0 725	3 764	5808	+ 27	0.964	3.716	5200	00
$\langle B\text{-}V \rangle_{int}$	0.785	3.748	5598	-183	1 000	3.708	5105	- 95
$-\langle V\text{-}B \rangle_{int}$	0.825	3.738	5470	-311	1.035	3.700	5012	-188

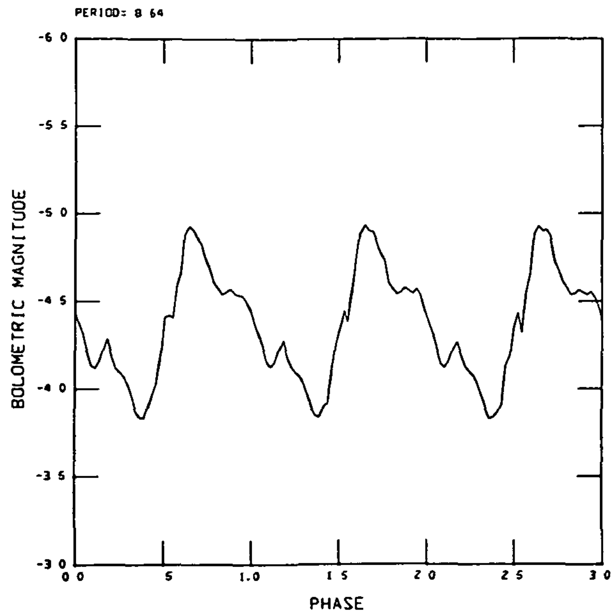

Figure 1. The bolometric magnitude light curve for model E7b for three periods. The magnitude mean is -4.48 compared to -4.44 for the nonpulsating model.

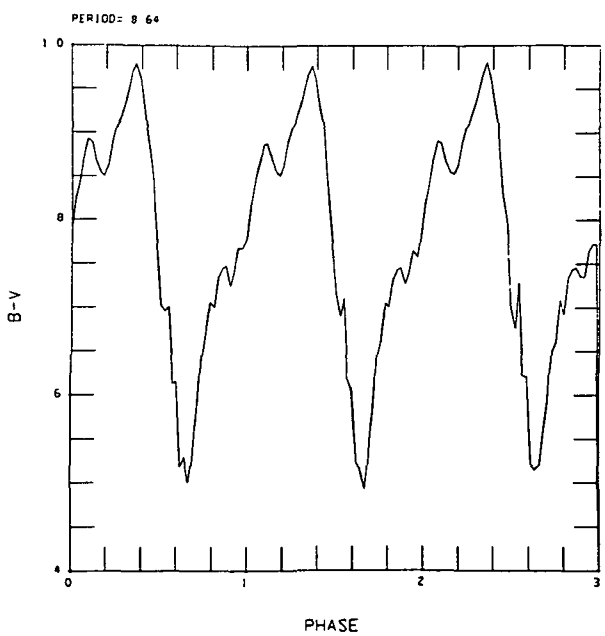

Figure 2. The (B-V) color variation for model E7b for three periods. Mean colors for various methods are given in Table 1.

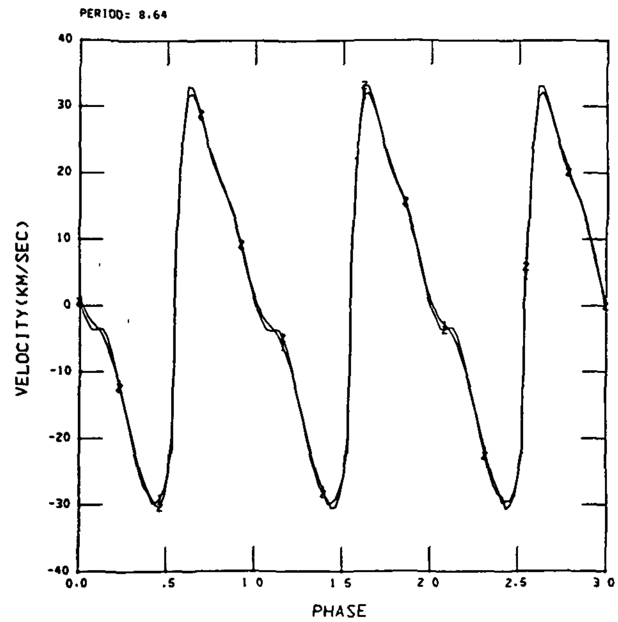

Figure 3. Velocity curves at Rosseland mean optical depths of 0.1 and 2/3 for three periods of model E7b.

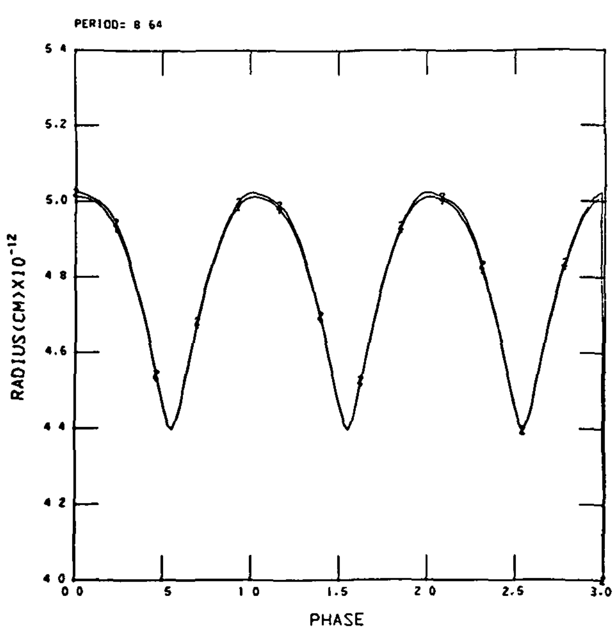

Figure 4. Radius variations for Rosseland mean optical depths of 0.1 and 2/3 for three periods of model E7b.

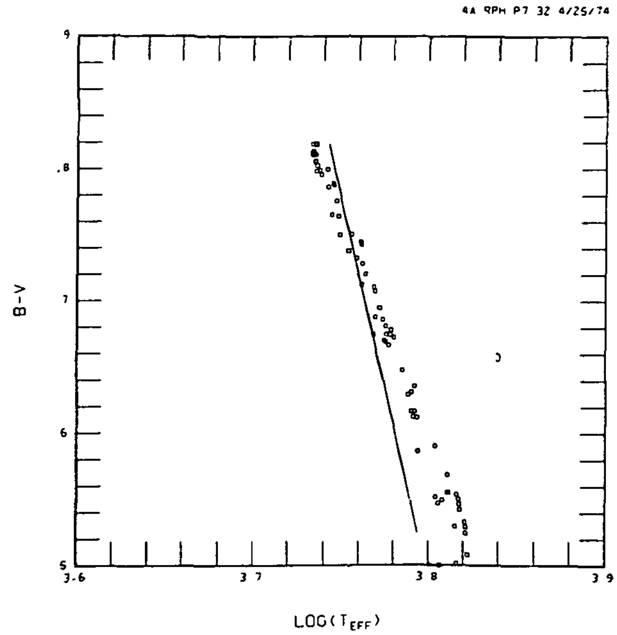

Figure 5. The (B-V) -log T_e relation for model 4a during three successive periods. The Kraft relation is given as a solid line.

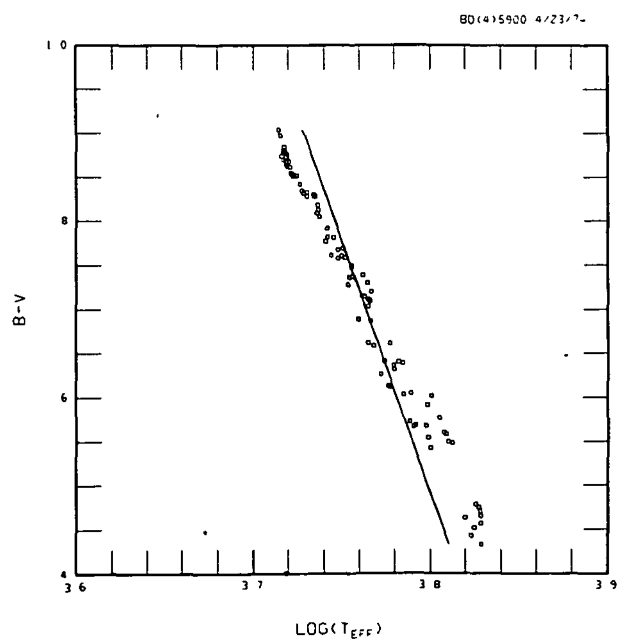

Figure 6. The (B-V) -log T_e relation for the β Doradus model during three successive periods. The Kraft relation is given as a solid line.

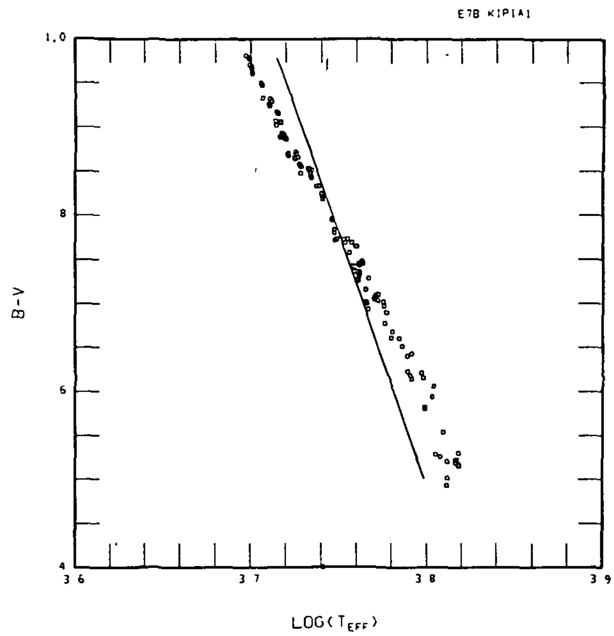

Figure 7. The (B-V) -log T_e relation for model E7b during three successive periods. The Kraft relation is given as a solid line.

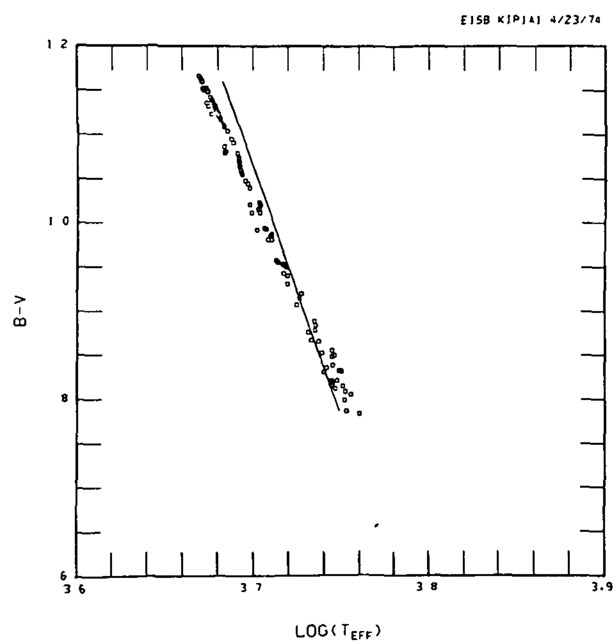

Figure 8. The (B-V) -log T_e relation for model E15b during three successive periods. The Kraft relation is given as a solid line.

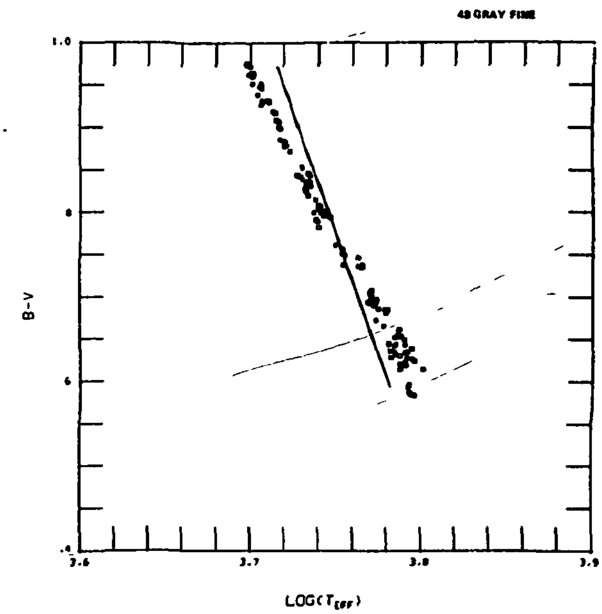

Figure 9. The (B-V) -log T_e relation for model 4b in the nonequilibrium diffusion, gray approximation during three successive periods. The Kraft relation is given as a solid line.

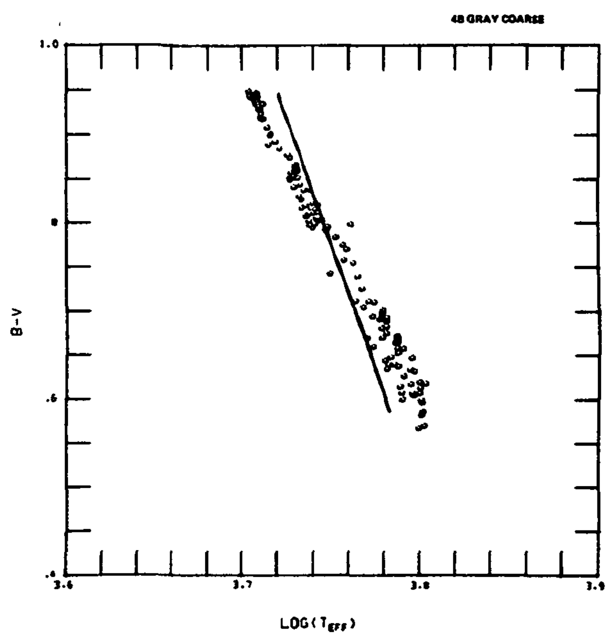

Figure 10. The (B-V) -log T_e relation for model 4b from a coarse zoned nonequilibrium diffusion, gray approximation calculation for three successive periods. The Kraft relation is given as a solid line.

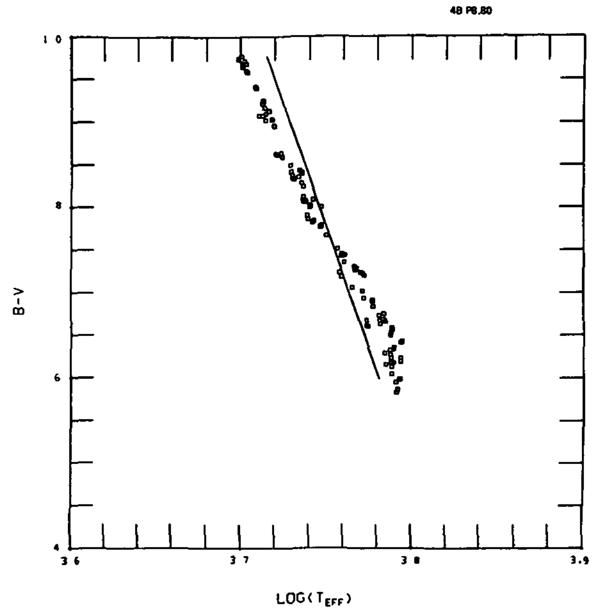

Figure 11. The (B-V) -log T_e relation for model 4b during three successive periods. The Kraft relation is given as a solid line. Mean colors for the four means in Table 1 are given on the figure. The second mean is preferred giving log T_e = 3.757 as compared to the identical value for the nonpulsating model.

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DISCUSSION

KARP:

When you did your color transformation from your arbitrary scale to Johnson's, you just used an additive constant; is that correct?

COX:

Yes. This 0.65 is part of our system, you see.

KARP:

I think that Caputo and Natta have shown that if you use the Matthews and Sandage filter sensitivity functions, the coefficient of B-V is slightly different from unity. I wonder if that would cure the problem? You did the actual transformation of A times color plus B.

COX:

Since we've talked about this, you know the answer. So let me say for the benefit of the rest of the audience, that all I'm saying is, let's calibrate it by some really good model. What we did was run five or six of Parsons' models through our system and we found a slope coefficient very near to 1. But the intercept coefficient is nearer 0.69. I believe that's the right way to do it. I'm not really able to answer your question directly because we didn't discuss the filter system in any great detail. We took them from either Allen or Sandage and Matthews, I don't recall which.

KARP:

I got something like 0.96, I think, for my particular filter sensitivities that I used, in front of B-V.

COX:

I can't tell you the coefficient right now. I know it's very close. But let me give you another answer. This number does change from 0.69 by as much as 2.02 over 6000 K to 5400 K, I guess. Our red colors should be redder.

KARP:

I'm just suggesting that if that were taken into account that would account for the change in slope.

COX:

Yes, and that is in the right direction.

KARP:

I've also done the averaging, and I got the best results in the intensity difference, so even though you have a problem, it still works. I get the same thing.

COX:

The point of my paper is, even though I don't think my colors are highly accurate, I don't like them too well–they certainly have very close to the proper excursion and are plenty accurate for this discussion of how to take the means.

ROSENDHAL:

In the case of RU Camelopardalis, which I think was the name of that thing that stopped pulsating some years ago, I think we have an example of where nature has provided an indication of how you ought to take the mean in the quiescent phase of it. Have you compared your methods of taking the mean to the color variations of RU Camelopardalis and compared it with the quiescent colors of that object?

COX:

No, it never occurred to me.

ROSENDHAL:

That might, in fact, give you some insight into what nature provided as an example of what mean colors ought to be in a pulsating star.

COX:

I'd like to look into that. Maybe you can help me find some of the references to the data. That would be fun to do. And indeed that has been discussed in Cepheid conferences in the past, to look at RU Camelopardalis.

HILL:

I'm wondering in the case of your B-V versus the effective temperature diagram if you could indicate what the Böhm-Vitense results were like so I could have an idea of how much of a difference . . .

COX:

I've got a Xerox copy of the articles here. But they ran 500 K cooler in this very region, and that's the key point.

HILL:

The whole thing would be displaced?

COX:

Yes. Pretty much the same slope. Maybe Parsons knows better than I offhand, but you can look at it. The plots in the Böhm-Vitense paper were not as nice as some we have had. It runs 500 degrees cooler; and that makes the radius bigger, and since the radius is bigger, you have to have the mass bigger to have a nice observed period.

ROSENDHAL:

With regard to the Böhm-Vitense temperature scale, Schmidt and I took a look at the slope and the zero point of her temperature scale. We analyzed one Cepheid through its cycle using her scale and determined that you get a variation of about a factor of 10 in the iron abundance, which suggests that the temperature scale had to be wrong. That's a ludicrous result using that scale. It's an argument that the slope is wrong if not the zero point.

COX:

That's a little bad, because I believe that Bell and Parsons like the slope a lot better than the intercept. I'll let Sid talk about that.

PARSONS:

It's the slope mainly—well, both. In B-V versus log T_e, the high temperature end is pinned down fairly well. Her slope is the main thing we don't like.

COX:

I see.

KING:

I'll show the slide in the next talk and compare Schmidt's temperature scale with Böhm-Vitense's temperature scale.

COX:

I think you ought to do that. It gets a little detailed. It's sort of off the subject of my paper, which is just to discuss how to take the mean, not alleging its high accuracy in

PULSATION OF DOUBLE-MODE CEPHEIDS AND COMMENTS ON THE PROBLEM OF CEPHEID MASSES

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DOUBLE-MODE CEPHEIDS

The double-mode Cepheids are stars, found near the low luminosity end of the Population I Cepheid strip, which display a mixture of modes where the longer of the two periods is between two and four days. It is usually assumed that the mixture is composed of fundamental (F) and first harmonic (1H) modes. These stars have been discussed by Fitch (1970), Stobie (1970, 1972), Stobie and Hawarden (1972), Rodgers and Gingold (1973), Petersen (1973), and Schmidt (1974).

In order to obtain the various physical parameters for the double-mode Cepheids, we note first that we can determine the pulsation constant, Q, in the period-mean-density relation $(Q = \Pi(\rho/\rho_\odot)^{1/2})$ for realistic stellar models, by making use of a linearized nonadiabatic analysis such as that outlined by King et al. (1973). By studying a large number of models that obey a variety of mass luminosity relations, we have obtained simple fitting formulas for the pulsation constants, Q_i, which are given by:

$$\log Q_i - g_i(M) = a_i \exp \left\{ -b_i [\log(M/R) - c_i]^2 \right\} + d_i \qquad (1)$$

where

$$g_0(M) = A_0 M \ , \qquad (2)$$

$$g_1(M) = A_1 \log M \ , \qquad (3)$$

and where M and R are the mass and radius expressed in solar units.

The results for the composition with hydrogen mass fraction X = 0.602 and heavy element mass fraction Z = 0.044, referred to as the Kippenhahn mixture, were presented in a previous paper by Cox et al. (1972) and are included for comparison purposes. Table 1 gives values for the coefficients in the above expression. We also present results for two additional mixtures, X = 0.70, Z = 0.02 and X = 0.80, Z = 0.02. All opacity and equation-of-state data were obtained using the Los Alamos code (Cox, 1965).

Table 1
Constants in the Fitting Formulas

i	A_i	a_i	b_i	c_i	d_i	Composition
0	0.003250	0.499	0.96	-2.4	-1.486	X = 0.602, Z = 0.044
1	0.02604	0.105	2.47	-1.5	-1.605	
0	0.002891	0.5822	1.0467	-2.4	-1.4877	X = 0.70 , Z = 0.02
1	0.02845	0.09996	2.5788	-1.5	-1.6077	
0	0.002386	0.6174	1.0555	-2.4	-1.4877	X = 0.80 , Z = 0.02
1	0.02577	0.09871	2.2467	-1.5	-1.6107	

Figure 1, adapted from Cox et al. (1972), shows the behavior of log $Q_i - g_i(M)$ versus log (M/R) for the various mixtures. The filled circles represent the individual models for the Kippenhahn mixture. The solid curves are least-squares fits to these points for the fundamental and first harmonic modes (that is, a plot of Equation 1). We have also plotted a few values for the compositions designated by X = 0.70 and X = 0.80 using Equations 1, 2, and 3 and the coefficients from Table 1. It will be noted that the effect of composition is rather slight. Equation 1 represents the detailed numerical results for these compositions to better than 1 percent in Q_i for both the fundamental and first harmonic modes. It should be cautioned that these results were obtained using purely radiation models.

We have applied these results to the problem of the double-mode Cepheids. Petersen (1973) has previously done this, making use of the Cox et al. (1972) results, and, as he points out, the fits to Q_i involve only the two parameters, M and R. Thus, given Π_0 and Π_1 of a double-mode Cepheid, and assuming these are true period identifications, M and R may be determined. For the eight stars he considered, Petersen computed M and R for the Kippenhahn mixture and fits to 0. In Table 2, values for the present calculations are given for the Kippenhahn mixture (which verify his results) and similar entries are provided for the two new mixtures.

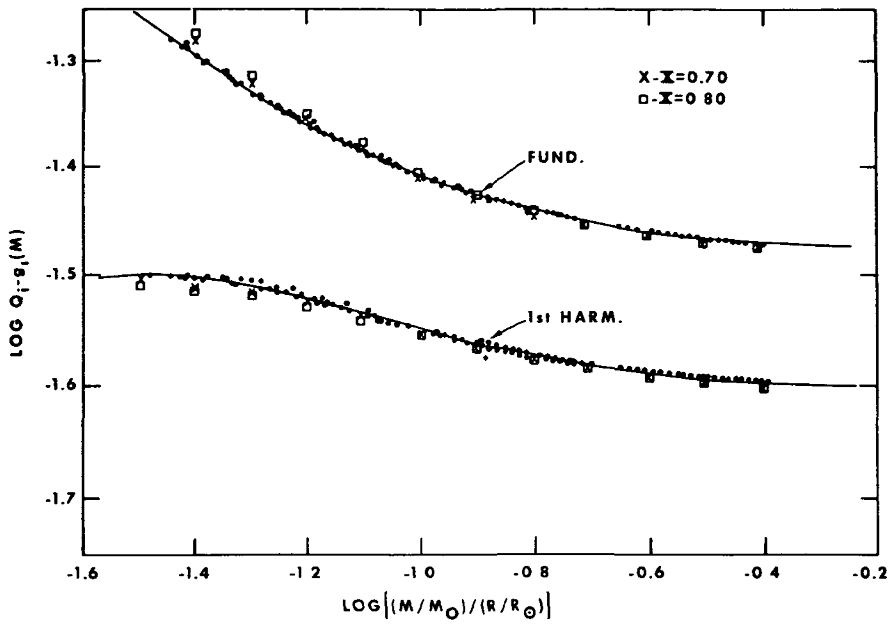

Figure 1. Log Q_i - $g_i(M)$ versus log (M/R) for fundamental and first harmonic modes. Solid lines are Equation 1 for X = 0.602, Z = 0.044, filled circles are computed models for the same composition, x = points for Equation 1 for composition X = 0.70, Z = 0.02, and □ = similar points for X = 0.80, Z = 0.02. M and R are in solar units and Q_i is in days.

Table 2
Masses and Radii of the Double-Mode Cepheids

Star	Composition		Kip Mix		X = 0.7		X = 0.8	
	Π_0	Π_1	M	R	M	R	M	R
V439 Oph	1.89	1.34	1.04	13.0	1.24	14.0	1.34	14.4
TU Cas	2.14	1.52	1.16	14.6	1.37	15.7	1.48	16.2
U TrA	2.57	1.83	1.13	17.3	1.60	18.6	1.74	19.2
VX Pup	3.01	2.14	1.50	19.8	1.76	21.2	1.92	21.9
AP Vel	3.13	2.20	1.40	19.6	1.64	21.0	1.81	21.8
BK Cen	3.17	2.24	1.46	20.1	1.70	21.5	1.88	22.4
Y Car	3.64	2.56	1.59	22.6	1.85	24.1	2.05	25.1
AX Vel	3.67	2.59	1.68	23.1	1.95	24.8	2.16	25.8

We note first the remarkable homogeneity of these objects in terms of Π, M, and R. Secondly, their masses ($1 \lesssim M \lesssim 2$) are distinctly lower than for the usual Cepheids ($3.5 \lesssim M \lesssim 16$). As Petersen (1973) points out, these results suggest a distinct class of objects. It is interesting to note that the values obtained observationally for Π_1/Π_g are of the order of 0.71. If one assumes evolutionary masses (≅3.5 M for $\Pi_0 \cong 2.6$, X = 0.70, $M_{bol} \cong -1^m.9$), or slightly less, then $\Pi_1/\Pi_0 \cong 0.74 - 0.75$.

In order to determine whether or not models having the lower masses are likely to represent double-mode Cepheids, it is necessary to test the models for pulsational stability in the F and 1H modes. We must first choose a luminosity or effective temperature. Unfortunately, it is only for the stars U Triangulum Australis and TU Cassiopeiae (Schmidt, 1972b) that one of these is available. Petersen (1973) quotes a Θ_{eff} of ≅ 0.84 ± 0.04 for the star U Triangulum Australis, based on Rodgers (1970) and Rodgers and Gingold (1973). This corresponds to T_{eff} = 6000 K ± ≅300 K. With a radius of 18.6 $R_\odot$ as given in Table 2 for X = 0.70, an effective temperature of 6000 K yields $M_{bol} = -1^m.78$. We have used the nonadiabatic analysis techniques discussed in King et al. (1973) to investigate a sequence of models with T_{eff} ranging from 5600 K up to 6400 K. The results are given in Table 3. The mass was fixed at 1.6 $M_\odot$ and the composition was given by X = 0.70 and Z = 0.02. The luminosity of the model was varied along with the effective temperature to yield a period close to that observed. We have also indicated the growth rates for the F and 1H modes, η_0 and η_1, as well as η_1/η_0 (for a definition, see King et al., 1973).

Table 3
Linear Models of U Triangulum Australis

M = 1.6 $M_\odot$			X = 0.70, Z = 0.02			
M_{bol}	T_{eff}(K)	Π_0	Π_1	η_0 (X10²)	η_1 (X10²)	η_1/η_0
-1.43	5600	2.57	1.86	5.52	7.79	1.4
-1.58	5800	2.57	1.85	5.43	8.32	1.5
-1.65	5900	2.57	1.85	5.05	7.38	1.5
-1.73	6000	2.56	1.85	4.92	7.21	1.5
-1.80	6100	2.56	1.85	3.58	5.89	1.6
-1.87	6200	2.55	1.84	3.51	6.42	1.8
-2.01	6400	2.55	1.85	0.932	0.666	0.7

We note that from the viewpoint of the linear analysis, all of these models seem to be acceptable (that is, they are all unstable in both F and 1H modes). Our previous results for Cepheids (King et al., 1973), which compared linear and nonlinear calculations, indicated that the 1H should be more unstable than the fundamental in order to survive at large amplitude. If this is also true for these stars, we might rule out the model with the highest T_{eff} (6400 K).

In order to choose among these models, it is necessary to do a nonlinear analysis to see whether or not both F and 1H modes will survive in some mixture at limiting amplitude. We have employed the nonlinear techniques described by Stellingwerf,* which provide the same information as those described by Baker and von Sengbusch (1969). In this preliminary study (which is still in progress), we are investigating two models, one at T_{eff} = 5800 K and one at T_{eff} = 6100 K. The mass for both models is 1.6 solar masses with luminosities (M_{bol} = $-1^{m}.6$ and $-1^{m}.8$, respectively) such that the fundamental period is close to that of U Triangulum Australis ($2^{d}.6$). These results indicate that the model at 6100 K is a fundamental pulsator at limiting amplitude, while the model at 5800 K has the aperiodic behavior expected of a double-mode Cepheid. We are still attempting to determine the limiting amplitude behavior of the mixture of fundamental and first harmonic modes, and we expect to find it similar to that found by Stellingwerf for the RR Lyrae variable AC Andromedae.

We conclude then, that in order to obtain models of double-mode Cepheids which have a value of Π_1/Π_0 close to the observed value, it is necessary to assume masses, a factor of two or more, less than evolutionary masses for normal Cepheids. Schmidt (1974) has recently found that for TU Cassiopeiae the observations may be compatible with a lower mass. This lends some support to the findings discussed here. In addition to the lower masses, our preliminary nonlinear calculations indicate that double-mode behavior is expected for such models if they are near the red edge of the instability strip. Because their masses and luminosities do not seem to fit in with traditional ΠLC or M-L relations, we suppose that they can be separated from the classical Cepheids along with some of the problems associated with the latter variables.

CEPHEID MASSES

The apparent discrepancy between masses of Cepheids obtained from pulsation theory and stellar evolution calculations is by now well documented in the literature (see, for example, Iben and Tuggle, 1972b; Schmidt, 1973 and references therein). One way in which this discrepancy has been exhibited is the following: Given an observed period and color, a ΠLC relation (for example, Sandage and Tammann, 1969) plus a bolometric correction can be used to find the star's luminosity, and, from an assumed color-effective temperature calibration, the temperature is derived. With the luminosity, effective temperature, and observed period, pulsation theory (either linear or nonlinear) will lead to a mass (M_{puls}).

*Stellingwerf, R. F., 1974, "Modal Stability of RR Lyrae Stars," unpublished thesis, University of Colorado.

Given the luminosity, however, evolutionary theory predicts a corresponding mass (M_{evol}). The usual result is that the ratio M_{evol}/M_{puls} is greater than unity, up to, perhaps, a factor of two for what are considered to be normal Cepheids.

To demonstrate the magnitude of the discrepancy, we use the Sandage and Tammann (1969) ΠLC relation (note, however, possible inconsistencies as pointed out by Iben and Tuggle, 1972b),

$$M_V = -3.425 \log \Pi + 2.52 (\langle B \rangle - \langle V \rangle) - 2.459 \tag{4}$$

and the relation

$$M_{bol} = M_V + 0.145 - 0.322 (B\text{-}V) \tag{5}$$

from Sandage and Gratton (1963). For our color-effective temperature law we use that of Schmidt (1972a), which is given by

$$\Theta_{eff} = 0.641 + 0.309 (B\text{-}V). \tag{6}$$

For the pulsation part of the analysis we choose the fundamental mode linearized results discussed in the first section of this paper, with X = 0.70. The luminosity -M_{evol} relation as taken from King et al. (1973) is

$$\log L = 3.48 \log M + 0.68. \tag{7}$$

Thus, given the period and color for a particular Cepheid, the above relations uniquely determine M_{evol} and M_{puls}.

We have analyzed 241 galactic and extragalactic Cepheids from various sources (detailed references may be found in King et al., 1975) with the following results. The ratio $\alpha \equiv M_{evol}/M_{puls}$ is given (to within 5 percent) by

$$\alpha = 1.14 + 0.40 \log \Pi + 0.13 (\langle B \rangle - \langle V \rangle). \tag{8}$$

For the ranges $0.03 \leqslant \log \Pi \leqslant 2.13$, $0.26 \leqslant \langle B \rangle - \langle V \rangle \leqslant 1.18$ of the 241 objects studied, this corresponds to $1.12 < \alpha < 2.06$. An important thing to note is that α increases monotonically as luminosity increases up the strip.

There have been two major suggestions for resolving this discrepancy. Iben and Tuggle (1972a, b) have suggested possible difficulties and inconsistencies in some of the basic relations used in this type of analysis. In their earlier paper they suggest that the magnitude scale of the ΠLC relation be readjusted by up to 0.3 magnitude (to higher luminosities) in order to have $\alpha \cong 1$. A further discussion of this can be found in King et al. (1975). Our present results indicate that, depending basically on the period of the object, the absolute visual magnitude must be adjustable over a range of about 0.4 to 0.6 magnitudes (the two numbers come from Iben and Tuggle's or our results). Similar conclusions are obtained when modest adjustments in the coefficients of Equations 6 or 7 are made.

Another possibility is to modify the M_{evol} – L relation, but retain the Sandage and Tammann ΠLC relation. In order to obtain a value of $\alpha \cong 1$, this leads to a relation which is completely at variance with evolutionary theory, assuming no mass loss. This has been discussed by Iben and Tuggle (1972b) and also by Lauterborn et al. (1971) who have shown that a modest mass loss of 10 percent in the red giant phase suppresses the Cepheid loop of a 5 $M_\odot$ star. Lauterborn and Siquig (1974) have obtained somewhat similar results for more massive objects.

Still another suggestion for resolving the problem is to modify the (B-V) – T_{eff} relation (Equation 6) but retain all else. We find that

$$\Theta_{eff} = 0.640 + 0.438\ (B\text{-}V) \equiv \Theta_{eff}(req) \qquad (9)$$

results in α differing from unity by, at most, 15 percent, except for four stars (all extragalactic) that appear to have anomalously small ⟨B⟩ - ⟨V⟩ for their periods. Equation 9 implies a readjustment of the temperature scale (to lower temperatures) by about 300 K for the shortest period objects to about 650 K for the longest. (A possible lowering of the temperature scale was earlier suggested by King (1970), Needham,* and was implied in the results of Cox et al. (1972).) That is, we require the greatest percentage changes (~15 percent) for the cool variables (where, we might point out, B-V is perhaps a poor temperature indicator; see Schmidt, 1973). In Figure 2 we show the above relation (Equation 9) and that of Schmidt (1972a), that is, Equation 6. The figure is a bit deceptive because the spread between the lines is really quite large. For example, if our Figure 2 is compared to Figure 2 of Schmidt we find that the relation in Equation 9 lies above almost all of the stars Schmidt used for his calibration. However, Figure 2 also shows the temperature scale proposed by Böhm-Vitense (1972) for Ib supergiants. We see that, except for small (B-V), this temperature scale is very close to what is required. It is also very similar to the scale of van Paradijs (1973), except that his scale is a bit hotter for (B-V) > 0.85 and, at least down to his last computed point at (B-V) ≅ 0.75, it is cooler. Both of these adjustments are desirable from our viewpoint. Schmidt et al. (1974) have criticized these scales on the basis that they tend to yield discrepant iron abundances in some observed stars according to their atmospheric analyses. We should like to urge that the source of the differences in these various scales be found.

Another attractive feature of the cooler scale is shown in Figure 3. Here, on an H-R diagram, are pictured the instability strip of Sandage and Tammann (1968) derived from their table A1 and ΠLC relation, but adjusted to lower temperatures according to Equation 9. Also shown are the new locations of the 241 sample variables. As expected, the vast majority lie in the strip except for a few extragalactic objects. In addition, our theoretical blue edge from the linearized calculations for Y = 0.28 and the M-L relation of Equation 7 is shown. The horizontal shift in the theoretical edge resulting from a change in helium abundance of Y = +0.10 is $\Delta \log T_{eff} \approx +0.02$, and from a change in mass at constant L by

*Needham, C. E., 1971, "Linear Pulsation Analysis of Cluster Cepheid Variables," unpublished thesis, University of New Mexico.

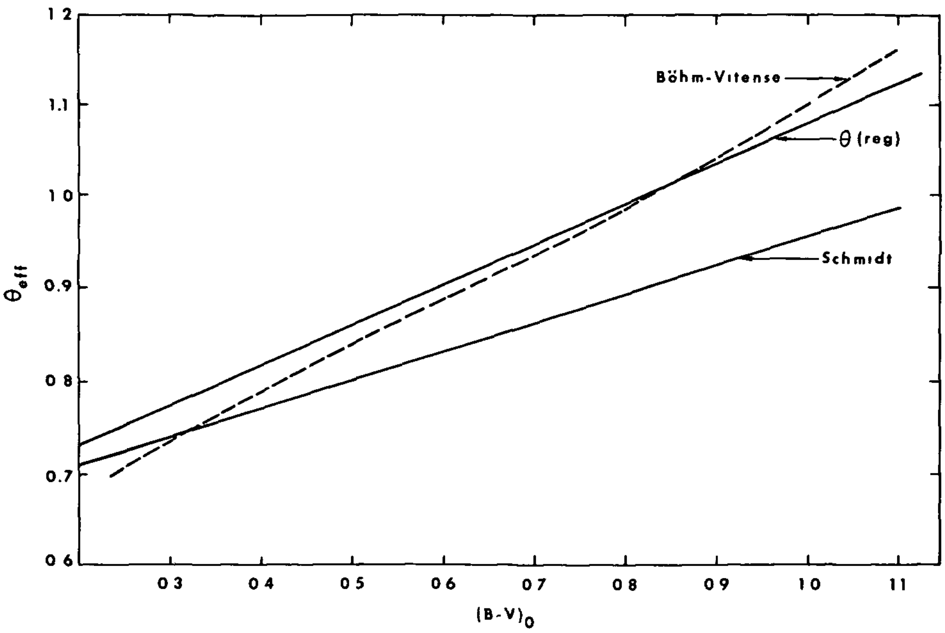

Figure 2. Color-temperature scales of Schmidt (1972a), Böhm-Vitense (1972), and Equation 9.

$\Delta \log M = -0.3$ yields $\Delta \log T_e \cong -0.02$. Thus, it would take rather radical changes in Y or in an M-L relation to modify the heartening result that the blue edge of the empirical strip (with the cooler temperature scale) is very near the theoretical blue edge. (We remark that the fundamental blue edge of Iben and Tuggle (1972a) for 5 $M_\odot$ and similar composition lies close to ours.)

The above analysis does involve some inconsistencies (apart from those discussed by Iben and Tuggle, 1972b), because the ΠLC relation of Sandage and Tammann (1969) does depend on the temperature scale assumed. However, it would require a full scale reanalysis of the observational data to construct a new, self-consistent ΠLC relation, and this is beyond the scope of the present paper. (We have also examined the more recent ΠLC relation of Sandage and Tammann (1971).) Except for requiring a temperature adjustment not quite as extreme as Equation 9—about 550 K rather than 650 K at the cool end—the results are substantially the same as we have discussed here.)

Although a fully self-consistent solution to the Cepheid mass problem has not been found, several lines of inquiry seem promising. Although it is not clear that the temperature scales of Böhm-Vitense (1972) and van Paradijs (1973) are correct, our results at least lend support to that supposition. However, it is not unlikely that a complex of factors enter into the result. An adjustment of the distance scale is quite attractive and recent suggestions (van Altena, 1974) for placing the Hyades at a greater distance are in the required direction. On the other hand, it is possible that either or both the evolutionary theory and the pulsation theory may be at fault.

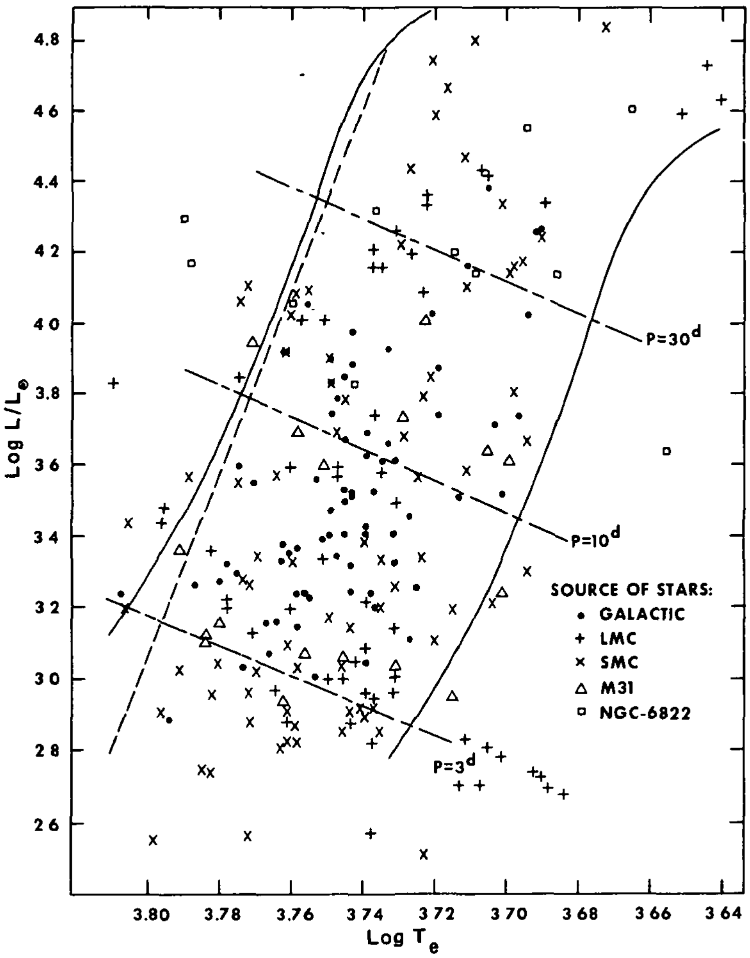

Figure 3. Positions of sample stars on the H-R diagram where the color-temperature scale of Equation 9 has been used. Constant period lines are shown as long dashes and the fundamental blue edge as a short-dashed line.

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DISCUSSION

KARP:

Didn't Fricke, Stobie, and Strittmatter compute evolution calculations and play around with the opacities? Didn't they show they could do almost anything they want with the loops, whether there was mass loss or no mass loss, they could still make the loops go away or reappear?

KING:

They did a number of things. I'm not really sure how reliable those things were.

KARP:

I'm just wondering if the changes that you have reported due to mass loss made the loops go away, couldn't the . . .

KING:

The kinds of things they did, I think, can only result in the loops going away, which is not a desirable change. You would like to still get the loops, for the kind of masses you are talking about. I don't think they found that's possible. Maybe you can't get loops at all if you do things right.

KARP:

The thing I was wondering about is, when you have problems with the mass loss models, even in the straightforward no-mass loss models that we think we understand, you are getting strange effects. Maybe that ought to be straightened out first.

KING:

I agree. I don't know which is first, but certainly that's one thing. Does anybody here know more about that particular evolutionary calculation?

COX:

Well, only that they are working on that problem, and it has to do with the early evolutionary stages just off the main sequence where the accuracy of how you burn hydrogen in the deep layers, and what happened way, way back in the earlier stage affects the looping.

KARP:

I remember they had some problem with shortening the time steps. The loop went away entirely, and then they stretched it out and got big loops.

COX:

Lauterborn and a number of people are working on that, but what I called for in my speech was more study of a mass loss evolutionary sequence.

BAKER:

At the low luminosities that he's talking about with these double-mode models, there really is no observation of that mass loss.

COX:

Do you feel, Norm or David, that those double-mode Cepheids have gone through a red giant phase, or do you think they are just off the main sequence?

KING:

I wouldn't have any idea. I mean, I have talked to people who do evolutionary calculations like Bob Rood, and he claims that it's just so difficult to do evolutionary calculations for those masses. It takes so much computing time, so it really hasn't been done. It's pretty hard to say. I think Petersen has suggested several possibilities, but they are such a range of possibilities that I don't think we can say too much about them.

BAKER:

There's a lot of difference between mass 1 and mass 2.

KING:

Right. I think that whole range is difficult to do, calculationwise. I was talking about two separate problems here. When I was talking about the mass problem, I wasn't really talking about the double-mode Cepheids, I was just talking about the general problem where we have things like 60 percent discrepancy with mass rather than factors of more than 2. We saw here that there was–if you just take the observation at face value, you can get factors of 2 for this value α for the higher luminosity options. I think it's a little bit strange that you would; a simple error in the distance scale wouldn't seem to give you that kind of thing, because you found this relation holds also in places like the Magellanic Cloud, where the distance shouldn't be a problem.

BAKER:

Changing the distance scale is a radical thing to do. It would make a lot of people unhappy.

COX:

I was just discussing this problem with Icko Iben just last week. He claims for the Magellanic clouds stars, which he and Tuggle have worked up, there is no problem anymore. The mass luminosity relation from evolutionary theory agrees with pulsation mass luminosity relation. That's a bit surprising. Of course, the problem with galactic Cepheids is still the same. He'd like to increase the distance to the Hyades and make them more luminous. He feels that's the best choice.

CHRISTY:

Are there other questions?

COX:

I'd like to make one more remark. Could you sometime show those of us who are really interested where the Kraft relation is on that slide [King, Figure 2]? After all, that's the standard workhorse relation.

KING:

I guess I don't have it with me. Someone must have it around.

PARSONS:

It's in between Schmidt and Böhm-Vitense, but much closer to Schmidt.

KARP:

Its slope is 0.174.

COX:

Can you fix it up that way?

PARSONS:

The main argument for a slope similar to the Kraft relationship is the work which Oke originally did and which I redid in matching the radius variations of Cepheids, matching model atmospheres to the observations over a large wavelength region. You get a radius variation matching the models to the observations and then a radius variation from the radial velocity curve. In order to get these two to agree you have to have a certain slope to the temperature scale. What I got was essentially what Oke got, which was the Kraft slope. That's the major inconsistency, if you change the slope. There are some uncertainties in that, due to blanketing, etc.

KING:

How about the shift itself? Can you justify just a shift?

PARSONS:

Not of 300 degrees. Then the models which I have some faith in for α Persei would be completely off and not in agreement with α Persei.

HYDRODYNAMIC EFFECTS IN THE ATMOSPHERES OF VARIABLE STARS

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ABSTRACT

Numerical models of variable stars are established, using a nonlinear radiative transfer coupled hydrodynamics code. The variable Eddington method of radiative transfer is used. Comparisons are for models of W Virginis, β Doradus, and η Aquilae. From these models it appears that shocks are formed in the atmospheres of classical Cepheids as well as W Virginis stars. In classical Cepheids, with periods from 7 to 10 days, the bumps occurring in the light and velocity curves appear as the result of a compression wave that reflects from the star's center in agreement with Christy (1964). At the head of the outward-going compression wave, shocks form in the atmosphere. Comparisons between the hydrodynamic motions in W Virginis and classical Cepheids are made. The strong shocks in W Virginis do not penetrate into the interior as do the compression waves formed in classical Cepheids. The shocks formed in W Virginis stars cause emission lines, while in classical Cepheids the shocks are weaker and their existence may possibly be evidenced in the recent ultraviolet data obtained from the OAO-2 satellite.

INTRODUCTION

The purpose of this study is to apply recent theoretical improvements in nonlinear pulsation theory to a general study of the hydrodynamic effects in the atmospheres of variable stars. The classes of stars included in this study are W Virginis and classical Cepheids. The mass discrepancy of classical Cepheids, which has been discussed extensively in the recent literature, is of principal interest here while the question of the proper mass for W Virginis stars appears to be resolved. Shocks which appear in the atmospheres of RR Lyrae and W Virginis stars may also appear in classical Cepheids. The reflected compression wave, responsible for the observed bump, may become a shock which then may cause excess ultraviolet radiation. A correlation of evidence for shocks with the observed bumps in the light curves of the "bump" Cepheids may help resolve the mass discrepancy. It was apparent in the study on W Virginis that the full radiative transfer approximation was needed. For classical Cepheids, a shock, if it exists in the atmosphere, is much weaker than it is in RR Lyrae and W Virginis stars, and the usual evidence such as hydrogen line bifurcation does not exist. The full radiative transfer treatment is apparently not needed to explain the light and velocity curves of "bump" Cepheids, but it may be necessary to explain the bump in some recent ultraviolet data and is probably needed, in a nonequilibrium treatment of lines, to explain the H & K and "satellite" lines observed.

NUMERICAL MODELING

The methods of nonlinear pulsation theory, originally described by Christy (1964), Cox et al. (1966), and Stobie (1969), have been modified to include the variable Eddington radiative transfer approximation (Davis, 1972) and velocity-dependent terms (Davis, 1974). This code has been augmented with recently improved opacity tables, Kippenhahn 1A (Kip 1A), which use the new iron abundances (Cox*). The selection of the model for W Virginis is described in Davis (1972), while the development of models for classical Cepheids will be described in the following section of this paper. The results and comparisons to observations will be made in the section on development of models, model calculations, and comparison with observational data and some general conclusions made in the final section.

From our previous studies (Bendt and Davis, 1971), we had determined that a model using 70 zones was reasonable in terms of the time spent doing the calculations and the resolution of the velocity and light profiles. The models contain approximately 1 percent of the star's total mass, having an inner radius near one-fifth the photospheric radius. For our present study, the inner radius is established at a location that ensures a reasonable number of thin zones in the outer atmosphere for a constant mass zoning.

The basic improvement in the physics of our models, over those of Christy, Cox et al., and Stobie, is in the use of the variable Eddington radiative transfer approximation instead of the diffusion approximation. The hydrodynamic theory used is basically the same. The Eddington approximation allows for the forward peaking, or streaming, of the radiation in optically thin and, in the spherical case, extended atmospheres. The Eddington factor (f=K/J) is obtained using a characteristic ray solution in plane geometry. The radiation field is treated separately from the material energy and due account can therefore be taken of the acceleration due to radiation pressure.

Numerical modeling is very sensitive to zoning and to the time step used, and, as we had determined in our previous studies, the spreading of the ionization region over at least two zones and the location of the region at least eight zones in from the boundary ensured relatively smooth light and velocity curves. Initially, we found that a time step of 300 s/cycle would cause the ionization region to move smoothly over five to six zones during a period. The model is initialized with a fundamental mode velocity field having about 10 percent of the equivalent kinetic energy of the limiting amplitude field. This method of initialization is similar to that of Christy (1964). Models have been allowed to grow from computer noise, and, as in Baker-Kippenhahn 7 (BK-7), the model may switch from the first harmonic to the fundamental at large amplitude (King and Cox, 1968). In our model of BK-7 we find the star still in the first harmonic after 200 periods. The time switching may be zone dependent. All of the models described here, though, have been initialized in the fundamental mode.

*Cox, A. N., 1973, private communication.

The boundary conditions applied to our models are similar to those used by Cox et al. (1966). A fixed inner boundary with a constant flux and zero velocity and a zero pressure outer boundary condition are imposed. The variable Eddington factor at the center is always one-third, since we ensure a diffusion condition there, while at the outer surface an appropriate extrapolation of the characteristic ray *f* factor is used. The material accelerations, due to radiation pressure in the zones, are calculated from

$$dP_R/dr = - \int_0^\infty F_\nu/\lambda_\nu \, d\nu \qquad (1)$$

where λ_ν is the mean free path for frequency ν in the zone and F_ν is the flux across the zone boundary.

SELECTION OF MODELS

The model of W Virginis, as selected by Christy, is based on a mass of 0.88 $M_\odot$ and a luminosity and T_{eff} from observations adjusted to give a reasonable period (20 days). The model luminosity is: $L = 1.795 \times 10^3$ $L_\odot$ and $T_{eff} = 5500$ K. The effect of the radiative transfer approximation used by Davis (1972) was an improvement in the light profile showing the definition of the shoulder after light maximum. A shock was shown to develop in the atmosphere during the phase when line emission and doubling occurs (0.65P-0.10P). The hydrodynamic motion in the atmosphere of W Virginis will be compared to that in a model of β Doradus in the following section.

In selecting models of classical Cepheids we decided to look at stars at the limits of the "bump" Cepheids from 7 to 10 days. The particular stars that we will be comparing to are η Aquilae (7.18 days) and β Doradus (9.8 days). For η Aquilae we will use a model initially studied by King et al. (1973) called 4A. The location of this model, on the H-R diagram, is shown in Figure 1. The empirical strip shown is from Sandage and Tammann (1968) and the transformation of color by Kraft (1961). This model will be compared to observations.

In our study of β Doradus we initially tried a model using the evolutionary mass (6.8 $M_\odot$) and the observed luminosity as determined from the P-L relationship by Christy (1966) ($L = 3.7 \times 10^3$ $L_\odot$). To obtain the observed period, for the fundamental, requires a $T_{eff} = 5300$ K which puts the model outside of the empirical instability strip (Figure 1). Next, we tried Christy's (1968) proposed mass (3.4 $M_\odot$) and T_{eff} (6100 K) and found as did Cox et al. (1972) that the model was stable in both the fundamental and first harmonic. As suggested by Christy,* this difference is due to his incorrect definition of the photospheric radius in these early models. For reasons that will become apparent, we found that a mass of 4 $M_\odot$ gave a better agreement with the observations and decided to select

*Christy, R. F., 1972, private communication.

three effective temperatures to surround the period observed. The effective temperatures and resulting periods were: 6000 K (9.22 days), 5900 K (9.8 days), and 5800 K (10.57 days). The effective temperature of 5900 K agrees quite well with the determination of effective temperature of 5870 K determined from line profiles (Rodgers (1970)). The locations of these three models relative to the instability strip are shown in Figure 1.

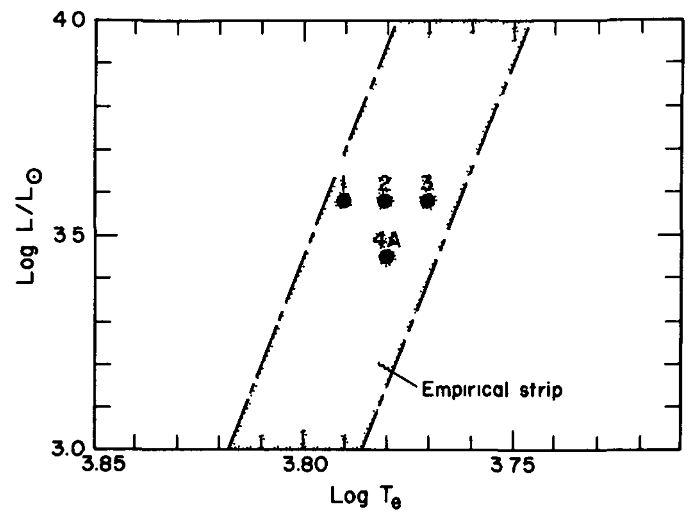

Figure 1. Hertzsprung-Russell diagram with the empirically derived instability strip (Sandage and Tammann, 1968). The locations of the three β Doradus models and the η Aquilae model (4A) are shown.

DEVELOPMENT OF THE FULL AMPLITUDE MODELS

As indicated, the models are initialized in a fundamental mode by the application of a velocity field. In this study we found that a pseudoviscosity method proposed by R. Stellingwerf* greatly accelerated the models growth to limiting amplitude. The method relies on continuing the application of pseudoviscosity into certain zones that are expanding, as well as all those that are contracting, which is the usual Richtmyer-Von Neuman application. From experimentation we found that the following formula for choosing the expanding zones,

$$q = \begin{cases} \dfrac{(\ell \rho_0)^2 (\partial v/\partial t)^2}{V} \\ \text{or } 0 \text{ if } (\partial v/\partial t) < -b\, C_0 \end{cases} \tag{2}$$

*Stellingwerf, R. F., 1974, private communication.

(where $\ell \approx a\Delta R\,(r/R)^2$ and $C_0 = \sqrt{\gamma P/\rho}$ = speed of sound in the zone), greatly reduced the noise in the grid and allowed the time step to be increased by orders of magnitude. The value of b = 1/100 was used in all of the present calculations with an increase in the time step by a factor of 10.

The models are started in the nonequilibrium diffusion gray approximation. A typical diffusion model will take about 1 hour of 7600 CDC computer time to go approximately 200 periods. After the model has reached limiting amplitude, we switch to the full multigroup variable Eddington approximation. Using 13 frequency groups, the transfer model takes another hour of 7600 CDC computer time to relax to limiting amplitude, going another four to six periods. During the last three periods, records of temperature, density, and radius are stored on tapes for future snapshot analysis.

THE RESULTS OF THE MODEL CALCULATIONS

The results will be described here and the comparisons to observations made in the next section.

During the course of the full nonlinear calculations, plots of velocity and luminosity at the surface are made. The surface velocities for the three β Doradus models are shown in Figure 2. A bump is clearly seen on the downward side of the $10^d.57$ model that appears to reach and cross the peak as the period decreases to $9^d.22$. Evidence that the bump reaches the surface at the maximum light for the β Doradus model (5900 K) is seen in the luminosity plots (Figure 3). The luminosity shows a maximum increase, at the peak, for the $9^d.8$ model.

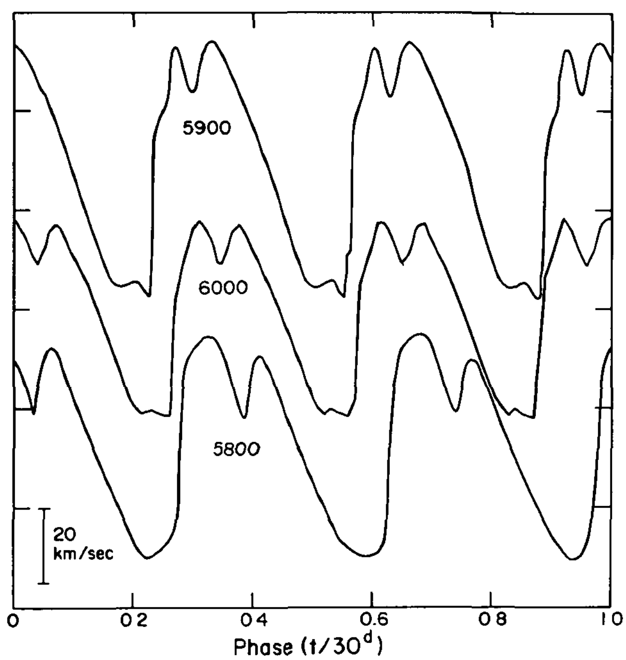

Figure 2. Velocity versus phase for the three β Doradus models.

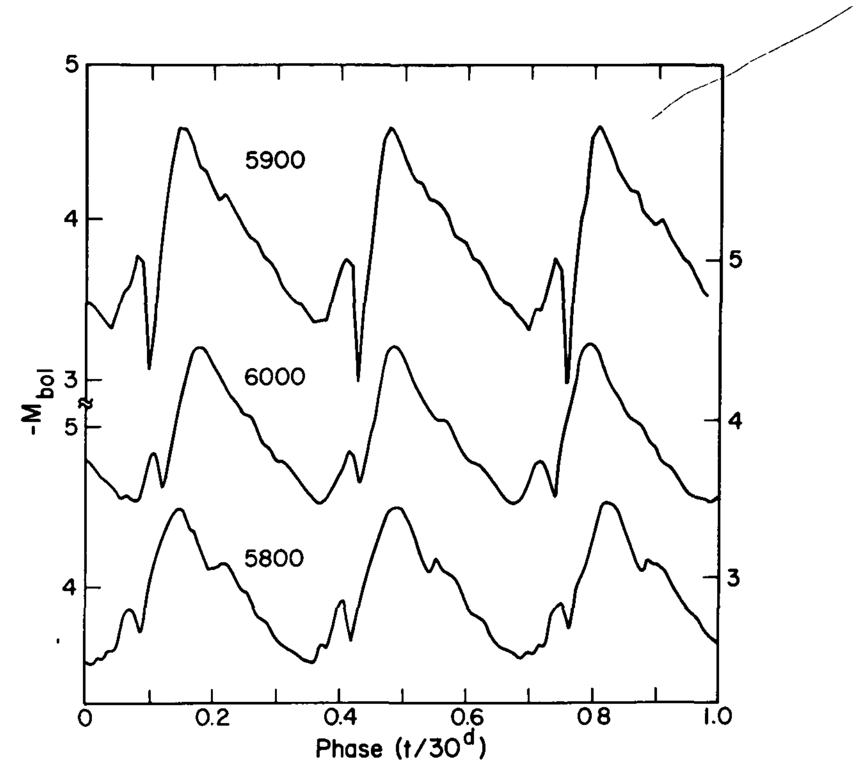

Figure 3. M_{bol} versus phase for the three β Doradus models.

To study the effect of the hydrodynamic motion in the stellar atmospheres, various plots of the velocity fields were made. A Christy-like plot, with ratios of velocity at zone boundaries over an average velocity, will show the compression wave reflecting from the star's core. A similar plot for W Virginis will show a null in velocity just inside the ionization region. To observe these motions more clearly we have utilized two-dimensional plots of velocity and radius at various perspectives as shown in Figures 4 and 5. The W Virginis model shows a larger dynamic motion in the atmosphere, due to a much stronger shock that develops, than in the classical Cepheid model. In Figure 5 we see that the continuation of the luminosity after maximum light produces a corresponding shoulder in the velocity profiles. Also seen in Figure 5 is the bump for the $10^{d}_{.}57$ Cepheid approaching the surface along the downward side of the velocity curve.

From the records of temperature, density, and radius we establish static atmosphere snapshots utilizing the following formulas:

$$\mu_{\ell}\, dI_{\nu}/\rho dx = K_{\nu}\,(I_{\nu} - B_{\nu}) \tag{3}$$

and

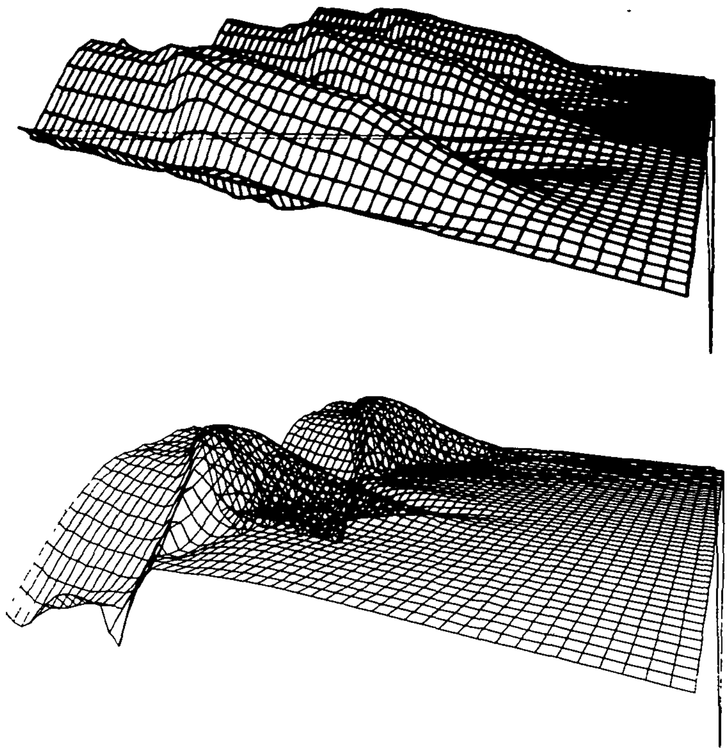

Figure 4. A two-dimensional plot of the velocity field for W Virginis (lower plot) compared to that for a classical Cepheid (10.57 days).

$$F_\nu = 2\pi \sum A_\ell \mu_\ell I_{\nu,\ell} \tag{4}$$

where the A_ℓ's are Gaussian weights and K_ν, the absorption coefficient. The flux (F_ν), emitted from the stellar surface, is calculated in a wavelength interval from 3000 to 10 000 Å. Normally, the results are presented in terms of the UBV colors, although the spectrum can be displayed if desired. Spikes appearing in the luminosity (Figure 3) and color results, during the phase just before light maximum, are the result of improper zoning. During this phase, the shock is forming at the surface of the ionization region and high compressions result. This perturbation, caused by the poor zoning, also reflects itself in the velocity profiles (Figure 2). To resolve the spectrum in the ultraviolet therefore, during this phase, it appears that it is necessary to use fine zones in the ionization region. A method where the static atmosphere is rezoned could be used, or preferably a time-dependent continuous rezone method. We are working on the improvement in zoning to resolve the continuum light curves, particularly in the ultraviolet portion of the spectrum.

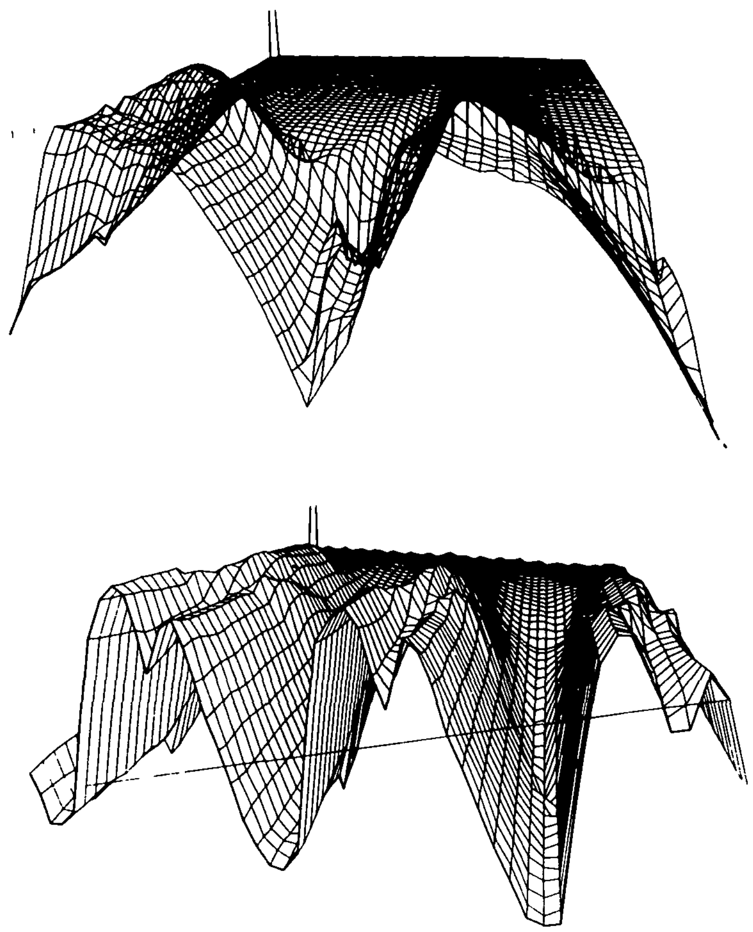

Figure 5. A front view of the velocity field for W Virginis (upper) and for a $10\overset{d}{.}57$ classical Cepheid (lower). The shoulder after light minimum is reflected in this velocity profile for W Virginis.

COMPARISON WITH OBSERVATIONS

The evidence for shock waves in the atmospheres of W Virginis stars is quite convincing. Emission lines of hydrogen appear just after light minimum and double absorption lines arise from this time to times just after light maximum. The phase of emission can be correlated with a shock that traverses the atmosphere of a model of W Virginis during this time. For classical Cepheids, the shock is apparently not strong enough to cause emission in hydrogen, though bumps do appear in the light profiles similar to the shoulder observed after light maximum in W Virginis. At present, we cannot resolve the shock structure in the atmospheres of classical Cepheids. A comparison of the density structure at times near light minimum, for β Doradus and W Virginis, is shown in Figure 6. From these models it is quite evident that shocks do exist in classical Cepheids.

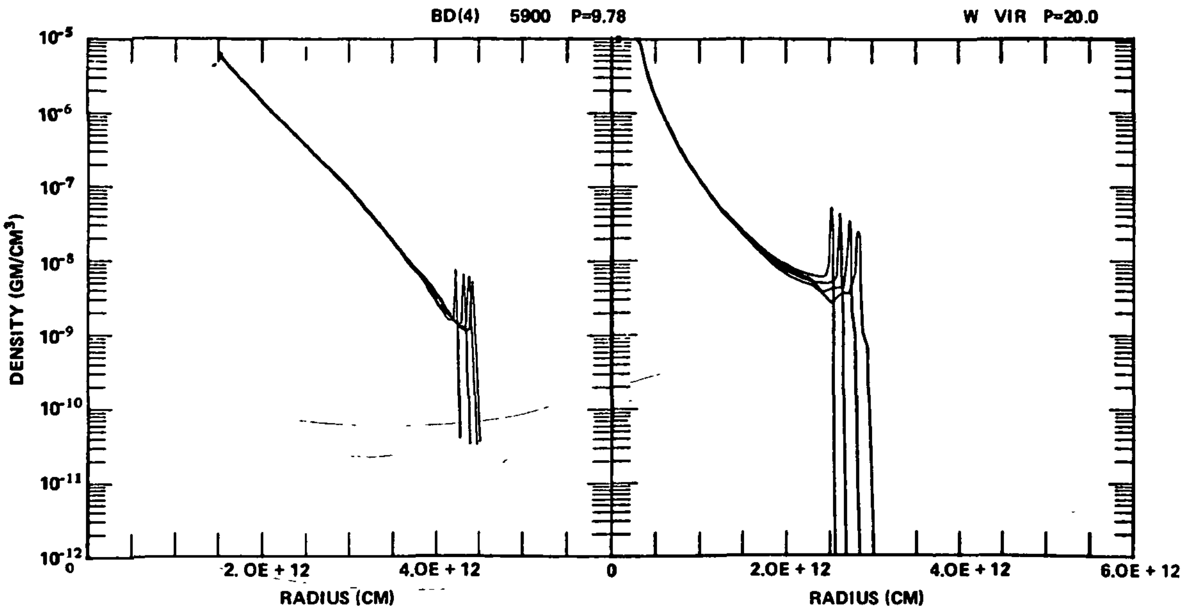

Figure 6. A plot of densities near light minimum for β Doradus and W Virginis.

Using the proposed luminosity for β Doradus ($L = 3.7 \times 10^3\ L_\odot$), we find that a 4 $M_\odot$ model will produce a bump in coincidence with peak light for an effective temperature of 5900 K. This may not be the correct interpretation of the phase of the bump, but a few percent change in T_{eff} will move the bump across the peak. Using the four solar mass problem (4A), which has a period of 7.32 days, we find that the bump in luminosity occurs at the same phase as observed in η Aquilae (Figure 7). Apparently a fit to the bump Cepheids can be obtained with lower than evolutionary masses, and for β Doradus it is necessary to use a mass less than the evolutionary mass.

The present attempt to resolve the luminosity in the ultraviolet part of the spectrum, by comparison with the OAO-2 data, has proved inconclusive. The ultraviolet data indicate that a bump occurs at a phase before light maximum that increases in size with decreasing wavelength. As we can see in Figure 8, the bump appears to be resolved in the 2980-Å band. Our calculations indicate fairly good agreement with the amplitude and location of the shoulder for the 4200-Å band (Figure 9). Unfortunately, the unresolved region, before the light maximum, is the region where the shock first forms in the atmosphere. For the lower wavelength bands the agreement gets worse in both amplitude and shape, but the observed increase in amplitude with decreasing wavelength is observed.

CONCLUSIONS

In the comparison of models of classical Cepheids and W Virginis stars we find some interesting similarities and differences. The mechanism sustaining pulsation is apparently the same—the Eddington "heat valve." The normal mode of pulsation would appear to be the fundamental, certainly for the W Virginis stars. We find that, for the classical Cepheids

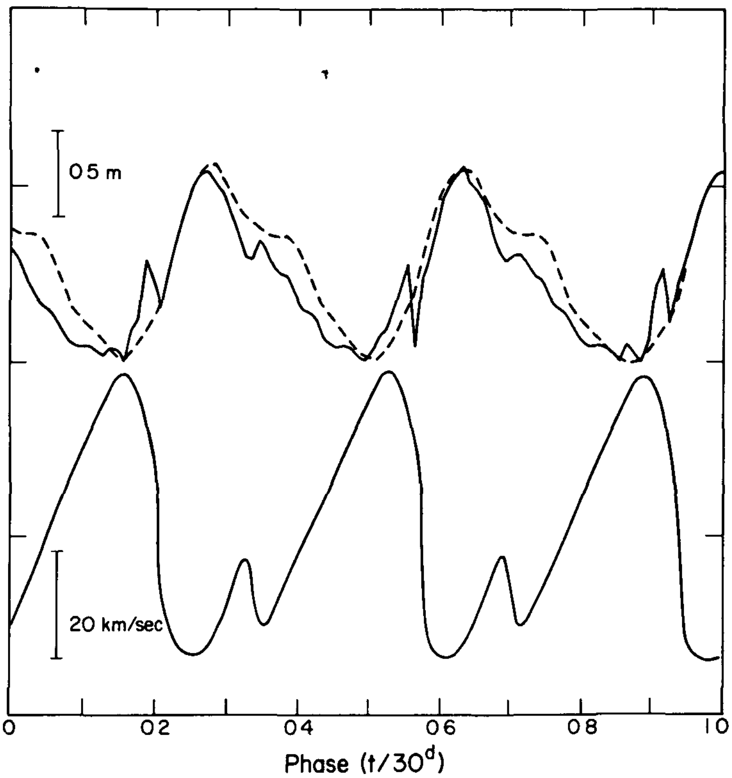

Figure 7. M_{bol} and velocity versus phase for model (4A). The observed light curve (dashed) is for η Aquilae.

in the 7 to 10 period range, a fundamental mode will fit the observations where less than evolutionary masses must be used. A reasonable mass to explain the correct phase for a model of β Doradus ($9^d_.8$) and η Aquilae ($7^d_.2$) is 4 $M_\odot$ about 0.6 of the evolutionary mass.

The dynamics in the atmospheres of classical Cepheids is considerably different from those of W Virginis stars. Assuming a zero pressure boundary condition and not including convection, we find that the low mass W Virginis stars develop strong shocks in their atmospheres. These shocks are evidenced in the hydrogen emission features starting near phase 0.65. For classical Cepheids the evidence for the calculated shocks is inconclusive. There is some evidence in satellite lines, but the principal evidence may be included in the recent ultraviolet data obtained from the OAO-2 satellite. So far, the zoning in the numerical calculations is insufficient to resolve the structure in the ultraviolet portion of the frequency spectrum.

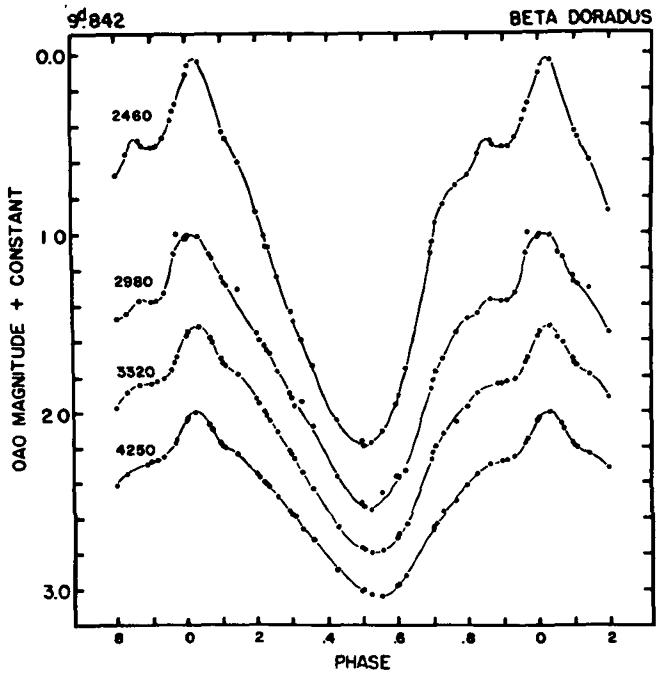

Figure 8. A plot of OAO-2 ultraviolet data obtained on β Doradus (plot provided by J. Hutchinson, Wisconsin).

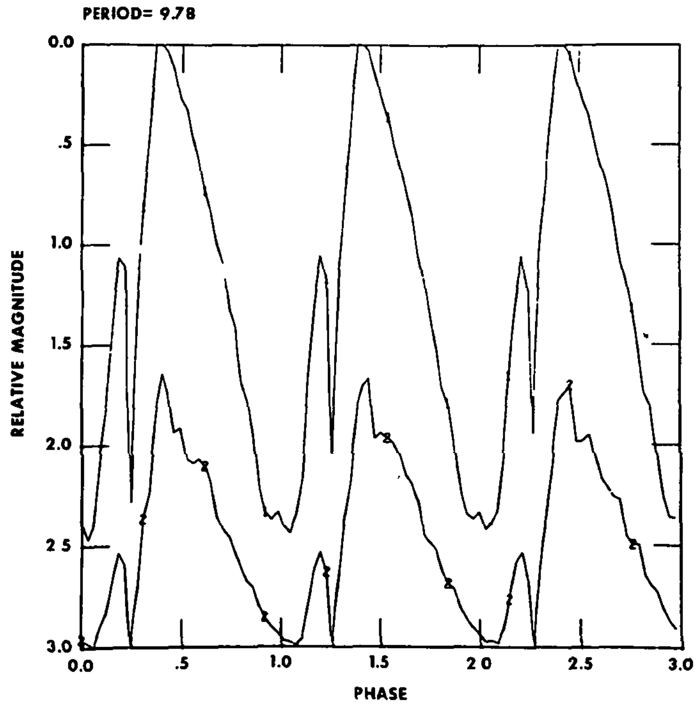

Figure 9. A plot of the calculated luminosity for the 5900 K β Doradus model for wavelengths centered at 4203 Å (lower curve) and 2592 Å (upper curve).

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DISCUSSION

HILL:

I would like to make a couple comments. I don't want to be known as the shock freak, but I should point out that your atmosphere stops at a level before the level of my second and third shock formation. So, if I stop my atmosphere where you stop yours, then I would agree with you that there would be only one shock.

I think if you extend your atmosphere out about six more scale lengths–here's one of the slides [Hill, Figure 1] I showed that indicates the depth of the shocks. Your atmosphere is stopping right underneath where I formed shocks two, three, and four. So I'm not too surprised that we would, at this point, disagree on the number of shocks.

DAVIS:

Yes. We don't generate more shocks.

SPANGENBERG:

What kind of ΔUs were you getting?

HILL:

The ΔU was largest for shock number 1. If you throw away everything less than a ΔU of 30 km/s, you wouldn't even count these other shocks. My subsequent shocks are by far weaker, but on the other hand, when you actually look at the energy loss, it does appear on the energy loss diagram, because the subsequent shocks are running into an atmosphere of increased density as a result of the first shock. So, therefore, in the energy loss formula ($\rho/\Delta UQ$), the density is much larger. So even though the ΔU is small with these, that's why they are still visible, I think.

COX:

Are they really visible? You predict the shocks will be visible?

DAVIS:

I predicted when they are visible. Jim [Hutchinson] shows from OAO-2 that they are visible then, for the ones that I predict.

COX:

I only saw two of them.

HILL:

There would be three of them that I predict would be visible.

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THE HERTZSPRUNG PROGRESSION IN CEPHEID CALCULATIONS

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ABSTRACT

The Hertzsprung bump in Cepheid models has been studied for models of a wide range of mass (M), radius (R), and effective temperature (T_e). This study showed that the delay of the bump (in days) measured from the time of minimum radius preceding the bump by one+period is dependent only on M and R and not on T_e. Over a range of models from $M/M_\odot \approx 0.5$ and $R/R_\odot = 10$ to $M/M_\odot = 13$ and $R/R_\odot = 160$, the delay of the bump (in days) is approximately proportional to the radius. This is consistent with a picture that the bump results from an acoustic signal that traverses the star from the ionization zone into the center and out again to the surface. The bump and other nonlinear phenomena has been studied over a series of Cepheid models from a period of less than one day to a period of 150 days. The results show characteristic changes in amplitude and in the appearance of bumps and are in good correspondence to actual Cepheids. The masses in this series are, however, characteristically lower than evolutionary masses.

THE SYSTEMATICS OF CEPHEID-TYPE VARIABLES

The Q values of Cepheid-type variables depend primarily on M and R and are, to a good approximation, a function of the parameter

$$X = \frac{(R/R_\odot)^{1.18}}{(M/M_\odot)}$$

Another feature of pulsation, the second bump, is determined by the value of $P/(R/R_\odot)$ where P is the period in days. These two quantities are very closely related however, since

$$\frac{P}{R/R_\odot} = Q\,\frac{(R/R_\odot)^{0.5}}{(M/M_\odot)^{0.5}}$$

which is very nearly a function of X. We may thus conclude that the value of Q and the appearance of a second bump are correlated by essentially the same parameter which may be represented either as X or as $P/(R/R_\odot)$. We should then see if all other characteristic features of pulsation can be similarly correlated with these parameters. These parameters can also be correlated in the period-luminosity diagram if we ignore variations across the instability strip by using a relation (Christy, unpublished) between T_e and period which describes well the trend of the blue boundary of instability for a wide range of models. It is:

$$T_e \propto P^{-0.06}$$

Then we have $L \propto R^2 T_e^4$ so $R \propto L^{1/2}/T_e^2$ and the relation $P \propto R/R_\odot$ becomes $P \propto L^{1/2}/T_e^2$ and with the dependence of T_e on period above, this gives $P \propto L^{0.57}$ or $\Delta M_{bol} = -4.4\ \Delta \log P$. This implies that we can correlate certain characteristic features of pulsation with lines in the period-luminosity diagram as shown in Figure 1, provided we ignore variations across the instability strip. In particular, the transition from fundamental to first overtone is found to follow such a correlation.

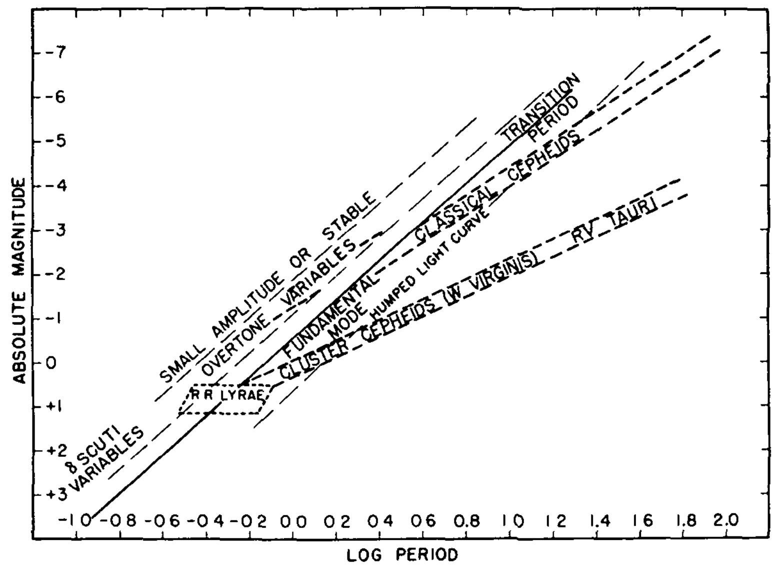

Figure 1. The period luminosity diagram.

Table 1 lists the features of pulsation in the Cepheid strip which appear to correlate with the above parameters. It is important to emphasize that the exploration which led to these correlations was carried out over a very wide range of luminosities and periods, and the correlations are intended to describe this macrostructure rather than the microstructure that may be found in a very limited exploration. Thus, the blue boundary of instability has been explored from $M_b = +3.0$ to $M_b = -6.0$. Overtones have been explored at $M_b = +3$, +0.75, -2, -4, and -6. The second overtone was explored only at $M_b = -2.8$. The bump associated with the Hertzsprung progression in Cepheids was studied at $M_b = -0.6$, -2.0, -4.0, and -6.1. The Cepheid strip (with luminosity depending on mass) was studied from $M_b = +0.125$ to $M_b = -7.4$. Irregular, very large amplitude pulsation which may be associated with RV Tauri behavior was studied at $M_b = -4.0$, -6.0, and -7.0.

Table 1
Pulsations in Cepheids

Feature	$X = \frac{(R/R_\odot)^{1.18}}{M/M_\odot}$	$\frac{P^{(days)}}{R/R_\odot}$	$\frac{P^{(days)}}{(L/L_\odot)^{0.57}}$
Transition second to first overtone	8	0.060	0.040
Transition first overtone to fundamental	10.5	0.100	0.0625
Maximum amplitude and asymmetry	12	0.115	0.08
Bump first appears in velocity curve	25	0.155	0.10
Bump is most prominent, amplitude is minimum	35	0.185	0.13
Maximum amplitude, bump disappears	50	0.235	0.17
Minimum amplitude	80	0.335	0.24
Pulsation irregular and of very large amplitude (RV Tauri type)	115	0.45	0.33
Pulsation tends to blow off envelope	200 ?	0.70 ?	0.45 ?

The existence of the transition from first overtone to fundamental has been discussed in a previous publication. More recently, von Sengbusch and also Stellingwerf have carried out some much more careful calculations of this transition in the RR Lyrae region. It would be interesting to see if their methods also found similar transition behavior for more massive and more luminous models as I report here.

Among the RR Lyrae variables, it is well known that the maximum amplitude and asymmetry occur among the shortest period fundamental mode pulsators, and the amplitude decreases as does the asymmetry for the longer period variables toward the red side of the strip. A similar series of changes in amplitude and asymmetry was found among Cepheid models for similar ranges of X and of $P/(R/R_\odot)$. A maximum amplitude and asymmetry were found for Cepheids of periods near 1.5 days with decreasing amplitude and asymmetry for longer periods to about 10 days.

These same characteristics are known for variables in the small Magellanic Cloud, and it is proposed here that this is a systematic property associated with the parameter X or $P/(R/R_{\odot})$.

Bumps in the falling half of the light curve and corresponding bumps in the velocity curve first become noticeable in the calculations for $X \approx 25$ and $P/(R/R_{\odot}) \approx 0.155$. For increasing X and P the bump becomes more prominent and occurs at an earlier phase until the light curve has two almost equal maxima and the amplitude reaches a minimum. Thereafter, for increasing X and P the bump appears to merge with the principal maximum of the light or velocity curve, and the pulsation amplitude reaches a maximum for Cepheids with periods near 25 days.

For larger values of X and P, the amplitude decreases again to a minimum for Cepheids with periods of around 80 days.

For still larger X and P, the models exhibit a new type of behavior. The density of the envelope is very low so that the kinetic energy associated with pulsation is very small, compared with the luminous flux emitted during one period. As a consequence, for unstable models, the amplitude of pulsation grows very rapidly, and, where $\Delta R/R$ exceeds ≈ 0.40, the amplitude quickly grows to $\Delta R/R \approx 1$, at which point the outer parts of the star fall back in free fall during a time longer than the period and meet the inner part during its phase of outward motion. This "breaking" of the pulsation wave greatly reduces the amplitude until a new cycle of growth and breaking sets in. In this way the amplitude grows for a few periods and then suddenly drops and grows again in a kind of relaxation oscillation. This peculiar behavior is found for models with $X > 115$ and $P/R > 0.45$. For Cepheids it occurs for periods greater than about 150 days whereas for mass $\approx 0.6\ M_{\odot}$ it is found for periods greater than about 30 days. It is suggestive of the irregularities in the pulsation of RV Tauri variables.

For $X > 150$ or $P/(R/R_{\odot}) > 0.60$, models tended to blow off the entire envelope, and it was difficult to establish models with deep envelopes and an insensitive inner boundary.

THE HERTZSPRUNG PROGRESSION

For Cepheids of 7- to 10-day periods, secondary bumps appear in the light and velocity curves. Calculations of this phenomenon were first reported in 1967.* Since then, extensive calculations have been made to explore and understand the phenomenon further.

In order to be sure that the phase of the bumps was not affected by the inner boundary, several models were calculated with the inner boundary ranging from 25 percent of the radius to less than 10 percent of the outer radius. There was no significant change in the phase of the second bump.

*Christy, R. F., 1968, *Quarterly J. of Royal Astron. Soc.*, 9, p. 13.

Four models with the same mass and radius, but with T_e varying from 5600 K to 6050 K, were calculated in order to see if the phase of the bump depended on T_e in addition to its dependence on M and R. No such dependence was found. The simplest parameter characterizing the phase of the bump was the period divided by the radius. As shown in Table 1, the bump first appears in the velocity curve at a delay of about 1.6 periods for $P^d/(R/R_\odot) = 0.155$. It merges with the next peak in velocity with a delay of about 1.0 period for $P/(R/R_\odot) = 0.235$. In these measures, the fiducial time is when V = 0, midway up the main rise in velocity, and the bump phase is measured midway up the steep rise in velocity. Then the time includes the time to traverse to the center of the star and out again, the delay of the bump is from 1.0 to about 1.6 periods, and the ratio of delay in days to $R/R_\odot$ is found to be remarkably constant in all cases where the bump is clearly visible and the value of this parameter was ≈0.24.

The dependence on mass and luminosity of the bump phenomenon was explored by means of a short period set with masses about 0.5 $M_\odot$ and periods from 1.5 to 2.0 days. The ratio of delay in days to $R/R_\odot$ was approximately equal to 0.26. A long period set was then tried with masses of 8 to 13 $M_\odot$ and periods of 35 to 25 days. The ratio of delay in days to $R/R_\odot$ was about 0.24. It thus appears that this parameter can be used to characterize the time of appearance of the bumps quite accurately. This relation, if accurately calibrated, can thus be used to determine the radius of an observed star that shows a prominent bump, and then, from the calculated dependence of the period on mass and radius, the mass can be found.

After this basic exploration, a series of Cepheid models was studied with an assumed mass-luminosity relation $M_b = -7.5 \log M/M_\odot + 0.3125$. This relation was chosen in order to reproduce the bump phenomenon for 7- to 10-day Cepheids. The series extended from $M=M_\odot$ where first overtone modes were excited to $M=10\ M_\odot$ where RV Tauri behavior was found.

A series of figures shows the systematic changes in the character of the pulsation. These figures are of two types. Type 1 are computer drawn representations of four observable quantities: the top strip shows the variation of T_e over a series of pulsations (900 time steps and 4 to 6 periods). The second strip shows the luminosity variation, and the third strip, the velocity variation at the second zone which lies at an optical depth of about 0.2 in the static model. The fourth strip shows the radius variation. In these plots, the coarse zoning in the region of hydrogen ionization leads to quite (and spuriously) bumpy behavior in T_e and in L. The V and R curves are relatively smooth. Time increases on the right on the abscissa.

The second type shows the velocity of each zone (usually about 42) with a displaced zero and a scale adjusted to a definite size for each zone. This size is chosen for clear display even though it distorts the velocities of the inner zones which are relatively much smaller than shown.

Figures 2 and 3 show a model with M = 1.50 $M_\odot$, M_b = -1.000, and P = 1.60 days. This has a large amplitude and asymmetry (the maximum in this region with $\Delta R/R = 0.20$, ΔM_b = 1.6, and ΔV = 106 km/s (extremes)). The velocity curve for the inner zones shows high overtones, but no significant traveling waves are visible.

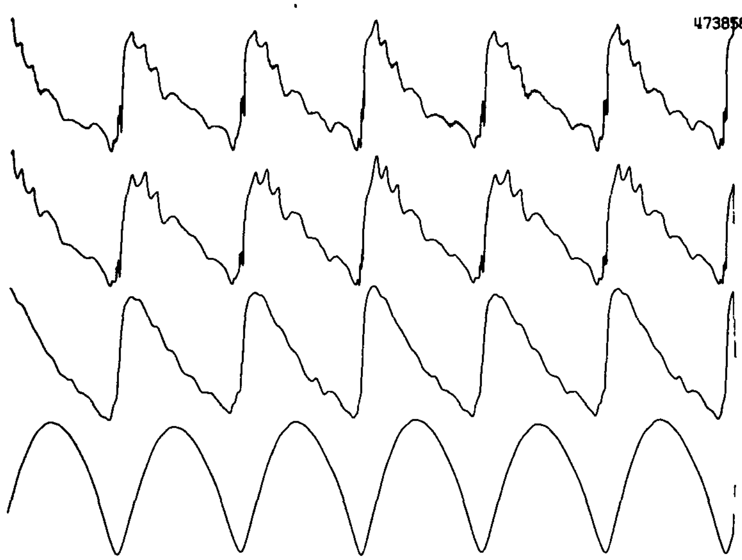

Figure 2. T_e, L, V, and R variations for M = 1.50 $M_\odot$, M_b = -1.000, and P = 1^d60.

Figures 4 and 5 show a model with M = 2.66 $M_\odot$, M_b = -2.875, and P = 6.03 days. The amplitude here is smaller with $\Delta R/R = 0.15$, ΔM_b = 0.8, and ΔV = 63 km/s. Here the traveling waves begin to appear in the interior, starting in the ionization zones at a time near minimum radius and progressing into the inner zone which responds in a characteristic way and sends the pulse out again where it nearly disappears in the ionization zones (where it travels very slowly) before appearing in the photospheric and reversing layers (the outer few zones) where it is clearly, though marginally, visible. Coincident with the outward acceleration phase of the velocity in this bump, there is a pronounced dip in the luminosity.

Figures 6 and 7 show a model with M = 2.99 $M_\odot$, M_b = -3.250, and P = 7.88 days. The amplitude is still a little smaller but the traveling wave phenomenon is very well developed (some numerical instability has appeared in the central zones but has no effect on the rest of the model). The delay of the bump in the velocity curve is 1.45 periods.

Figures 8 and 9 show a model with M = 3.35 $M_\odot$, M_b = -3.625, and P = 10.4 days. This model has the least amplitude in this region with $\Delta R/R = 0.15$, ΔM_b = 0.8, and ΔV = 46 km/s. The delay of the second bump in velocity is 1.34 periods. Because of the small light amplitude of these models with prominent bumps, the calculated light curves are of

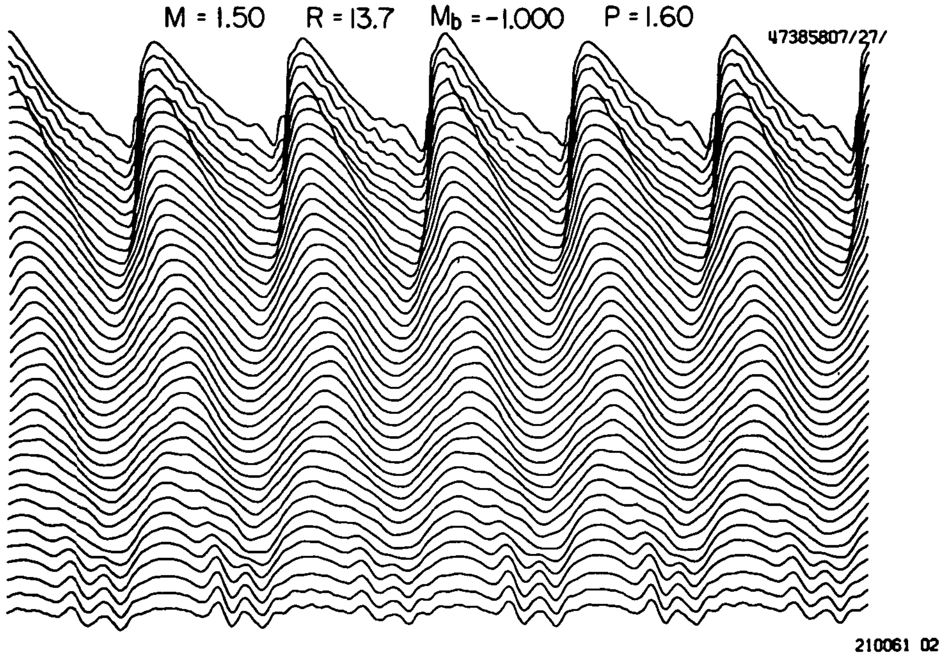

Figure 3. Internal velocity variations $M = 1.50\ M_\odot$, $M_b = -1.000$, and $P = 1\overset{d}{.}60$.

very poor quality. Others at this meeting are reporting calculations with much finer zoning in the ionization region which gives much clearer light curves. In each case, a dip in the light curve coincides with the peak outward acceleration associated with the bump traversing the photosphere.

Figures 10 and 11 show a model with $M = 4.73\ M_\odot$, $M_b = -4.750$, and $P = 24.2$ days. Here the bump has coalesced with the main velocity peak, and the pulsation motion reaches a maximum amplitude. This model shows $\Delta R/R = 0.33$, $\Delta M_b = 1.8$, and $\Delta V = 72$ km/s. For this model the amplitude of the inner zones is a relative maximum and reaches 0.15 km/s at R = 0.1R star.

Figures 12 and 13 show a model with $M = 7.50\ M_\odot$, $M_b = -6.250$, and $P = 76.7$ days. The amplitude here is a relative minimum both in the inner zones and for the surface phenomena where $\Delta R/R = 0.22$, $\Delta M_b = 0.66$, and $\Delta V = 54$ km/s.

Finally, Figures 14 and 15 show a model with $M = 9.45\ M_\odot$, $M_b = -7.000$, and $P = 148$ days. Here the model shows the increasing amplitude until at $\Delta R/R = 0.95$, $\Delta M_b = 2.3$, and $\Delta V = 105$ km/s the wave "breaks;" the amplitude drops sharply to a small value; and the growth starts again. The compression wave traveling to the center is very strong and reflects out again in less than one period. The amplitude of the inner zone at 0.1R star is 2 km/s.

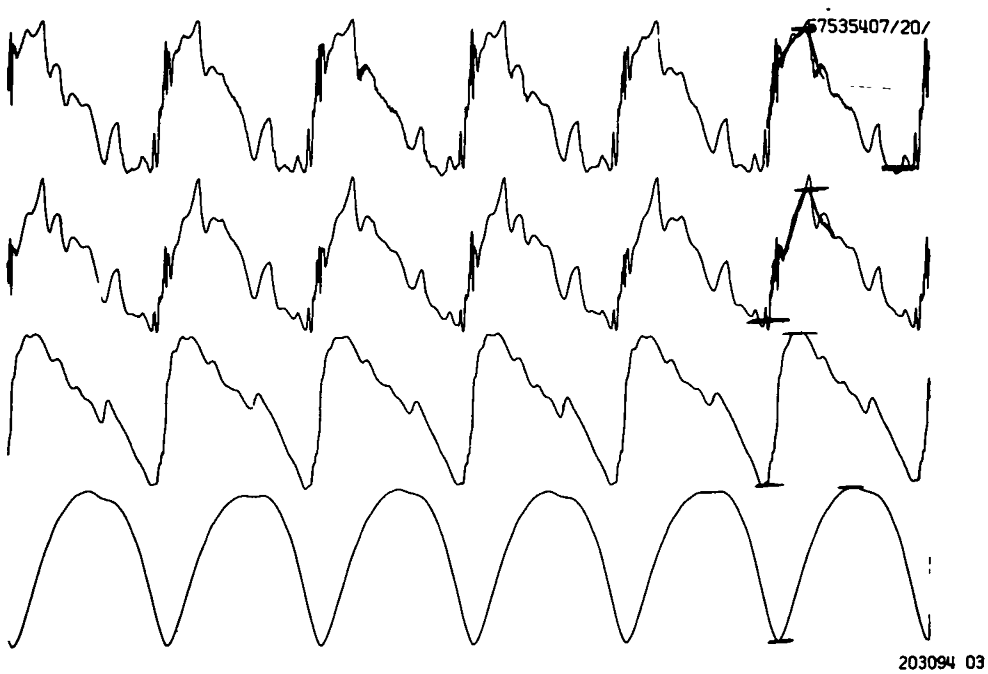

Figure 4. T_e, L, V, and R variations for M = 2.66 $M_\odot$, M_b = -2.875, and P = $6\overset{d}{.}03$.

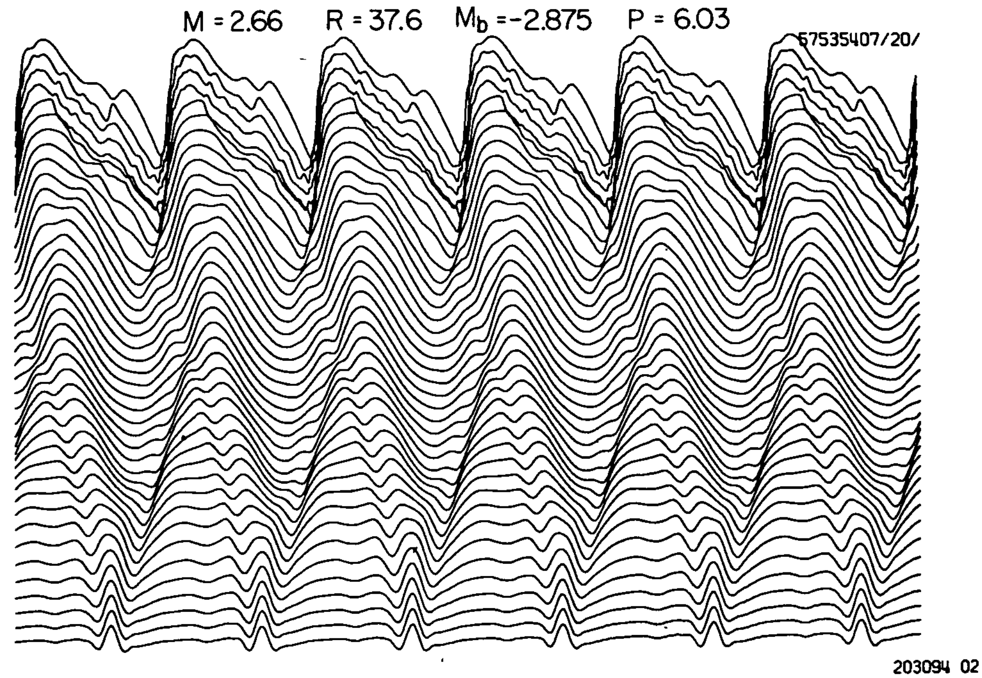

Figure 5. Internal velocity variations for M = 2.66 $M_\odot$, M_b = -2.875, and P = $6\overset{d}{.}03$.

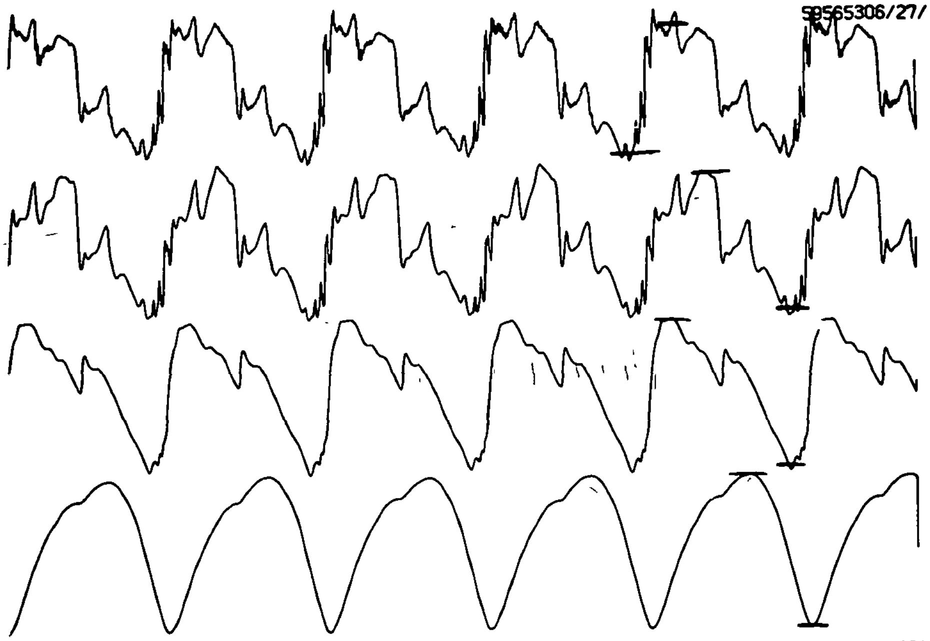

Figure 6. T_e, L, V, and R variations for M = 2.99 $M_\odot$, M_b = -3.250, and P = $7^d_.88$.

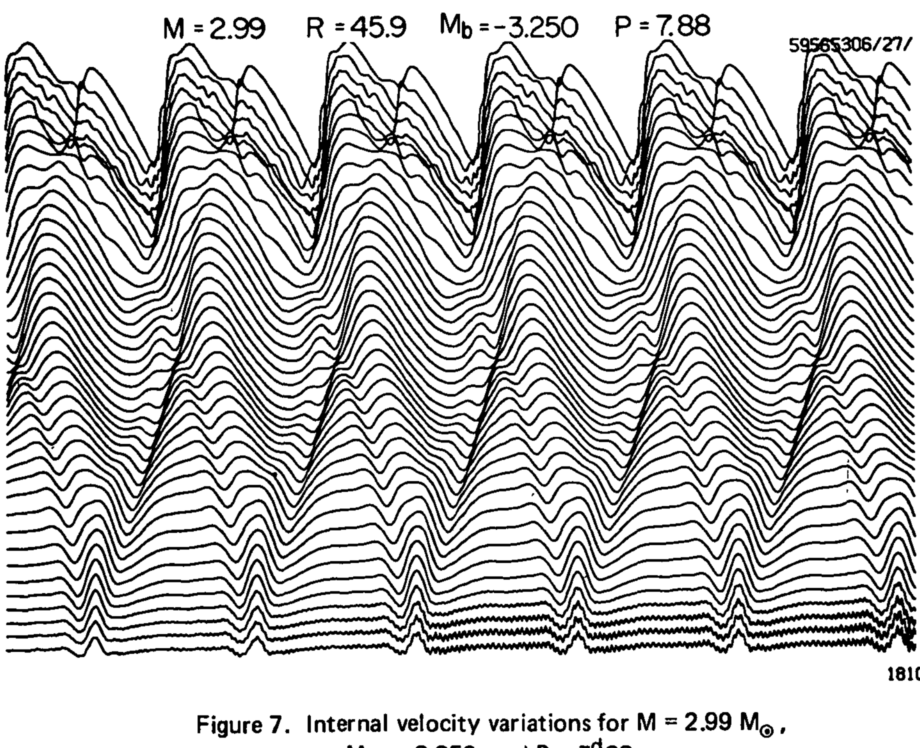

Figure 7. Internal velocity variations for M = 2.99 $M_\odot$, M_b = -3.250, and P = $7^d_.88$.

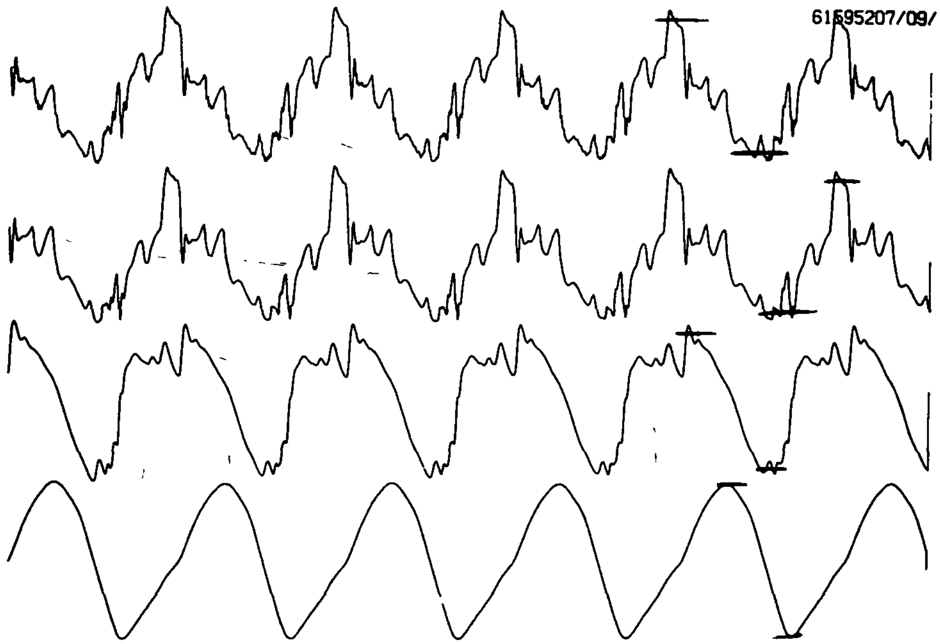

Figure 8. T_e, L, V, and R variations for M = 3.35 $M_\odot$, M_b = -3.625, and P = $10^d_.4$.

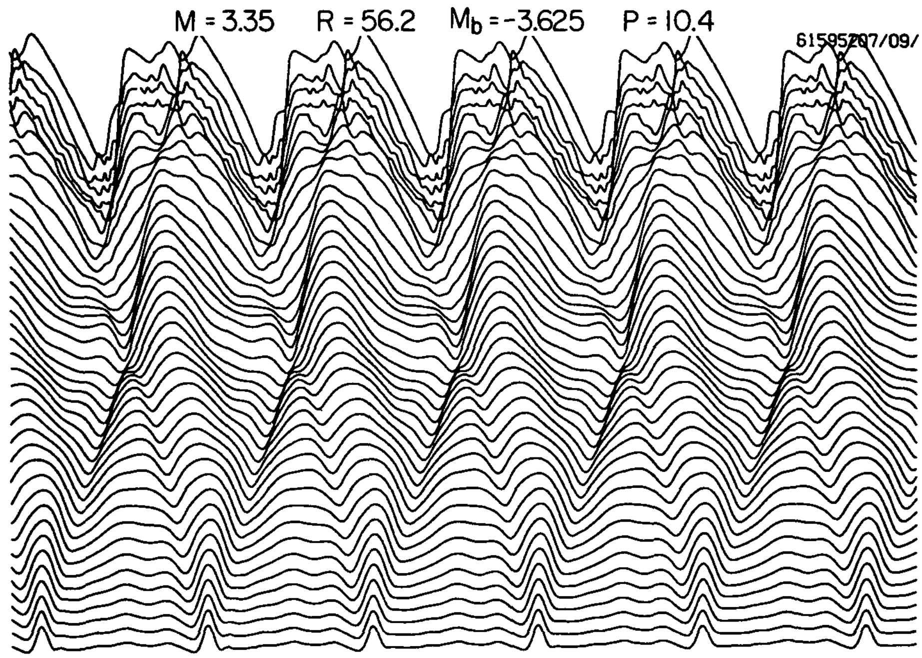

Figure 9. Internal velocity variations for M = 3.35 $M_\odot$, M_b = -3.625, and P = $10^d_.4$.

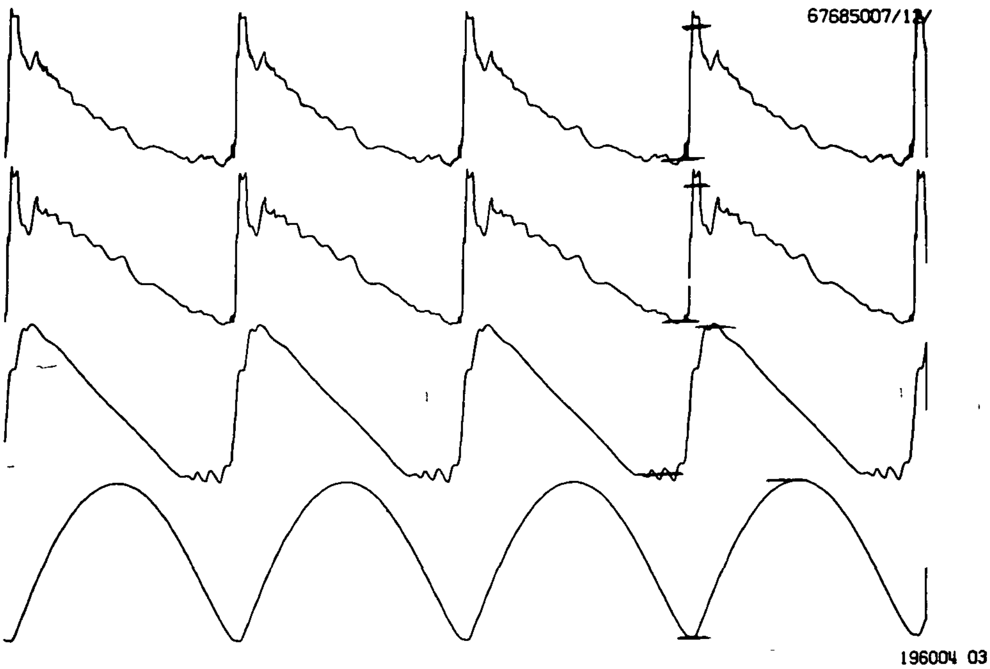

Figure 10. T_e, L, V, and R variations for M = 4.73 $M_\odot$, M_b = -4.750, and P = $24^d_.2$.

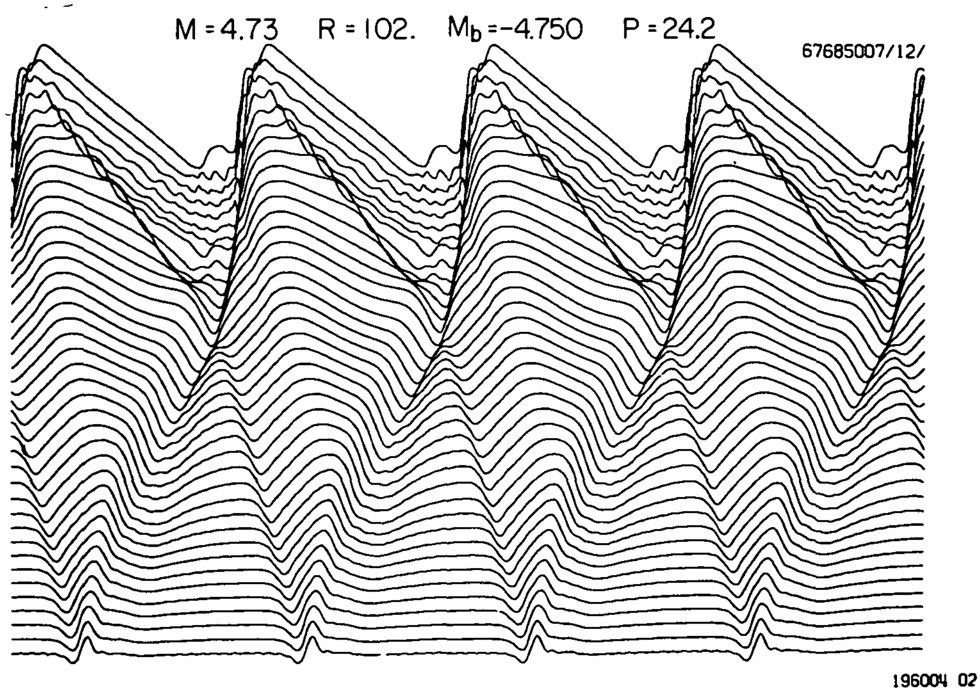

Figure 11. Internal velocity variations for M = 4.73 $M_\odot$, M_b = -4.750, and P = $24^d_.2$.

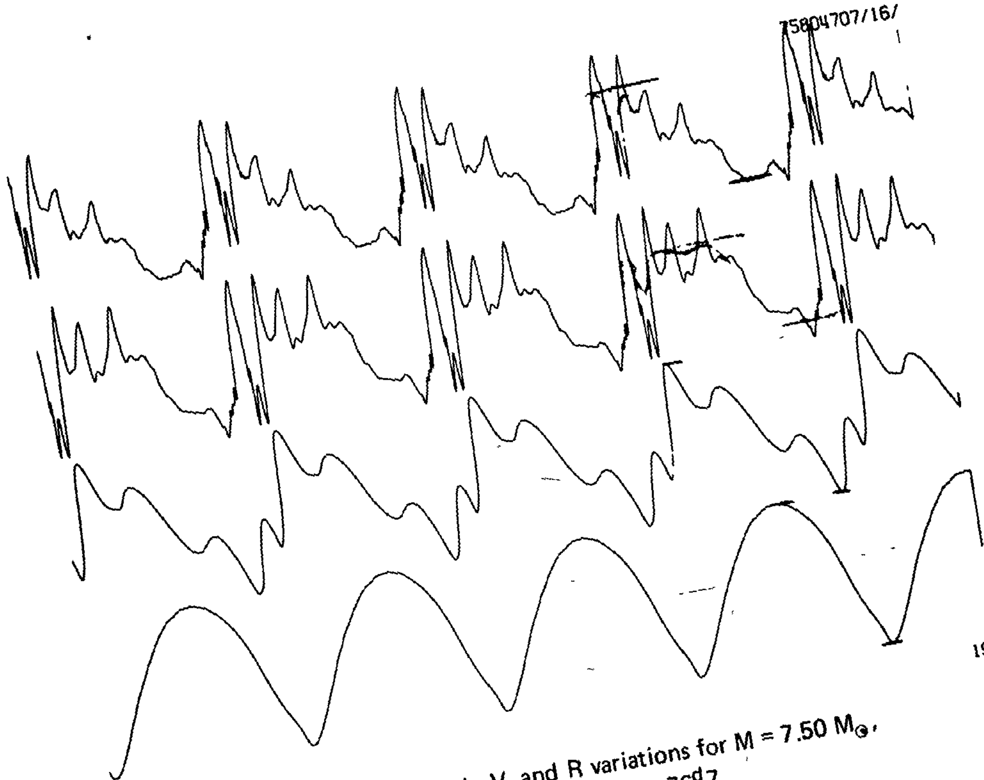

Figure 12. T_e, L, V, and R variations for M = 7.50 $M_\odot$, M_b = -6.250, and P = $76^d.7$.

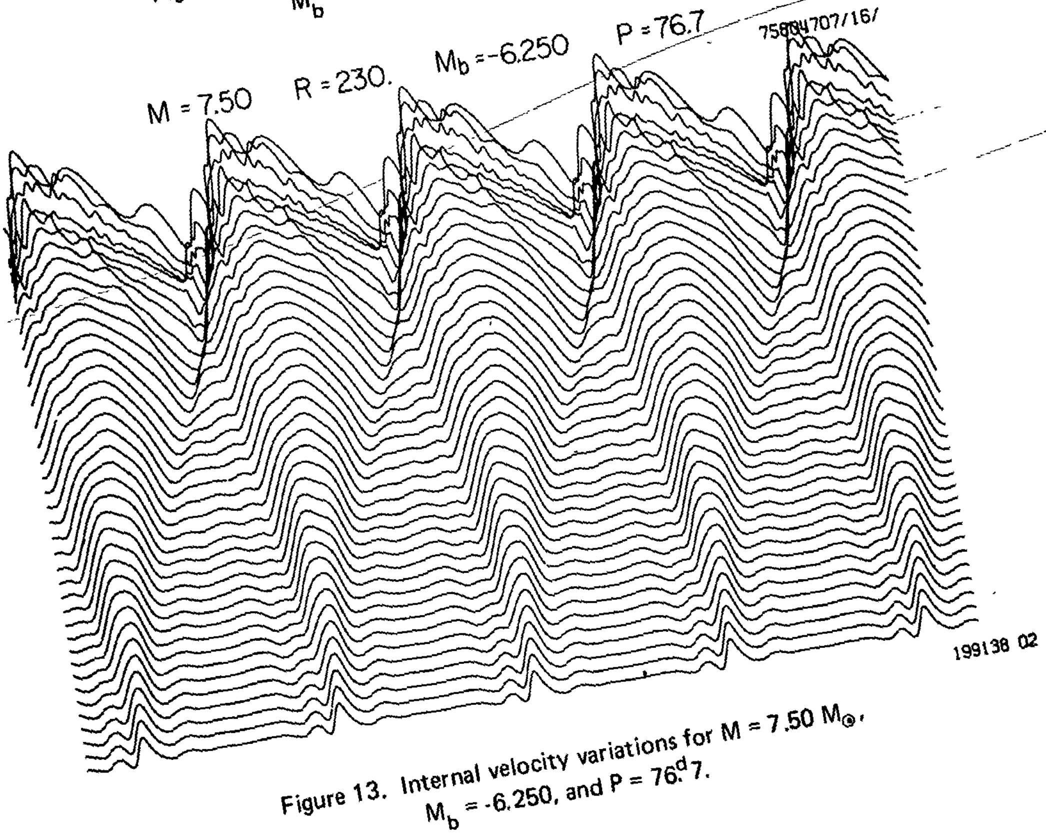

Figure 13. Internal velocity variations for M = 7.50 $M_\odot$, M_b = -6.250, and P = $76^d.7$.

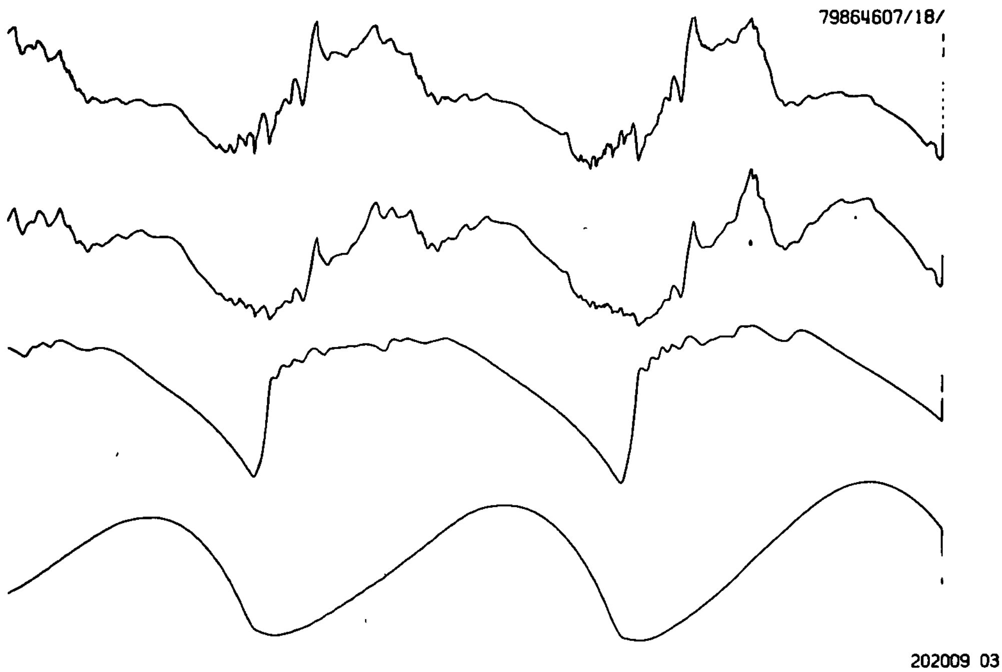

Figure 14. T_e, L, V, and R variations for M = 9.45 $M_\odot$, M_b = -7.000, and P = 148^d.

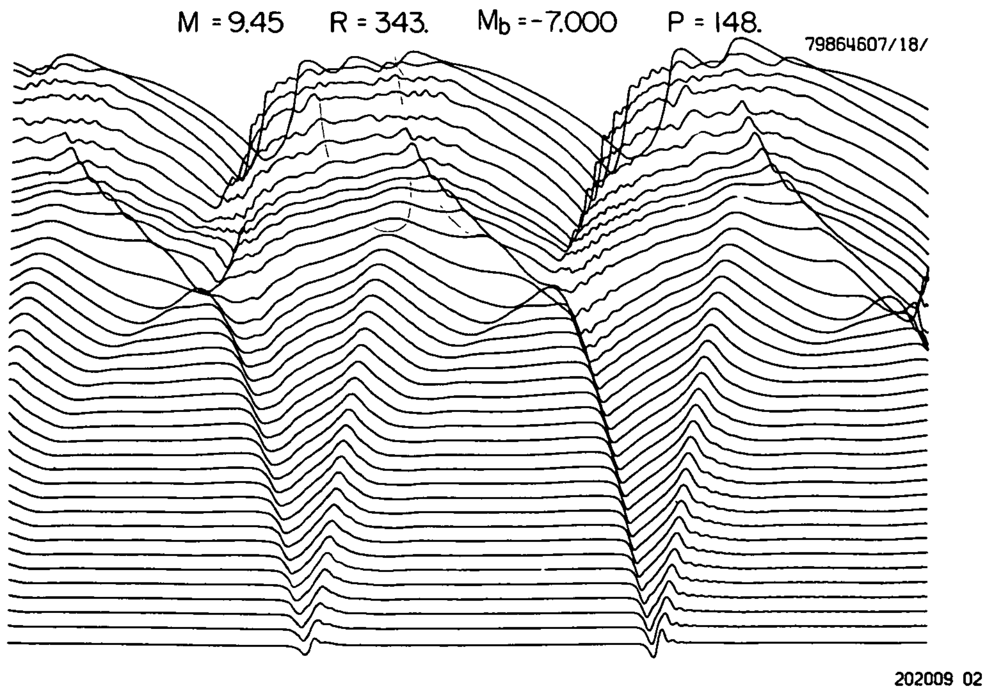

Figure 15. Internal velocity variations for M = 9.45 $M_\odot$, M_b = -7.000, and P = 148^d.

These figures have been chosen to represent the models corresponding to the lines in Table 1. They give a good impression of the complicated interior dynamics of pulsating stars.

CONCLUSION

The systematics of Cepheid variables show a wide range of interesting dynamical phenomena which can be correlated with observed variables. The masses which seem to give best correspondence with observation are, however, only about 60 percent of those from evolutionary calculations. We note that convection has been ignored in these calculations, and it is possible that the discrepancy can be removed by a dynamic treatment of convection.

DISCUSSION DURING PRESENTATION

HILL:

Is it possible to average out the periods, and then see that the ones due to zones would cancel themselves?

CHRISTY:

No, it's periodic. They repeat every period even when things have settled down.

HILL:

It looks like [on Figure 11] they are displaced from each other from period to period.

CHRISTY:

There's not precise regularity. The differences are due to not computing ad infinitum. You could do it by eye, but not by computer.

HYDRODYNAMIC MODELS OF A CEPHEID ATMOSPHERE

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INTRODUCTION

I have chosen an approach different from that of most of the preceding speakers. Instead of computing a large number of coarsely zoned models covering the entire instability strip, I have computed one model as well as computer limitations allow.

METHOD

The implicit hydrodynamic code of Kutter and Sparks (1972), which uses the Henyey method, has been modified to include radiative transfer effects in the optically thin zones. The Henyey method is just a special case of Newton's method for solving systems of non-linear equations, and the modification for computing radiative transfer hydrodynamics is:

$$x^{(n)} = x^{(n-1)} - J^{-1} f(x)$$

where:

$\mathbf{x}$ = vector of unknowns,

$\mathbf{f}$ = vector of inhomogenous terms of linearized equations

J = Jacobian of $\mathbf{f}$ with respect to $\mathbf{x}$, that is, $J_{ij} = \partial f_i/\partial x_j$, n = number of iterations.

Code uses:

$$x^{(n)} = x^{(n-1)} - J_D^{-1} f_D(x)$$

Try:

$$x^{(n)} = x^{(n-1)} - J_D^{-1} f_R(x)$$

Define:

$$R = J_R - J_D$$

Convergence if $\|R\| \cdot \|J_D^{-1}\| < 1$

The only change needed to include the solution of the transfer equation in the hydrodynamic code is to compute f using the transfer equation. Even though J is computed using the diffusion approximation, the iteration can still converge. If $R = J_R - J_D$ is the difference between the exact and the diffusion approximation Jacobians, it can be shown that the iteration will converge if $\|R\| \cdot \|J_D^{-1}\| < 1$. Unfortunately, R is not known, so the convergence cannot be determined a priori. The first guess is kept within the radius of convergence by limiting the time steps taken by the model.

MODEL PARAMETERS

The equilibrium model chosen for this study has

$M = 5\ M_\odot$

$L = 5000\ L_\odot$

$T_{eff} = 5730$ K

$R = 71.7\ R_\odot$

King 4a composition

100 zones

Mass ratio 1.15

25 optically thin zones

Δt_N = optical thickness of top zone = 4.6×10^{-4}

At full amplitude the model has a period of $12^d.05$.

Figure 1 shows the light curve of the full amplitude model. The small ripples are zoning effects and are small enough not to interfere with the discussion of the other features. There are three features of interest on the light curve. The sudden dip at phase $\phi = 0.15$ has been attributed to the use of artificial viscosity by Hillendahl.* Next, there is a shoulder on the rising branch near $\phi = 0.2$ and then a distinct bump on the falling branch.

These features have zoning ripples superimposed on them and are, therefore, probably real. Figure 2 shows several velocity curves. Notice that the feature at $\phi = 0.2$ appears only on the curve for $\tau = 10^{-3}$. Since velocity curves are usually used to classify bumps of the models, care must be used when comparing with observed light curves. Note, too, that the velocity curve for a given mass shell is nearly the same as for $\tau = 1$.

*Hillendahl, R. W., 1968, Ph.D. Thesis, University of California, Berkeley.

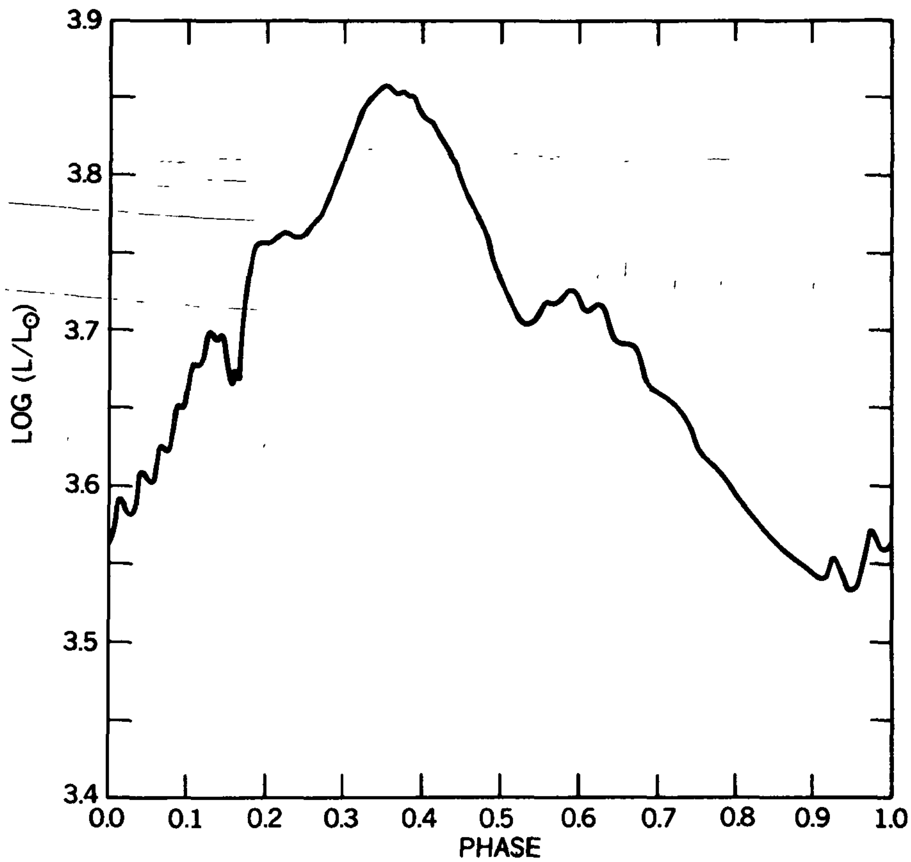

Figure 1. Log $L/L_{\odot}$ versus phase for the full amplitude model.

SECOND BUMPS

Dr. Christy has just discussed one mechanism for producing secondary features on the light curves. This model has two such features. Since van Genderen (1970) has suggested that two separate mechanisms are responsible for these features, I would like to propose another mechanism. Figure 3 shows the velocity as a function of mass point and phase. The inset will be used to define points of interest in the figure. Effects due to the zoning have been labeled "z" and are quite small compared to the main features. The shaded area in the inset shows the bottom side of the surface, while the dashed line follows the hydrogen ionization region (HIR). The point corresponding to maximum light is labeled A; B is the shoulder on the rising branch of the light curve; and C is the bump on the falling branch. The line marked D is an inward moving pressure wave discussed below, and line E indicates the location of the inward moving pressure wave described by Christy (1970).

Figure 4 is a different view of the same data. After reaching maximum expansion velocity near D, the atmosphere begins to slow down under the influence of gravity along line E. However, a disturbance originating at point A changes the sign of the acceleration and propagates both outward toward B and inward toward C. The velocity reaches a second

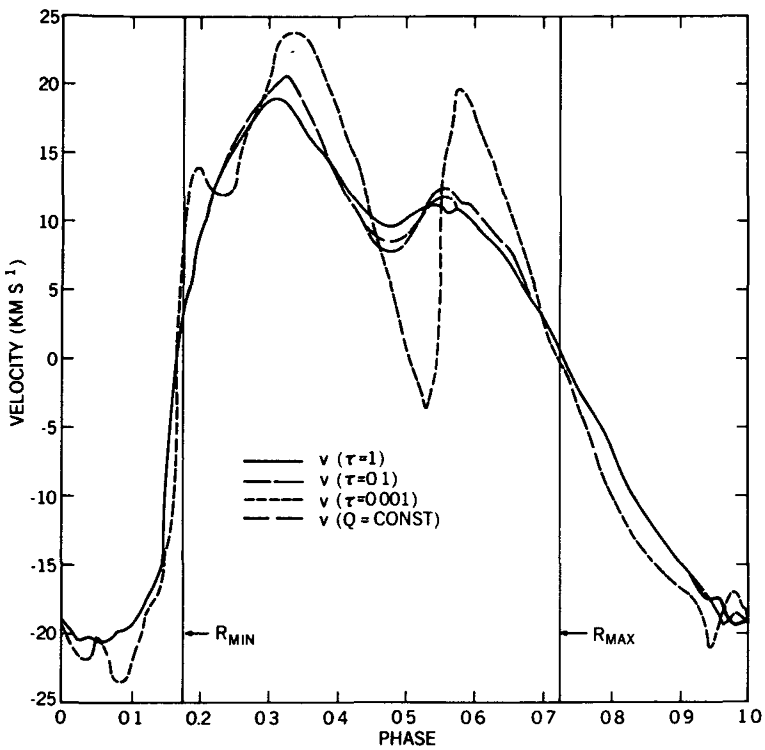

Figure 2. Velocity curves for the full amplitude model.

maximum near F and then decreases under the influence of gravity along G. The curve marked H indicates the velocity curve deeper in the envelope. Figure 5 shows the origin of the inward and outward moving pressure waves. The line marked A is the locus of points of maximum velocity in the atmosphere; line B marks the locus of points of maximum velocity in the envelope. As is evident, the atmosphere reaches maximum velocity before the envelope and starts to slow down while the envelope is still accelerating, compressing a region near the HIR. This compression generates pressure waves moving both outward (line AB on Figure 4) and inward (line AC on Figure 4). Line D(E) shows the inward (outward) moving pressure disturbance described by Christy (1970).

Figure 6 clearly shows the Christy pressure wave from the preceding cycle (line C) arrives at the surface of the model near light maximum (A), not near the second bump (B). In fact, this pressure wave reaches $\tau = 1$ very near $\phi = 0.2$ and is responsible for the shoulder on the rising branch of the light curve.

Inspection of Figure 4 shows that the surface layers appear to be pulsating with a period roughly two-thirds of the period of the envelope.

The period of the atmospheric oscillation can be estimated as follows:

According to Lamb (1932), the natural period of an isothermal atmosphere is given by

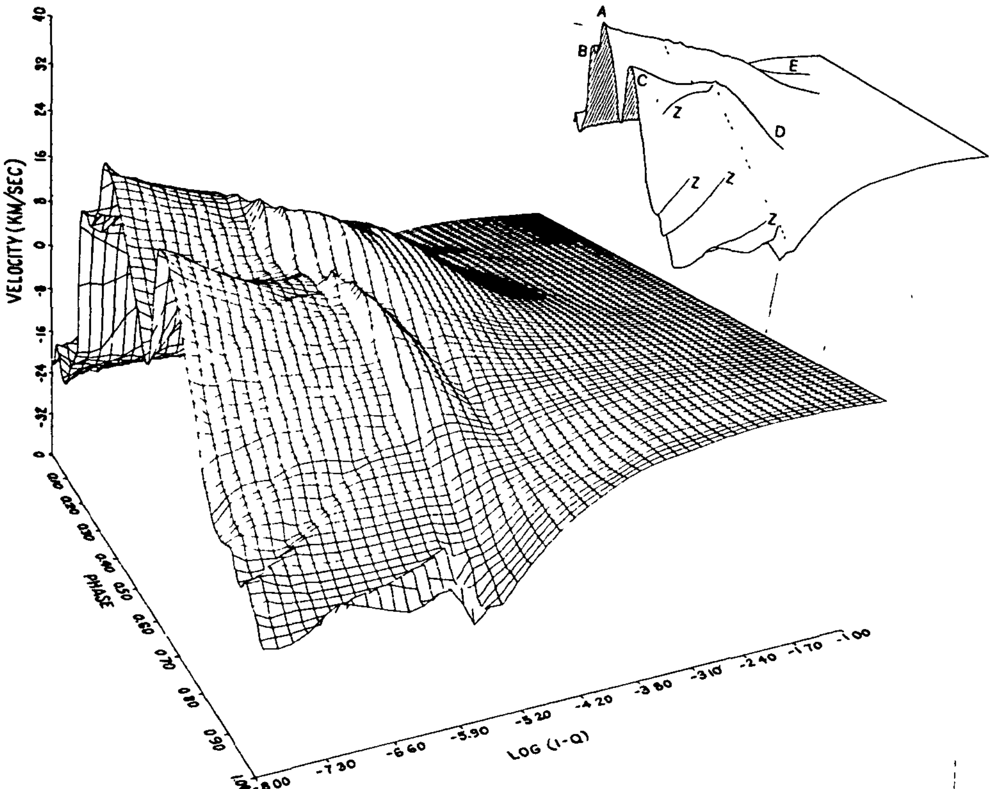

Figure 3. Velocity versus mass point and phase.

$$P_a \sim \frac{N_s}{g} \sim R^2 M^{-1}$$

while the envelope period is given by

$$P_e \sim \frac{1}{\sqrt{\rho}} \sim R^{3/2} M^{-1/2}$$

The ratio of the periods is then

$$\frac{P_a}{P_e} \sim \sqrt{\frac{R}{M}}$$

Applying the period-radius and period-mass relations gives

$$\frac{P_a}{P_e} \sim P_e^{\,0.21}$$

A more careful analysis shows that $P_a = P_e$ near 30^d.

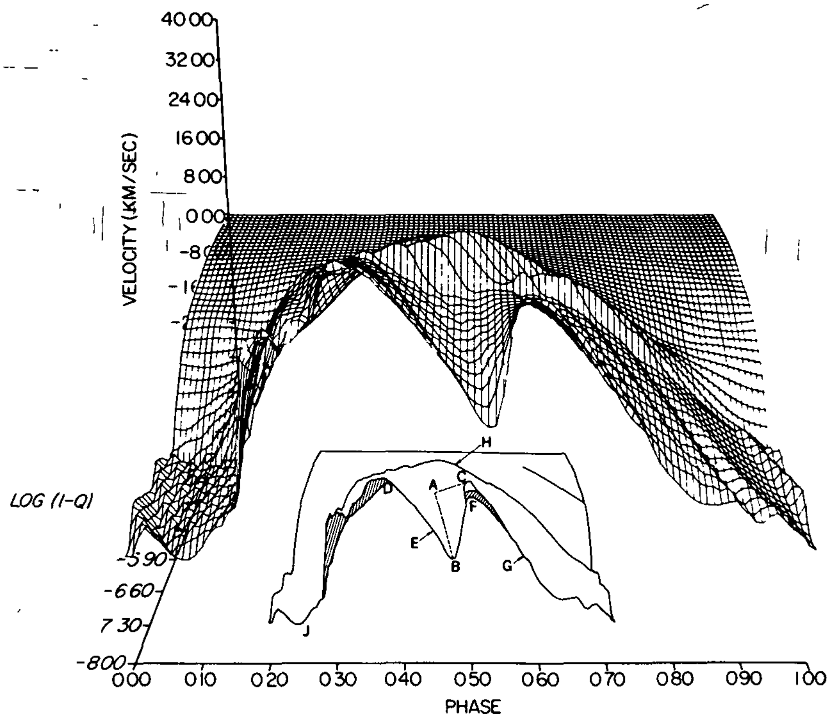

Figure 4. Velocity versus mass point and phase.

The occurrence of two bumps on Cepheid light curves can be explained qualitatively as follows: Cepheids with periods less than 7^d have $P_a < 0.5\ P_e$. Since the atmosphere is being driven far from resonance, the amplitude of the atmospheric mode is low, and the atmosphere follows the motion of the envelope. Starting at 7^d, the driving frequency approaches the resonant frequency of the atmosphere, but not until about 10^d is the amplitude of the atmospheric mode large enough to produce an observable shock from the compression of the HIR. The multiple bumps seen on the rising branch of ultraviolet light curves of β Doradus by Hutchinson* may indicate that the atmosphere is beginning to produce these shocks. In the period range 7^d to 10^d, the bumps appearing on the falling branch are produced by the Christy mechanism. From 10^d to 12^d, the Christy bump appears on the rising branch and the atmospheric oscillation bump appears on the falling branch. As the period increases beyond 12^d, the amplitude of the atmospheric mode grows, but the compression of the HIR decreases as the atmosphere and envelope begin to oscillate in phase. There are no bumps from 15^d to 30^d since the compression of the HIR is too small to produce observable shocks. At about 25^d $P_a = P_e$, the atmosphere is oscillating at its maximum amplitude, but there are no bumps since the atmosphere and envelope are always in phase.

*Hutchinson, J. L., 1974, in press.

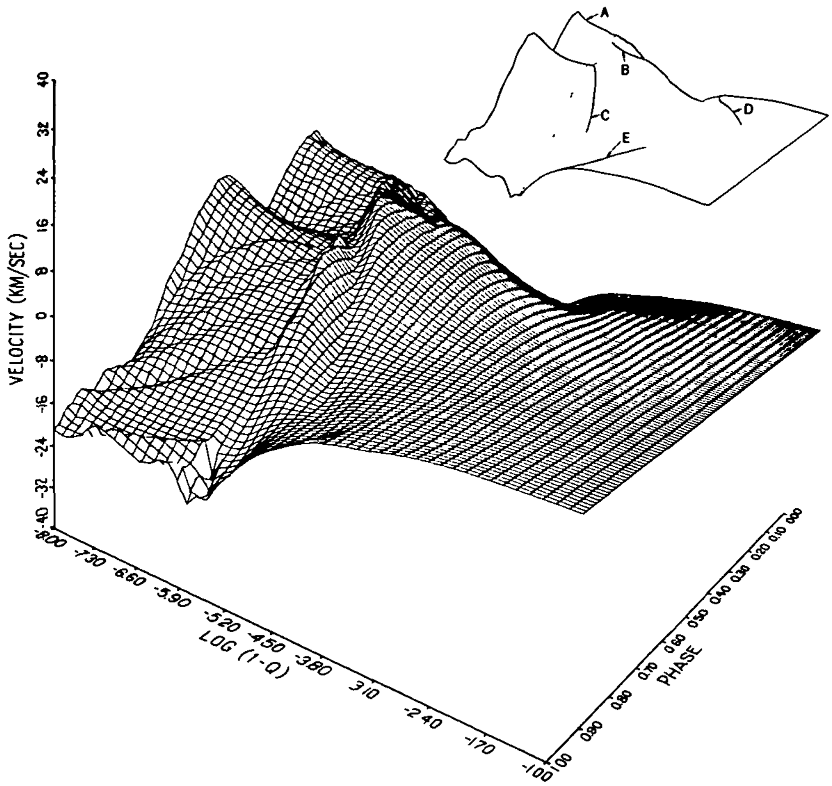

Figure 5. Velocity versus mass point and phase.

One prediction of the atmospheric oscillation mechanism is that in the period range of 10^d to 15^d, the bump on the falling branch of the light curve should become weaker and come later after maximum light as P_a/P_e (that is, R/M) increases.

PHASE LAG

Figure 7 shows another interesting phenomenon. Castor (1968), treating the HIR as a discontinuity, has suggested that a theory proposed by Eddington (1926) is correct. The large heat capacity of the HIR delays light maximum. Since the HIR lies at the top of the transition region between the quasiadiabatic envelope and the nonadiabatic atmosphere, the luminosity gets "frozen-in" at the top of the HIR.

The phase lag can be seen in Figure 7, which is a 3-dimensional plot showing the variation of luminosity, L/L_{STB}, as a function of mass point and phase as viewed from the center of the star. Note that phase increases from right to left. The inset is a schematic representation and will be used to define points of reference in the figure. Point A is in the He II ionization zone. This part of the model is nearly adiabatic, and, as expected, the luminosity maximum coincides with radius minimum. By the time the He I ionization zone is reached at point B, there is a substantial phase shift. A further, small phase shift is introduced in the HIR, the region between B and C. The "freezing-in" of the flux in the atmosphere, point C, is seen as luminosity perturbations moving outward at constant phase.

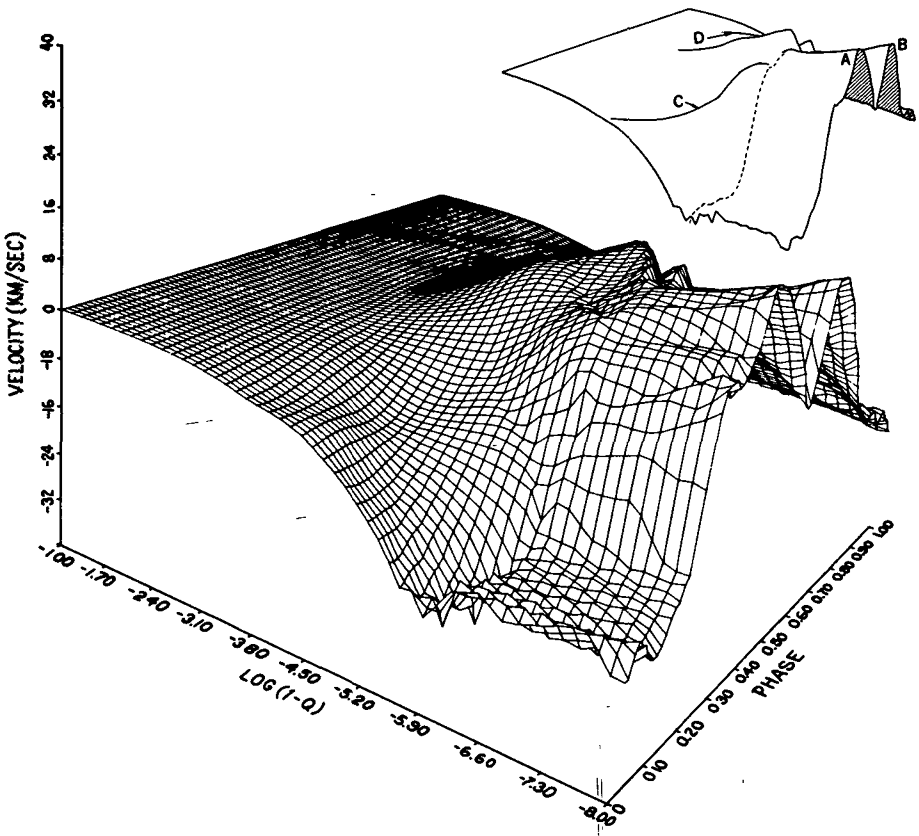

Figure 6. Velocity versus mass point and phase.

The model, therefore, suggests that the HIR plays only a small role in generating the phase shift. The phase shift appears to vary continuously through the transition region between the quasiadiabatic envelope and the nonadiabatic atmosphere.

CONTINUOUS SPECTRUM

Since fine zoning was used in the hydrodynamic calculations, the optically thin zones can be used as snapshots of the atmospheric structure and used to compute spectral energy distributions and broadband colors. The line-blocking approximation was used to account for the effect of spectral lines on the emergent flux. Figure 8 illustrates the difference between the unblocked and the line-blocked spectral energy distributions for one of Parsons' (1969) models. The dashed line is for continuous opacity sources only, and the solid line includes the line-blocking factors of Bell. It is clear that the spectral lines will have a reasonably large effect on the U, B, and V magnitudes, but will change the R and I magnitudes only slightly.

After converting the magnitudes to Johnson's system, the model was compared with observations. Such quantities as light and velocity amplitude, asymmetry, and phase shift with

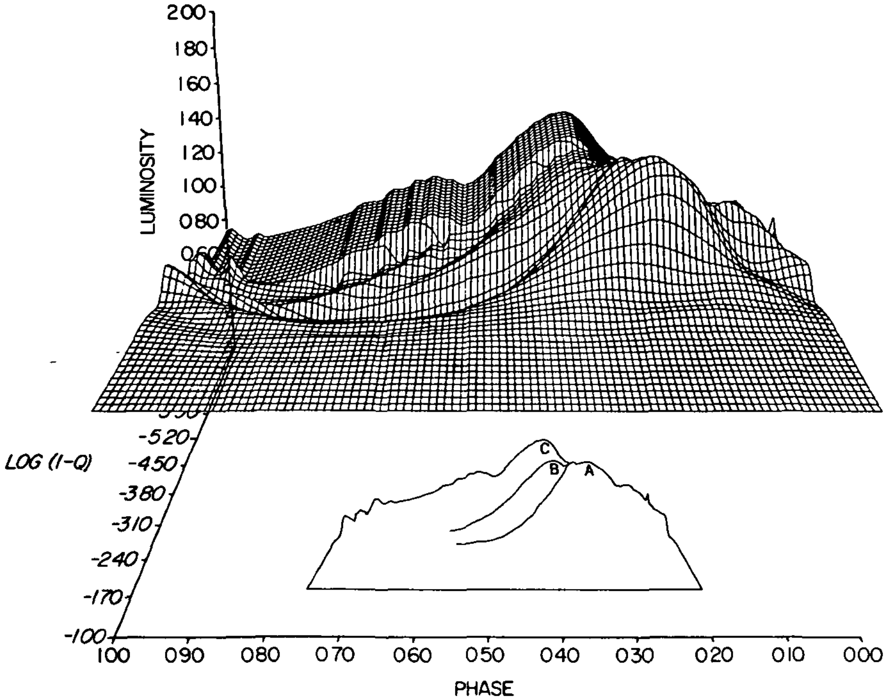

Figure 7. Luminosity versus mass point and phase.

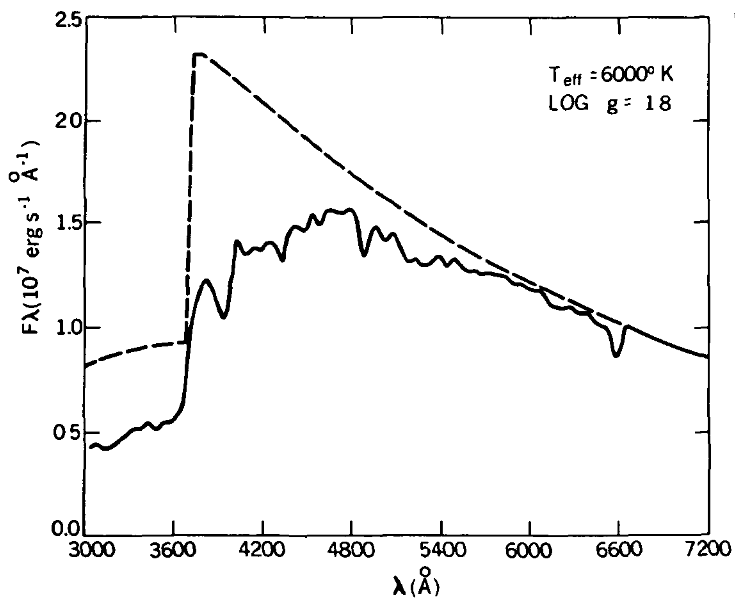

Figure 8. Spectral energy distribution for a model with T_{eff} = 6000 K and log g = 1.8. Dashed line is without blocking; solid line is with blocking.

increasing effective wavelength all fall within the observed ranges. Figure 9 shows M_v versus phase. While the zoning effects are smaller than about $0^m_.05$, they are large enough to mask features on the H-R and color-color diagrams. To remove this difficulty, the light and color curves were smoothed (Figure 10). These curves were adopted to represent the model.

Since the colors of the model approximate observations, the model can be used to answer questions raised by the observations.

One interesting problem is the cause of the loops in the (U-B) - (B-V) diagram. Abt (1959) has suggested that the loops are caused by:

a. Excess ultraviolet emission from shocks,

b. Sensitivity of the continuous opacity, κ_C, to the electron pressure, P_e,

c. Unusual line blanketing,

d. Lines being partially filled in by emission lines,

e. Continuous emission possibly originating in a chromosphere, or

f. Nonthermal dependence of line strength (that is, with P_e).

The top of Figure 11 shows the color-color diagram for the model. The curve resembles that of η Aquilae and would be classified by Nikolov and Kunchev (1969) as being linear or nearly linear. The curve is noticeably open from phase $\phi = 0.1$ to $\phi = 0.5$, having a maximum width of $0^m_.04$ in (U-B). Since neither emission lines nor chromospheric emission was included in the model, the loops could not be produced by (d) or (e). The bottom of Figure 11 shows the color-color diagram for the model excluding the line-blocking factors. Due to the change of scale, the curve appears to be much more open but still has a maximum width of $0^m_.05$ in (U-B). Since lines have not been included in calculating this case, the openness cannot be explained by (c) or (f). There is a strong shock in the atmosphere only near $\phi = 0.5$, and the excess emission amounts to about $0^m_.02$ in (U-B). Possibility (a), therefore, can be excluded. Only (b), the sensitivity of κ_C to P_e, remains. In the temperature range of the model, the continuous opacities of H and H^- are the primary opacity sources. In the atmosphere, most of the free electrons come from the metals, and the ionization of the metals depends linearly on P_e through the Saha equation. Since the wavelength dependencies of the H and H^- continuous opacities differ, a change of P_e at fixed temperature will change H^-/H and, therefore, the wavelength dependence of κ_C. Since the U filter contains the Balmer jump, this effect will be more pronounced in U than in B or V, producing a loop in the color-color diagram.

LINE SPECTRUM

The line spectrum was also studied. One of the most interesting phenomena is the occurrence of the "Cheshire cat" lines. This phrase was used by Underhill to describe the extra component of strong lines often observed in Cepheids. These extra components seem to

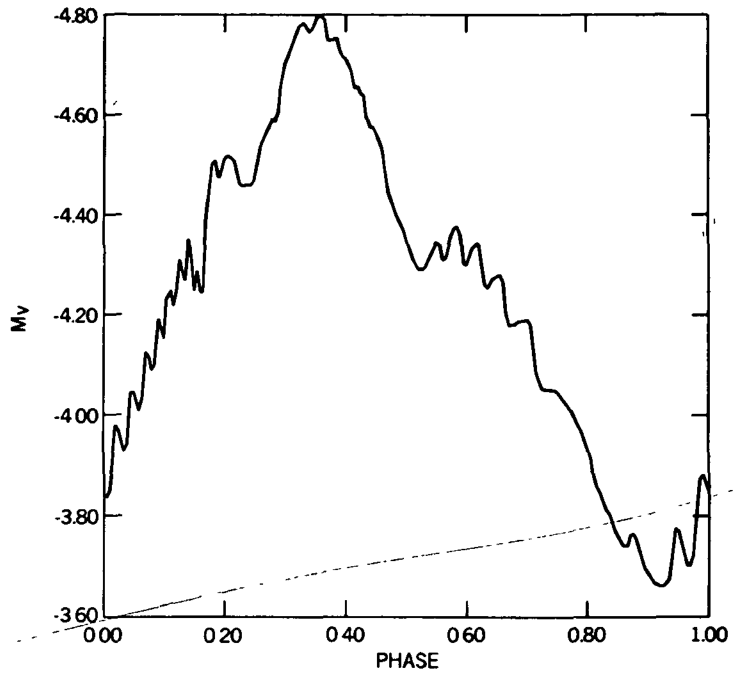

Figure 9. Absolute visual magnitude versus phase.

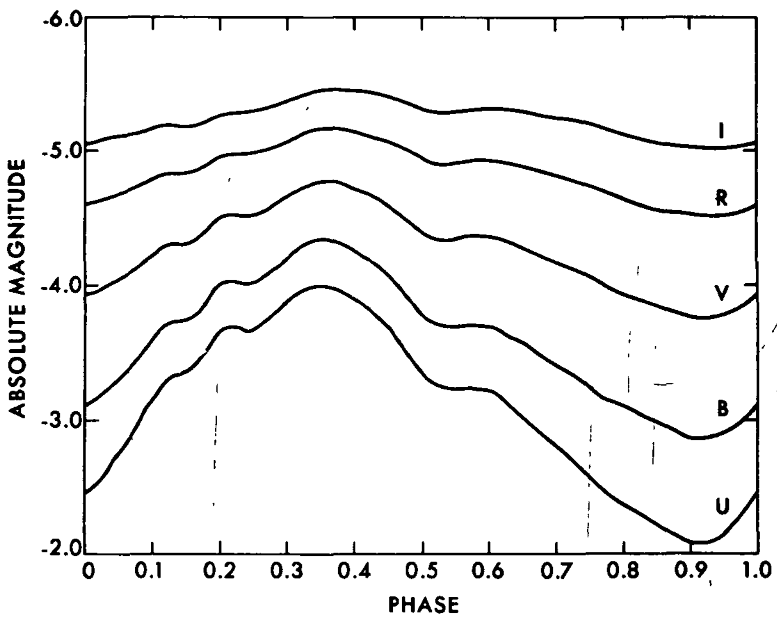

Figure 10. Smoothed light curves in U, B, V, R, and I.

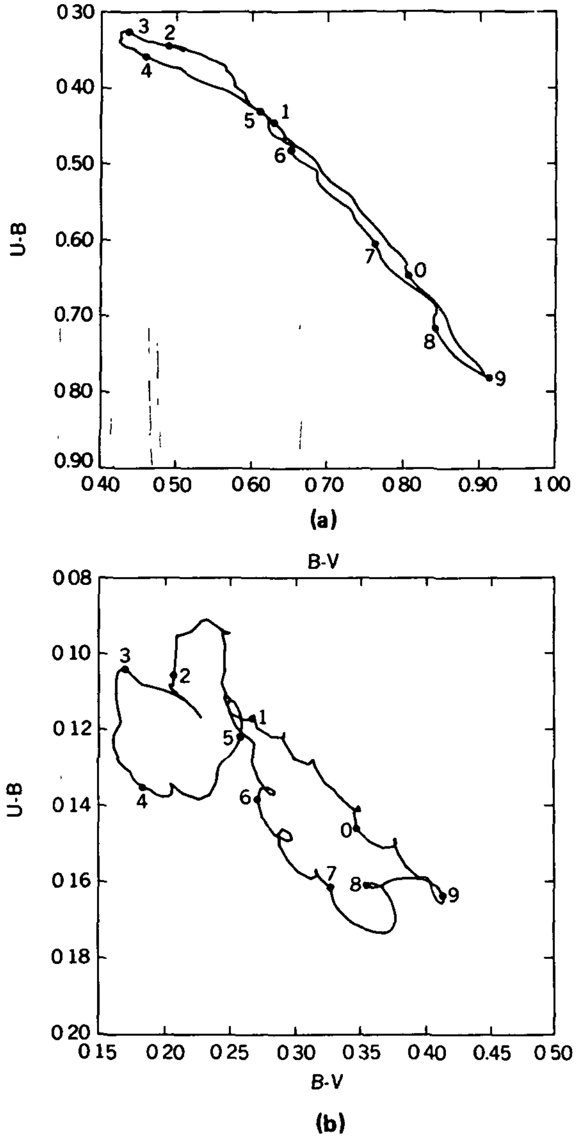

Figure 11. (a) (U-B) versus (B-V) including line blocking factors. The numbers indicate phase X 10. (b) (U-B) versus (B-V) without line blocking factors.

have no antecedents, but suddenly appear at phases when strong shocks are expected in the atmosphere.

Kraft (1967) has stated that the velocity difference between the two components may be as large as 30 to 40 km s^{-1} for the low excitation metal lines while Grenfell and Wallerstein (1969) and Wallerstein (1972) report velocity differences of up to 100 km s^{-1} in the Hα lines of SV Vulpeculae and T Monocerotis. Since the atmosphere of the model never has velocities greater than 30 km s^{-1} or velocity differences greater than 15 km s^{-1}, the occurrence of the "Cheshire cat line" shown in Figure 12 indicates that the splitting is not due to differential motions. The deeper component has a velocity of 19 km s^{-1} while the shallower

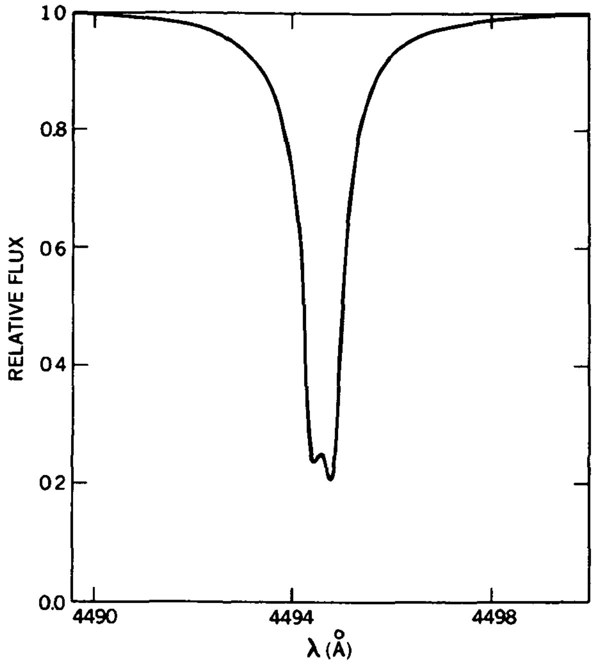

Figure 12. Line profile showing a "Cheshire cat" line.

has -3 km s^{-1}, a difference of 22 km s^{-1}. Examining the model shows the maximum velocity difference is 3 km s^{-1} and the mean velocity is 10 km s^{-1}. The splitting of the line is obviously not caused by differential motions.

Further inspection of the model revealed that there was a temperature inversion of about 300 K near $\tau = 3 \times 10^{-3}$, the region in which the line core is formed. To verify that the temperature inversion is responsible for the line doubling, the same line was computed with all velocities set to zero. The result is shown in Figure 13. The central reversal is characteristic of a line core formed in a region in which the source function increases outward. While a more detailed, non-LTE calculation would probably not show as large a central reversal since the source function in the line core could be smaller than the Planck function, the line doubling could still occur for the strongest lines.

Care must be taken when measuring velocities of strong lines. The practice of treating each component of the line as a distinct layer of gas produces erroneous velocity curves. Neither component represents the motion of the atmosphere. Even if the splitting is not observed and the line core appears to be symmetric, there may be incipient splitting that has been masked by macroturbulence, microturbulence, or instrumental broadening. Velocities of strong lines should, therefore, be measured at some point in the wings of the line.

RADIUS DETERMINATIONS

Profiles were computed for lines of varying strengths and velocity curves were computed for each line. Figure 14 shows the displacement curve for a line of intermediate strength, that is, 85 mÅ $\leqslant W \leqslant$ 150 mÅ. The solid line was taken from $\tau = 1$ of the model and the

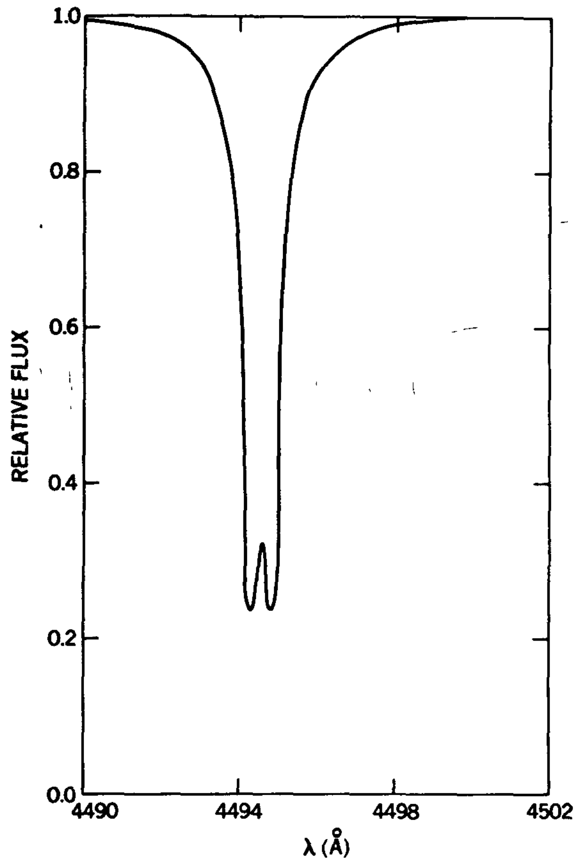

Figure 13. Same case as Figure 12, but with all velocities set to zero.

dashed line from the half-intensity point on the profile. The two curves are in good agreement with each other, and it appears that a Wesselink (1946) calculation is valid. In addition to a radius determination using the Wesselink method, Baade's (1926) method and the bolometric luminosity were also used. The results are summarized in Table 1.

Table 1
Radius Determinations of Hydrodynamic Mode in Solar Units

Method	R_{STB} = 71.7			Expected Error	
	B-V R ± s.d.	V-R R ± s.d.	R-I R ± s.d.	Random	Systematic
Wesselink (1946)	58.5 ± 4.0	73.5 ± 8.7	67.3 ± 5.2	8%	2%
Baade (1926)	77.2 ± 7.8	79.6 ± 4.7	73.6 ± 5.5	10%	2%
all points					
some points*	71.0 ± 5.5	74.2 ± 3.8	73.6 ± 2.8	10%	2%
$L = 4\pi R^2 \sigma T^4$	69.4 ± 2.0	69.5 ± 2.2	69.2 ± 1.6	8%	7%

*Excluding points near maximum light.

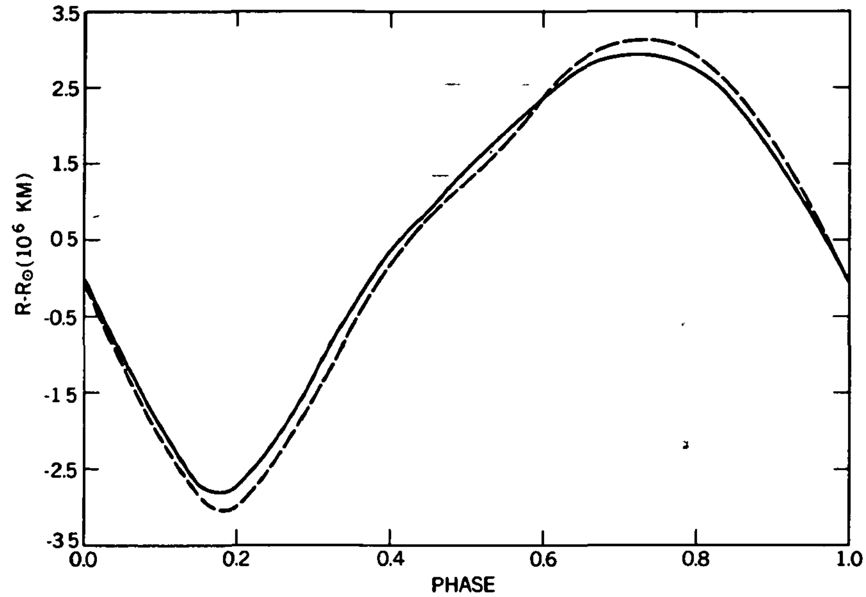

Figure 14. Change of radius, $R\text{-}R_0$, versus phase. Solid line is from models; dashed line is from adopted velocity curve.

The standard deviations were computed from several pairs of radii, and the quoted errors, in percent, are estimates based on the quality of the light and velocity curves in Table 1. All three methods produce radii accurately to about 10 percent. Results are somewhat better if (V-R) or (R-I) is used as a temperature indicator instead of (B-V). It would appear to be unrealistic to expect radii from observations to be more accurate than about 10 to 15 percent. This means that the period-radius law cannot be used to detect overtone pulsators unless some method can be found to reduce the errors in the radius determinations.

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NUMERICAL TECHNIQUES FOR THE LINEAR, NONADIABATIC STELLAR PULSATION PROBLEM

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ABSTRACT

The linear, nonadiabatic eigenvalue problem is formulated using Castor's method. Both left and right eigenvectors are calculated. Initial eigenvalues for the linear, nonadiabatic solutions are obtained from the adiabatic eigenvalues and left and right eigenvectors. The orthogonality relation is obtained. Simple formulas for the Newton method are given. The iteration procedure is constrained to improve convergence. The Newton method is less satisfactory than the secant method for difficult cases. The linear, nonadiabatic solutions are shown to be sensitive to $N_<$, the number of zones with $\tau < 2/3$, and the value of $(P/P_r)_{surface}$ or τ(surface). Optimum values can be determined for $N_<$ and τ(surface). Application of the method to Population II Cepheids is briefly presented.

INTRODUCTION

Castor (1971) has developed a new fast method for the calculation of linear, nonadiabatic radial pulsations of stellar models. In applying the method to Population II Cepheids, we found the method to be unstable if the surface boundary condition was applied near $(P/P_r)_{surface} \sim 1$, or if the model had a high luminosity relative to that at the intersection point of the fundamental and first harmonic blue edges. The techniques used to stabilize the method are described in the following sections. Tests to determine the optimum point of application of the surface boundary condition are also given, and the application of the method to Population II Cepheids is discussed briefly.

THE LINEAR, NONADIABATIC EIGENVALUE PROBLEM

The eigenvalue problem arises through the linearization of the difference equations describing the stellar model. This technique is described by Castor (1971) and need not be repeated here. We shall only examine the method of solution of the resulting eigenvalue problem. Castor's notation is retained.

The eigenvalue problem for the right eigenvector Z can be written in general matrix form as

$$(A - \lambda) Z = 0 \tag{1}$$

$$A = \begin{bmatrix} G_1 & G_2 \\ K_1 & K_2 \end{bmatrix}, \quad \lambda = \begin{bmatrix} \omega^2 I & 0 \\ 0 & i\omega I \end{bmatrix}, \quad Z = \begin{bmatrix} X \\ Y \end{bmatrix} \tag{2}$$

The column vectors X and Y have components X(J), J = 2, N+1 and Y(J), J = 1, N, respectively. The submatrices of A and λ are all N X N. I is the identity matrix. We define a left eigenvector W* by

$$W^*(A - \lambda) = 0 \tag{3}$$

$$W^* = (U^*, V^*) \tag{4}$$

where the * denotes the complex conjugate transpose operation. The row vectors U* and V* have components $\bar{U}(J)$, J = 2, N+1 and $\bar{V}(J)$, J = 1, N, respectively, where the bar denotes complex conjugacy. In general, the 2N equations (Equation 1) have N physically relevant solutions (Z_K, ω_K), K = 1, N. The N solutions $(Z_K, -\omega_K)$, K = 1, N, can be discarded because they correspond to negative periods.

To solve Equations 1 and 3, we first transform them to band form as follows:

$$P(A - \lambda) P^T(PZ) = 0 \tag{5}$$

$$(W^*P^T) P(A - \lambda)P^T = 0 \tag{6}$$

$$P_{ij} = \max(\delta(i, 2j), \delta(i, 2j - 2N - 1)) \tag{7}$$

$$P^T = P^{-1} \tag{8}$$

where T denotes the transpose operation, and $\delta(i, j)$ is the Kronecker delta. The transformed Equations 5 are in the band form described by Castor (1971). We further partition the matrices as follows:

$$PAP^T = \begin{bmatrix} \hat{A} & \hat{C} \\ \hat{R}^T & A_0 \end{bmatrix}, \quad P\lambda P^T = \begin{bmatrix} \hat{\lambda} & 0 \\ 0 & \omega^2 \end{bmatrix} \tag{9}$$

$$W^*P^T = (\hat{W}^*, \bar{U}(N+1)), (PZ)^T = (\hat{Z}^T, X(N+1)) \tag{10}$$

where $\hat{C}$ is a column vector, $\hat{R}^T$ is a row vector and $A_0 = G_1(N+1, 2)$. The vectors $\hat{Z}$ and $\hat{W}^*$ are obtained by choosing values for ω, X(N+1), $\bar{U}$(N+1) and solving

$$(\hat{A} - \hat{\lambda})\hat{Z} = -\hat{C}X(N+1) \tag{11}$$

$$\hat{W}^*(\hat{A} - \hat{\lambda}) = -\bar{U}(N+1)\hat{R}^T \tag{12}$$

We have used a complex form of the band equation solver, with partial pivoting, described by Martin and Wilkinson (1967).

CHOOSING AN INITIAL EIGENVALUE

Initially, we solve the linear adiabatic eigenvalue problem,

$$(G_1 - \omega_{ad}^2)\, X_{ad} = 0 \tag{13}$$

The adiabatic eigenvalues are obtained by the method of bisection and Sturm sequences described by Barth, Martin, and Wilkinson (1967). The eigenvectors X_{ad} are calculated using Castor's (1971) method.

Next we set $\omega = \omega_{ad}$ and solve Equations 11 and 12. We do not make use of the adiabatic eigenvectors X_{ad}. Since the N*th* equation involving G_1 and G_2 was omitted from Equations 11 and 12, we can write

$$E = G_1 X + G_2 Y - \omega^2 X \tag{14}$$

where E is an error vector. If we choose E so that it is orthogonal to U*, we can calculate the new eigenvalue from

$$\omega^2 - \omega_{ad}^2 = \frac{U^*(G_1 - \omega_{ad}^2)\, X}{U^* X} + \frac{U^* G_2 Y}{U^* X} \tag{15}$$

by insisting that $U^*E = 0$.

This method is similar to the method of the Rayleigh quotient (Martin and Wilkinson, 1967). Further improvement of the eigenvalues and eigenvectors can be obtained by the secant method described by Castor (1971) or by a Newton iteration described below. In the secant method, further iteration requires the solution of only Equation 11, apart from a final solution of Equation 12, to compute the orthogonality condition.

THE NEWTON CORRECTION

Any solution of Equations 5 and 6 must satisfy Equations 11 and 12 and reduce to zero the residuals F_1 and F_2 defined by

$$F_1 = \hat{R}^T \hat{Z} + A_0 X(N+1) - \omega^2 X(N+1) \tag{16}$$

$$F_2 = \hat{W}^* \hat{C} + A_0 \bar{U}(N+1) - \omega^2 \bar{U}(N+1) \tag{17}$$

By differentiating Equations 11, 12, 16, and 17 with respect to ω, and eliminating all derivatives, the Newton correction to the eigenvalue is given by

$$\Delta\omega = \frac{\bar{U}(N+1)F_1}{2\omega U^*X + iV^*Y} \tag{18}$$

$$\Delta\omega = \frac{F_2 X(N+1)}{2\omega U^*X + iV^*Y} \tag{19}$$

With $\bar{U}(N+1) = X(N+1)$, we have verified numerically that $F_1 = F_2$.

In practice, the Newton method has proven to be less satisfactory than the secant method for difficult cases. It can be seen, from the denominators of Equations 18 and 19, that the Newton correction makes use of information from the total eigenvectors. If the initial estimate of ω is not close to the true eigenvalue, the Newton correction is highly contaminated by information from other modes and may vary erratically. The secant method, based on the residual F_1, only includes information from the slowly changing behavior of the eigenvectors near the surface of the stellar model.

THE ORTHOGONALITY RELATION

If ω_i and ω_j are distinct eigenvalues, it follows, from Equations 1 and 3, that

$$W_j^* (\lambda_j - \lambda_i) Z_i = 0 \tag{20}$$

Since $\omega_i \neq \omega_j$,

$$(\omega_i + \omega_j) U_j^* X + iV_j^* Y_i = 0 \tag{21}$$

which is the orthogonality condition. Typically, the orthogonality condition is satisfied to the same degree of accuracy as the eigenvalues.

SPECIAL TECHNIQUES TO IMPROVE CONVERGENCE

The secant method was applied to the residual F_1 divided by X(2), as suggested by Castor (1971). The real part of the eigenvalue, ω_r, was limited to a change of no more than 5 percent each iteration. The imaginary part of the eigenvalue, ω_i, was constrained, so that $|\omega_i| < 0.4\ \omega_r$. Sometimes the eigenvalue for the fundamental mode, as predicted by Equation 15, satisfied $|\omega_i| > \omega_r$. Then, a better value for ω_r was obtained from $|\omega_i\omega_r|^{1/2}$, while ω_i was chosen to satisfy the previous condition. If the fundamental mode still did not converge, a new value for ω_r was obtained by increasing the adiabatic eigenvalue by 10 to 20 percent. This final procedure was rarely required. The iteration procedure was limited to 30 iterations for each initial estimate of the eigenvalue. The average number of iterations per mode was 10.

OPTIMUM PLACEMENT OF THE SURFACE BOUNDARY CONDITION

A number of test models were calculated to determine the best positioning of the surface boundary condition. Opacities, for the composition Y = 0.30, Z = 0.004, were calculated using B. Paczyński's opacity interpolation program. One model, corresponding to Christy's (1966) model 5g, had mass $M/M_{\odot} = 0.5776$, luminosity $\log_{10} L/L_{\odot} = 1.5895$, and effective temperature $\log_{10} T_e = 3.8129$. More luminous models, with $M/M_{\odot} = 0.6$ and $\log_{10} L/L_{\odot} = 3.0$, were constructed for effective temperatures $\log_{10} T_e = 3.73, 3.75$, and 3.77. The model with highest temperature lies $\Delta\log_{10} T_e = 0.0096$ to the red of the fundamental blue edge.

For model 5g, only the growth rate,

$$W = -\frac{4\pi\omega_i}{\omega_r} \tag{22}$$

was sensitive to the positioning of the surface boundary condition. Variations of the model were constructed by changing the ratio of total to radiation pressure, $(P/P_r)_s$, at the surface of the model, and by altering the number of zones, $N_<$, with $\tau < 2/3$. Figure 1 shows the variation of the growth rate with $N_<$. The higher the mode, the greater the variation. In particular, W(2H) diverges if $N_<$ is less than 20 or greater than 40. The effects of truncation error in integrating the static model are the primary cause of the divergence for large $N_<$. If $N_<$ is too small, the steep variation of the eigenvectors near the surface is not adequately represented. The variation of W(1H) is similar to that of W(2H), but smaller in magnitude. The fundamental mode is not very sensitive to changes in $N_<$.

Figure 2 illustrates the effect of changing the value of $(P/P_r)_s$. For large values of $(P/P_r)_s$, W(1H) and W(2H) are strong functions of $N_<$. These models were calculated with an earlier version of the program which did not converge for the second harmonic for small surface pressures. The optical depth at the surface was 1.17, 1.39, 1.78, and 2.74 (in units of 10^{-3}) for values of $(P/P_r)_s$ equal to 75, 85, 100, and 135, respectively.

Other models of 5g were constructed including the variation of optical depth at the surface and a finite pressure outside the model (King et al., 1973). The effect of these modifications was negligible in comparison to the effects produced by varying $N_<$ and $(P/P_r)_s$.

The higher luminosity models were calculated with $N_<$ set at approximately 35. Figure 3 shows that the period of the fundamental can be calculated to within 1 percent. But the period of the first harmonic varies by as much as 5 percent for different choices of τ_s. The pulsation energy, KE, called K by Castor (1971), varies by as much as a factor of 10 for the first harmonic, as illustrated in Figure 4. The general trend for the higher luminosity models is a divergence in the period and KE as τ_s tends to zero. This effect is greater at lower effective temperatures. A similar effect was found by Unno (1965). The effect is caused by the sensitivity of the equations to the surface conditions near the mathematical surface, $P_{gas}=0$. In our models, for $\tau_s < 10^{-3}$, the sound travel times through the first few surface zones were much greater than the pulsation periods and indicated an adiabatic behavior there. This situation seems unrealistic considering that the pulsation amplitudes are large at the surface compared to the interior regions of the model. The pulsation energy decreases with decreasing τ_s because the pulsation amplitude at the surface increases relative

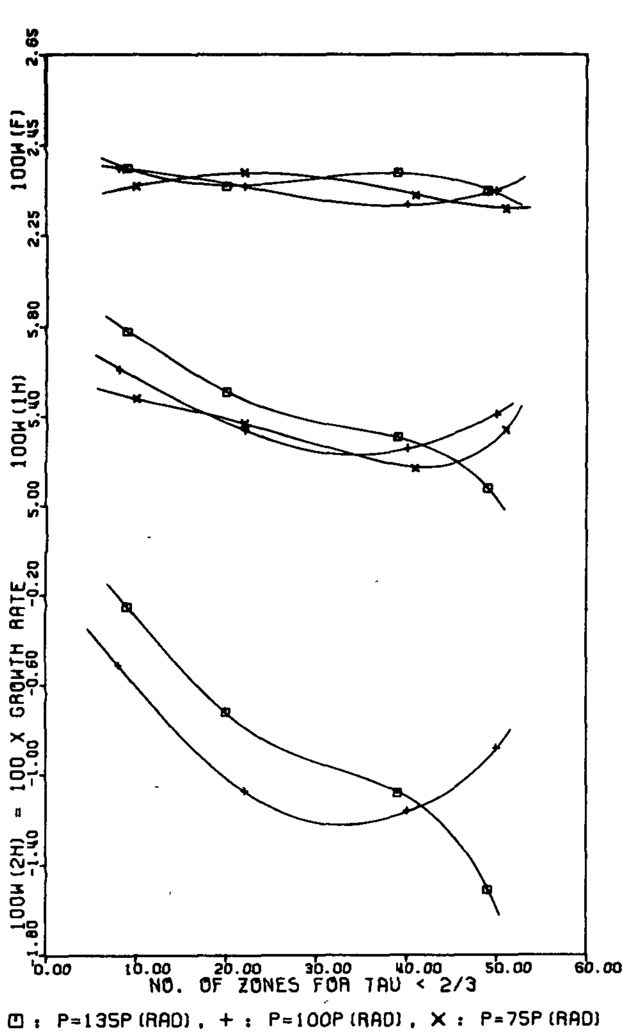

Figure 1. Growth rate versus $N_< \times 100$ for model 5g for the fundamental (F), first harmonic (1H), and second harmonic (2H) modes.

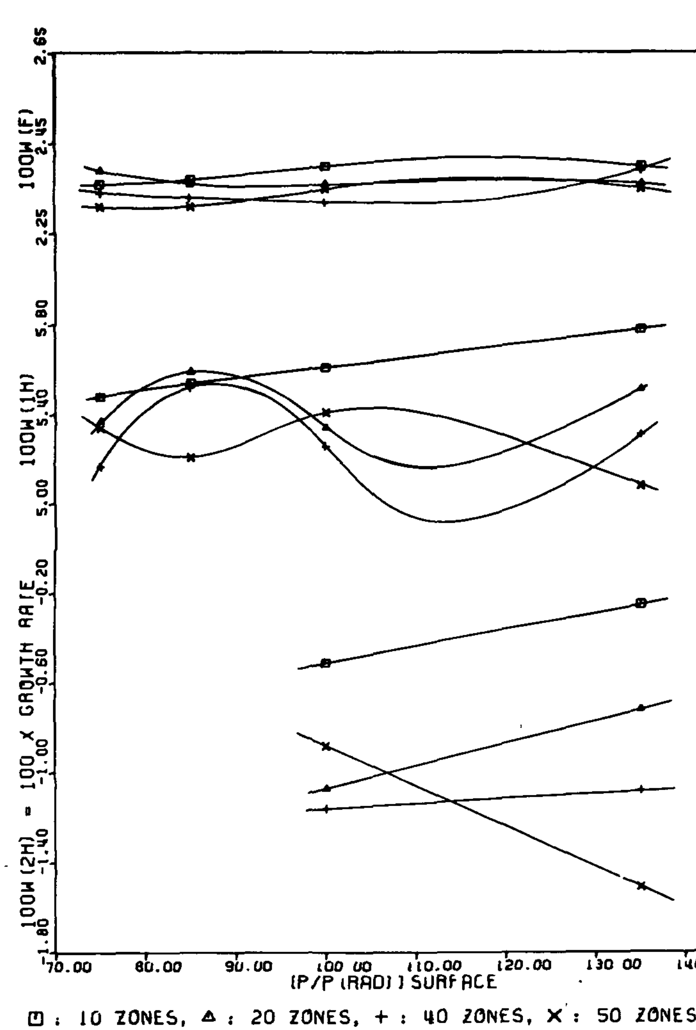

Figure 2. Growth rate versus $(P/P_r)_s \times 100$ for model 5g

to the interior. However, despite these effects, for the range of conditions in Figures 3 and 4, the fundamental blue edge could be calculated to an accuracy of ±0.005 in $\log_{10} T_e$. From an examination of these results and others not presented here, the boundary condition at the surface was set at $\tau_s = 1.4 \times 10^{-3}$ with $N_<$ in the range 30 to 40.

APPLICATION TO POPULATION II CEPHEIDS

Using the methods described in this paper, several hundred models of Population II Cepheids were calculated. We found that at luminosities characteristic of the slow loop and final crossing phases of the post-asymptotic-branch models computed by Mengel,* the first harmonic blue edge approaches and crosses the fundamental blue edge. The periods

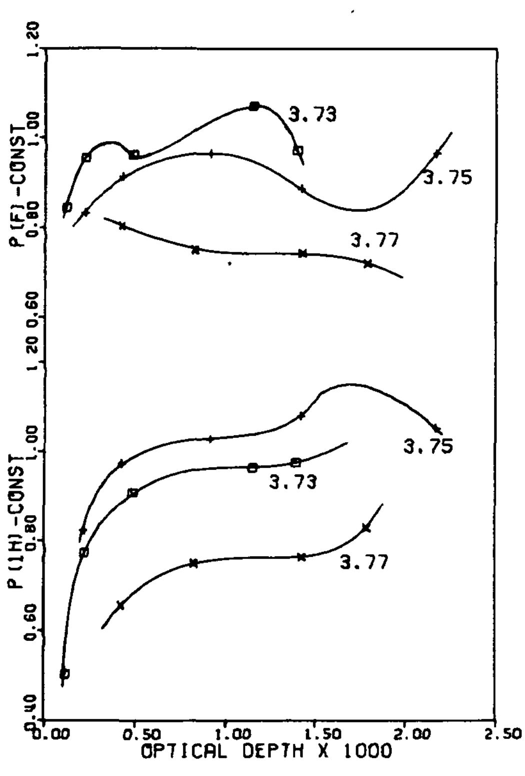

Figure 3. Period versus $\tau_s \times 1000$ for models with $M/M_\odot = 0.6$ and $\log_{10} L/L_\odot = 3.0$. The number beside each curve is the value of $\log_{10} T_e$. The curves have been shifted by arbitrary amounts (minus some constant) to bring them onto the same scale.

*Mengel, J., 1972, Ph.D. thesis, Yale University.

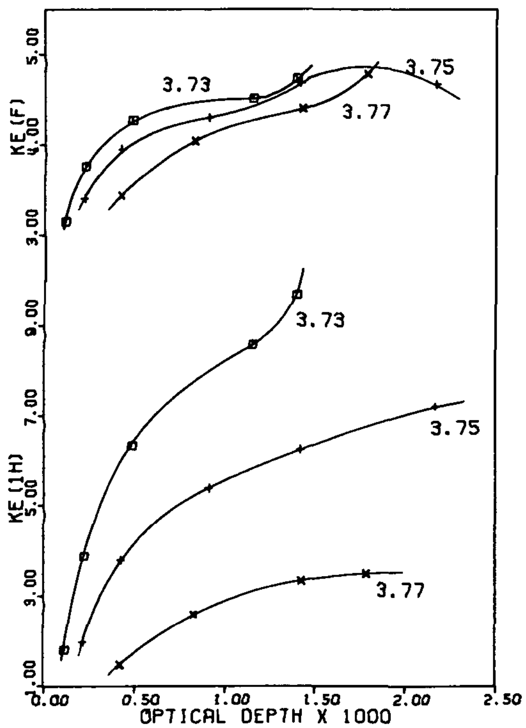

Figure 4. Pulsation energy (KE) versus τ_s X 1000 for models with $M/M_\odot = 0.6$ and $\log_{10} L/L_\odot = 3.0$. The number beside each curve is the value of $\log_{10} T_e$.

and luminosities of these models are comparable to those of the brightest long period Cepheids in globular clusters. For some models, the third harmonic was also unstable. Nonlinear, nonadiabatic calculations are in progress. These results will be published in detail elsewhere.

ACKNOWLEDGMENT

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DISCUSSION DURING PRESENTATION

BEDNAREK:

. . . depending on how wide the Population II instability strip is, this model may be unstable.

COX:

But is it unstable for you?

BEDNAREK:

Yes, it is for me.

COX:

Can you tell us about that scale [in Figure 3]? I'm just amazed. How much is the variation?

BEDNAREK:

For models near the blue edge it is not much more than 1 percent and never more than 5 percent for the first harmonic nor 1 percent for the fundamental.

KING:

Are you changing the zoning on the outside?

BEDNAREK:

Yes.

KING:

Does the zoning inside change?

BEDNAREK:

No, it changes very little as I use finer zoning on the outside.

COX:

Are you sure this isn't an effect of the lower boundary?

BEDNAREK:

The lower boundary is fixed in temperature.

COX:

No, I mean its mass, or fractional radius.

BEDNAREK:

I maintain the lower boundary at the same position at all times.

COX:

What is the fractional radius?

BEDNAREK:

It's less than 0.01 of the radius of the core. I set the radius at the boundary where the temperature is about 2×10^6 K. I have also arbitrarily added 50 zones to the interior regions, and it makes little difference.

KING:

Although it could make little difference in what you're doing here, changing the zoning can significantly affect the growth rates, since in one set you'd pick up a driving zone and in another you wouldn't.

BEDNAREK:

I left the surface zones alone and inserted zones between all the ones below $\tau = 2/3$ and found little difference in the model.

DISCUSSION

COX:

I might say that Iben uses this so-called spline fitting technique in fitting the opacity.

BEDNAREK:

Yes.

COX:

In the work I did with David King and Jim Taylor we didn't get so fancy. We merely wanted to locate the blue edge to some degree of accuracy and find out the helium content of the stars. The difference between us and Iben is due to the fact that he has done it better.

BEDNAREK:

I used a different set of opacity tables.

COX:

Oh, well, then you get anything you want. (Laughter) I don't know what Bob Stellingwerf is going to show but he has a beautiful picture in this thesis, starting with the difference between the Christy formula of opacity which has been widely used and more detailed spline fits, which makes a tremendous difference. At least the crossing of the blue edges of the first harmonic and the fundamental is very sensitive to the opacities.

BEDNAREK:

Yes. If Iben's results are correct, then this destroys the agreement between the observationally determined blue edge and the theoretically determined one.

KING:

Well, but you said that your results diverge from his, and I couldn't really tell exactly where it was but it was out fairly far. Now, where are the RR Lyrae stars with respect to that crossing that you showed?

COX:

They're below.

BEDNAREK:

It doesn't affect the RR Lyrae stars.

KING:

In the case of W Virginis stars, it may very well be that there might be something else in Iben's theory where he differed from us. How he does a linear analysis, I don't really know. He wasn't really addressing himself to those models anyway. What you found, basically, was that those stars up there were pulsing in the fundamental. Right? You had a crossing above where the loop was. Now, those models, since the fundamental edge was to the left of the first harmonic edge, are bound to pulsate in the fundamental.

BEDNAREK:

Yes. If you go to a high enough luminosity, they eventually do cross.

KING:

Well, I don't know how high you would go over there, actually, whether W Virginis stars are there or not.

BEDNAREK:

At slightly lower masses, the crossing again occurs on the lower luminosity.

CHRISTY:

What is the growth rate where this second crossing occurs? My impression is that the region where the instability grows so rapidly . . .

BEDNAREK:

It depends how close you are to the blue edge. If you are close to the blue edge, the growth rate can be as small as you want.

COX:

Oh, yes, but of course. You have to get awfully close. Their e-folding times are from one to ten periods.

BEDNAREK:

Yes, for some nominal width of the instability strip of 0.01, 0.02 in log T_e. Well, up to about 0.01 we can see growth rates of about 30 percent.

CHRISTY:

This tends to be the region where I found this irregular pulsation that did not settle down to a stabilized large amplitude pulsation.

BEDNAREK:

Yes. I think the mass of that variable is possibly too high to fit the Population II Cepheids in globular clusters.

CHRISTY:

Then you must find that, as I was trying to say, over a great range of masses?

BEDNAREK:

Yes.

CHRISTY:

And you would find that, also, for a mass of 1 solar mass or less?

BEDNAREK:

Yes.

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STABILITY OF NONLINEAR PERIODIC PULSATION OF STELLAR ENVELOPES

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The eigenvalue method (von Sengbusch, 1973) to calculate models of periodically pulsating stars can be used to study the stability of the resulting oscillations. This paper will discuss results for a series of RR Lyrae models.

The periodic motion of a stellar envelope is calculated by solving an eigenvalue problem (Baker and von Sengbusch, 1969) as was first proposed by Baker and Lucy*. Other than in the determination of the linear stability, where the equilibrium model is altered only by small disturbances, the nonlinear approach consists of looking for the finite amplitude behavior, where disturbances may no longer be small. A set of models adequately distributed over one period will describe this motion in time, and it is on this set of models that we are iterating. Every two models adjacent in time at $t = t_{k-1}$ and $t = t_k$ are coupled by a set of nonlinear difference equations derived from the differential equations of stellar structure. If each model is described by a vector z ($z^k = z(t_k)$), whose components are the unknown dependent variables of this model, then

$$G^k (z^{k-1}, z^k, t_k - t_{k-1}) = 0 \tag{1}$$

is the general form of these equations. An initial approximate distribution of models will in general not satisfy these equations as well as the demand for periodicity $z(\Pi) = z(0)$. We solve these equations with the linear Newton-Raphson Method by calculating corrections δz for all model parameters from the variation of Equation 1 as follows:

$$\frac{\partial G^k}{\partial z^{k-1}} \delta z^{k-1} + \frac{\partial G^k}{\partial z^k} \delta z^k + \frac{\partial G^k}{\partial (t_k - t_{k-1})} \delta(t_k - t_{k-1}) + G^k = 0 \tag{2}$$

If we assume that the matrix $\partial G^k / \partial z^k$ is not singular, Equation 2 can be given the form

$$\partial z^k = A_k \delta z^{k-1} + c_k \, \delta(t_k - t_{k-1}) + d_k \tag{3}$$

which indicates how corrections at a later timestep can be derived once the timestep and the corrections to the previous model are known. If we simplify the scheme by allowing

*Baker, N. H. and L. B. Lucy, 1968, private communication.

only relative changes in the timestep ($t = \tau \cdot \Pi$ and $\delta t = \tau \delta \Pi$), one easily recognizes that consecutively replacing δz^{k-1} will yield

$$\delta z(\Pi) = A^* \, \delta z(0) + c^* \, \delta \Pi + d^* \tag{4}$$

which couples the corrections of the model at time $t = \Pi$ to the corrections at $t = 0$. If this time Π is assigned to the period of the wanted solution, and we ask for

$$\delta z(\Pi) - \delta z(0) = z(0) - z(\Pi), \tag{5}$$

this will yield an iteration toward the periodic solution. With one additional equation which fixes the solution in phase, and once A^*, c^*, and d^* are built up, $\delta z(0)$ can be calculated.

Experience with this iteration scheme has shown its usefulness, but one must point out that the linearity of the scheme either requires considerably small undercorrection factors or else one has to introduce some nonlinear iteration step which speeds up convergence. Different procedures are now available (Stellingwerf;* Baker and Lucy) for obtaining a best choice of a set of initial models as well as recipes for a nonlinear intermediate iteration, but they will not be discussed in detail here. Once the solution is established ($d^k = 0$ in Equation 3 or $d^* = 0$ in Equation 5), Equation 5 characterizes the evolution of a model perturbation through one period. Eigenvalues λ of A^* with $|\lambda| > 1$ will indicate growth of the associated eigensolution $\delta z_\lambda(0)$ during this evolution and show that the periodic solution obtained is not stable. Figure 1 shows a typical distribution of eigenvalues of A^*. There are many different characteristics of these eigensolutions possible, although the most interesting are those which show close resemblance to the linear eigensolutions of the equilibrium model. The phase angle λ determines eigenfrequency of the solution, and nodes in the eigensolution may correspond to fundamental, first overtone, etc. like behavior of the perturbation. For a sequence of models of 0.7 $M_\odot$, 44.7 $L_\odot$, $X = 0.7$, and $Z = 0.0004$ (Figure 2), the periodic solutions have been obtained and the behavior of the most unstable modes has been studied. Those were for the periodic solutions of the fundamental mode eigensolutions with one mode and an eigenfrequency close to the eigenfrequency of the linear first overtone solution and vice versa for the period solutions pulsating in the first overtone.

Although in a large range of T_{eff} the two types of periodic solutions were obtained, both solutions were never found stable at the same time. It might be expected, however, that in other cases both modes or even neither mode could be stable.

These results have been checked by forward integration techniques. A perturbation of the most unstable mode was, with suitable amplitude, superimposed on the model parameters at the beginning of the period. The evolution of the perturbation was then followed through several hundred periods. The behavior of the resulting motion was studied by Fourier analysis (Figures 1 and 3). In both cases the theoretical excitation rates could be verified. The first overtone periodic motion could be followed as long as necessary to be sure that the switchover to the fundamental mode behavior was clearly established (Figures 4 and 5). The final behavior of the fundamental mode solution was not determined as well.

*Stellingwerf, R. F., 1974, Ph.D. thesis, University of Colorado.

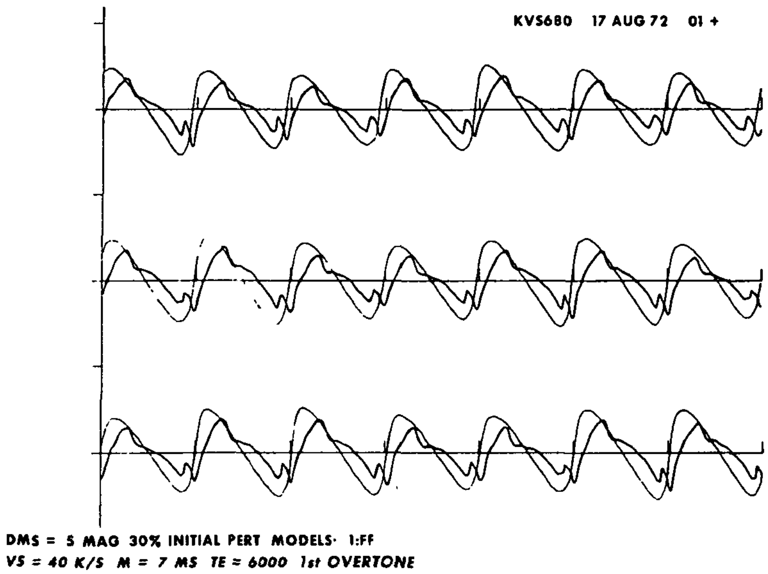

Figure 1. Radial velocity and luminosity at the beginning of the integration of the perturbed model.

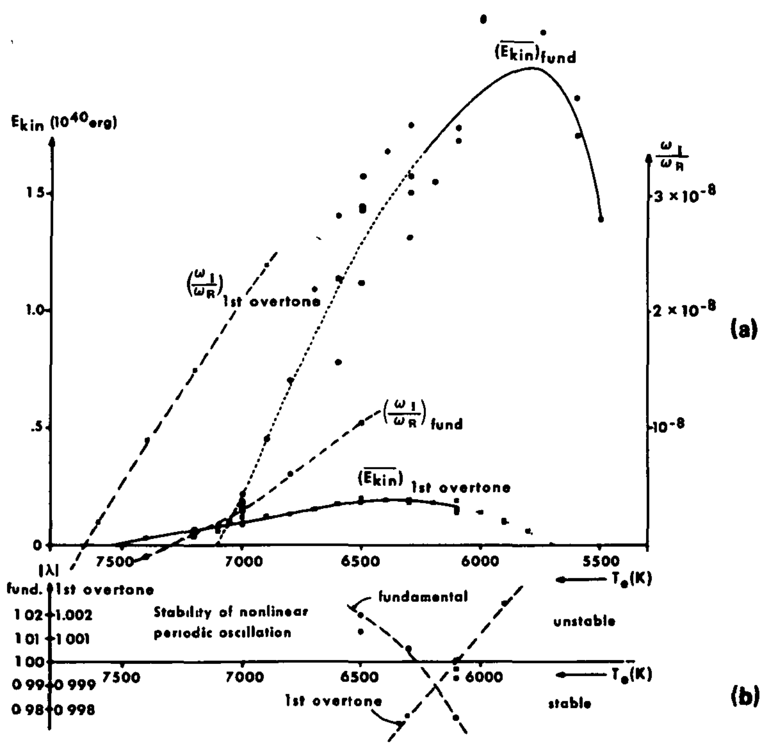

Figure 2. (a) Linear (ω_I/ω_R) and nonlinear (E_{kin}) results of stability analysis for M = 0.7 $M_\odot$, L = 44.7 $L_\odot$, X = 0.7, and Z = 0.0004. (b) Stability analysis of nonlinear periodic oscillations at full amplitude.

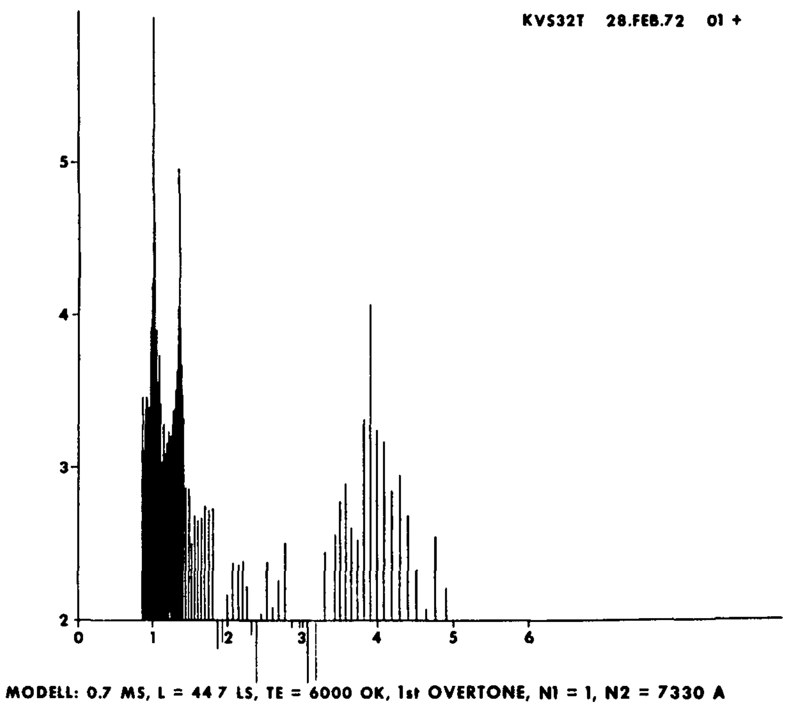

Figure 3. Fourier analysis of first 60 periods of integrations.

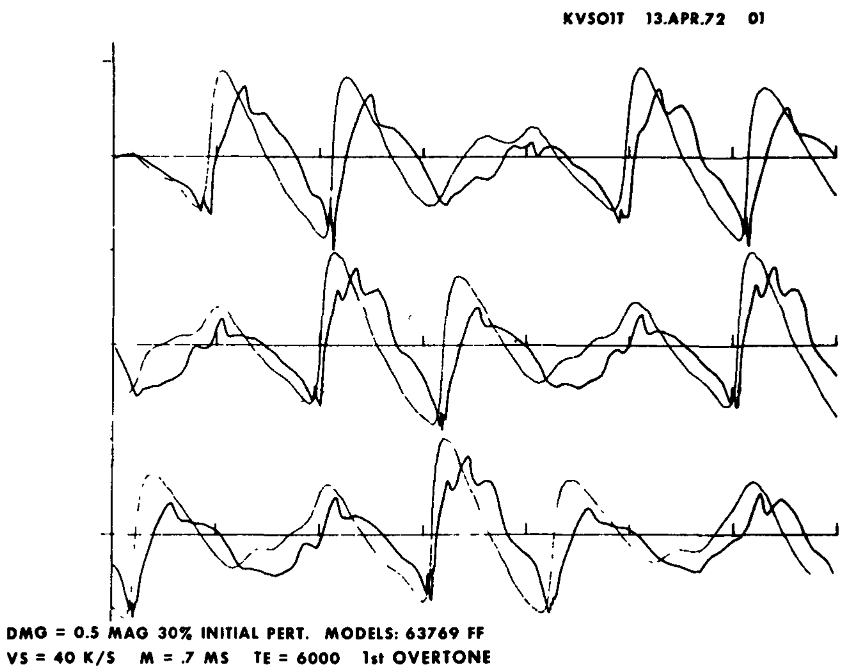

Figure 4. Radial velocity and luminosity after ~200 periods of integration of the perturbed model.

The behavior of the Fourier coefficients corresponding to the two modes under consideration (Figure 6) shows that the fundamental mode is decaying and the first overtone is excited. But after 300 periods of integration the final behavior cannot yet be determined, since the energy contained in both modes is still very different.

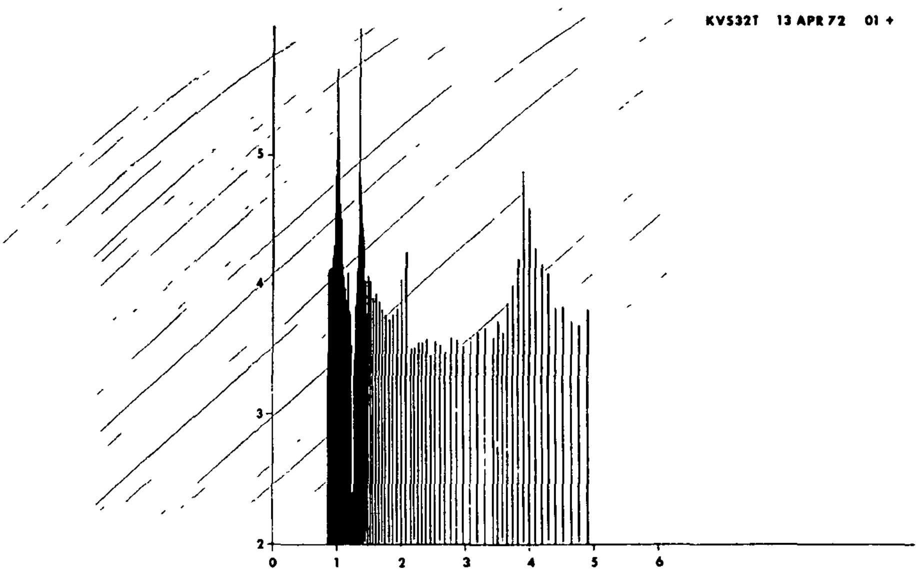

Figure 5. Fourier analysis of 60 periods after ~200 periods of integration of the perturbed model.

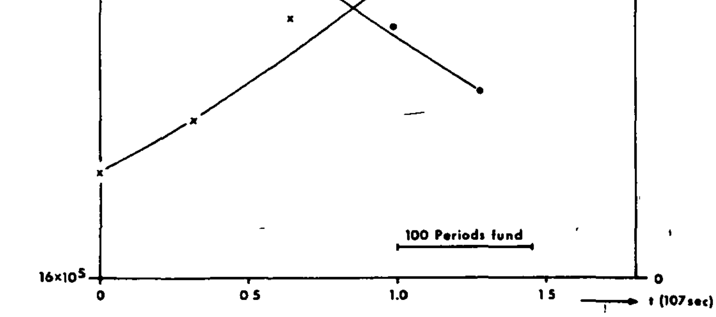

Figure 6. Development of Fourier coefficients for the integration of the unstable perturbed fundamental mode model.

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DISCUSSION DURING PRESENTATION

COX:

Kurt, let me ask you, what opacities were you using?

VON SENGBUSCH:

This RR Lyrae analysis was done with a mixture of Cox 2 and Cox 3 opacities in the Russian tables. Of course, the tables are rather coarse and some of the results are due to the coarseness of the tables, and I will stress one point, we are not very happy about that respect.

COX:

People have concerned themselves with spline fitting and Christy formula and all these things. Do you find that the blue edge positions make a lot of difference?

VON SENGBUSCH:

Yes, that's right. We have checked linear and nonlinear results and used the same table with some kind of interpolations and got rather good agreement. Of course, in all these cases we have not used spline fitting, but I agree that we used the linear code with spline fitting and that it gave completely different results. But in order to be consistent with nonlinear results, we have plotted here the linear results with the same kind of . . .

DISCUSSION

SPANGENBERG:

Is it possible to run very many optically thin zones with this method?

VON SENGBUSCH:

It is no more difficult than any other code.

SPANGENBERG:

So you could do a model with shocks and follow them?

VON SENGBUSCH:

We usually do models, equilibrium models, showing about six to eight optically thin zones. But of course, if you have very much more optically thin zones you get all of these physical difficulties.

SPANGENBERG:

You could have shock complication during the period. You could have trouble with convergence.

VON SENGBUSCH:

No, not really. I mean, there is no difficulty in getting the solution even with shocks.

COX:

Warren, did you have a question?

SPARKS:

Yes. Did you have more slides that you wanted to show when you ran out of time, because we have an extra half hour.

VON SENGBUSCH:

Yes. The point is that most of these things are not related to Cepheids, and they are all used to cover the last statement that stability analysis has been checked by time forward integrations. I do not think that these are too relevant to the subject.

BAKER:

I think it would be very interesting after Stellingwerf's talk to compare things that . . .

COX:

Well, Warren is perfectly right. We have almost an hour before we are slated to break up, and we only have Bob Stellingwerf's speech.

I know what you are talking about–you're talking about the Fourier analyses and these things. They are rather interesting, but why don't we take you at your word at the moment, anyway, and get to Bob, unless we have further specific questions to Kurt.

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THE CEPHEID MODE PROBLEM

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ABSTRACT

The modal behavior of RR Lyrae stars and low mass Cepheids has been investigated using a nonlinear relaxation technique and stability analysis. The advantages of this type of numerical approach in investigations of preferred mode of pulsation are discussed. The results obtained for both classes of variable stars are quite similar: first harmonic pulsation toward the blue, fundamental pulsation toward the red, and mixed-mode behavior at the extreme red edge of the instability strip. In addition, stars near the center of the strip can pulsate in either the fundamental or the first harmonic mode. Possible implications for observational results, including the Oosterhoff dichotomy of globular clusters and the beat Cepheids, are discussed.

Observationally, we know that the modal behavior of stars in the Cepheid strip is quite complicated, but proper interpretation of the observational evidence depends in many cases upon theoretical results. Although standard nonlinear techniques can provide us with some information on modal behavior, these results are always somewhat uncertain because mode switching rates are not known beforehand; it is impossible to know how long to continue the calculation. Today I would like to briefly describe a new approach to this problem that avoids many of these difficulties and outline the type of results obtained to date. For details see Stellingwerf* (1974, 1975).

The approach is basically a sophisticated mechanism for accelerating the approach of a model toward periodic motion. Information obtained in the form of derivatives from forward integration of one period is used to update the time-zero radii, temperatures, and velocities, as well as the period, using the Newton-Raphson algorithm. The mathematical details are based upon a similar scheme developed by Baker and von Sengbusch (1969), and a nonlinear stability analysis as described by von Sengbusch (1973) is employed. This method is applied as follows: Motion is initiated as usual from the static model with an imposed velocity distribution. Single periods are then calculated until the motion is periodic–periodicity to six significant figures is usually obtained in less than eight iterations. Convergence is, to a large extent, independent of growth rate and initial guess. With this

*Stellingwerf, R. F., 1974, Ph.D. thesis, University of Colorado.

method, the periodic limit cycle is obtained even when this limit cycle is unstable and tends to switch to another mode. In the stability analysis, however, growth rates for a complete set of perturbations to the nonlinear solution are calculated, so orbitally unstable modes are detected, and the switching rate is known. These mode switching rates are smooth functions of the model parameters; interpolation therefore produces accurate transition lines, even from a coarse grid of models.

Figure 1 shows the nonlinear eigenvalues (multiplicative scaling factors for each perturbation per period) for a typical RR Lyrae model, fundamental mode. Complex eigenvalues correspond to perturbations with periods different from the fundamental period–these generally correspond to other modes, as indicated. One eigenvalue falls at (1,0) and corresponds to a pure phase shift (the accuracy in this case serves as a check of the overall accuracy of the calculation). Other real eigenvalues correspond to a pure amplitude change (the pair at 0.98) and to modifications to the thermal/shock structure in the ionization zones. Only the largest eigenvalues are shown. Since all moduli are less than unity, this is a stable, or "good," mode. As parameters are varied, we are primarily concerned with the 1H eigenvalue; if its modulus exceeds unity, the model will switch toward the 1H. Of similar importance is the fundamental perturbation to the 1H limit cycle.

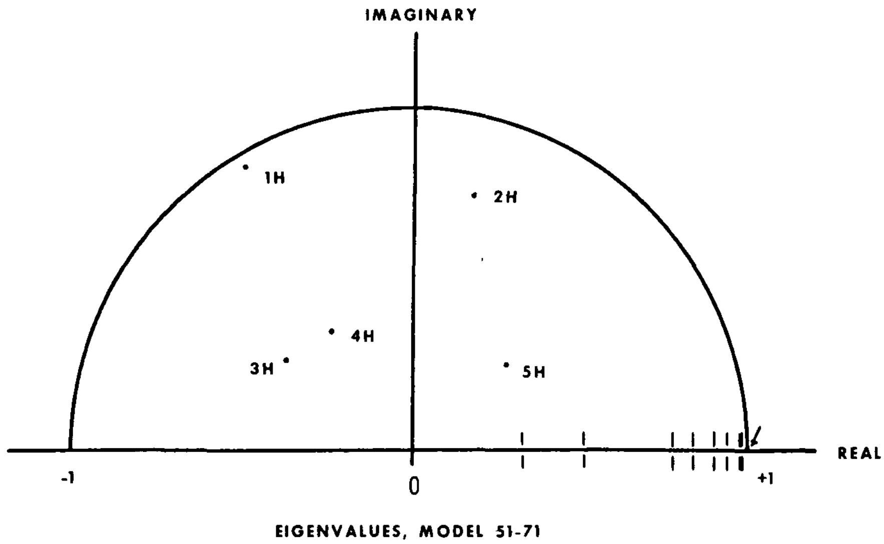

Figure 1. Distribution of the largest eigenvalues on the complex plane for a typical fundamental RR Lyrae model.

As effective temperature is varied for a given luminosity, mass, and composition, we can combine the nonlinear switching rates with the linear growth rates to obtain a "composite growth rate diagram" as shown in Figure 2, again for RR Lyrae models. Here the solid lines are linear growth rates (F and 1H), and the dashed lines are nonlinear switching rates (F in 1H means switching rate of 1H toward F). A dashed line passing through zero defines a "transition line" (the actual line is generated by varying the luminosity); three such lines are

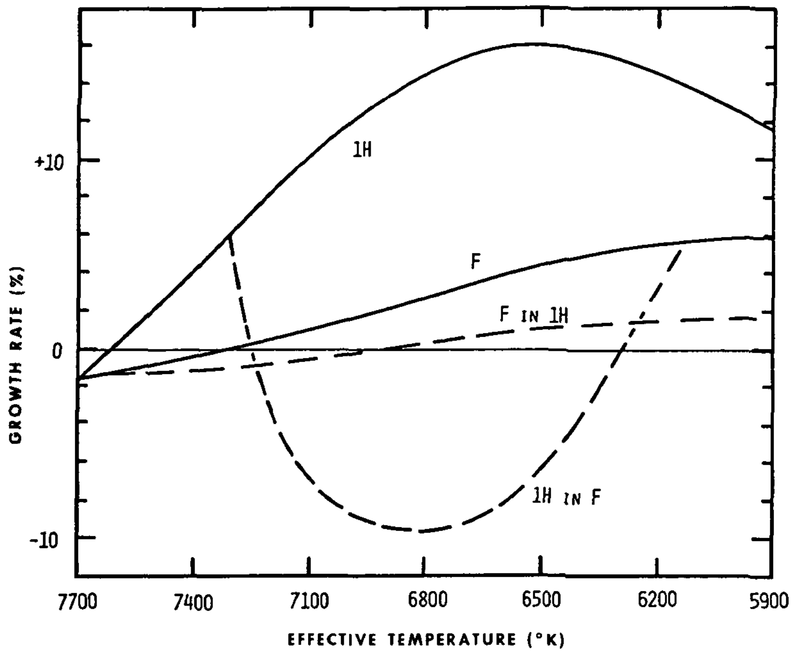

Figure 2. Composite growth rate diagram for a sequence of RR Lyrae models with $M = 0.578\ M_{\odot}$, $X = 0.7$, $Z = 0.001$, and $L = 2 \times 10^{35}$ erg/s.

indicated by this diagram. These lines delineate regions in the H-R diagram of different modal behavior, the types indicated here being (left to right) 1H only, F or 1H, F only, or *neither* F nor 1H. The last category can be shown to correspond to an aperiodic mixture of modes and occurs quite near the observational red edge of the instability strip. Only one case of an RR Lyrae star showing this type of behavior is known. The effect of the "either-or" region (near 7100 K) is that the position of the observed F/1H transition lines will depend upon direction of evolution, being located either at 7250 K (blueward evolution) or at 6950 K (redward evolution). This is consistent with the hysteresis mechanism proposed by van Albada and Baker (1973) as the explanation of the Oosterhoff dichotomy of globular clusters.

The shape of the various regions in the H-R diagram is shown in Figure 3. The transition lines show a tendency to run parallel to the blue edges in the region of interest. At high luminosities the fundamental dominates, at low luminosities the either-or region tends to expand. Although not shown here, the 2H is expected to appear (according to linear results) at the extreme blue edge of the strip below $M_{bol} \approx 1$. Van Albada and Baker (1973) suggest the detection of 2H pulsators in M68, also appearing toward the blue.

Finally, I would like to say a few words about mixed-mode Cepheids. Peterson (1973) pointed out that the knowledge of the F and 1H periods for an observed star allows an accurate determination of the mass and radius (the periods depend primarily on M and R alone). This method as applied to the RR Lyrae star AC Andromedae (Stellingwerf, 1975)

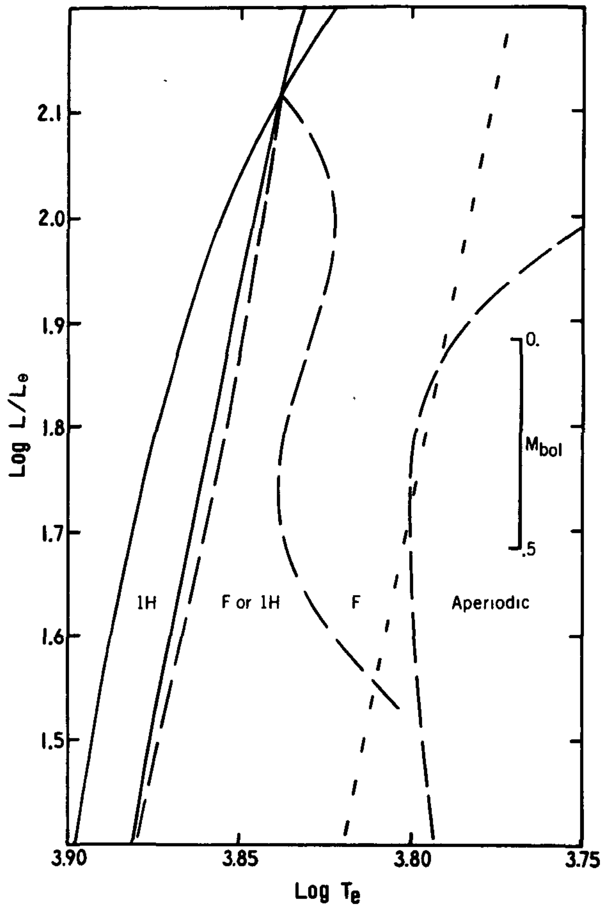

Figure 3. Transition lines and linear edges in the H-R diagram for RR Lyrae models with M = 0.578 $M_\odot$, X = 0.7, and Z = 0.001.

yields the reasonable mass 0.6 $M_\odot$, but when applied to the beat Cepheids, masses in the range 1 to 2 $M_\odot$ result, the expected evolutionary mass being about 4 $M_\odot$ (see King et al., 1975). In view of this discrepancy, it is of interest to know if the nonlinear models permit mixed-mode behavior at the pulsational mass. Figure 4 shows the composite growth-rate diagram for a 1.6 $M_\odot$ model with King 4a composition (X = 0.7, Z = 0.02) taken along the constant period line $P_F \approx 2^d.5$. The overall similarity to the RR Lyrae diagram is evident. Among various slight shifts, the mixed-mode region occurs nearer the blue edge, so mixed-mode stars can be expected near the red edge of the strip in agreement with temperature determinations for U Triangulum Australis and β Doradus (Rodgers and Gingold, 1973). The major uncertainty in these predictions for low-mass stars is, of course, the effect of convection—especially since it appears to be a toss-up as to whether mixed-mode behavior is encountered at all before arriving at the red edge. Although this argues against a detailed theoretical interpretation at this time, it is nonetheless encouraging that the nonlinear models exhibit the full range of observed modal behavior.

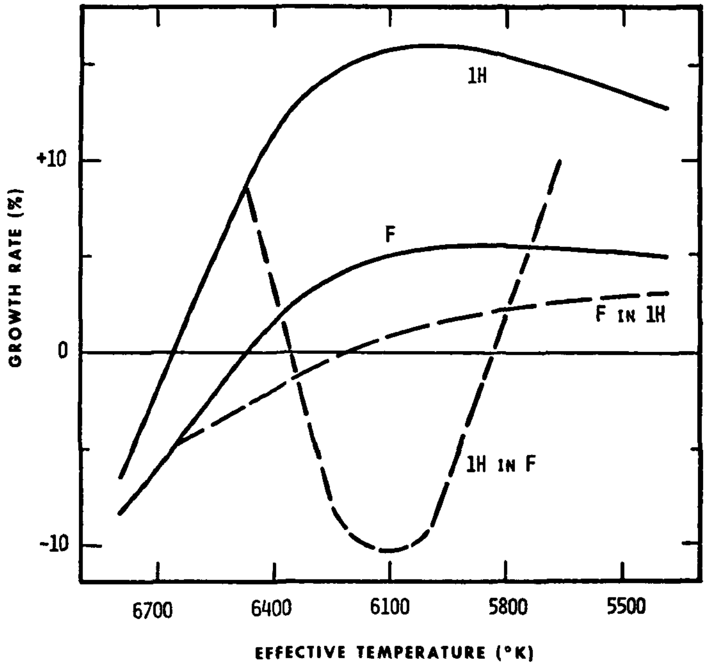

Figure 4. Composite growth rate diagram for a sequence of beat Cepheid models with $M = 1.6\ M_{\odot}$, $X = 0.7$, $Z = 0.02$, and $P_F = 2\overset{d}{.}5$.

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DISCUSSION DURING PRESENTATION

CHRISTY:

The stability that you show on that first diagram [Figure 1] is for the first harmonic calculated on a stationary star or is it the first harmonic calculated on the fundamental?

STELLINGWERF:

This is the first harmonic perturbation to the fundamental limit cycle.

CHRISTY:

It is based on the fundamental as the basic oscillation. This doesn't say that the first harmonic might not be unstable if you calculated that as your basic.

STELLINGWERF:

No. In fact, this model is unstable in both modes at small amplitudes and stable in both modes at large amplitudes. So this is in the either/or region where it can pulsate in either mode depending on initial conditions.

COX:

I misunderstood those hash marks.

STELLINGWERF:

There is an eigenvalue on the real axis at each point indicated [Figure 1].

COX:

Are there an infinite number of them?

STELLINGWERF:

No, you have as many eigenvalue values as you do dimensions in this matrix. In a real star you have an infinite number of perturbations that you can apply. But there is a finite number of linear-independent perturbations that you can apply with a finite zoning.

COX:

Is there the same number of those as zones?

STELLINGWERF:

As variables. I have three variables per zone. There are 89 eigenvalues in this particular problem.

COX:

But they are referring to perturbations which ultimately lead to a fundamental period again.

STELLINGWERF:

All the real eigenvalues, yes. In other words, one of these here, I investigated a little bit, and what it corresponds to is just taking all the zones above the hydrogen zones and adding a constant velocity, so this has the effect of changing the way hydrogen interacts with the zones above it. Otherwise, you get very nearly periodic motion; very close to the fundamental.

BAKER:

Every relative motion the zones can undergo will be represented somewhere there. Just as if the thing becomes continuous, then you go to an infinite number of nodes, some of which have practically zero wavelength. Whereas, here the limit on the size of the wavelength is the zone size.

STELLINGWERF:

These real eigenvalues correspond in some way to secular eigenvalues in the normal pulsation problem.

CHRISTY:

Right now, how close those are to one will tell me how fast my system will relax to a stable mode.

STELLINGWERF:

Right. The difference between the modulus of the eigenvalue and one tells you how fast that thing will die away as you integrate. In this case the amplitude variation is the last one to die away. That is not always the case.

* * * * *

STELLINGWERF:

Basically, what I wanted to point out was that this stable mixture of modes which you can see on the temperature curve [Figure D1] does permeate the whole model. The modes

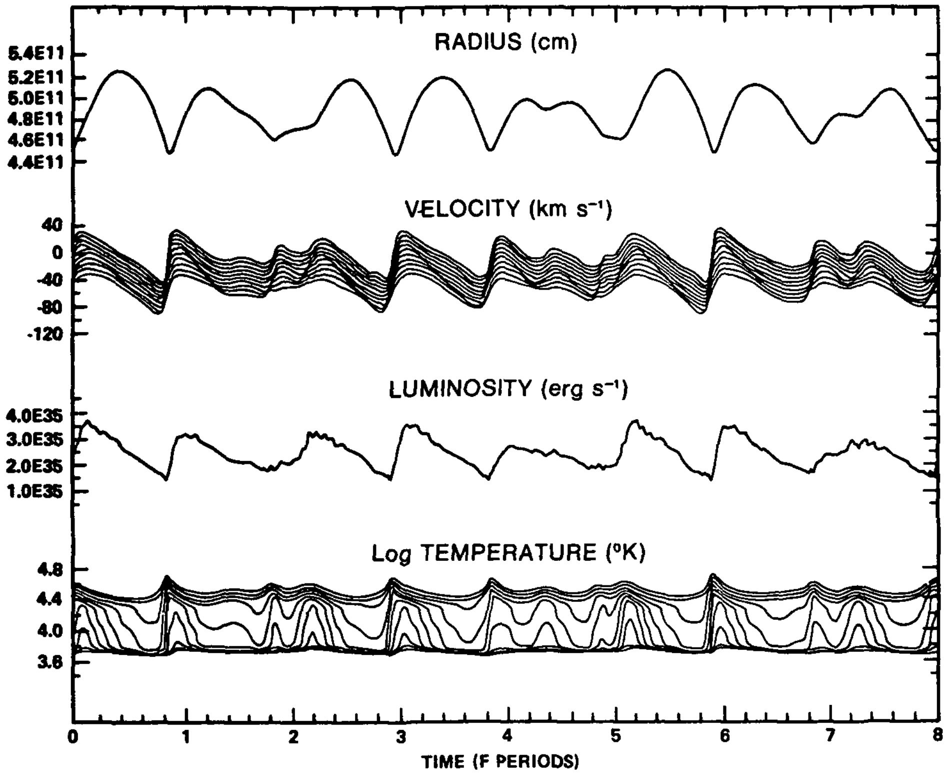

Figure D1. Radius, velocity, luminosity, and log temperature for eight fundamental periods of the mixed mode model. Successive velocity curves are shifted by 6 km s^{-1}.

themselves can be represented quite closely by just a straight combination of the limit cycle and the fundamental limit cycle in the first harmonic. There is a nonlinear interaction, but it is really not all that big. You can just add both of those velocity curves together and get something that looks very much like this velocity curve.

COX:

But that's because mostly it's a low amplitude pulsator, isn't that right?

STELLINGWERF:

Yes and no, actually. The velocity amplitude turns out to be very close to the sum of the two velocities of the two modes, but that's not true for the luminosity.

COX:

What I'm worried about is that you were saying you can decompose this thing by the very classical methods and get the individual periods and the individual light curves as if they were isolated. Whereas, I'm sure if you have larger amplitude variations, larger amplitude stars, the interaction of these two modes will actually upset the periods, amplitudes, and everything else, and observers should be careful if they are going to decompose some extreme light curves and not just use linear peiods, for example.

STELLINGWERF:

That's quite right. The period changes are likely to be larger for Cepheids, let's say. This is an RR Lyrae model. This amplitude is not negligible; it's a magnitude and a half, or something, and I tried decomposing this and I did get within a percent of the normal periods. I have a feeling that that is probably something like the errors you can expect. Of course, the nonlinear effects do come in very strongly and very suddenly. That's not to say that if you didn't go to a slightly larger amplitude you wouldn't get much worse results. So you're right.

BAKER:

But there again, to the extent that you can look at this thing as nonlinear interaction between the modes that more or less have the linear periods, you found in this case that the higher modes are pretty well damped so that, while you certainly will excite some of these higher ones in the periodic mode, you expect that excitation to die out rather rapidly. In another case that might not be true.

STELLINGWERF:

That's correct. That would definitely complicate matters, if that were true.

COX:

What I'm grasping is whether Dave King and Petersen have done the right thing in decomposing light curves of beat Cepheids. They have assumed the periods they can stick into the business just from linear theory, and yet these stars are wiggling around in these mixed modes and maybe everything is affecting everything else. It's dangerous to use linear velocities here.

STELLINGWERF:

The way I checked this is that I actually did the same type analysis as Stobie did to decompose into the two modes and check the two periods. As I said, in this case they do agree very closely with the linear numbers. Probably a good estimate of how well this agreement will be is just to compare the nonlinear and linear results. I have a feeling that you aren't going to get any more problem this way. There doesn't seem to be too much interaction, as I said.

KING:

Probably the only way to tell, for the double-mode Cepheid, is to do the nonlinear model that we think best represents it, then decompose that and see if you get the same result.

STELLINGWERF:

Analyze it, yes.

COX:

When you go from linear to nonlinear theory you increase the period a little bit in both fundamental and harmonic modes. So maybe the ratio stays okay. On the other hand, I'm worried about the interaction of these things. It may be bad enough to really upset things.

STELLINGWERF:

It's possible.

COX:

I don't know. I guess David wouldn't allow me to speculate on that to solve the mass discrepancy.

STELLINGWERF:

You would need quite a bit of shift.

SPANGENBERG:

Did you say the two velocity distributions for harmonic and fundamental added together pretty well but that the light curves didn't ?

STELLINGWERF:

Well, the velocity amplitudes are about 20 in both of those, and, as you can see, it adds up to about 40. The luminosity doesn't quite add up, so there is nonlinear interaction present. You would get a somewhat larger luminosity amplitude by just adding them up.

BAKER:

I think in connection with this it might be interesting to see Kurt's Fourier analysis.

COX:

I had that in mind. Let's let Bob finish here and we can go back to Kurt.

STELLINGWERF:

I have only one more point to make, actually, and that is in reference to Figure 4. Dave King described the mixed mode analysis. These were the growth rates when I did the mixed-mode models. These are based on points at 5800 and 6100 K so this is in part schematic. These are the growth rates along a constant period line, fitted to the period of U Triangulum Australis which is about 2.6 days, I believe. And these models all have a mass of 1.6 solar masses. This shows where you would expect to find mixed mode models. This region of stability for the fundamental is somewhat narrower than the RR Lyrae case, so you may just have a little bit larger region here in mixed mode behavior.

COX:

Before the red edge?

STELLINGWERF:

Before the red edge.

COX:

That's an awfully small region there, where you have Christy type 3 behavior.

STELLINGWERF:

As I said, this is based on points here and here [at 6400 and 6100 K], so that's pretty much uncertain.

COX:

Maybe it is 100 K. That's what Kurt got, about 100 K.

STELLINGWERF:

I'm not sure about the slope of this line[1H in F], for instance. I don't have any points in this region [near 6300 K]. So I wouldn't base too much on that, but I do have a point right here [at 5800 K], so I knew where the aperiodic behavior began. The basic overall behavior of these growth rates for this low mass Cepheid case is very similar to what happened to the RR Lyrae case; although these are approximate models in some sense of the word, this may very well be what you find for the higher masses for the Cepheid as well. This would mean that the reason you do not get mixed mode behavior with the higher masses must depend on either moving that line [1H in F] or some sort of interaction with the convection, which is quite possible as well and probably should be checked.

COX:

Is this a 1.60 solar mass?

STELLINGWERF:

A 1.60 solar mass model, at 2.6-day period—this is quite far down the instability strip. Although the effects of convection do influence the situation, at least this result presents an alternative to the hypothesis that the star observed with a mixture of modes must be at this point or at this point (i.e., in the process of switching modes). And a simple calculation will show that a model evolving across the diagram with an evolution rate given by Iben and growth rate slopes that are given by this line [F] or this line [1H], you find that the switch occurs in 100, 200, or perhaps 300 periods.

The mixed-mode stars that we know of haven't been all that well observed, and it would be interesting to see whether any changes of the relative amplitudes do occur. This would probably be visible if the star were at this point. They should be changed secularly with time. But not if the star were at that point, so that would be an interesting thing to have.

BAKER:

Of course, you do have in the clusters the irregular variables that are more or less near the transition lines quite frequently—I am talking about RR Lyrae stars now. Often you find the stars just about where you switch between the Bailey type c's and the type a's and b's, and that a number of those stars are irregular.

STELLINGWERF:

Yes.

BAKER:

What that means in these terms, I don't know.

STELLINGWERF:

Well, you certainly will have stars. If the stars are evolving across, they must switch from fundamental to the first harmonic at some point. If you have enough stars you will be able to see that.

BAKER:

They are not likely to be ones that you caught in the act. This is only for a hundred periods.

STELLINGWERF:

Right. It depends on both the growth rates and the evolutionary rates, so you can only make rough estimates of that. It would be nice if these things turn out to be very stable, to actually have a nonlinear area in which they could stay showing a mixture of modes.

DISCUSSION

KING:

The thing I was wondering about is the physical reason for it. If you go from the blue first harmonic into the fundamental, you tend to get a feeling for physically why this happens. When you get over there, what physically causes the things to go from, say, being fundamental to being aperiodic?

STELLINGWERF:

That's a difficult question. About the only thing that I can think of, when you talk about the physics of the situation, is that we have a difference between the fundamental and first harmonic, obviously. The difference in the nonlinear results [Figure 4] shows up as a difference between this slope at the blue edge and this slope at the red edge; one is positive and one is negative. Intuitively, the only difference between the modes is the presence of the node, so that must cause it in some way. However, Iben and Tuggle have shown that nodal quenching for this type of luminosity doesn't come in in the normal way.

COX:

Well, at linear amplitudes but not at nonlinear.

STELLINGWERF:

The next slide [Figure D2] shows the work of the interior and the hydrogen zone and the helium zone in the fundamental and the first harmonic as functions of effective temperature for models in that particular sequence that I showed you. Now, if nodal quenching were present, you would expect to find the helium zone work for the first harmonic, the upper right, to taper off rather rapidly toward the red. Instead, you find that these curves here [F] and here [1H] have slight shifts, but they are really very similar. So that doesn't look like nodal damping in the usual sense. This is linear work in units of the luminosity times the period.

COX:

What was the bottom curve?

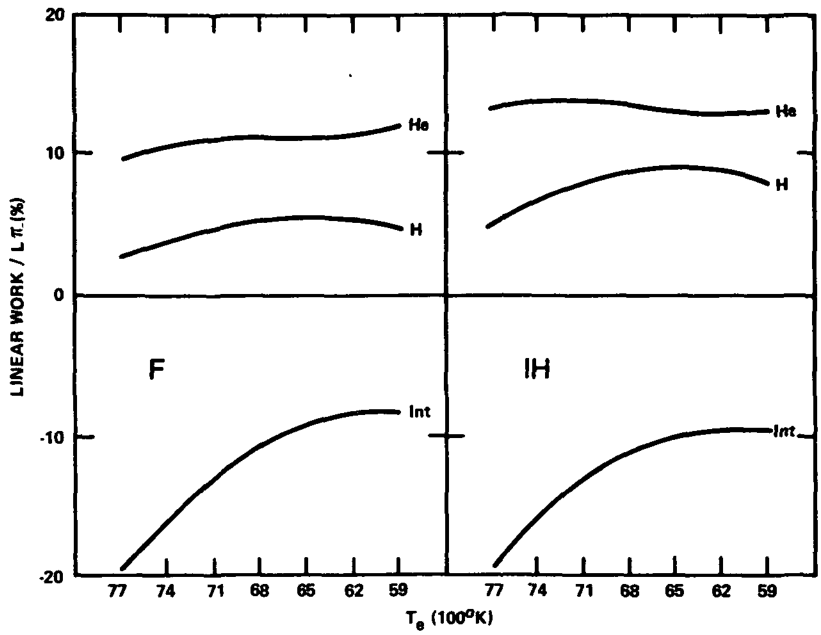

Figure D2. Linear work of the hydrogen ionization zone, the helium ionization zone, and the interior for the fundamental and the first harmonic as functions of effective temperature.

STELLINGWERF:

Interior damping. I just divided the work up to three terms.

CHRISTY:

Do you have the final amplitude for the fundamental and the first harmonic plotted across here [Figure D2], too? You have shown growth rates, but I haven't seen final amplitudes of the final stage.

STELLINGWERF:

I don't have a slide on it. They reach a maximum and then they taper off. This is the standard type behavior. I do have one idea for this; and that's shown in the following slide [Figure D3]. There is a definite difference if you plot the linear kinetic energy of the first harmonic and the fundamental. It turns out that in regions of low linear kinetic energy these numbers are luminosity, but as you go to higher luminosities, the linear kinetic energy tends to come down. Where linear kinetic energy is low, you tend to have stable fundamental modes. Where it's high, you tend to have unstable first harmonic modes.

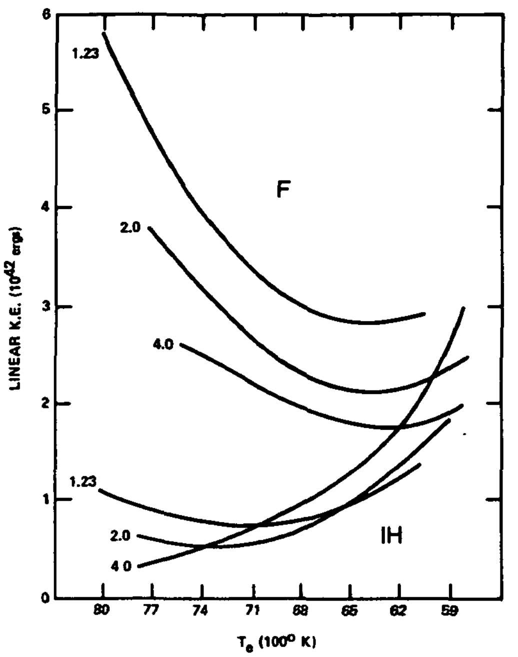

Figure D3. Linear kinetic energy of the first harmonic and the fundamental as functions of effective temperature.

As you can see, that does go through a minimum here, the fundamental. It starts to climb up a little bit. All these linear eigenfunctions have been normalized to unity at the surface. Perhaps this increase in the linear kinetic energy, which occurs at about the same place as the minimum switching rate, somehow reflects the inertia of the envelope and tells us how the kinetic energy goes for a fixed amplitude. What it means, the kinetic energy going up a bit, is that the fixed amplitude in the ionization zones have to drive more inertia in the envelope itself. It appears that when the inertia of the envelope goes up it wants to switch to another mode, perhaps more favorable conditions. This is very sketchy, but it's the only kind of reason I can come up with for that change. This was suggested, a sort of overall mechanical behavior of the envelope itself that causes that behavior.

COX:

All of these models are unstable, is that right?

STELLINGWERF:

At low amplitudes.

COX:

These are all linear kinetic energy which means that they are all growing? Over this whole band?

STELLINGWERF:

No. This is linear kinetic energy, which means that I have just done linear analysis. Independent of what the growth rate is, I find this for the kinetic energy of an envelope normalized to unity at the surface.

COX:

Can you plot the instability strip on there [Figure D3]?

STELLINGWERF:

You can easily do that by comparing with the H-R diagram [Figure 3].

COX:

There were a couple of rules of thumb given by King, Cox, Eilers, and Davey that if the fundamental was, in the linear theory, driven twice—what is it, twice?

KING:

The growth rate of the first harmonic was about twice the fundamental.

COX:

. . . then it would be in the fundamental.

STELLINGWERF:

This agrees with that in sort of a general way. You notice that at high luminosity, the fundamental kinetic energy is low, and the first harmonic kinetic energy is high. So the fundamental is favored. At low luminosities you get just the opposite, so the first harmonic tends to be the more favored mode.

COX:

How have you been able to find out from your work how you characterize, from the linear theory, which models would be in the fundamental and which would be in the harmonic?

STELLINGWERF:

This is the best I could do. It is very crude. It gives you sort of a general idea. I haven't come up with any way of predicting that, no.

COX:

Let me go back and let Kurt show his switching. I don't think that you have done that, having actually followed in time, except for the painful process of watching one pulsation go and switch to another. I know that Kurt has done it, I guess in a couple of cases.

VON SENGBUSCH:

We have done two cases, yes.

[Von Sengbusch's discussion on Fourier analysis has been removed because he presented it in his paper, although he deleted the material in his talk.]

COX:

Let me ask a question of Kurt and Bob. I guess you both firmly believe that there is in reality the so-called Christy type 3 behavior at the RR Lyrae luminosity. Yet this is not observed in any cluster except possibly ω Centauri.

STELLINGWERF:

If the stars are all evolving in one direction of course you would not observe this.

COX:

The evolution boys say that isn't the case. They go both ways.

STELLINGWERF:

I think I've changed Icko's mind about that. I think he will at least accept the possibility.

* * * * *

COX:

Did you find the growth rate of the fundamental into the harmonic?

VON SENGBUSCH:

I did not really check on that. . . . because the switch would appear and I did not check on that. But in the other case we were somewhat suspicious about whether the motion really shows a calculated behavior. On the stability analysis we checked and . . .

COX:

The point I'm making is, there was a point that was made earlier, that the growth rates from the linear theory seem to be kind of hard to get with precision. It depends on zoning and things, as we heard.

VON SENGBUSCH:

Well, I only checked this to be of the correct order of magnitude.

COX:

Yes, that's right. Actually, if you start with, not the stability of one mode against another, but just the growth rate of a single mode, you will find that agreement between the linear and the nonlinear theories in the growth rates is rarely better than a factor of 2 or so. If you really work hard at it, you might get it down to what–30 percent? It's hard to actually get the growth rate of any of these things. Your problem, I think, would be more complicated, because you have a mode growing already and you are testing the stability of that mode.

VON SENGBUSCH:

On the other hand, the effect of zoning is not so prominent because coarse zoning would make for difficulty, especially at low amplitudes where a zone is not really moved through the hydrogen ionization region. There, the difference between linear and nonlinear results is much more prominent. Here, the linear excitation rates are a result of the nonlinear finite amplitude motions and contain all the information which . . . And I would think the difference should not be that big.

SPANGENBERG:

I guess I would say that comparing my work with Bob's would support the same thing, because some of the models which I've had persist in the first harmonic at rather red temperatures, which Bob's stability analysis would indicate should be unstable in fundamental perturbation, but they are very close to the hemisphere from plus to minus one . . .

COX:

So they would switch modes very slowly?

SPANGENBERG:

And, indeed, over what I could afford to run them, the harmonic persisted, even though one would like to see them switch.

COX:

So what kind of agreement in these growth rates would you say—within a factor of 2 or so, maybe?

SPANGENBERG:

I think within that [is about right]. The agreement becomes a lot better when you go over toward the blue side where things are varying faster. Then you can get fundamentals to switch to harmonics rapidly, as one would expect.

COX:

Well, for those of us who doubt everything, it would be fun if you guys sometime could take a mode which is unstable to the other mode, very greatly unstable, and then follow the direct nonlinear integration and watch it really move. The reason why I expressed a doubt is that you have, after all, nearly linear stability analysis of the nonlinear mode and it may not go; and maybe, if I can always grasp at things, that may still explain the very sharp transition line observed in the globular clusters.

BAKER:

Somebody will have to ask for a very large grant for computer time to do that. (Laughter) [A. Cox was on one-year temporary appointment to NSF at the time.]

COX:

Not necessarily. Oh, I see. But, of course, Norm, those switchover rates for the RR Lyrae stars are hundreds of periods. What I'm asking is to try to find a case where you haven't switched completely from one mode to the other in an e-folding of about 10 periods.

BAKER:

Do you think there's a chance there are any like that?

STELLINGWERF:

I had switching rates of 5 and 7 percent, so I expected switching in 10 periods but it took 400.

COX:

You told me the agreement was good.

SPANGENBERG:

That was a very small perturbation. You start with a very small perturbation to make sure you are at the limit cycle. It is cheating to put in a big perturbation.

COX:

Sure, sure . . .

BAKER:

The eigenvalue only tells you how to start out . . .

COX:

That exactly is my point. To really get there . . . I am not sure anybody knows how to really get there. I guess nobody has ever followed it. Kurt has done the closest.

WORKSHOP

WORKSHOP FIGURES

WORKSHOP PROCEEDINGS

During the Workshop proceedings, the model makers discussed the various parameters used in their models. The following list indicates the code designations for the models discussed. These codes are referred to throughout the Workshop and are used to identify the Workshop figures.

MODELER	ABBREVIATION
Bednarek	Be
Christy	Ch
Cox and Davis	CD
Fischel, Sparks, and Karp	FSK
King	Ki
Spangenberg	Sp
von Sengbusch	Se
Stellingwerf	St

Table W1 was created during the Workshop and is referred to as "table" throughout the following discussion. It was displayed on a blackboard and the data were contributed by each speaker during the first portion of the Workshop. The phase shifts in Table W1 are relative to the shortest period models: FSK for 1.24×10^{37} and Ki for 1.1×10^{37} erg/s.

The model marked Be II, included in Table W1 and in Figures W3a and W4a, is a recalculated model by Bednarek submitted after the workshop. He is not the first to be caught by a subtle "bug" and, by no means, the last, so we are happy that the Workshop provided the symptomatic diagnosis to catch the problem. Discussion of the Be model is preserved to maintain as much continuity as we could, and to provide an insight for diagnosing such problems in the future. (See the Appendix.) We sincerely appreciate Dr. Bednarek's submission of the corrected model and discussion.

FISCHEL:

Yesterday went very well, thanks to the speakers and our two chairmen. Today we have viewgraphs of all of the graphical material that the model makers have provided. Projectionists in the back will be able to overlay any combination of viewgraphs. The abbreviations we have arbitrarily added to all of the graphs are on this blackboard [See Introduction]. There are two models by von Sengbusch; the one with the King opacities is marked Se V.

Table W1
Summary of Models

	Be Bednarek	Ch Christy	CD Cox & Davis	FSK Fischel, Sparks, & Karp	Se III von Sengbusch	Se V von Sengbusch	Sp Spangenberg	St Stellingwerf	Ki King	Be II Bednarek
No. zones/ No. Thin Zones	36/4	42/4	72/11	100/25	48/7	48/7	59/14	29/4 (46/4)	50/3	36/4
Opacity	Russian Tables Linear interpolation	Christy Opacity X = 0.70 Y = 0.28	Kip 1a Tables Linear interpolation	King 4a Tables Linear interpolation	Kip 1 Tables Linear interpolation	King 4a Tables Linear interpolation	King 4a Tables Spline (cubic)	Formula based on King 4a	Kip 1 Tables Linear interpolation	Russian Tables Linear interpolation
Viscosity	Q1	Q1 Linear at Bottom	Q4 Cutoff at 1%	Q Variable	Q1	Q1	Q4 Cutoff at 10%	Q4 Cutoff at 10%	Q1 Linear at bottom	Q1
Program	Christy	Christy	VERA	Kutter-Sparks	Eigenvalue von Sengbusch	Eigenvalue von Sengbusch	nonequilibrium diffusion	Eigenvalue Stellingwerf	Los Alamos	Christy
Number of Periods	97	25 with acceleration	200	50 with acceleration	10-15 iterations	10-15 iterations	95	8 iterations	150 with acceleration	0.92
Period (days)	10.7	9.59	8.8	9.55	8.84	9.73	9.88	9.98	8.8	9.87
Luminosity ($\times 10^{37}$ erg/s)	1.24	1.24	1.1	1.24	1.1	1.24	1.24	1.24	1.1	1.24
T_{eff}	5700	5700	5715	5700	5700	5700	5700	5700	5715	5700
Fractional Radius at Bottom	0.15	0.09	0.17	0.16	0.10	0.10	0.09	0.58 (0.09)	0.07	0.15
Boundary Condition	P = 0	Pr at Surface	Extra Pr	Pα	P = 0	P = 0	Castor	Castor	P = 0	P = 0
Christy Parameter		0.24	0.24	0.22	0.23		0.24	0.16	0.24	0.24
Shift	FSK + 0.04	FSK + 0.415	Ki – 0.045	0	Ki + 0.08	FSK – 0.05	FSK – 0.08	FSK + 0.13	0	FSK – 0.15

If you would like, we can view the one with the Kippenhahn mix (Se III), which would also give us a comparison of the mixes.

There will be students noting the speakers and recording on the blackboard significant points of comparison between different techniques and different approaches to solving a given problem. I have no a priori prejudice as to how to conduct this workshop. I will leave that to Norm.

BAKER:

I think what I want to do is to more or less to stand back and let the fur fly. So often we have had numerical work–comparable numerical work–done by different people in which one is just not able to sort out why one paper differs from another. You cannot tell clearly why the details of one person's calculations differ from those of another. I hope today we will have a chance to really sort out the differences. We have the model makers here and they can be asked exactly what they did do.

Dave Fischel suggested that each person on the list talk about their models for about 10 minutes. I would think that in cases where the work has been published previously, or where the methods were described yesterday in some detail, it will only be necessary to add a few things for clarification. We will leave that up to the speakers, but at least we could have a bit of an introduction if each of you would tell what you do that you think is different from that done by most of the others.

Approximately 10 years ago, all of this was begun by Bob Christy, so I think it appropriate that he start this Workshop off and talk about what he has been doing. In particular, Bob, I suppose there isn't anyone here who isn't familiar–very familiar–with your work, but you might describe specifically what you do differently these days.

CHRISTY:

Unfortunately, I don't have very much to say. Because of pressures of other duties, I haven't been able to pursue this work, particularly in recent years. [Dr. Christy is Provost of the California Institute of Technology.]

The method that I used, I think is familiar to all of you. Basically, it has not changed. In the models that we are going to describe, I used my opacity formula as I have in others, although I put in whatever the appropriate hydrogen and helium abundances are. In particular, I got the tabulated opacities that we are supposed to use with $X = 0.7$ and plotted them in comparison with my own, just to see what the distinction was. I might point out the basic features of those plots.

The basic features of the opacities are as follows: If I plot log opacity against log temperature for different densities, [see Figure W1], there tends to be, for a given density, a very rapid rise in opacity in the photosphere: a peak somewhere between 4 and 4.5 in log T; a decrease,

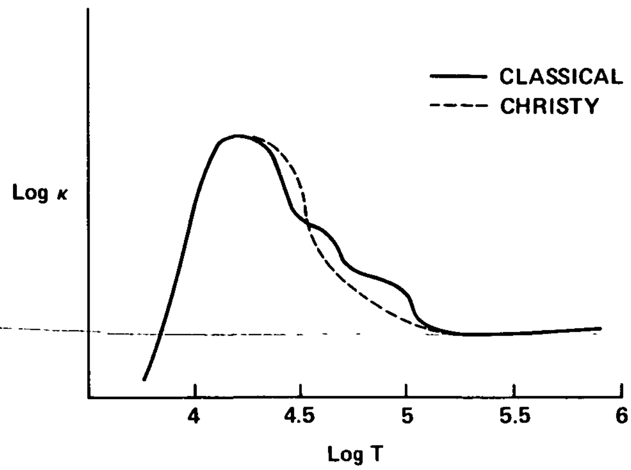

Figure W1. Log opacity versus log temperature.

a secondary peak, more or less noticeable around 4.5; in some cases a tertiary peak; and then flattening off to free electron opacities.

The tabulated opacities of the mixture that we are supposed to use show these peaks. The distinctions between those and my own are basically that my curves were a little fatter in this region [near the peak], come down a little bit low in this region [between 4.5 and 5], and then agree here [at larger temperatures]. There are rather minor discrepancies of perhaps a few percent–10 percent or so–in the relevant region of opacity, which usually is a band where the opacity is fairly constant through here [between 4.5 and 5] for different densities. The density increases if the temperature does, the opacity stays almost constant, and the calculations follow this band. The discrepancies there are not large. I would say 5 or 10 percent, probably.

I might say that from time to time there has been occasional discussion as to what is the origin of these peaks. They did not appear previously in the calculated opacities of Cox and company, but now they apparently do. I don't think I have ever seen an explanation of their cause. I have always believed that this one is due to the peak in the Planck distribution passing over the ionization edge of hydrogen, but I have not checked recently.

There are no other basic changes in what I've done that I have to report on. The number of zones used in this calculation is about 42, and in the static model there are approximately 4 zones above the hydrogen ionization region. This does not give a very clean light curve, as you will see when we come to compare these.

HILL:

Can we now ask the opacity people, from their tabulated values, what the origins of the two peaks are?

BAKER:

Does anybody care to comment?

COX:

There is a paper I wrote having to do with why there are nonpulsating stars in the Cepheid instability strip, actually inspired by our Los Alamos conference some time ago. In it, because opacity is a favorite hobby of mine, I put some opacity curves like those Bob [Christy] has traced for three mixtures. This one is called Iben 1, X = 0.999, and Z = 0.001.

Perhaps these aren't of interest to us here, because these are Population II mixtures, but what I want to show is that in these modern things—Iben 1 was published in 1970, considerably later than when you [Christy] got your stuff from mine—these wiggles are present, and that's hardly any helium at all. Then you see this side has a little more helium, and then on this side [past the H peak] you can see a bump of helium I and a bump of helium II ionization.

CHRISTY:

Could I see that? I want to see if the bump is bigger or if the main peak is smaller.

COX:

The main peak is the ionization of hydrogen. Then there is one that is the second ionization of helium. This is typically at 40,000 K. If you have no hydrogen in the mixture, then there is another bump in here, which is the first ionization of helium. I'm unable to comment, Bob, about why these bumps seem to appear to you now and they didn't before.

CHRISTY:

They always appeared in my calculations, but I never found them in yours until recently.

KING:

I think if you were looking at very coarse tables. . .

* * * * *

CHRISTY:

That's right. It was not easy to see them; the tables were very coarse there.

KING:

How about the other bumps? I think they would show in the old Vitense opacities.

COX:

David King is asking about this third bump [Figure W1]. Where is this now; is this up about 100,000?

CHRISTY:

It's almost invisible, but it appears to be just around 10^5 K.

COX:

Any element that is undergoing ionization will give you a bump. The reason why you will not see it there is because the Z of those mixtures is 0.001; it's a Population II mix. In Population I mixes, it might be a confluence of carbon, nitrogen, oxygen, or something like that.

HILL:

But is this the ionization and not the excitation that causes the increased opacity?

COX:

There's a grouping of lines toward the absorption edge. The weighting function moves in to pick it up. It can give you an actual increase in the opacity of some of those plots shown.

HILL:

Do you mean bound-free, instead of bound-bound, primarily?

COX:

Both. There are a whole bunch of bound-bound transitions clumped toward the edge, and then there is the bound-free edge itself.

BAKER:

I don't think there is any reason now why we shouldn't continue with the next person on the list.

COX:

Norm, one thing. Could the table also include the opacity formula?

BAKER:

Yes, but how would we designate opacity formula?

COX:

For Christy, he said, "Christy formula."

CHRISTY:

I used my own opacity formula, but there probably will be some distinction with some people who use tables with different mixes.

BAKER:

For Christy's model use the Christy opacity formula. Some opacity formulas will have more complicated designations.

Now I guess we will ask Dr. Bednarek to speak. He spoke about linear models yesterday, and I assume he will talk about the nonlinear ones today.

BEDNAREK:

The program that I used was essentially the Christy program. I used the program developed by Paczynski, which interpolates the tables that were published in the Russian journal. I don't remember the reference. [A convoluted discussion established that these were the Cox, Stewart, and Eilers opacities published in Russian*.]

In my static model there were four optically thin zones for $\tau < 2/3$. In total, there were 36 zones in the static model. For the artificial viscosity, I used a value of C_Q equal to unity. I found that, in the interior, using the Christy method involves taking differences in the internal energy in the core where the internal energy is not changing very quickly, and this results in a lot of spurious oscillations in the center. So, for temperature greater than 100,000 K, I calculated all the equations of state variables analytically, tying them on smoothly to those at lower temperatures, and I calculated the internal energy difference, ΔE, from the difference in the temperature from one time step to the next. This reduced rather enormously the spurious oscillations that the models have in the core. It also improved considerably the total energy and the internal energy conservation.

ROSENDHAL:

Did you plot those opacities to see how they came together with some of the others?

BEDNAREK:

I did plot them. I did not plot these for this particular model, but I have plotted the opacities generated by this program before.

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*In *Scientific Information of the Astronomical Council, U.S.S.R. Academy of Sciences*, **15**, 1969.

COX:

May I ask you about something? I'm confused on your opacity. You said you used the Christy formula.

BEDNAREK:

No. Paczynski's interpolation formula in the Christy program.

* * * * *

VON SENGBUSCH:

The tables in the journal [*Scientific Information of the Astronomical Council, U.S.S.R. Academy of Sciences*] are much coarser.

KING:

Do they show the bumps?

BEDNAREK:

They show the main peak and the one at log T of 4.5. I don't remember if they show the other secondary bumps.

VON SENGBUSCH:

I have compared the Kippenhahn mixture you [Baker] gave me with X = 0.60 with the old table. There were new values in the table due to the finer grid, but the other values were just the same.

BAKER:

I have the feeling that opacity is the subject we are going to return to.

SPANGENBERG:

Before we get too far along, would you include a column in the table for representation of the viscosity used?

BAKER:

Yes. Viscosity formula.

ALEXANDER:

Is the internal energy that you mentioned in the Paczynski formulation?

BEDNAREK:

This is just the specific internal energy that I was referring to.

ALEXANDER:

Do you use the same technique for decomposing as Paczynski uses for the evolution program?

BEDNAREK:

No. I solve the Saha equation analytically; otherwise, I interpolate in tables.

BAKER:

Is this interpolation likely to be why you had to solve the energy equation differently? I don't think that's generally a problem.

BEDNAREK:

I don't think that would generate any problems.

COX:

Unless it has been done incorrectly. (Laughter)

We can keep that in reserve as the final explanation at the end of the day.

BAKER:

That may emerge, too.

BEDNAREK:

Under viscosity, I used a value of unity. This is the quadratic as described by Christy, Q1.

CHRISTY:

You can put me down for Q1, also.

BAKER:

Is there anything else that you feel is of interest to everybody?

BEDNAREK:

As far as the model is concerned, there is really nothing additional I can think of at this time that is really important.

As far as the iterations of each time step are concerned, when one solves for the temperature corrections, one can write this in the form of a Newton correction procedure for a matrix A times the temperature corrections equal to some right-hand D. Rather than checking to see how well the temperatures converge, I found that after three or four iterations the value of the bottom zone of this matrix element usually converged to a constant value; therefore, I stopped the iteration when the bottom zones in this vector D tended towards constancy. This usually gave—down to about 2 percent—the temperatures accurately to better than 1 percent throughout the model at all times.

As far as initiation of pulsation in my model, I used the velocity distribution suggested by Stobie in his calculations, which is a power law type of distribution. Velocities are just given by minus 10 times the relative radius ζ, raised to the power of 4.9, where ζ at the surface is equal to unity. I tried to use the adiabatic velocity distribution, but this produced a mixture of what appeared to be the fundamental and the first harmonic. I could not seem to get rid of the first harmonic integrating over fairly large temperature . . .

COX:

But it was a pure linear mode that you used?

BEDNAREK:

Yes. This was the linear adiabatic model that I tried to start with. Just before I left Toronto, I also tried the linear nonadiabatic, and that produced essentially the same result as the linear adiabatic results. It appears that the linear adiabatic and also the nonadiabatic velocity distributions, at least for my program or my models, are too steep in the core regions.

COX:

The amplitude variations, you mean?

BEDNAREK:

Yes. So this particular power law gives a larger amplitude.

COX:

At what relative amplitude did you introduce that? Maybe you introduced it at too high an amplitude.

BEDNAREK:

I tried a variety of amplitudes from the smallest of about 1 kilometer per second on the surface up to about 10 kilometers per second.

CHRISTY:

It doesn't affect the end result, does it? How did you initiate it?

BEDNAREK:

I never carried the other models to limiting amplitude waiting for the harmonics to die out. I just used this particular velocity distribution I described.

KING:

In your adiabatic solution or nonadiabatic solution for your linear code, were you actually analyzing exactly the same equations as you were in your nonlinear code? Because if you weren't, this would probably introduce a difference.

BEDNAREK:

I wasn't. I actually was using the difference equation described by Castor. But I have initiated other models using the same code—using the adiabatic solution. I have tried it on RR Lyrae stars, and it seems to work successfully for them. But it didn't work for this particular model.

CHRISTY:

What do you mean it didn't work?

BEDNAREK:

For RR Lyrae variables, if you initiate them using the linear adiabatic velocity distribution, if you want to initiate the fundamental, then you will get a relatively pure fundamental. But for this particular model there was a mixture of modes.

KARP:

I tried the converse of that with my nonlinear models. I tried taking this Stobie velocity distribution at very low amplitudes, several hundred centimeters per second, and the thing started going crazy with all kinds of modes in there. It wasn't even close. So that is the converse of what you tried to do.

COX:

In the same model?

KARP:

No, it wasn't this model. It was my 12-day model.

CHRISTY:

Did you eventually get to maximum amplitude with relatively little . . .

BEDNAREK:

Yes. I reached maximum amplitude within about 100 periods, and I ran it through an additional 15 periods to get a relatively pure fundamental mode.

STELLINGWERF:

We ought to add that to the list.

BAKER:

How many periods did you run?

BEDNAREK:

The total number is 97.

BAKER:

Bob, do you have any figures?

CHRISTY:

About 25 periods, but I accelerated the approach to the maximum, so it doesn't occur naturally.

BEDNAREK:

I didn't accelerate this model at all.

COX:

Norm, maybe David can tell us. I can't find it in the paper here–the linear growth rate of this model. That would probably be useful to put up. It's probably about 100 periods e-folding time?

CHRISTY:

Linear growth rate in energy or velocity?

KING:

I have it in energy right now.

BEDNAREK:

I obtained a linear growth rate of eight percent.

KING:

This, of course, will vary a lot within the zone. I did it for 195 zones through the hydrogen region, so that should, hopefully, be closer to large amplitude pulsation where it's sweeping through a number of zones. For the fundamental, I have here ζ, which is 1 over the e-folding time for kinetic energy. It's 5.6×10^{-2}.

COX:

He gets 8 and you get 5.6. That's not bad.

SPANGENBERG:

I get 7.

COX:

Lucky 7, that would be 14 periods.

BEDNAREK:

For my nonlinear model, the growth rate was only about 2.5 percent.

COX:

That's because your zones were coarse.

BEDNAREK:

Yes.

BAKER:

I think that it is obvious that everybody has done linear models; it probably isn't necessary to keep track of that. Who is speaking for Cox and Davis?

DAVIS:

Let me write down our basic references: (1) Bendt, J. E. and C. G. Davis, 1971, *Astrophys. J.*, **169**, p. 333, and (2) Davis, C. G., 1974, *Astrophys. J.*, **187**, p. 175.

The first reference contains the basic code with the hydrodynamic equations and the original variable Eddington formulation. The second has the code as modified to run these particular models. It has the added velocity-dependent terms that were worked out with Castor and

Freeman almost directly from Frazier's work. It probably has no real effect on the results; but it has been added as a slight improvement to the calculations for W Virginis models.

We did 4b, we did not do the 4bx, I call it, that Warren [Sparks] gave us, the new luminosity and the new T_{eff}. We did 4b from the King, Cox, Eilers, and Davey paper, which has a slightly different luminosity. My period is 8.8.

BAKER:

So the opacity is Kippenhahn?

DAVIS:

Kippenhahn 1a. There will be three models that will be slightly different from the basic set of models. The period is 8.8. The linear is 8.8, too. It's slightly different from the nonlinear, but put 8.8 in the table. The opacity is Kippenhahn 1a, with the fitting procedures used by the Los Alamos group in a table look-up—very fine tables that Dave [King] has worked out that have a lot of points and fill up the machine. The Kippenhahn 1a has the advantage over Kippenhahn 1 in that it includes the new iron abundance, but I don't know what effect it has. However, such things as metal abundances probably don't have much to do with this calculation.

The number of zones is 72. I have two models for this, actually, but the basic model has 11 thin zones. There is a 7-thin-zone model, too. The 11-zone model is the one I will discuss, unless there is some reason to talk about the other.

The model has 11 zones in and above the hydrogen ionization region. Viscosity is Q with a 4 and a Stellingwerf-type cutoff at 1/100*th* the speed of sound. Maybe I'll just call it Stellingwerf and blame him for it. It really helps running the calculation.

I ran 200 periods. I left it overnight for an hour run and came back and it was done in the morning, so I didn't have to decide when it was at limiting amplitude. The luminosity is the one for 4b in the *Astrophysical Journal* articles.

CHRISTY:

What's the radius of your star?

KING:

I don't have the radius written down, but the effective temperature is . . .

DAVIS:

. . . 5715 K, a slightly different effective temperature.

CHRISTY:

The period is the simplest thing. We had better see if . . .

DAVIS:

It won't be the same, we're using a different luminosity. This is the 4b model in the paper and not the 4b model as described in Warren's letter.*

CHRISTY:

I think my period is 9.59 days.

DAVIS:

Yes, we did that as a linear code and it came out to be that–9.58 or so.

BEDNAREK:

I got a period of 10.7, although the linear code . . .

VON SENGBUSCH:

What was the luminosity you were using?

CHRISTY:

1.24×10^{37} ergs per second.

* * * * *

KING:

The other one should be 1.1×10^{37} ergs per second.

BEDNAREK:

Mine is the same as Dr. Christy's.

BAKER:

And the effective temperature?

* See Appendix for the letter sent to participants describing the "standard model."

BEDNAREK:

Again the same.

CHRISTY:

Why do we have different periods?

BEDNAREK:

I don't know. For my linear nonadiabatic case, I got 9.7 days.

CHRISTY:

You were not concerned about this difference between 9.7 and 10.7?

BEDNAREK:

I could not find any explanation for it. When I initiated the oscillation using a different velocity distribution, I did get a slightly shorter period, about 9.8 days.*

KING:

Sounds like you have a mixture of modes.

* * * * *

COX:

Gil, is that what you get; is the nonlinear period 8.8?

DAVIS:

Yes. Nonlinear and linear periods, with the Kippenhahn 1a and the luminosity in the *Astrophysical Journal* article, are 8.8. The linear period, with the model Warren gave us in the letter, is 9.8.

BAKER:

So these are different models?

COX:

Then he agrees pretty well with Christy, whose period is a little longer.

---

* See the Appendix for Dr. Bednarek's elucidation of this problem.

KING:

There has been a suggestion, also, that people have used not only different numbers of zones but the bottom of the model in different cases is different. In those cases where it is easy to get at the information, could we have the information on the depth included in the table? Perhaps, the radius fraction at the bottom.

CHRISTY:

Put down 0.09 as the radius fraction for me. That is the fraction in the core.

COX:

The label is a little wrong. Why not call it the fractional radius at the bottom?

* * * * *

BAKER:

Shall we move on, then? Who is talking for the home team?

SPARKS:

The number of zones for the FSK model is 100. We used the King 4a tables.

COX:

How many thin zones?

SPARKS:

There are 25. The code—this is Kutter-Sparks computer code—has been published, and was slightly modified by Al Karp. This is an implicit hydrodynamics code.

COX:

This is not Karp's code?

SPARKS:

He has modified the code.

COX:

But this is with his modifications?

SPARKS:

Yes.

FISCHEL:

He made modifications for the thin zones.

SPARKS:

Yes.

COX:

One point, which hasn't been brought up yet, is that Gil's work and your work are transport models. The others are diffusion models.

SPARKS:

We did the diffusion on the outside.

COX:

It was only Gil who did transports?

SPARKS:

Yes. We did not really have time to do the radiative transport. For viscosity, we used a viscosity that varies. It is about six on the outside, and it is a function of pressure.

Our period, Dave?

FISCHEL:

The period is 9.55 days. Fifty periods with acceleration.

CHRISTY:

Finally someone agrees with me.

SPARKS:

And luminosity is 1.24; T_{eff} is 5700 K. We don't know the fractional radius, but we'll present it when we look it up.

COX:

That is King's 4a table, right?

SPARKS:

Yes.

BAKER:

Any other comments?

SPARKS:

No.

BAKER:

What I think is already becoming clear is that there are certain families of models that are . . .

KING:

Three out of four, so far, have what looks to me like a rather large value of "X4." ["X4" is King's notation for the fractional radius of the core.] What was the temperature in that first zone? That might be of some interest–how deep you are in temperature.

KARP:

About 10^6 K in this one.

BAKER:

Is that worth keeping score on?

CHRISTY:

I never found that the difference between 0.1 or 0.2 makes any real noticeable difference.

KING:

I have, in some models, in terms of period. The period would be different by 5 percent or something like that.

COX:

I think not, Dave, for a highly condensed model like this.

BAKER:

It depends a great deal on the model. I think this would be interesting to discuss later in connection with . . .

CHRISTY:

I doubt that it's a major effect.

BAKER:

I think some people believe it has something to do with the bumps. We can see about that. I think we should have von Sengbusch and Stellingwerf at the end, because they are in a separate category, in a way. So maybe Dave King should speak now, because his models are very similar to some of these others.

KING:

I would be looking at the same one that Gil Davis and Art Cox looked at. In other words, I didn't have time to do the standard model or to finish it. So I don't have anything except linear results on that.

I have 50 zones with 3 optically thin zones; Kippenhahn 1a, Los Alamos code, and Q is equal to 1. This is quadratic viscosity every place except the inside zone. You may have noticed the little ripples that Christy had at the bottom of his model, and other people have mentioned this. I found that, if you run for a while, it can even cause the inside to start moving rather violently in some cases. I put in a linear viscosity rather than a quadratic on the inside zone with the same factor of 1; that damps out those oscillations.

CHRISTY:

I might say in this particular case I did that, too, because I did not do it at first and I was getting oscillation on the inside zone. I put in linear viscosity and killed it off in that zone. It did not make any other difference.

KARP:

That is not needed by us. We didn't have that problem at all.

CHRISTY:

It depends on where you take the inner boundary. When you take it farther in you get more problems.

KING:

I think it may depend on other things, too. Your code is different.

KARP:

Right. I was wondering how close to the Courant condition you are with your time step for that inner zone.

KING:

Not too close, I don't think, on that zone.

CHRISTY:

I ran fairly close, and I was taking the zones close in. That was when I got into trouble.

KING:

We don't really understand exactly what causes this. We ran about 150 periods altogether. There was some acceleration involved. This was not a growth from the noise level or anything like that. The period is 8.8 days. Luminosity is 1.1, T_{eff} is 5715, and the fractional radius is 0.07. The code was described many years ago by Art Cox and company.

BAKER:

This is quite different than the one that Gil used?

KING:

Yes, this is quite different. There is no radiation transport. It is pure diffusion throughout. We just used linear interpolation in both equation of state and opacity tables as produced by the Los Alamos group.

COX:

I would suggest two other items here. One of them is something characterizing the outer boundary condition. The other is pulsation amplitude at maximum amplitude. These are two suggestions for two more items in the table.

BAKER:

I would think, as far as the second suggestion goes, maybe there will be a number of characteristics of the pulsations that we will want to keep score on as we go along. Maybe we will put that in a second category. But I think if there is some way that we could easily characterize the outer boundary condition, or whatever you want to call it, that might be a good thing. Does anyone have any suggestions?

COX:

We do have one—radiation pressure on the outside beyond the star, that kind of thing. Castor has discussed this problem, and he has something he calls alpha that characterizes the rest of the mass of the star.

BAKER:

That has become quite popular, I think, to use now.

KING:

But not everybody is using it. I did not use it in this particular model, for example.

COX:

Maybe it doesn't make any difference.

BAKER:

I think it might very well make a difference.

KING:

I guess you would have to have something simple. You would need to know exactly what formula is being used for the boundary condition on the hydrodynamics.

BAKER:

People can tell about it, but if we just put down something to remind us it might not be a bad idea. Let's have a try at it.

CHRISTY:

I have the radiation pressure on the surface.

BAKER:

What about Los Alamos?

DAVIS:

We used the variable Eddington factor extrapolated from the previous zones.

CHRISTY:

He should not need to worry about that with radiative transfer.

COX:

Is there any mechanical outside pressure at all?

DAVIS:

I think there is just the extrapolated radiation pressure to the outside.

COX:

Maybe the way to get around it is to call it a transport method.

CHRISTY:

If you have very, very thin zones, then you do have to be rather careful about it. Otherwise, it doesn't make much difference.

KARP:

We used the p-alpha boundary conditions, which have the same pressure ratio as the mass ratio. The only thing that did was to keep the top radius, the top zone, from being two or three times thicker than the next zone in. But that is about the only difference.

CHRISTY:

Was the radiation pressure computed at the top zone?

KARP:

In the diffusion code, yes.

CHRISTY:

And there was no balancing pressure?

KARP:

Yes. We put a surface pressure on it. Without it the only difference was that the top zone got to be two or three times thicker than the other.

CHRISTY:

If you don't watch out, it will fly off entirely.

KARP:

It didn't. I just put it on to make . . .

CHRISTY:

You did put a surface radiation pressure on it?

KARP:

We just used an arbitrary p-alpha.

COX:

What do you mean by p-alpha?

KARP:

Extrapolate the pressure using the ratio of the mass zones. The ratio of the pressure outside the top of the star was in the same ratio as the ratio of the mass zones.

COX:

That is the Castor method.

KARP:

No, it isn't.

BEDNAREK:

I just set the pressure outside equal to zero.

BAKER:

But do you have radiation pressure?

BEDNAREK:

No, I just set it to zero.

KING:

No, I didn't in this model. I essentially have P = 0.

BEDNAREK:

In static models, I find if you just have small surface zones, you can compare that model with essentially the same model, but having more optically thin zones. Then, if you apply this outside pressure, the coarse zone models tend to be distorted from the fine zone models.

BAKER:

I see. I guess logically the next one to speak is Bill Spangenberg.

SPANGENBERG:

I used my own computer code, which is something of a hybrid between the straight diffusion codes and full-blown radiation transport code. The terminology used at Los Alamos is nonequilibrium diffusion, which is a gray treatment. What I solve are the two coupled equations, a gas energy equation. V is specific volume, B is the Planck function, and J is the angle average radiation field.

Then you have the zero moment transfer equation with velocity dependent terms:

$$\frac{\partial E_g}{\partial t} + P_g \frac{\partial V}{\partial t} = 4\pi K_R (J - B)$$

$$\frac{\partial}{\partial t}\left(\frac{4\pi VJ}{c}\right) + \frac{4\pi}{3c} J \frac{\partial V}{\partial t} = \frac{\partial}{\partial m_r}\left[\frac{(4\pi r^2)^2}{K_r} \frac{\partial}{\partial m_r}\left(\frac{4\pi}{3} J\right)\right] - 4\pi K_R (J - B)$$

This set of equations will give you a diffusion-type solution in optically thick regions, but does allow you to do something a little better than diffusion in thin regions.

And I had a 59-zone model with 14 thin zones. The opacity was the King 4a of the requested model, and there is a two-step process here. I have made fine opacity tables by splining up all of the King mixes, which are available to anyone who wants to use them. This process generates a fine table, it adds points. It doesn't do a calculation, but it will put points in the table. It also supplies opacity derivatives. As a result, then, taking this table, I used it in the code. I did a consistent cubic interpolation within the code, but one could do that with a coarse table or any other table.

COX:

It is a spline?

SPANGENBERG:

I used the spline table and then I also did a cubic interpolation within the splines. So just for information's sake, one could take these spline tables, and, if you think they are fine enough, you could use your standard linear interpolation. If you did not want to do the coding, by the same token, one ought to be able to take one of the coarser, older mixes and use a cubic interpolation.

BAKER:

How fine are they? Can you characterize them?

SPANGENBERG:

Yes, I think from memory I can. They are functions of log T_e and density. They are not square tables. I think the bottom point is 3.5 in log T_e. Then they go by 0.05 to about 3.7 I think, and then by 0.02 well up beyond the helium region, 4.5 or 4.6, 4.7; then they come back to 0.05, up past that region of, perhaps, the carbon-nitrogen-oxygen bump.

COX:

How many points in the table?

SPANGENBERG:

It depends on what you want to do. I think there are 96 for an envelope. I believe it has 110 altogether, but that would allow you to do a complete core.

COX:

I am talking about the number of opacity values. There must be 1000.

SPANGENBERG:

I run it 96 temperatures by 20 densities for an envelope, but this is a function of temperature.

COX:

Two thousand points.

SPANGENBERG:

Right. The table is quadrilateral, but it does have parallel edges.

ALEXANDER:

Do other people who use tables use linear interpolation?

SPANGENBERG:

I don't know.

ALEXANDER:

The people that spoke already used linear, quadratic, or what?

CHRISTY:

Linear.

COX:

In logs, though.

SPANGENBERG:

Yes. These are actually log tables. But it is a cubic in the log.

BAKER:

One other thing we might note is that this model [Spangenberg] and the Cox-Davis model are transport-types. In that way I believe they are different from all the others.

COX:

The terms are this: for Gil, you might call it a transport code. Bill [Spangenberg] would call his a nonequilibrium diffusion code. This means that the matter temperature is not equal to the radiation temperatures.

SPANGENBERG:

So that one has a hope of treating optically thin regions without coupling them too strongly to the local temperature. But the code was developed totally independently of Gil's code or Dave's [King] nonlinear code. So somewhere at the bottom of it all, the three are not tied together.

BAKER:

Apart from this, I think that this whole question of opacities is something we will get into and, in particular, I think it is very interesting that these various very finely treated opacity tables are available. I hope we will have a few minutes this afternoon to talk about this whole thing, but we should go on now.

HILL:

While we have the equation on the board, did you run a case where you did not have the time derivatives in your radiative transfer equation? Are they necessary? And are they now becoming standard with everyone else for their models?

SPANGENBERG:

For one thing, I don't believe anyone else is using them. They don't cost you anything to run. Now, for the Cepheids, I never got terribly strong shocks, but for the RR Lyrae models you can get 100 kilometers per second at thin zones. And so when you are dealing with corrections, you are dealing with small corrections. There seems to be no computational reason why you shouldn't write down the best equation.

CHRISTY:

There are some physical reasons, however. It is very difficult to be consistent in those terms because, if you start thinking of opacities when there are relative motions, the emission and the absorption occur at different velocities and this again is a v/c effect, which is the same thing as the time derivative terms.

SPANGENBERG:

You certainly could have time scales where you do not have LTE holding, and where you would have trouble, say, evaluating all your opacities or for equilibrium considerations.

CHRISTY:

I'm just saying, in terms of that size, it is very complicated to be really consistent.

COX:

Even in the transport calculations?

SPANGENBERG:

Yes, you can get competing time scales.

DAVIS:

Pomeraning shows you have to have those forms for the transport equations to reduce to the diffusion equations consistently.

COX:

I don't know what the results will be when we see all these pretty curves but, I think for Cepheids, this fancy business of radiation treatment is not necessary.

BAKER:

I hope so. (Laughter)

COX:

We don't need to get into this argument as we do at Los Alamos many times.

BAKER:

I am sure it is an approximation.

ALEXANDER:

I just wanted to ask if there was a public reference for this code.

SPANGENBERG:

Oh, for the theory. There is no coding in there, but it is from a 1972 or 1973 paper. Since I have a preprint, I never wrote down when it came out in *Astrophysical Journal.* "Radiation in Spherical Symmetric Flows" is the title. However, the arguments are somewhat political over the years, of what to include and what not to include. It could make up a two-day conference, at least. (Laughter) Let's see. Where are we? Viscosity: I used Q4 with—I don't know if we should characterize it as a cutoff or turn-on or what. But we can go into those details. There is a tenth of sound speed for a threshold compression cutoff. Less than a tenth of a sound speed was not considered as requiring viscosity. The number of periods is about 95, with a period equaling 9.88 days. The standard luminosity and temperature. About 0.09 for the fractional radius of core. I would characterize my boundary condition as an imaginary atmosphere, the Castor formulation, which is not exactly the same as p-alpha.

COX:

It is called Castor.

SPANGENBERG:

It is Castor's imaginary atmosphere. . . (Laughter)

COX:

An unusual talent. (Laughter)

SPANGENBERG:

If people want to talk about this, certainly we can, in terms of formulas. But it lends itself to a consistent treatment of both the hydrodynamic boundary condition and the radiation transport boundary condition.

I have a single angle, discrete ordinate-type condition on J rather than T which, if this were diffusion, there would be a σT^4 in there, so I deal with the J quantity at the surface, but one can get a temperature from that. I believe that is all of the quantities for now.

BAKER:

I guess, Kurt, you would be next. There seem to be two different von Sengbusch models.

VON SENGBUSCH:

I haven't got the plots with me, but the optically thin-zones in the equilibrium mode are of the order of 7. Now, this [opacity table] is what I call Kippenhahn 71. I got this table in 1971 from Dr. King, and I think that is referred to as Kippenhahn 1.

Now, I don't know whether this "a" is correct here, because the table itself does not contain the "a." The label on the tape does not contain the "a."

KING:

I think that those others should not have an "a" on them either.

VON SENGBUSCH:

When did the "a" tables come out?

KING:

Only a year ago or so.

COX:

Sometime about the end of 1971. What happened was they increased the iron abundance in the sun by a factor of 10, and for a while what we agreed to do was signify these with little "a's." I believe—we redid Kippenhahn 1 so there is both Kippenhahn 1 and Kippenhahn 1a. I believe [for the King mixtures] that we never calculated anything except for the increased iron, and we called them for a while 1a, 2a, 3a, 4a. But there exist no [King] mixtures without the a's.

VON SENGBUSCH:

Then Kippenhahn 1a is correct. Next, an eigenvalue code. Viscosity is Q1. [von Sengbusch is filling in the table for his models Se III and Se V during the following transcription.] The number of periods—I would rather say number of iterations—I think is of the order of 10 or 15. King 4a is the opacity table for Se V. The period is 9.73 for Se V and 8.84 for Se III. The luminosity here is 1.24 and 1.1, for Se V and Se III, respectively. In both cases, T_{eff} is 5700 K. The fractional radius is about 10 percent, and the boundary condition is P = 0. We used the tables with linear interpolation. The difference scheme is fully backward-implicit in the energy equation and is time-centered in equations of motion. Any further information?

BAKER:

I guess the essential difference between this work and also the work that Stellingwerf will talk about, and the others, is the business of looking for the periodic nature and, because of that, there are some differences in the way the equations are treated.

VON SENGBUSCH:

I should mention that these 15 iterations are necessary to establish convergence to 1 K.

BAKER:

I see. Maybe we should just mark iterations there rather than periods. Now, we have Bob Stellingwerf.

STELLINGWERF:

Immediately, I am in trouble because the number of zones category has to be qualified. It is done with a fancy inner boundary condition. The linear model is calculated with a certain eigenfunction where the ordinate is δr, let's say, and the abscissa is radius, and that is done with standard nonlinear zoning, 46 zones with 4 thin zones. (See Figure W2.)

I have a special treatment of the interior in which I throw away all of this [below 0.58] and it picks up the boundary condition here [at 0.58] so that the nonlinear model has the same behavior in the interior as the linear model shows. The way you do that is to put some kind of a simple harmonic oscillator boundary condition here.

CHRISTY:

At what radius is that?

STELLINGWERF:

The first model is at 10 percent of . . .

CHRISTY:

No, what radius is your dynamic model interior boundary?

STELLINGWERF:

This one is 0.58.

CHRISTY:

And there is no time delay? That is, if a signal hits that, it doesn't come back after a time?

STELLINGWERF:

Well, I am not sure about that. We will have to discuss that. You see, it is tuned to behave just right at the fundamental and at the first harmonic frequencies simultaneously. So the nonlinear and linear periods will agree, and the growth rates will agree almost exactly. But I don't know what happens if you hit it with a delta function. That may just bounce off and may affect the phase of the bump. We'll have to see.

BAKER:

But certainly the sound speed interior to that is pretty high already, isn't it?

CHRISTY:

But 0.58 is not so far in as all that. You'll have to be careful at that point.

STELLINGWERF:

I have 29 zones in the final model with 4 thin zones based on a model with 46 and 4.

COX:

Bob, let me ask you here, because this is very interesting. You actually have some kind of moving piston there. Is that the idea?

STELLINGWERF:

I attach a weight to the inner boundary. This is the nonlinear code.

CHRISTY:

No, there is an awful lot that goes on inside that.

STELLINGWERF:

That is a mass; that is a spring [Figure W2].

COX:

That's going to affect the Christy bump.

CHRISTY:

Unless you have some great magic in that boundary condition!

STELLINGWERF:

No, that's it. A driven oscillator.

COX:

Tuned apparently to two frequencies simultaneously, the fundamental and the first harmonic . . .

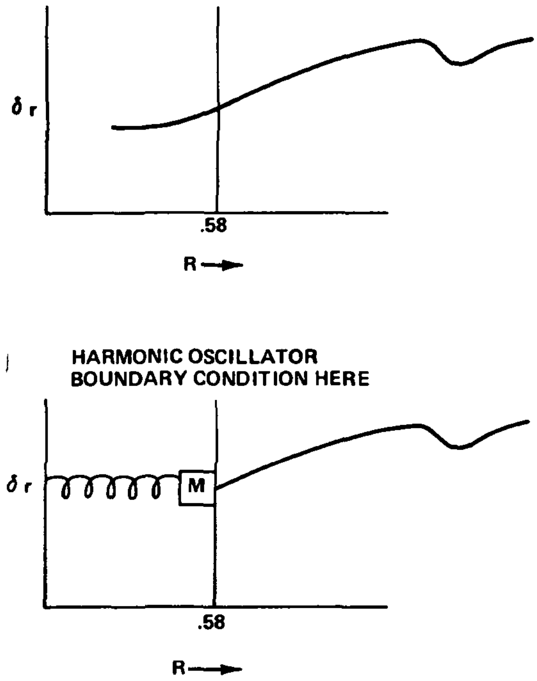

Figure W2. Stellingwerf's harmonic oscillator as a boundary condition.

STELLINGWERF:

That's correct.

COX:

. . : but it isn't tuned to Christy bumps.

STELLINGWERF:

No. As I said, if you hit it with a delta function, I am not sure what would happen.

CHRISTY:

We will see when we compare the results.

STELLINGWERF:

I wasn't too concerned with RR Lyrae models, but it may have an effect on this one.

HILL:

When you say that it is tuned, do you mean that you specify what its velocity is as a function of time?

STELLINGWERF:

Well, you have two parameters. You have the spring constant and the mass.

HILL:

So it is a resonance tuning.

STELLINGWERF:

Not quite. What I do is the complete linear analysis all the way in and that gives me the slope and the value of the eigenfunction at this point. For the first harmonic and fundamentals, I have used that to specify these two qualities.

HILL:

Just the mass and the spring constant.

STELLINGWERF:

Only two parameters. But it is sufficient to get the periods right and get the growth rates right. So it is not that bad.

BAKER:

Fill in the fractional radius there also . . .

STELLINGWERF:

It is 0.58, 0.09 in parentheses. The 0.09 is the proper one for the period, but the 0.58 may be the proper one for the bump. For opacity, I am using my own formula based on King 4a.

ALEXANDER:

You used a formula, not a table look-up?

STELLINGWERF:

It is a formula, a smooth formula. Maybe I should say a word about that. It is basically the same type of formula as Dr. Christy's, but is not tremendously accurate. But I did fit it to the new King 4a tabular points. So that at the main peak it is a little bit narrower, but it fits quite well at large temperatures, which is what you expect. The viscosity is Q4 and the program is eigenvalue. I use the Fraley difference scheme.

KARP:

Isn't that more of a hybrid between the standard Christy and the Baker-von Sengbusch?

STELLINGWERF:

I don't know how to answer that.

KARP:

I'm trying to find a link between the two kinds of codes.

STELLINGWERF:

Mathematically the relaxation scheme is similar.

COX:

But, that's a detail. I think you ought to put "Stellingwerf" in the program column.

STELLINGWERF:

Stellingwerf program, yes. I will take responsibility. (Laughter)

COX:

After all, he wrote it. He made it up of little pieces all over, like everybody else.

STELLINGWERF:

But the difference scheme is a time-centered, energy-conservative difference scheme.

COX:

A Fraley system.

BAKER:

Everything is time-centered. All the equations?

STELLINGWERF:

Except for the luminosity.

CHRISTY:

The hydrodynamics are time-centered.

BAKER:

Is that different from von Sengbusch?

STELLINGWERF:

No, my hydrodynamics are also time-centered.

VON SENGBUSCH:

Yes, but your luminosity is not completely time-centered.

STELLINGWERF:

That may be a minor point.

BAKER:

You use a fully implicit code.

VON SENGBUSCH:

But I don't use a Fraley-type difference scheme.

STELLINGWERF:

That is probably a detail.

KARP:

Should we note which are explicit and which are implicit? Is that significant or not?

CHRISTY:

I doubt that it is terribly significant. It seems to me there have been other differences that are going to be more significant.

STELLINGWERF:

Viscosity is Q4 with a [cutoff] point—what do we have, 10 percent? The same as Spangenberg. There are eight iterations, and the period is 9.98. Luminosity is 1.243, T_e is 5700 K, and boundary condition is Castor.

CHRISTY:

There is one other question that I would like to ask: What is the photosphere radius in each case? Normally it doesn't make an awful lot of difference, probably, except if you use a lot of optically thin zones, then where do you start this and where is the photosphere?

COX:

Where do you start what, Bob?

CHRISTY:

The simplest thing to do is to calculate a radius according to $4\pi R^2\ T^4$ as the luminosity.

COX:

Which you can do.

CHRISTY:

But if you start at the outside of a very optically thin layer there, your photosphere is not at that point, and so your photosphere does not radiate according to the Stefan-Boltzman law. So our question is: What in fact was the radius of the photosphere in the model calculated?

KARP:

In our model, which has a lot of optically thin zones, the thickness of the atmosphere is only one percent. But we did pick the hydrogen ionization region as the . . .

CHRISTY:

It may be worthwhile writing down what the radius of the photosphere was in the model calculated.

BAKER:

Is that available in most cases?

SPANGENBERG:

In the scheme I use, I make it consistent with the specified luminosity and temperature. My boundary condition on the static model is derived by specifying an optical depth of the mass zone that I want, and that optical depth includes an imagined atmosphere. You then pick a reasonable number for the radius and have two iteration schemes. You play with your mass . . .

CHRISTY:

Yours is consistent.

SPANGENBERG:

You play with your mass to get the optical depth you want. Then you play with the radius until you get the photosphere where you want it.

KING:

This is basically the same way I did it.

COX:

How to interpolate back into the zones was described in the original method of Los Alamos.

CHRISTY:

Mine is not consistent. I don't go thin enough so that it makes perhaps half a percent difference.

COX:

In what?

CHRISTY:

In the radius. Then that shows up in the period.

KING:

Right.

CHRISTY:

The periods will be off a few percent, a percent or something like that, if you are not careful with where you put the photospheric radius. This may account for some small difference in periods I think.

VON SENGBUSCH:

I used the inner radius of the outermost zone.

CHRISTY:

As the boundary condition?

VON SENGBUSCH:

As the boundary condition radius; it could differ by about two percent from the photospheric radius.

BAKER:

It seems to me it is a little bit difficult to characterize this succinctly.

CHRISTY:

In the end, we can compare photosphere radiuses if we want. I think it will account for some difference in the periods.

KING:

It looks to me as though it is exactly the right direction, the way you say you do it. Your period is shorter than what I got in my linear model, Bob, by a couple percent.

CHRISTY:

It is quite possible my photosphere radius is a little bit too small.

BAKER:

I think we are gradually getting to the point of discussing the results and seeing how they compare. The one output quantity you could say that we have here is the period. To what extent should they be comparable?

CHRISTY:

There are at least two models being discussed with different luminosities. Also two opacities, but that shouldn't make so much difference.

BAKER:

It shouldn't matter for the periods so much.

CHRISTY:

No.

BAKER:

Let's take the 1.24 [luminosity model]. We have periods going from 9.55 to 10.7.

CHRISTY:

I would settle for 9.7.

BAKER:

As an average, yes.

COX:

Let me ask about that, Norm. Of course, in years past I tried to get a period from a non-linear calculation, and it is not easy to do.

SPANGENBERG:

But, if you use kinetic energy maximums, it comes out pretty nicely.

COX:

You get the peaks of kinetic energy and divide by two.

SPANGENBERG:

Yes. I would say if you can't get the kinetic energy smooth, you are in trouble.

CHRISTY:

If you can't get the oscillations smooth enough to get a period out of it, you haven't got a . . .

COX:

What I am really saying is, can you really get that more accurate than 2 or 3 percent?

CHRISTY:

Oh, yes.

COX:

You think so?

CHRISTY:

Yes. You can certainly read it with an accuracy of a 10*th* of a percent. If you have got a well-defined—and presumably yours iterates to something like . . .

VON SENGBUSCH:

These iterations get the period down at least to 10^{-6}. But I think that has no relevance because . . .

CHRISTY:

No. It just means that you define the period for a given . . .

COX:

What I am saying is that you are hacking away and you are only taking 100 or fewer time steps per period. You must jitter a percent or so.

KARP:

What were your time steps? How many time steps per period?

COX:

David [King] has only run the problem now, but it usually runs like 100 time steps per period.

KING:

A hundred to two hundred.

COX:

I guess I'm wrong. That isn't the source of inaccuracy that I thought it could be.

BAKER:

The other differences in the models can certainly explain these differences in the order of 10 percent or so. They are much less than 10 percent, really, most of them. Except for this one [Be]. Now, do we know why?

BEDNAREK:

I could get a different period when I start the model off using the linear adiabatic velocity distribution.

BAKER:

Your linear period was different?

CHRISTY:

You got different periods with different initial conditions. That's very strange . . .

BEDNAREK:

At small amplitudes I was getting a different period, but then it was difficult to determine . . .

CHRISTY:

But you did not run it out to full amplitude?

BEDNAREK:

No, I didn't, but it was 9.7 to 9.8. There was a mixture of modes. From my linear non-adiabatic program I got 9.7.

CHRISTY:

Which seems to be consistent.

BAKER:

Yes, that looks consistent. Was it possible you didn't run the program long enough?

BEDNAREK:

No, it was a fairly pure fundamental mode.

KING:

It would also be surprising that the period would be that much longer than what other people got. If it is a mixture of modes, you might expect it to be a little shorter. But that is much longer.

BAKER:

It certainly does seem strange. What about with the other luminosities?

COX:

They agree very perfectly. But I think it is all because they come from the same code. (Laughter) You got to watch that around Los Alamos.

KING:

They don't really come from the same code.

BAKER:

Well, von Sengbusch, of course, has a different code.

COX:

Just the three of them.

KING:

That number is already published. Maybe that's why. (Laughter)

BAKER:

That helps a good deal. Yes. Well, I guess we should go on, then, to some comparisons.

CHRISTY:

I think the velocity is more apt to show agreement—also the structure—but not the temperature and luminosity. Therefore, start with things that are more likely to agree than disagree.

BAKER:

I think that's right.

CHRISTY:

The structure as a function of depth is apt to agree.

BAKER:

Let's write down what we have.

[The following list reflects what Dr. Baker wrote on the blackboard.]

Velocity	versus	Phase
M_{bol}	versus	Phase
T_{eff}	versus	Phase
Log P	versus	Log Q at minimum and maximum light
Log ρ	versus	Log Q at minimum and maximum light
Log T	versus	Log Q at minimum and maximum light

CHRISTY:

I don't think the last three will be detailed enough to reveal anything very fancy. And the temperature and luminosity will be so detailed that they will reveal all sorts of peculiarities.

BAKER:

Right. So the velocity certainly is something that is interesting.

CHRISTY:

I think it will indicate the amplitude and general character of the solution.

[The conference room was equipped with two large translucent screens on one wall, illuminated from behind. This provided the capability of dual projections with no obstructing projectors and, of course, no obstruction of the light path. The intent of the organizers was to have the viewgraphs all on the same scale, but technical difficulties obfuscated our intent. Much of the following discussion is somewhat confused due to the slight difference in scale of the viewgraphs, the fact that there were two screens operating, and that the viewgraphs were being overlaid during the discussion. These figures of the models have been properly scaled and are ordered on increasing period. Because there were two luminosities discussed, the models are separated by luminosity.]

BAKER:

Now, how shall we show them? Do you want to first put them on individually and then in groups, or how. . .

FISCHEL:

We could run the velocity on one screen and the luminosity on the other.

KING:

Why don't you separate the two sets of models by luminosities so that there is some separation?

BAKER:

Yes. Well, is there one that would be particularly good to choose as a kind of standard with which we could compare the others? It is going to be pretty hard to compare them all at the same time.

COX:

Well, I guess you should eliminate maybe Stellingwerf and von Sengbusch and also Davis and Spangenberg because all are somewhat different. The others are the classical diffusion cases.

KING:

Why don't you use Christy's?

BAKER:

Fine, let's use Christy's to compare then.

[For the following discussion, refer to Figures W3a and W4a.]

BAKER:

I think the first one to compare with Christy is the Goddard one, the Fischel et al., FSK.

COX:

Are these made by the individual authors?

CHRISTY:

Is the scale different or what?

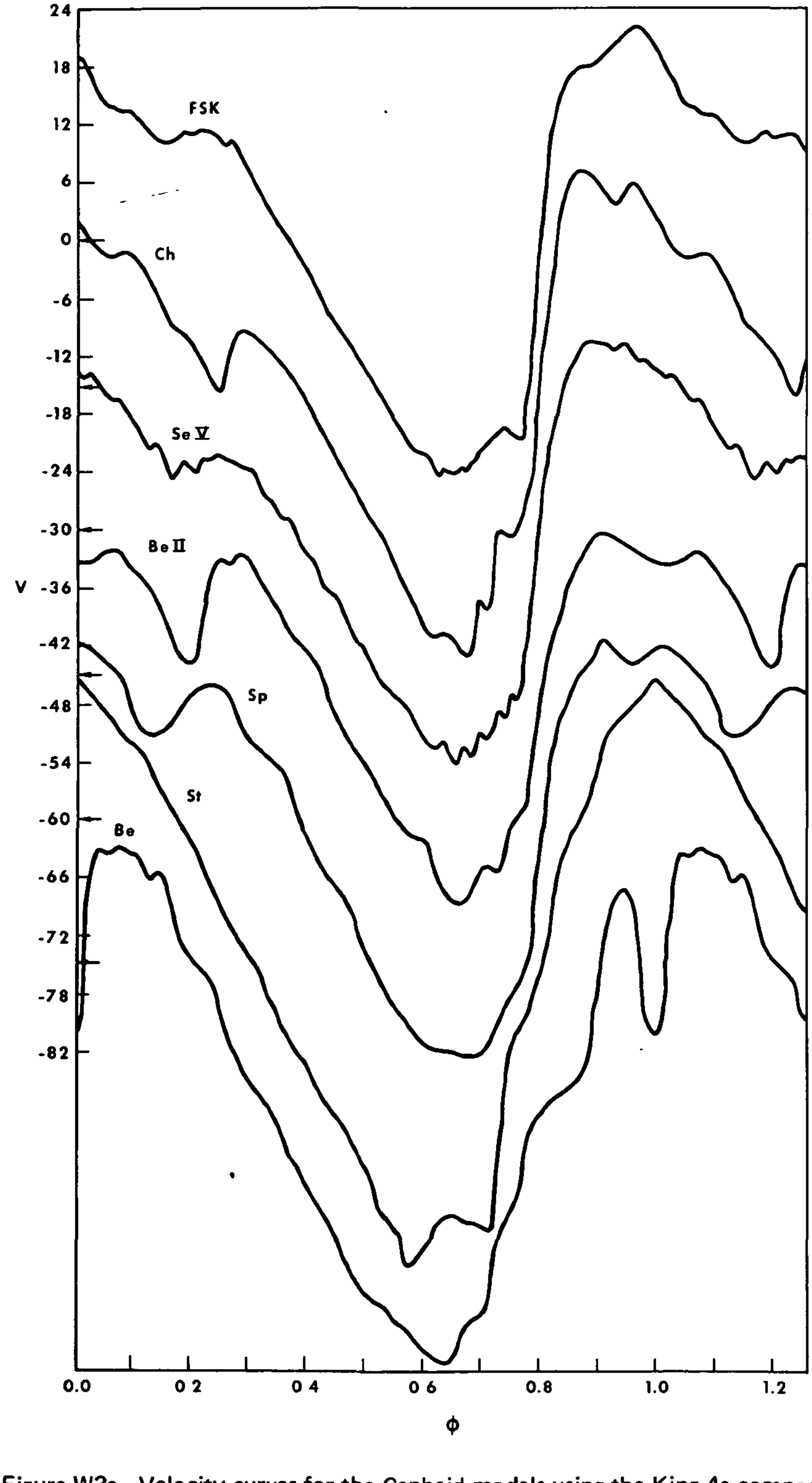

Figure W3a. Velocity curves for the Cepheid models using the King 4a composition.

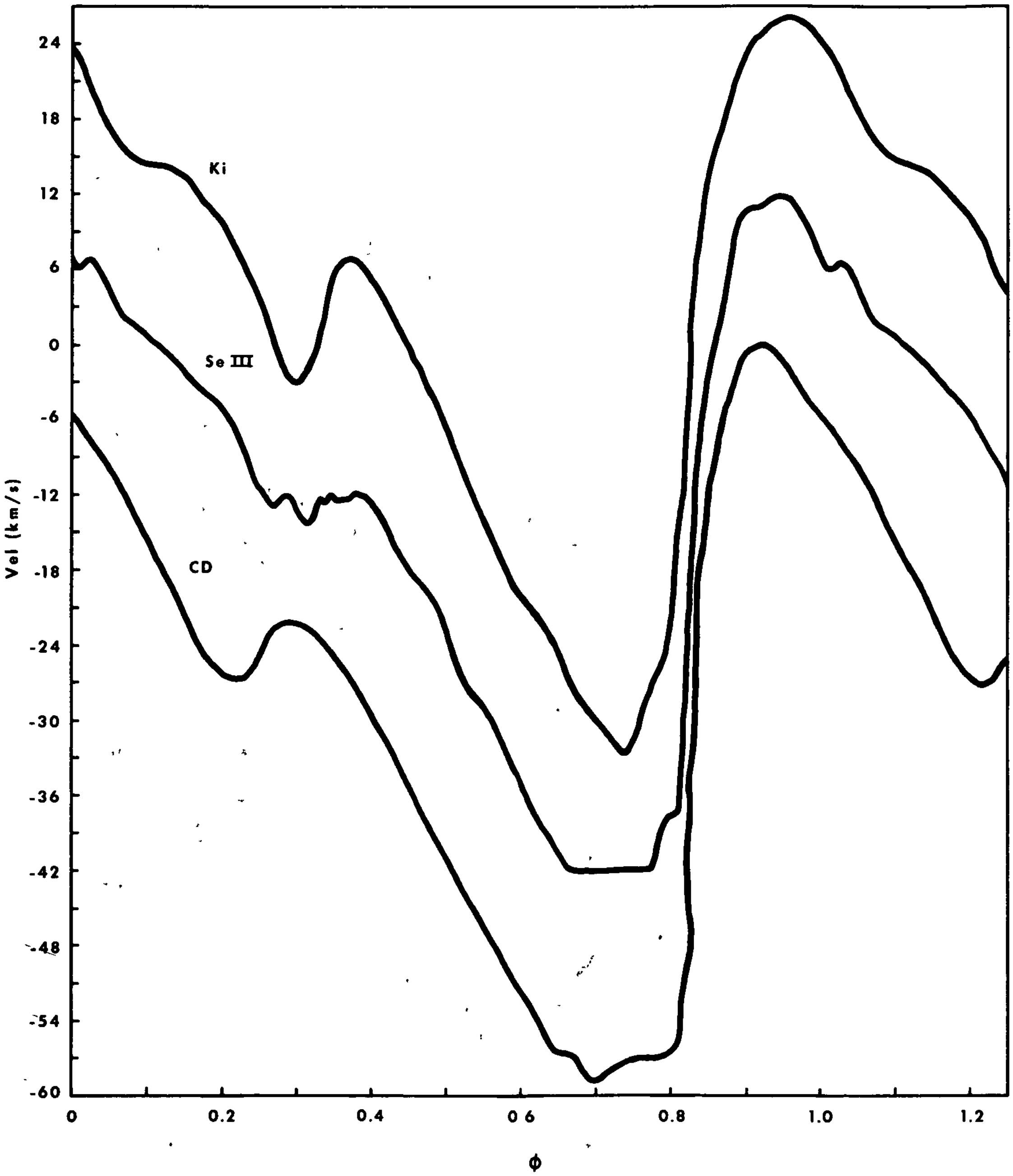

Figure W3b. Velocity curves for the Cepheid models using the Kippenhahn 1a composition.

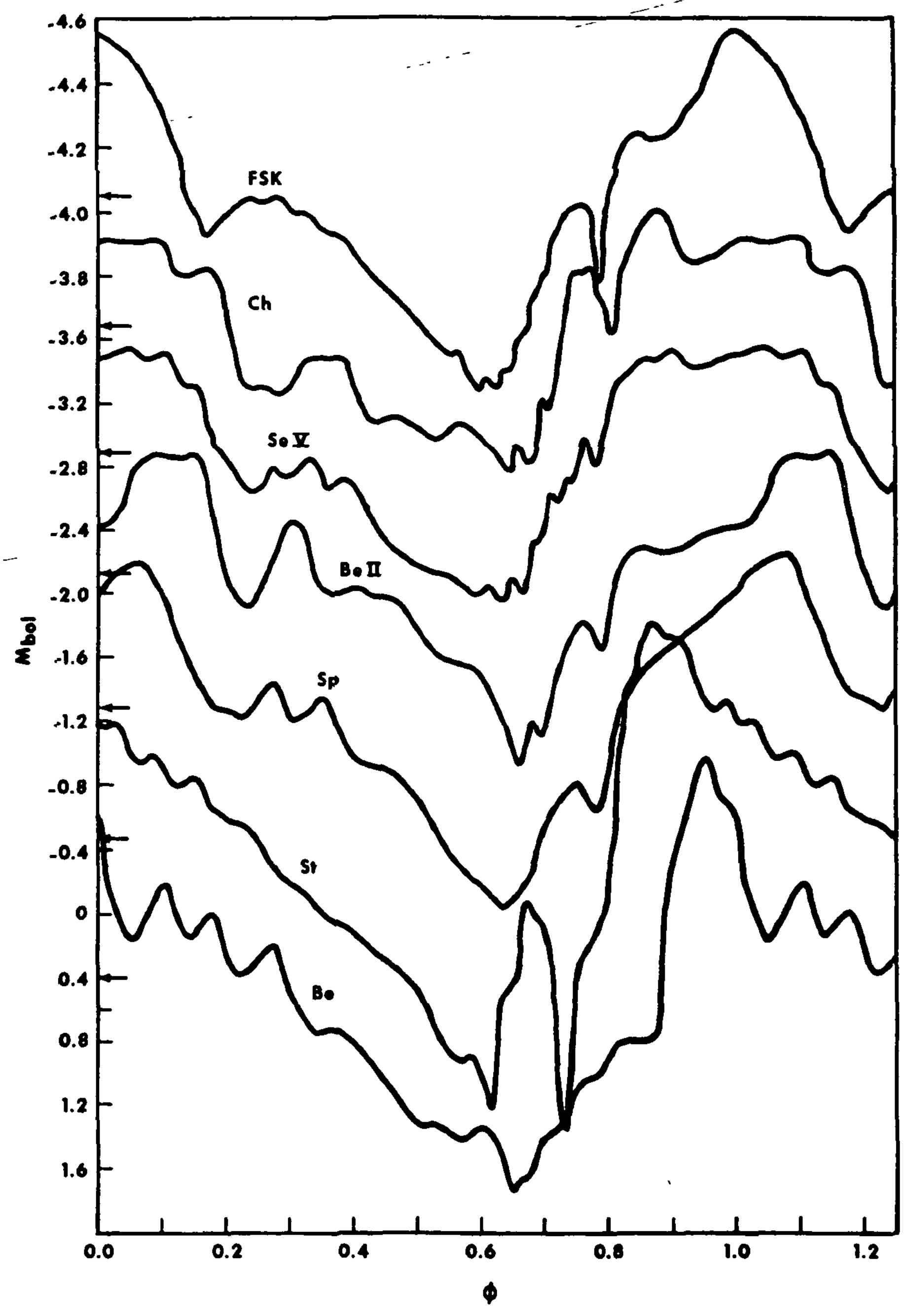

Figure W4a. Light curves for the Cepheid models using the King 4a composition.

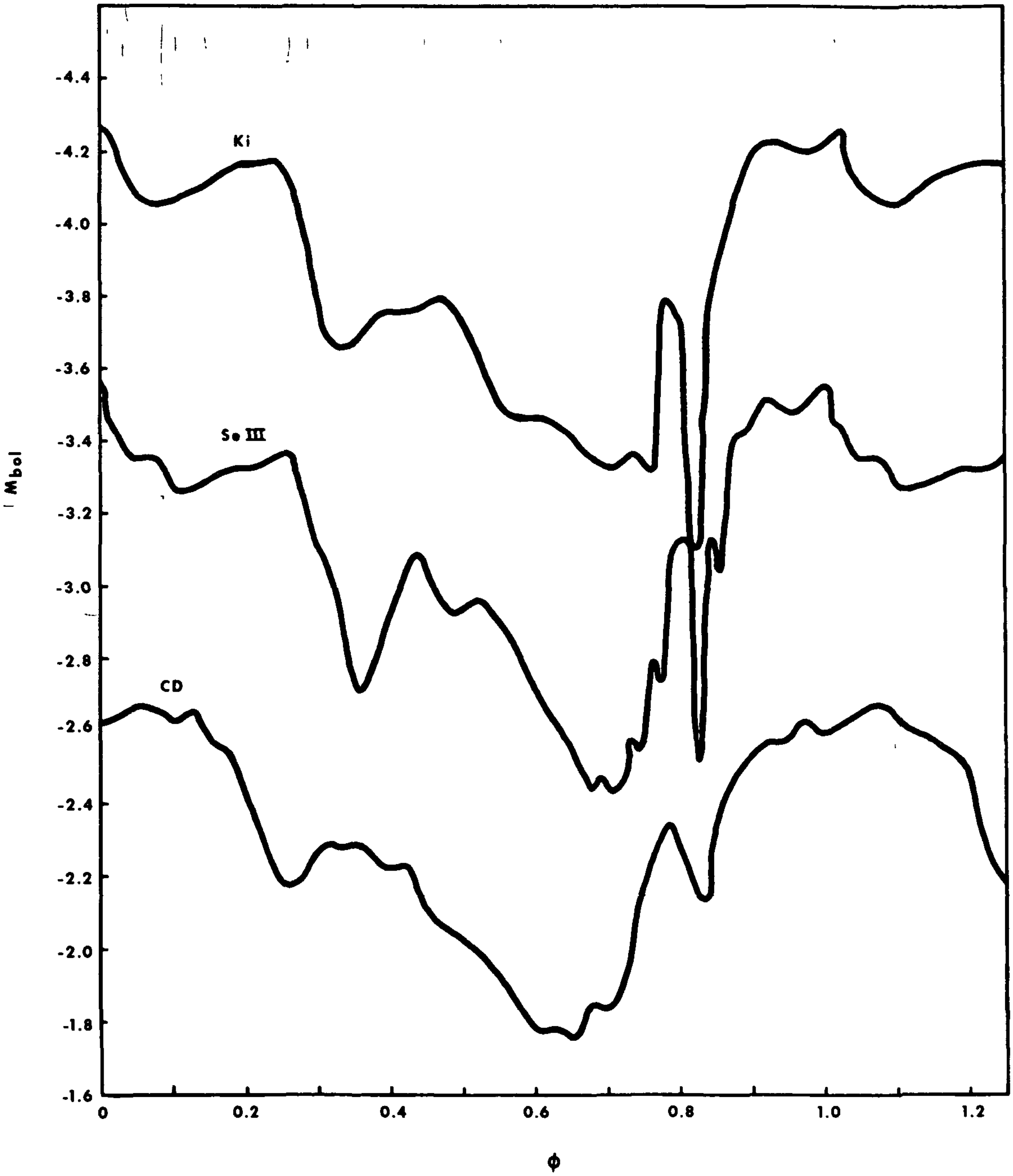

Figure W4b. Light curves for the Cepheid models using the Kippenhahn 1a composition.

COX:

The arrows, yes. Okay.

BAKER:

The scales are slightly different. The phase scale is slightly different. [The arrow pointed to V = 0.]

CHRISTY:

I would say shift the phase because the timing of peak luminosity is the worst place to identify phase zero. So shift the phase and the velocity until the rising branches coincide.

[After the conference, the editors placed all velocity curves and luminosity curves on the same scale and shifted them such that each model agreed in phase at the zero velocity crossing at the rising branch. The phase shifts are specified in Table W1. All reference phases in the discussion are in the shifted scale as in Figures W3a and b and W4a and b. The "bump," unless otherwise modified, refers to the "Christy bump." In these models, it occurs at phase 0.3.]

That is almost a one-tenth shift in phase. I would say to now make the same phase shift in the luminosity that you just did there [in the velocity curve]. What are the features that show up? The amplitudes are comparable.

BAKER:

They are certainly comparable.

CHRISTY:

What optical depth is this velocity in FSK? What zone or where is the velocity?

SPARKS:

I think 0.1.

CHRISTY:

Mine is a fixed mass zone, which is about 0.1 or 0.2 in the static model.

BAKER:

Yours is around what?

FISCHEL:

Ours is two-thirds.

CHRISTY:

Yours is then somewhat deeper. It is a fixed mass zone, is that it. . .

SPARKS:

No. It is two-thirds.

CHRISTY:

It is optical depth at two-thirds at all times. I see . . . What I have identified as a bump [at the same phase] coming out here [at phase 0.3 in Figure W3a] may or may not be their feature, and I wouldn't know without looking at other velocity curves of theirs whether there was an identifiable bump or not.

SPARKS:

Yes. It repeats.

KING:

Does it shift over, become more prominent, as you get closer to the surface?

SPARKS:

That I don't know.

CHRISTY:

I have this plot as a function of depth, so I can see it.

BAKER:

Would you say that most of this structure that you see in there and the difference is probably simply due to zoning?

CHRISTY:

I certainly know in mine, all of this [ripples at phase 0.7] I think is zoning, and I think theirs is zoning too. So I don't identify anything there that I would believe to be different from mine. The only feature in my velocity curve that I believe means anything is this [bump at 0.3].

In the luminosity, I don't know which is which, but it is apparent that they are comparable in character, except mine clearly does not reach the same kind of peak structure as FSK. I think this [at phase 0.8] must be the outward acceleration compression wave coming out through, getting this dip in each one [Figures W3a and W4a]. The amplitudes are comparable, but not very identical.

* * * * *

CHRISTY:

Probably that feature [at phase 0.3] is the same that goes with this in their velocity curve. In my own case, I don't know whether I identify anything very much in the light curve that occurs at about phase 0.27. And I guess this bump is not very good. So I don't believe that I can identify any particular feature. This is a low amplitude noisy light curve. I don't like it.

COX:

Norm, could I ask Bob [Christy] a question here? Could you tell us in your table of systematics what you would expect? Would you expect a bump in this light curve, for example?

CHRISTY:

I prefer to talk about the velocity curve. This certainly should have one and it does, and I have the kind of detailed structure as a function of depth, which is not the most prominent case, but it is there.

COX:

At what phase would you expect that bump to occur?

CHRISTY:

Just where it occurs, here [at phase 0.3]. This is the bump I am referring to in the velocity.

COX:

Surprisingly enough, FSK seems to have got it, not in the velocity curve so much, but in the light curve. About the right phase, though.

SPANGENBERG:

If they plot slightly later velocity, slightly higher . . .

CHRISTY:

If they plotted a velocity of a thinner zone, I think it would have shown up more prominently.

KARP:

If you want to see it. From my 12-day model, I have got velocity plotted at different optical depths, and I am sure you can see the progressive nature and . . .

CHRISTY:

But I wondered if you had the same thing for this model.

KARP:

We haven't plotted it, but . . .

CHRISTY:

That is all I have to say about mine at the moment.

BAKER:

Now, it is a little hard to know what to compare next.

CHRISTY:

I think it would be useful to compare other velocities. As I say, I think there would be a straightforward comparison in velocity curves, although it helps to know what phase shift to use in comparing light curves.

ALEXANDER:

Write phase shift down.

BAKER:

Shall we look at Bednarek now?

SPARKS:

Would you want to say that the velocity curves agree reasonably well . . .

CHRISTY:

I think that there is reasonable agreement there with the exception of the bump, which is unclear as to whether there is agreement or not; this is my conclusion.

COX:

I think the agreement is—I am astounded that they are even there on the same piece of paper. (Laughter)

BAKER:

Let's take the Bednarek one now and compare them.

CHRISTY:

The scale is different, so they are not strictly comparable.

BAKER:

How different—let's see.

SPARKS:

I guess they were done at different times by different photographers.

[Different photographers effected different photographic reduction in size of the viewgraphs used.]

BAKER:

Unfortunately, we can't see the velocity scale.

SPARKS:

The arrows should line up. Dave [Fischel], on the velocity, can you line the arrows up?

BAKER:

The luminosity amplitudes are certainly comparable.

COX:

We should ask, I guess, the same question, for the velocity curve; at what level does this refer?

BEDNAREK:

Yes. This is $\tau = 2/3$.

KING:

It would be interesting to compare this with their plots.

COX:

Yes, FSK had $\tau = 2/3$.

[FSK and Be were overlaid]

VON SENGBUSCH:

The Be has no bump in the descending branch at all.

BAKER:

Right.

COX:

The other two have bumps at different places.

CHRISTY:

I just compared my calculation of this. At a greater optical depth corresponding to the optical depth at which FSK is and at an optical depth around 1.0, my bump becomes very much like this shape. It is very much washed out and very slightly earlier in phase. It is much closer to this at optical depth 1.0.

BAKER:

So the differences are probably due to different depths.

CHRISTY:

I think it is an optical depth difference and a slight timing difference, but primarily an optical depth difference.

ALEXANDER:

At shallow optical depth in Be, is there a bump if you go further out to $\tau = 0.1$?

BEDNAREK:

Yes, I believe so, but I can't recall for sure.

KING:

If this is a longer period, it may very well be that the thing that–if you look over here on the right [at phase 1]–shows up as looking like the biggest thing may actually be a mixture of the peak and a bump. Because he has a longer period . . .

ALEXANDER:

He also has the spike in the luminosity.

KARP:

That dip [at phase 1], though, looks like the artificial viscosity dip.

CHRISTY:

This is kind of a big peak in the velocity curve here [at phase 0.9]; I must say this is an unusual structure.

SPANGENBERG:

It looks very much like a harmonic.

BAKER:

It looks like a peak in a rising branch, really.

CHRISTY:

Not the sort of thing that I normally see.

ALEXANDER:

How does the spike in the luminosity agree in phase with what's happening in the velocity curve?

KING:

Could we lay the velocity curve and the light curve on top of each other? They should at least match in scale, I suppose.

CHRISTY:

Yes.

KING:

For that one case, Be, overlay light and velocity curves on top of each other.

BAKER:

Then the velocity is the one that . . .

KING:

The one with the double bump.

BAKER:

Yes, the one that goes way down.

COX:

Could we ask Bednarek about that big dip in the right there in the velocity curve, I guess it is. Is that zoning?

BEDNAREK:

That dip still doesn't persist for smaller optical depth.

CHRISTY:

Let me say that what worries me about it is that it's an inward acceleration that looks to me as though it is faster than gravity. That is, you can push things out fast, but it is very hard to get them to accelerate inward fast. How do you do it?

SPANGENBERG:

Did you have to interpolate to get the $\tau = 2/3$?

BEDNAREK:

This may be a problem in the way I plot velocity. I was just taking the closest zone to $\tau = 2/3$.

CHRISTY:

Zone switching.

COX:

It isn't a smooth interpolation.

BEDNAREK:

I could plot it up for one zone.

CHRISTY:

I think you had an inward acceleration faster than gravity, which is disturbing.

BAKER:

You didn't try to interpolate between zones, but just took whichever one was closer.

I was going to say that I thought the ones that were used in more or less transport-type approach might differ, but they really shouldn't differ so much in the velocity. Why don't we look at Spangenberg.

SPANGENBERG:

There should be three velocity curves out there at $\tau = 0.1$; a surface zone; and an intermediate zone, about five zones in. The $\tau = 0.1$ is interpolated.

BAKER:

When you say five zones, which is the one that is, say, most comparable to. . .

SPANGENBERG:

I think you can watch the feature move and perhaps put the $\tau = 0.1$ on with the Christy velocity first. I get a bump there at about 0.23.

CHRISTY:

It hasn't quite repeated.

BAKER:

But the phase scale on the two graphs is different, you see.

CHRISTY:

Is this the bump?

SPANGENBERG:

That is the bump.

CHRISTY:

This is the same model. Most interesting! Something caused different phasing there.

KING:

Maybe you ought to look at the ones where you had a constant zone.

SPANGENBERG:

Right. Now you could watch that bump. If it moves later in phase . . .

BAKER:

Wait a minute. The phases are not comparable. Let's take the rising branch. Here it is at 0.78 in Christy. So in fact, there is a displacement of 0.2 relative to Christy.

COX:

Yes, we tried to match the bumps.

CHRISTY:

So the difference in phase between here [the bump] and here [the zero crossing of the rising branch] with me is approximately 0.5. . .

* * * * *

CHRISTY:

They are not so different in phase then. Maybe a tenth difference in phase.

BAKER:

How much of that can be because of depth?

SPANGENBERG:

Mine is probably a little higher up than yours.

COX:

Want to try another one of your levels, Bill?

SPANGENBERG:

I have a few more plots of two zones higher up. One says zone 55 and the other says the surface. Zone 55 is an intermediate depth.

ROSENDHAL:

Could we also compare the three levels?

BAKER:

Okay.

CHRISTY:

The amplitude, by the way, goes to minus 18–the amplitude is very similar because this is 26 or 27, isn't it?

SPANGENBERG:

Yes, and it goes up to about 18 or 19.

CHRISTY:

Whereas mine goes up to about 23 or 24.

COX:

So that, unfortunately, Bob, is highly dependent upon the viscosity.

KING:

And he is using more viscosity than you are.

SPANGENBERG:

No, I am probably not, really, because I have the sound speed cutoff.

BAKER:

Yes, but he is using the cutoff.

SPANGENBERG:

Right. I have done this–and maybe we are getting to the afternoon discussion topics. Try turning on different viscosities at full amplitude and we can make some comments about these effects. But maybe not now.

I would like to see the three Sp curves [Figure W5].

BAKER:

All right. After that we will compare one of the other Spangenberg and Christy.

CHRISTY:

This is optical depth 0.1.

SPANGENBERG:

Zone 55 is the next.

COX:

Maybe you can tell us already, Bill; maybe they all agree.

SPANGENBERG:

I think you can see the bump move.

CHRISTY:

That is zone 55.

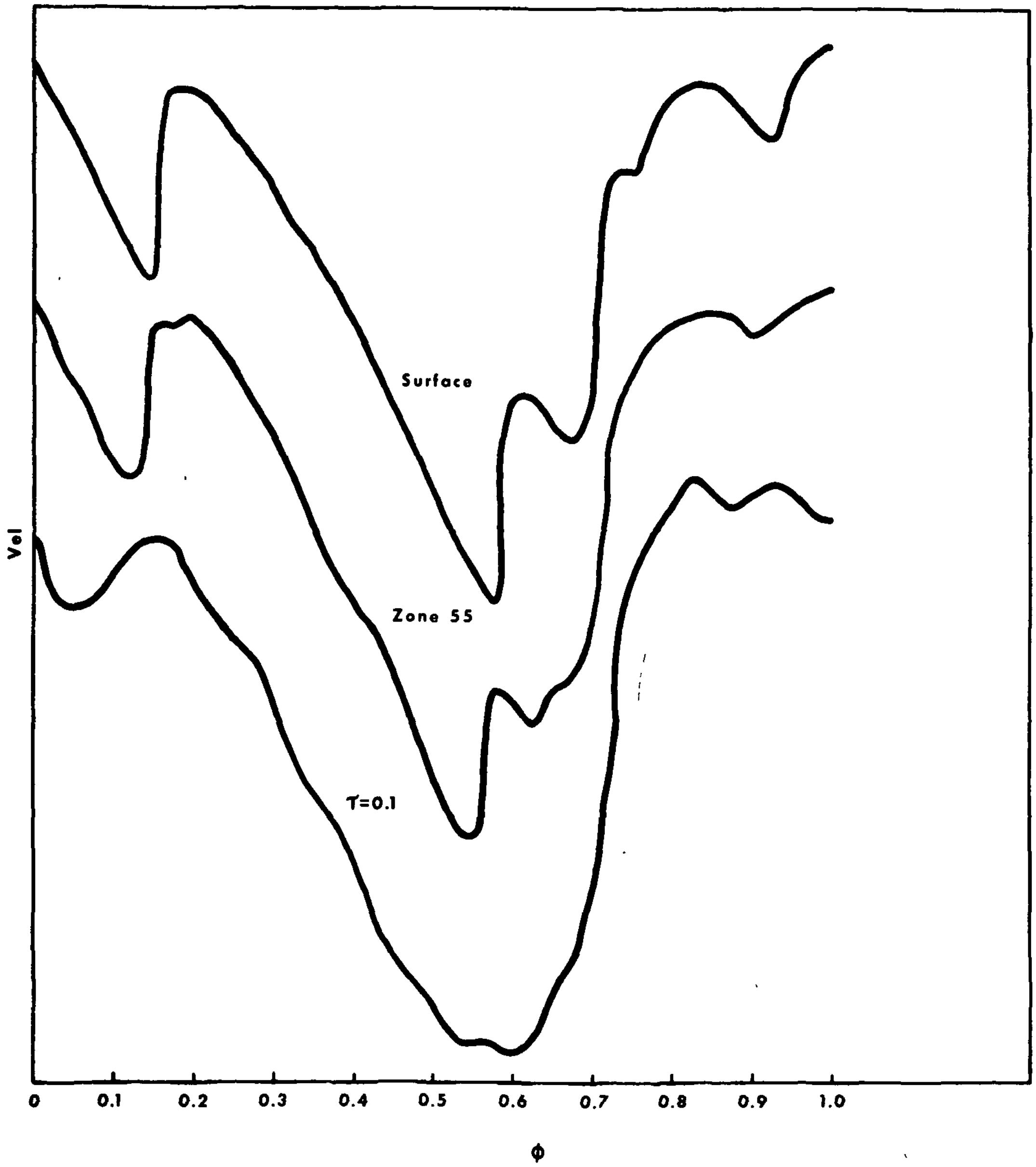

Figure W5. Velocity curves for three different levels in Spangenberg's model.

SPANGENBERG:

See, it has moved out in phase. It is deeper and has moved out.

CHRISTY:

Here it is now in 0.1. It is more prominent here, and it has moved out to about 0.14.

SPANGENBERG:

The velocity amplitude scale has been changing as well. You can't get it on a prescribed scale.

CHRISTY:

At the surface it is getting more prominent still, and moving out a little bit more.

COGAN:

Displace the three graphs vertically a little bit so we can distinguish between them.

BAKER:

Okay. The upper one is the zone 55 [middle graph in Figure W5], and the bump is getting later.

COX:

At the very thin level?

SPANGENBERG:

Yes. Probably about a 25-kilometer jump.

BAKER:

That, in essence, increases the discrepancy with yours.

KING:

It decreases the discrepancy.

BAKER:

I thought yours was deeper.

CHRISTY:

Mine is at a fixed mass zone. At this phase, his optical depth is less than mine.

BAKER:

I see.

CHRISTY:

At the static model, it is about at optical depth 0.1. At this phase, the atmosphere is expanding.

BAKER:

I don't think there is any need to compare it further.

COX:

What we learned is that he does indeed get better agreement with Christy; is that right? Because he moves a little bit later?

CHRISTY:

No. I would say that the existence of the bump is clear in yours, and that there is a progressive wave going out and a phase that is slightly earlier than mine, but it is comparable in amplitude to the one that I get.

SPANGENBERG:

And the disturbance grows.

CHRISTY:

Qualitatively, it is in agreement; and quantitatively, you would have to compare more carefully to see how big the agreement or disagreement is.

HILL:

I think it is also interesting to observe that the second bump was also in good agreement with yours and that was one of the . . .

SPANGENBERG:

Which?

HILL:

Over on the right-hand side of phase . . .

BAKER:

The maximum.

CHRISTY:

I see. I said that I had no faith in its reality—the bump over here [at phase 0.9].

SPANGENBERG:

How did that feature get over there?

CHRISTY:

I admit that I had a bump there that I did not know the meaning of. So that may have some significance, too. Can you slide those curves a bit so that we can separate these over on the right.

CHRISTY:

Whatever you have there, it is consistent.

ALEXANDER:

Does FSK have anything there?

HILL:

No, I don't think so.

STELLINGWERF:

There is also a feature that starts near minimum. It is not visible on the $\tau = 0.1$, but it is visible on the zone 55 and appears to be moving outward on the surface [Figure W5].

KING:

Like the features near the bottom.

SPANGENBERG:

Yes. It is not on the [$\tau = 0.1$] curve. But the next curve has a disturbance and it has grown in the later zone at a slightly later time.

KING:

Isn't that the viscosity bump?

SPANGENBERG:

It would be nice to clear that up.

CHRISTY:

In your light curve, there may be something in there. Your bump was coming through at about this point [at 0.6 in Figure W5] and. . .

SPANGENBERG:

That surface zone is where the luminosity is determined. You might want to see what the disturbance looks like a little later.

BAKER:

Otherwise the luminosity amplitude seems to be a bit higher.

SPANGENBERG:

Are the scales the same?

CHRISTY:

Yours goes from about 4.5 down to 3.4 or 3.5, so it is about one magnitude, whereas mine was only about 0.6 or 0.7 of a magnitude. So you have a larger luminosity amplitude.

BAKER:

We no longer have this problem about difference in treatment of radiation pressure, do we? Do you have radiation pressure now in the usual way?

CHRISTY:

Yes. You have the same minimum in this region. But I think that you agree better with FSK in the general trend of the luminosity. I didn't like my luminosity curve. I don't have any faith in it.

BAKER:

Can you compare one of these with FSK?

CHRISTY:

Compare the luminosity with FSK. I think it will show better agreement. In fact, I would be happy if you just eliminated my luminosity. (Laughter) Yes. I think that, apart from a difference in scales, the general features are rather similar.

There is a structure here [at phase 0.8] that I would say corresponds to that structure.

BAKER:

Yes, Spangenberg's data are very smooth, presumably because of the large number of zones. But the structure is similar.

DAVIS:

And the damming-up due to the pseudoviscosity toe reflects itself in the model because of the perturbation of the velocity curve. The spike in the luminosity due to pseudoviscosity should be correlated in phase with velocity.

CHRISTY:

This spike you mean, here? Normally the dip comes through right around 0.8.

HILL:

I have a question about that, if I may. You talk about damming-up due to the viscosity toe.

CHRISTY:

I don't use those words. (Laughter) What I said was that roughly at the time when you get the steepest rise in the velocity is the time when you get this dip in the luminosity.

HILL:

I wonder if without artificial viscosity, you would also get it, because you are having the greatest compression itself which would . . .

CHRISTY:

Well, that is what I said. It occurs after that time.

HILL:

Yes, I don't know if it is an artificial viscosity.

DAVIS:

But it is an artificial effect. I don't think it is real. I think it is artificial, due to the artificial viscosity or the compression.

SPANGENBERG:

What kind of time resolution are we talking about? I don't want to ascribe reality to all those wiggles, but that is a very short time scale.

COX:

But that is exactly the kind of time scale that would arise.

* * * * *

BAKER:

Perhaps we should discuss the details this afternoon.

I think it ought to be interesting now to compare, at this luminosity, the von Sengbusch V, I think it is called, and the Stellingwerf model, so could we put on the Se V and compare. I think you should compare with Christy since I am a little bit worried about having . . .

CHRISTY:

No, eliminate my luminosity curve, which I think is confusing more than anything else.

BAKER:

All right. We will adopt FSK as the standard luminosity and Christy as standard velocity.

CHRISTY:

Sengbusch V—there is remarkable agreement.

COX:

Yes, except it is off in phase by 0.2 [relative to Christy].

KING:

Yes, that is the photosphere, though, again. That is the same thing you had . . .

VON SENGBUSCH:

This is the photospheric velocity.

KING:

Looks just like the FSK.

CHRISTY:

Yes, looks very much like the FSK velocity of the photosphere. Put the FSK velocity in there instead of Christy. Very close agreement. You line up those phases—well, the phasing is very close.

COX:

The arrows are different by about one-tenth of a phase.

CHRISTY:

That is just the peak luminosity. As I say, the worst place to identify phase is the peak luminosity. If you use this point [the zero velocity at the rising branch] to identify phase, then people will agree. That is pinned down much better.

BAKER:

There is a slight difference in the structure of the bump, but really those differences are so small compared to . . .

CHRISTY:

I am curious that von Sengbusch's luminosity is more like mine. Except that it is better, of course. But its feature of not showing this late maximum here [at phase 0.4], but more of a broad flatness, is more like mine. It shows this bump tolerably well, which mine does not.

KING:

The FSK has lots of thin zones, which is quite a difference. Your two are about the same number of thin zones.

BAKER:

Yes. That would affect luminosity more than the velocity, wouldn't it? Yes.

CHRISTY:

The flatness here is more like mine, which is probably the result of our similar zoning.

BAKER:

Yes.

CHRISTY:

You may eliminate my luminosity again. (Laughter)

BAKER:

Bob, you don't seem to like your luminosity.

CHRISTY:

No, I would say this feature is rather similar in both these.

COX:

To be fair, one ought to shift the Se V a little bit more as it has been in the other pair.

* * * * *

VON SENGBUSCH:

I don't get this big peak in maximum light.

BAKER:

The phase of the bumps is really just the same. It occurs in both cases at about 0.3. The agreement looks amazingly good. Let's compare Stellingwerf, then, with FSK. Let's see what the differences are first. What about the amplitude?

CHRISTY:

The amplitudes are comparable, although Stellingwerf is a little bit higher. What is the scale?

BAKER:

Stellingwerf is 20.

COX:

It goes to about 25, almost 30.

CHRISTY:

Almost 30, whereas von Sengbusch is more like 21 or 22.

STELLINGWERF:

That is at the surface.

BAKER:

This is the surface. Fixed mass zone at the surface.

CHRISTY:

That usually shows a somewhat higher velocity.

BAKER:

Yes, so it goes down to almost 30 and up to almost 30.

ALEXANDER:

Where's the bump?

CHRISTY:

No bump.

STELLINGWERF:

I would say the bump is halfway up the rising branch. When you plot the inner velocities along with that one you can see a definite, well-defined shock coming out at phase 0.60 there, steep and then not so steep.

CHRISTY:

An inner boundary to where you have it. I would think that requires looking at to see what happens.

HILL:

Would that explain why it is coming back so soon?

CHRISTY:

It could very easily, because it is so much shallower.

STELLINGWERF:

That is what I would say. That also produces a large feature at 0.8 in the luminosity curve.

BAKER:

You say there is a bump here [at phase 0.8]. Is that right, Bob?

STELLINGWERF:

Yes. There is definitely a shock wave coming out there.

BAKER:

There is a shock coming out there. Would you identify that with the bump? Now, FSK has a bit of a—well, let's be careful here because they have a dip also. It comes a little bit later in phase, but it is there. And it is not so big, of course.

CHRISTY:

I would bet that your inner boundary is distorting these particular features.

STELLINGWERF:

I would say that the bump arrives too early because of that. I think it is interesting that the FSK bump, when compared to my model, which presumably doesn't have any bump, shows up not as a bump but as a depression at phase 0.2.

CHRISTY:

This is what we have been referring to as the bump, and this [at phase 0.8] is a depression associated with the main wave coming up out through the surface.

ALEXANDER:

The scales have been changed. That would change the slope of the curve.

STELLINGWERF:

I am looking at 0.33 now. I am assuming that in my curve there is no bump at that phase.

CHRISTY:

There is no evidence in the velocity, anyway.

STELLINGWERF:

It comes at a similar phase. If there really is a bump, I would expect FSK to be higher in that region.

KARP:

I think if you lay a pointer along the FSK curve, it looks like it is joining and that really it is a darkening [at phase 0.2] rather than a bump [at phase 0.3].

STELLINGWERF:

Could that be just an effect of the scales?

KARP:

You see what I am saying about that straight line at minimum light?

CHRISTY:

You are saying that FSK shows a depression rather than a bump.

KARP:

If you lay a ruler right along where it comes in almost tangent to the peak.

STELLINGWERF:

Could that be caused by increasing opacity caused by a shock coming out?

COX:

That is the wrong time. The shock comes out on the rising part of the light curve.

CHRISTY:

It is coming through the photosphere somewhere around 0.2, so it is conceivably making a depression rather than a bump.

VON SENGBUSCH:

That is something I observed in many light curves. It never got a feature, which you would obtain from an observational curve where you really have a little bump on the descending branch, but rather a depression on the. . .

CHRISTY:

I noticed in the number of light curves I showed yesterday that the clearest thing was a depression in the light curve at the time the bump comes through. So this may be a way to identify it in the light curve.

BAKER:

A different question that I think we shouldn't get into is how this might be compared with the observational bumps.

HILL:

This comparison is interesting, because we don't have a pressure wave going down to make the bump, and it appears that what is going on in the light curve is not really being triggered by the ionization zone or by the energy it's releasing. Instead, we have an extra depression associated with that bump, which then means we are no longer looking as deep into the atmosphere. Then my luminosity is going to drop off. The luminosity comes back to normal once that compression wave has gone out through the atmosphere. That would explain why you could draw a straight line and it looks like a depression.

COX:

Somewhat the same words that Hillendahl might use for his artificial viscosity toe–blanks it out for a while.

HILL:

No, not the artificial viscosity. I am making the argument that you throw away viscosity and my argument would still apply.

COX:

I understand. You are not having any artificial viscosity in your explanation, but it is the same kind of physical phenomena. You see deeper for a while and then you can't.

HILL:

Yes. Does that sound like good reasoning or not?

CHRISTY:

I think it may well be the physical explanation of this that, with the compression wave, you see less deep into the atmosphere and therefore it shuts off the light for a while. That may be the explanation.

HILL:

What I am thinking is that it would be awfully nice if that were the case, because then, without looking at the velocity curves and only looking at luminosity curves, one could say something about the velocity curves.

CHRISTY:

Is that the last of them?

BAKER:

Yes, that is the last of them, I think, that are comparable.

CHRISTY:

Should we try some of the others, then?

BAKER:

Yes. I believe we just have three at 1.1. Maybe we can look at CD, King, and Se I.

COX:

One must remember, Norm, and I guess we are aware of it, that what you are doing here is comparing three different theories: the von Sengbusch periodic-type theory, versus the King standard classical diffusion-type, or the rather fancy Davis transport theory.

BAKER:

Yes, right.

[For the following discussion, refer to Figures W3b and W4b.]

CHRISTY:

Hopefully, it is the same underlying mathematics, and they should be the same physics. For von Sengbusch I and King, I would say there is fairly good agreement there.

BAKER:

Yes. Again we have some scale problems.

CHRISTY:

Not too bad on the horizontal scale. Se I is a different scale. That is not going to work. But I would say qualitatively, they seem to agree very well.

KING:

My velocity is a fixed interface. Next to the outside interface going from the boundary.

CHRISTY:

I would say on the whole there is rather good agreement between these.

BAKER:

Yes, certainly more agreement than we were having in the other model. But the difficulties here are scale. The general features are there even in von Sengbusch, aren't they?

CHRISTY:

The general features of luminosity are the same in all of these.

BAKER:

We have got the one big dip in the King. The King dip is really quite a big depression.

CHRISTY:

I think that they are the same general feature of either the dip or the bump in the light curve for each of them at this stage, so that it seems to be fairly . . .

BAKER:

Do any of the manufacturers care to comment on their product here?

DAVIS:

At one stage, Janet Bendt and I made the attempt in our nonequilibrium diffusion gray approximation to agree with King, Cox, Eilers, and Cox. To do that, we had to find out what John Cox's boundary conditions were and make a judgment.

We found that within the approximations we could agree exactly. They're independent codes; they're not related except for the same opacities and equation of state. So you can find agreement in the gray case. In this case, I don't think that with transport—this is a full multigroup transport calculation—it makes a difference.

COX:

That's what we have been saying for a long time. What model did you check?

DAVIS:

This is Kippenhahn VII. We agreed in detail, even to the little wiggles.

BAKER:

What I'm wondering about is why the luminosity curves are or are not different?

CHRISTY:

We're comparing 1.1 luminosity here.

VON SENGBUSCH:

Se I is the wrong luminosity. It's luminosity is 1.24, but it has the Kippenhahn mixture.

If we had Se II or Se III we could compare. They differ in the number of zones. The one has 40 zones and the other has 48.

BAKER:

Take off Se I and put on Se III.

KING:

This is the Kippenhahn mix, but with a luminosity that is higher.

VON SENGBUSCH:

A higher luminosity.

KING:

And yet they look very similar.

COX:

The important thing, as we know, is that the pulsation is caused by helium, and hydrogen you always have to contend with. Those calculations are the same. I'm not sure the opacity makes any difference.

CHRISTY:

It doesn't.

KING:

The radius will be the same.

VON SENGBUSCH:

That's the fine grid model.

CHRISTY:

Where is your velocity curve plotted?

VON SENGBUSCH:

At $\tau = 2/3$.

CHRISTY:

At a photosphere again.

COX:

Gil has it there at 0.1.

CHRISTY:

The bumps don't show up so clearly at that point.

BAKER:

The phases are shifted the same amount.

VON SENGBUSCH:

The point I want to make on this curve is that one sees the bump as a dip going down, rather than a bump going up.

SEVERAL:

In the luminosity?

VON SENGBUSCH:

Yes.

CHRISTY:

I think this may very well be the most characteristic thing, that there is a dip in luminosity.

COX:

That's the Davis one?

CHRISTY:

Which is this one?

BAKER:

That's CD.

CHRISTY:

I can't tell at which scale it is. It's probably around 0.24.

BAKER:

Yes.

CHRISTY:

And CD—the compression's going through at 0.24. I think if you identify halfway up the rise here, which is when the compression is at about its maximum acceleration, it would correspond pretty closely to the dip that you see in the light curve.

BAKER:

Gil, at what depth is it?

DAVIS:

This is $\tau = 0.1$.

BAKER:

And the emergent luminosity. What about the amplitudes? The amplitudes really are comparable, aren't they? Von Sengbusch goes up to about 24. The others go up about 27 or so. King goes down to minus 28; von Sengbusch has this bottom at about 25.

VON SENGBUSCH:

That's because it was out of range. (Laughter)

CHRISTY:

This [graph] is plotted by a machine that didn't know enough to go over the scale.

BAKER:

The agreement is not quite as good as it was with the wrong one, but it's still pretty good.

COX:

I'd like to make a couple remarks here. Apparently, in almost every case, the Christy bump appears, although we have cases where we didn't get a Christy bump. We've seen some slides without a bump.

CHRISTY:

Stellingwerf's was the only one in which there is clearly no bump.

COX:

I thought Be didn't have a bump either. It appears one has to be awfully careful or otherwise you won't see the bump.

CHRISTY:

You have to look in the right place for the right thing.

COX:

Secondly, if you do find the bump, that's fine, except that there is an uncertainty in both the velocity and light curves by about one-tenth of a phase; so that even if you do find the bump, you may not get it in the right place, whatever the right place is.

I guess my question to Christy is, it appears as though there may be something like a 10 percent timing difference in the echo time, which would then imply a 10 percent radius difference.

CHRISTY:

I think we would have to look at the numbers a little more carefully to be sure. You see, there are some differences in periods, some differences in scale, and I would like to more carefully identify what the phasing differences are when this bump occurs.

In that connection, I would find it most useful to time the bump in days from the previous peak, essentially one period plus the fractional period, and to divide it by the radius in solar units to see whether that number agrees with mine, because this is something that is somewhat independent of the exact model you have used.

COX:

I agree 100 percent. I was just wondering whether you may get a jitter in that number by about 10 percent, which would imply an uncertainty of 10 percent in the radius.

CHRISTY:

I would like to see this in order to find out what difference the different programs actually give on this thing.

KING:

If the periods are different, though, then . . .

CHRISTY:

If you take that number in days and divide it by the radius, it turns out it's rather independent of the period and everything else in my calculations.

BAKER:

I certainly would like to see it cleared up a bit. I think Stellingwerf's calculations will be useful in this respect, but it looks very much as though in his case the bump is being reflected from the inner boundary, and yet I still worry. Well, we can do some back of the envelope calculations. I find it rather strange that the phase should have been changed that much by the reflection at that point, because I would have thought . . .

CHRISTY:

I am not surprised.

COX:

Christy talks an awful lot about the deep part in the 10-percent level of the stellar radius, and he found that the echoing was not important at that level.

BAKER:

What I don't understand is why the wave isn't propagating so fast down there.

CHRISTY:

Not at 58 percent of the radius.

KING:

It's about 150,000 K there.

BAKER:

Has anyone actually drawn the characteristics?

CHRISTY:

I have done some calculations, yes. I have drawn some. I can show you, if you would like. There is a peculiar behavior.

STELLINGWERF:

One of the slides you had yesterday shows the characteristics quite well. You can identify 0.58 on these. You can see when we would expect it.

CHRISTY:

I find the characteristic goes in here [left dashed line in Figure W6], and the characteristic comes out here [right dashed line]. But if I trace the characteristics down, they don't meet. So it looks as though a sound wave coming in at this time reflects from the center a short time later, the time being essentially the shortest period I find at the innermost point I take.

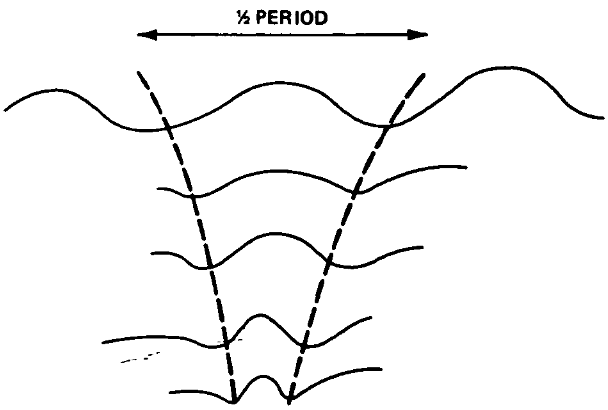

Figure W6. Velocity curves for several zones near the lower boundary.

COX:

What level are you talking about down there?

CHRISTY:

That's around one-tenth of the radius. What I assume is that there is a very short period twitch down [at the bottom], and it seems to be initiated by [the ingoing wave]. That causes the upward-going wave. But from the characteristics, one sees that this signal coming in does not seem to precisely give that signal going out. There is a slight delay corresponding to one oscillation at that bottom point.

ALEXANDER:

Can you point out where 0.6 radius is?

SPANGENBERG:

That would tell us what phase difference to expect with respect to Stellingwerf.

CHRISTY:

The 0.6 radius is at approximately the number 16 zone, and the number 16 zone looks like this [top curve]. These waves still have a long way to go.

ALEXANDER:

What's that phase shift.

CHRISTY:

About a half a period.

BAKER:

It might very well explain the difference that Stellingwerf found.

CHRISTY AND STELLINGWERF:

Yes.

STELLINGWERF:

This is the velocity increase here [at the bottom] and this is your extra wave here.

CHRISTY:

It's around zone 16.

BAKER:

We can at least say that there seems to be some agreement that that is probable.

COX:

Somebody suggested that over lunch time maybe people could actually try that Christy formula. All we have to do is look at our curves and find the time between the rising part of the velocity curve and the bump.

BAKER:

Are there any other comments or suggestions? One comment I would like to make is that I think it would be good, if after lunch, we agree in a general kind of way on the subjects that we want to talk about. We can't talk about every detail, and I think it would be nice to make a list of the things we want to get through in the afternoon. That would help me in my job, too. You might think about that over lunch.

[Immediately following the lunch break, the Christy phase values were entered into the Table W1 showing general agreement.]

COX:

Do we have any more curves to compare?

BAKER:

We have some other von Sengbusch models, but I think that's all.

KING:

We have other things, such as the structure. I don't think the structures are going to show much difference on the scale that has been plotted.

BAKER:

My thinking is that, contrary to anything that I think we expected, if there were any chemical inhomogeneities, if there were sudden jumps in density somewhere, say, exterior to the inner boundary, this might well change the bump.

CHRISTY:

It certainly could, if there was a sudden jump at 30 or 40 percent of the radius.

BAKER:

That seems pretty unlikely.

COX:

Except to put together a story that is tenuous; maybe there is mass loss in spite of the fact Warren [Sparks] doesn't know how to get the stars to evolve there. So maybe the outer 30 or 40 percent of the star is stripped away. The rest of the star might have some inhomogeneities down near the bottom.

KARP:

I found something interesting in the models, when I plotted the velocity, or the kinetic energy. They both went asymptotically to zero as they approached the bottom boundary, so I plotted the momentum. Because the density rise was so steep, it kept going up. I got within a few zones of the bottom boundary, where I forced it to a velocity of zero. It came crashing down to zero, almost like it's really held by that lower boundary condition. I'm wondering if anyone has ever tried computing a model with a free lower boundary?

BAKER:

Certainly Christy has played with that boundary condition.

CHRISTY:

I've moved the lower boundary around.

KARP:

But have you ever just extrapolated the pressure gradient across the lower boundary and let it move?

CHRISTY:

No. Stellingwerf does that, but at a rather shallow point.

KARP:

It's just in the momentum, not in the kinetic energy or velocity.

BAKER:

I found this an interesting point, but could I suggest that we make a list of what we want to talk about so that we don't find when we get to 5 p.m. that there is some important topic we have left out. I think certainly the opacity is something . . .

CHRISTY:

I don't know if there's any real evidence that the complications are associated with opacity differences.

BAKER:

I think it would be very nice if we could have a discussion—particularly since Spangenberg's work—about what's available. Should there be a standard model from now on with a standard set of opacities that everybody has to do before they can publish anything. (Laughter)

I'm certainly intrigued by these opacities.

KARP:

Also, do you want to look at the structures?

CHRISTY:

I think it would be useful to look at them. I don't think we will see anything except the extent of the model. It will show very clearly where the model went.

COX:

I don't particularly want to talk about opacities in the sense that you do. However, I would like very much to try to ferret out one more time from Bob Stellingwerf's very interesting graph of the transition line, using the Christy formula. I guess I do agree that we should go into opacity, not in the light of phasing a bump, but in the light of transition edges, intersection of fundamental and first harmonic blue edges, and modal behavior.

BAKER:

I think that's true, although not very many people have data on that.

COX:

Only Bob Stellingwerf; it's only a 10 or 15 minute conversation. Call it transition lines rather than opacity.

BAKER:

Yes. Transition lines, certainly, at some point. We were going to talk some about structure. I guess I misread the sentiment about opacity. Nobody thinks we need to bring that up again?

COX:

Perhaps it can come up again. People want to have accurate transition lines, because they need these things.

BAKER:

Sure. To the extent that it comes in there.

What about the artificial viscosity conditions? Certainly there are some varied formulas. Some people have some experience with them. It seems to be a fairly viable thing. Is there any sentiment for putting it down on the agenda?

STELLINGWERF:

There is no real way to discuss it.

CHRISTY:

I gather that some people who have cut off the artificial viscosity can report the good things it does for you if you do that.

BAKER:

I think it would be nice to know briefly about—in a little more detail than we have on the list there—just what was done, so let's put it down as a minor topic at least.

Are there any other topics that somebody wants to be sure we get around to?

CHRISTY:

I think the factor that's fairly relevant in how smooth your light curve ends up is how many zones go through the hydrogen ionization transition in a period in these various models. I think that would be a number we can compare—how many zones make the hydrogen ionization transition in each period. It would give some indication of the smoothness that should show up in the light curve.

BAKER:

There may be several topics in regard to zoning, but that's certainly a very interesting one.

I don't see any reason why we have to structure this, but I just want to make sure that we don't miss something that somebody was dying to talk about.

KARP:

Do you want to discuss the possibility of defining an archetype 9.8-day Cepheid?

BAKER:

I think it would be useful, since we seem to be close to that now. The divergences there aren't that big.

COX:

All we have to do is have a little pact. I guess we represent all the referees in the world, except for Icko and John. (Laughter)

BAKER:

We'll give them their orders, too.

FISCHEL:

I'd like to hear a few words from anybody on how they want to see the results that have been compiled, presented.

BAKER:

The things that were prepared?

FISCHEL:

We can take care of the scale shifts and things and do exactly what we have done, or we can do anything that you feel is more reasonable.

BAKER:

Since you brought it up, why don't we dispose of it now?

FISCHEL:

The only two things that are really current are the luminosity and the velocity.

BAKER:

I would think that would be the next thing to go to, look at the structure. But I don't want to prematurely cut off the discussion of bumps.

CHRISTY:

I think we could leave it for a while.

BAKER:

It will doubtless come up again.

DAVIS:

I just want to have a few comments on the effect of the transfer approximations on the results and show a few slides.

BAKER:

I think that would be nice.

SPANGENBERG:

Unfortunately, my plot routine broke down, so I haven't been able to follow some shocks out through the atmosphere, but I think on my one temperature plot you can see some. So I think there is a possibility of putting on enough thin zones that one can see atmospheric phenomena.

BAKER:

You and Gil could both bring this up during the discussion of the structure.

I guess the first thing is effective temperature versus phase, and then we will get . . .

CHRISTY:

My feeling is, the temperature won't show anything different from the luminosity, except the people who do transport may have a different definition of temperature.

* * * * *

[The discussion of the structure plots has been severely edited, because, as R. F. Christy suggested, they did not show much of interest. Comments of major importance are preserved, but brief or minor comments are summarized below. The only graphs of the structure reproduced here are those of the FSK model, because they show the largest variation between maximum and minimum light and reach the smallest log Q. The Appendix contains all of the tables submitted by the model makers.]

The summary points are:

- D. S. King's technique at the surface essentially places a point at log Q = ∞.
- Although almost any surface boundary condition yields a good temperature structure (Figure W7a), fine zoning in the outer layers provides a smoother, more detailed structure.
- G. Davis reported that Janet Bendt studied the causes of the fluctuations found near log T = 4.6 in this model and deduced they were real effects of the opacity in the helium ionization zone.
- K. von Sengbusch actually made a movie of the density plot, especially near log Q = –6 in Figure W7b, for a time sequence of one model and satisfied himself that the density inversion moves in and out with the pulsation.]

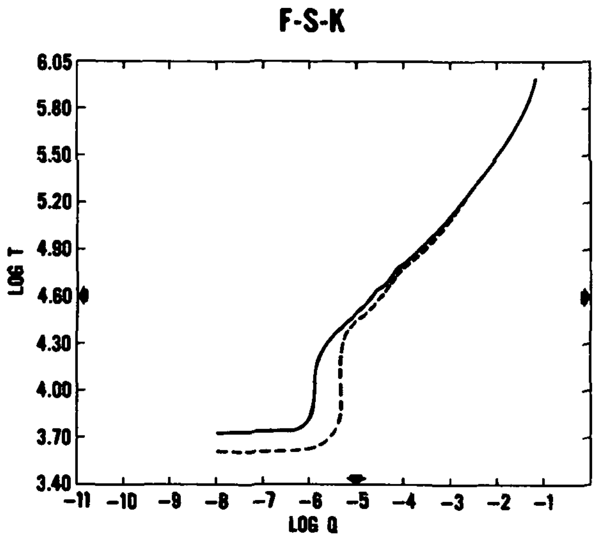

Figure W7a. Log-temperature plots at maximum and minimum light for FSK.

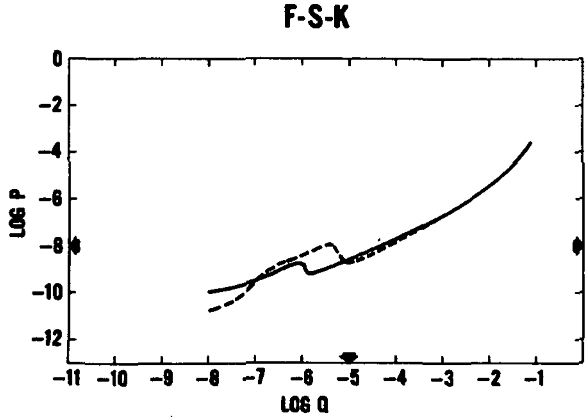

Figure W7b. Log-density plots at maximum and minimum light for FSK.

COX:

What we should have done was plot the amount of work done per gram over one period as a function of depth.

* * * * *

CHRISTY:

The only thing that's perturbing is that there are certain parts of the star, namely in the ionization regions, where the waves do not show up in the velocity plots. It would be nice to be able to see what's going on there.

BAKER:

That certainly would be very interesting.

COX:

I'm afraid they won't show in the density plots either, because if you don't have the gradient in velocity, you're not going to cause any compression.

CHRISTY:

If things are ionizable, they may not behave in the normal manner.

BAKER:

Maybe [one should look at the] internal energy or something like that.

CHRISTY:

There must be something happening somewhere.

* * * * *

VON SENGBUSCH:

The pressure is something which is not an unsteady function through the hydrogen ionization region, and so features on the pressure distribution are, in general, features that have something to do with the motion, or with the driving of certain waves [See Figure W7c].

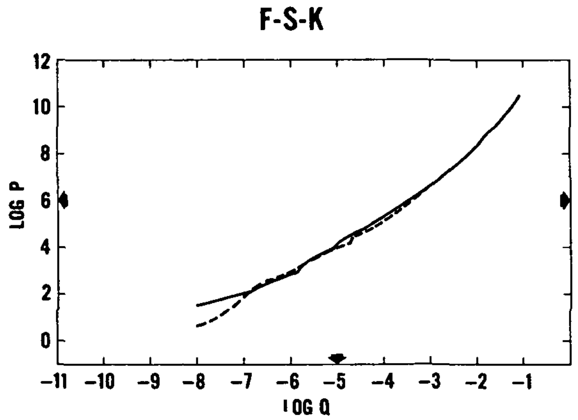

Figure W7c. Log-pressure plots at maximum and minimum light for FSK.

* * * * *

BAKER:

Let's go on to the driving now.

STELLINGWERF:

This plot [Figure W8] is the driving per zone in the units of kinetic energy. It's 2.5 percent of the total kinetic energy, minus or plus. This is the interior of the star. The inner boundary for my model is at zone 0. [The right peak is] the hydrogen zone, [the central feature is] the helium zone, [and the dip] is the interior damping. The solid line is the limit cycle driving, and the dashed line is the linear driving. So you can see what happened when it attained limiting amplitude. The helium zone driving went way down. There is a big difference [in the ionization zones]. The differences in this model are much larger than the differences I got with the RR Lyrae models, especially [in the damping part]. The damping for the nonlinear models also is smaller than damping for the linear models.

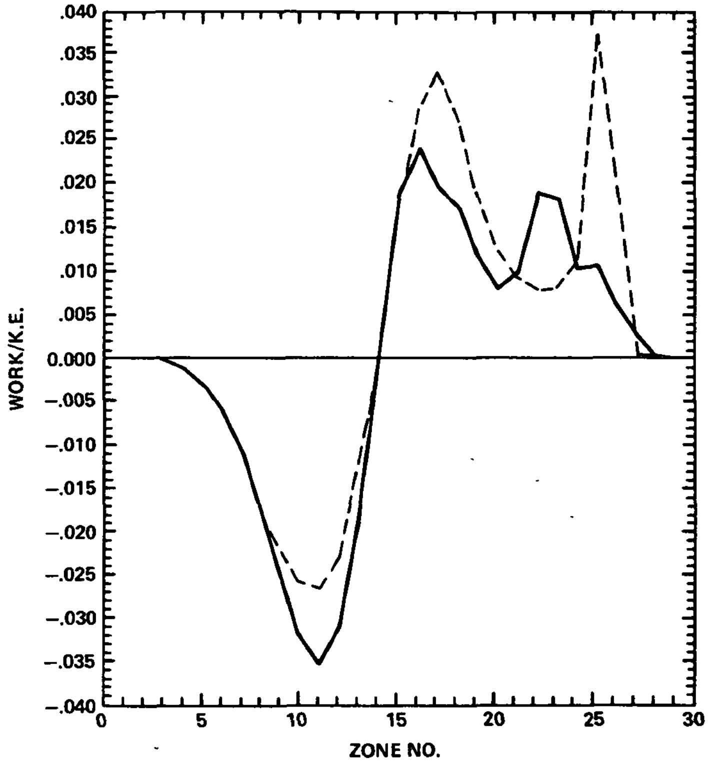

Figure W8. Work versus zone number.

VON SENGBUSCH:

How did you scale the linear [case]?

STELLINGWERF:

The linear is scaled with kinetic energy. The nonlinear is also. So it is possible that there may be a problem in scale just because of the nonadiabatic effects at the surface or something.

COX:

There is one thing you pointed out here—that the integral under the dashed curve has to be positive, because the linear theory says stars are going to grow; whereas, the integral under the solid curve has to be zero, because it is at the limit cycle.

STELLINGWERF:

It's exactly zero.

COX:

So there will be a difference. But the thing I found interesting is that you get the Christy saturations, especially in the hydrogen ionization region and even in the helium ionization region. But the most puzzling thing is that little filling in of the dip down below.

STELLINGWERF:

In the RR Lyrae models that I did, I looked at a lot of these curves, and I always found the same effects. It seemed to me to be the same sort of thing as you're getting here—you're getting less driving and more damping. This one is much more pronounced than the others that I have seen. I don't know exactly what it is, but you're getting a lot more damping. It happens at quite small amplitudes. Nonlinear effects come right in.

The area under the linear hydrogen peak is the dashed line, the other is the nonlinear hydrogen peak; they're practically identical, even for the survey of RR Lyrae stars.

COX:

So there isn't really a saturation at that point?

STELLINGWERF:

It spreads out quite a bit, especially for hotter models. There is a big spread in the hydrogen zone driving region, but the total area depends on the opacity you are using. The total area for this opacity tends to be just about constant, and the big thing, in terms of limiting amplitude, is [the helium peak]. There's a big difference in these areas.

BAKER:

The spread, of course, tends to make saturation harder to occur. It brings in more material. The saturation of hydrogen [is not easily obtained].

COX:

If I remember, Bob, you got the hydrogen driving to saturation first.

CHRISTY:

I don't happen to have it. My recollection is the other way. It fooled me. Where I expected it to saturate, it didn't.

COX:

That's what he's saying, too.

STELLINGWERF:

The helium saturation; yes. In fact, the models I did with the Christy formula actually showed a gain in driving in the hydrogen zone at some amplitudes. My models differ in some respects. The primary thing seems to be the helium zone. Just barely below it, you also get some extra damping.

KARP:

Is that the $\int PdV$ per zone, as work per gram, or do you multiply by the mass?

STELLINGWERF:

It is multiplied by the mass, that is, the work per zone in units of the kinetic energy.

BAKER:

I think we might start on the question of the edges, transition lines, and so on.

STELLINGWERF:

This is the plot that was called for [Figure W9]. What I did here was try to duplicate as close as I could Dr. Christy's sequence 5; his 1966 paper. [They are all RR Lyrae models, M. = 0.578, He = 30 percent]. I used the Christy formula. The other plot is the plot I showed yesterday. [See Stellingwerf's paper, Figure 3.] They are exactly the same models. The only things that are different are the different treatment of viscosity and the different opacities.

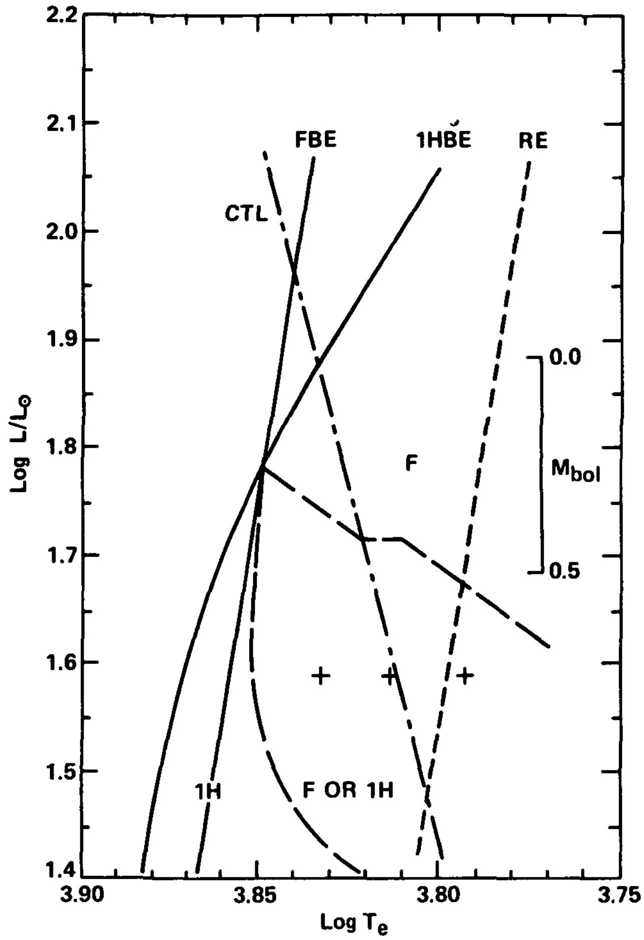

Figure W9. H-R diagram showing the overall stability results using Christy's analytic formula.

HILL:

When you say the Christy formula, do you mean your modified Christy formula or . . .

STELLINGWERF:

The Christy formula. The other ones use my formula, which I don't distinguish from David King's. This line [CTL in Figure W9] is the Christy transition line, or close to it, using my periods. It may not correspond exactly to somebody else's.

CHRISTY:

What are the axes?

STELLINGWERF:

H-R diagram. These crosses represent models in sequence 5. These two models [to the right] Dr. Christy found to be unstable, in both the fundamental and the first harmonic. These [solid lines] are linear left-hand edges. They agree pretty well with other people; Iben's, for instance, I looked at very closely. The dashed lines are taken from nonlinear models. This model [the cross on the left] was reported by Dr. Christy as being unstable at small amplitudes in both modes; but I only found the first harmonic for that. That's a possibility. That may be unstable in both modes, also.

CHRISTY:

I might add that Iben once wrote me about some of these points, asking whether I had used the radiation pressure. I had not used it in these calculations. I redid them with radiation pressure, and I did find a nonnegligible shift.

STELLINGWERF:

Shift toward the blue?

CHRISTY:

I forgot the direction. I did write to him, though.

STELLINGWERF:

I was about to mention that. This is your transition line, and of course Iben would like that transition line to go through the intersection point, and putting in radiation pressure could very well . . .

COX:

Well, it does, doesn't it, pretty closely.

STELLINGWERF:

The long dashed lines based on the nonlinear results do go through that point. As far as that goes, that's okay.

The point here is that my nonlinear transition lines are [on the left] and all models between the long dashed lines [F or 1H] pulsate in either mode; and there are a lot of models. I think 60 models or something spread all over this area. The first harmonic-only area is here [1H], and this [dashed line] curves away. So you can see a difference at low luminosities for this transition line. It moves over toward the right. That, I think, is the effect of artificial viscosity.

At very low luminosities, your growth rates are sufficiently low, so that the viscous damping in the interior, down below where any shocks are formed, can dominate. Apparently, it has this effect. I did run a test model here, which pulsated only in the first harmonic. I only changed the viscosity, and that transition line moved right over to the edge.

SPANGENBERG:

In my RR Lyrae nonlinear models, I had the same type topology on this on the left-hand edge of our diagram, using an artificial viscosity, which gives the contribution down to zero difference. That was one of the problems with my survey—I got a first harmonic region that curved down and to the right using the standard viscosities on the left-hand edges.

COX:

Apparently what it does, this artificial viscosity, in the classical way, damps the fundamental mode more than the harmonic mode. I would gather that the physics of the situation is that, in the fundamental mode, amplitudes are larger everywhere, compared to the harmonic mode, and that is why the damping is more effective. Do you suppose that is the proper physics?

STELLINGWERF:

What happens is, I get damping when I plot viscous dissipation versus zone number. With the normal artificial viscosity, I get some damping in the ionization zones, due to shock waves probably. This is just the viscous dissipation integrated around one cycle divided by the kinetic energy per zone. With the normal artificial viscous formula, this damping occurs in the deeper regions also. This can amount to 1 or 2 percent of the kinetic energy—so if you have growth rates around 1 or 2 percent, that can stabilize it.

COX:

We have had that happen.

STELLINGWERF:

The first harmonic has a node right in the deeper regions, and it does not affect the first harmonic quite that much.

COX:

What does this look like when you use your new technique of having a cutoff?

STELLINGWERF:

The extra viscous dissipation below the ionization zone completely disappears.

SPANGENBERG:

This is viscous work only?

STELLINGWERF:

Just viscous work, yes. Amount of dissipation. This work is included in the work that I showed you a minute ago. Viscous dissipation is included in the total work.

BAKER:

But it's hardly visible, is it?

STELLINGWERF:

In some cases it makes a little bit of difference. It is not the major component. A small shift, just 0.1 or 0.01 of the sound speed, is sufficient to eliminate the dissipation in the deeper regions.

COX:

At one time, when we plotted the final limiting amplitude versus viscous factor, we found a very discouraging result. Things were still growing when we decreased the viscous factor. This led me to believe that we can never, from these kinds of calculations, really know an accurate full amplitude behavior. Do you feel that you now know the full amplitude behavior rather accurately?

STELLINGWERF:

You should phrase that a little bit differently. I will answer another question.

I have fiddled with C_Q, with this type of behavior, and I found that plotting velocity in the outer two zones. With $C_Q = 1$, the outer zone shows a little noise. When I change C_Q to 4, the kinetic energy amplitude remains exactly the same, and the amplitude in velocity goes down a bit, but I found that that is entirely due to just smoothing out the behavior of the outer zone or two. So changing C_Q within reasonable ranges, 1 to 4 or 1 to 10 perhaps, wouldn't change the amplitude very much. A few percent or so.

COX:

That's wonderful, because we got tremendous effects . . .

STELLINGWERF:

That still may not be the right amplitude because of convection.

SPANGENBERG:

I did this model with a one-tenth sound speed cutoff and ran it up to what would appear to be the limiting amplitude. Then I switched to a coefficient of 4, but [the model] seemed to be somewhat insensitive.

At that point I attempted to run for a few more periods, changing the viscosity formula. The first thing I did was go to the standard ΔU^2, but reduced the C_Q to 1 and found that it did not change the amplitude. By making $C_Q = 4$ with the standard formula, the model was then pulsating at too high a kinetic energy, and it started dissipating. It started losing kinetic energy, without the cutoff with a C_Q of 4. For the conference model, $C_Q = 1$. I think the model was pulsating strongly enough that I did not have large velocity gradients in that interior region.

COX:

As I recall, even with $C_Q = 1$, we thought it was really not small enough.

BAKER:

Yes, that's right.

COX:

And you are saying it is.

SPANGENBERG:

No. For this model it is. But I would still want to make that cutoff.

STELLINGWERF:

If the linear growth rate is greater than a few percent, I wouldn't expect this to show up.

KING:

The growth rate is shorter or a lot faster in this case than some of the models we were investigating.

BAKER:

While we are on the subject, I noticed that there are a number of different recipes here. Did anybody else want to say something about it?

DAVIS:

Our factor of 4 in the viscosity came from a previous study that Janet Bendt and I did.

COX:

What I am saying is that if you had, following David's [King] Q, a slow growing pulsation, then you would be in bad trouble.

DAVIS:

Yes.

COX:

You would get the wrong amplitude.

DAVIS:

Yes.

COX:

You made a lucky choice as to which model you used.

BAKER:

I don't understand the remark. Why would he have trouble?

COX:

With such a large Q, he would get limiting amplitudes that would be considerably below the right one.

KARP:

But he's got the cutoff.

COX:

Now that he has the cutoff, it shouldn't hurt.

BAKER:

What about your work, Al?

KARP:

I found the same problem, that I could not get some stars to pulsate that should. So I just put in a variable viscosity. I took e to the minus pressure over P_0 times Q_0. I used 4, but I found that it didn't change the amplitude over a range of 2 to 6.

STELLINGWERF:

But it has the same effect?

KARP:

It has the same effect. All I did was tune it so that there was a viscosity between 3 and 4 in the atmosphere, and by the time you got to the helium 2 zone it was essentially zero. So there was no viscosity inside the helium 2 ionization zone. It just does the same thing.

STELLINGWERF:

Does everybody realize what we are talking about here? When I say cutoff, what I mean is that for artificial viscosity I plot viscous pressure versus ΔU, the velocity grading across two interfaces. It is just a straight quadratic formula through zero whose coefficient is C_Q [Figure W10]. All we do is move this over a bit so it doesn't turn on, except for some finite ΔU. What we are trying to do is cut it off.

KARP:

Do you turn it on at zero Q, or do you pick a different point to start?

STELLINGWERF:

Just pick a different point where it starts.

KARP:

I mean, you said that at some finite ΔU you have a very, very small artificial viscosity pressure.

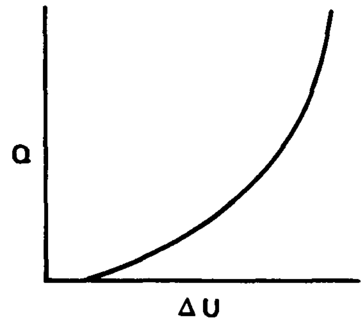

Figure W10. Artificial viscosity factor versus velocity difference with a cutoff.

STELLINGWERF:

There's none here (below ΔU); ΔU has to get up to the cutoff before anything happens.

KARP:

You don't pick up a point off that first curve to the left? It's not a step function there?

STELLINGWERF:

No. What we use is this scheme.

CHRISTY:

What do you take as a constant shift from ΔU?

STELLINGWERF:

It's indicated up there [see Table W1]. I take one-tenth of the sound speed, the square root of PV, and apparently Gil has found that one-hundredth of the sound speed works better?

DAVIS:

Yes. I notice it doesn't turn the viscosity off very deep below the helium ionization region.

STELLINGWERF:

I checked how much viscous pressure I was getting in here, and I think that, for my models, one-hundredth and one-tenth will produce about the same results.

DAVIS:

Yes.

STELLINGWERF:

The difference in the velocities in these interior zones is extremely small, and the viscous pressure is very small compared with the total pressure; but the masses are so large you can get dissipation.

COX:

It may be, Bob, that in your technique, your 10-percent cutoff is appropriate. Maybe Gil's is more sensitive and you can't cut if off at such a high point.

STELLINGWERF:

Basically, you must have viscous pressure where you have any kind of shock.

SPANGENBERG:

I ran with one-tenth and it didn't seem to have undue jiggles.

KARP:

You have to tune it, right?

STELLINGWERF:

It depends on the code, probably.

BAKER:

The criterion for the artificial viscosity, after all, is that it be insensitive to the zoning. If you make the zoning finer, you shouldn't change the result. But if it's both insensitive to the zoning and to the coefficient, then it seems to me whatever you are doing is satisfactory. Does everyone agree on that?

CHRISTY:

Going the other way around, if it weren't, then it would be unsatisfactory.

BAKER:

This is what most of us have been operating with until now, I think.

STELLINGWERF:

If everyone is satisfied with viscosity, there is just one more thing I would like to say about this diagram. [Figure W9 and Stellingwerf, Figure 3.] I would say that viscosity is the main reason for this difference. Now, as you can see, the long dashed line, which is the reddest extent of the first harmonic pulsation, has also changed. It has come down. At the same time, this intersection point has moved way up. I have the feeling that viscosity may have something to do with that transition line, but it's mainly an opacity effect.

I did one test of a model on this diagram [Stellingwerf, Figure 3] right in here [Log $L/L_\odot$ = 1.8, Log Te = 3.82], and I changed the viscosity using the King 4a opacity. In other words, it is the model here with only the viscosity changed. The amplitude changed just a few percent; you could hardly tell. But that model will pulsate at either mode. It looked like it was very near the transition line. In other words, I would say that this transition region got wider.

COX:

Wait a minute. You're mixing things up here–4a is a Population I mixture.

STELLINGWERF:

That's the King 1a, right. So I'm saying now that the effect of just changing the viscosity presumably moves the two long dashed lines to the right. So the effect of the opacity is just to move the intersection point way up.

CHRISTY:

In the projection of the slide, you have a line that has an "S" curve to it [Stellingwerf, Figure 3].

COX:

That's the red edge of the type 3 behavior.

CHRISTY:

That was done with your best opacities; is that it? Whereas, [in Figure W9] you have a line with a different kind of [shape]—more or less straight.

STELLINGWERF:

Right. That's with your formula, and this is approximately constant M/R—constant radius line.

CHRISTY:

You think it's very sensitive to your opacities, then?

COX:

This, in my view, is the most sensitive and most flagrant exhibition of the problems you can get into with just slight changes in opacity. Your [Christy] opacity formula fits very well, as you showed today. Yet if you want to worry about transition edges, transition lines, blue edges, type 3 behavior, and all that, you have to be very, very careful.

STELLINGWERF:

I should make a point here, that it may depend somewhat on my boundary conditions, too. It's hard to tell. For the low growth rate models, I found that shifting to the Christy formula, with the new viscous pressure, limiting this dissipation in the deeper regions, can cause really large amplitudes. Amplitudes near the blue edge, which used to be like 20 kilometers per second, would go up to approximately 80. Very large. Then, when I shift over to the new opacity, they come back down again, primarily because you're not picking up that extra driving in the hydrogen zone. The peak in the opacity formula is narrower, so zones that get beyond that peak are no longer driving. I think that's basically what is happening. There are a couple of competing effects.

KING:

Eighty kilometers per second?

STELLINGWERF:

Yes, that was the model right here, a sequence 5 model with a growth rate of 0.2 percent per period—very low amplitude, with the usual viscosity. That's the one I found to have this big velocity change.

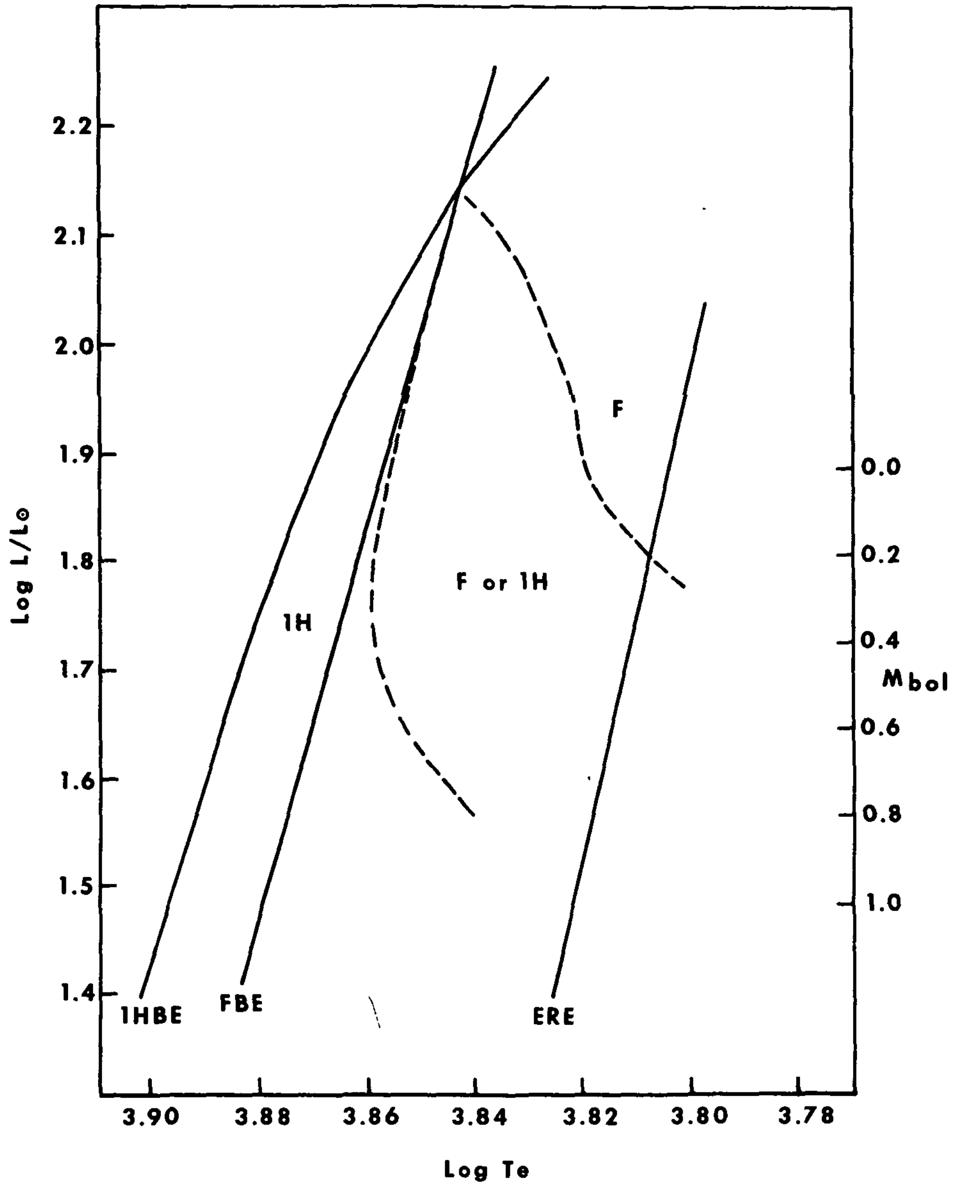

Figure W11. Spangenberg's H-R diagram of the RR Lyrae instability strip for 0.6 $M_\odot$ stars with the King 1a composition ($X = 0.7$, $Z = 0.001$). The solid lines labled 1HBE, FBE, and ERE stand for the first harmonic blue edge from linear theory, the fundamental blue edge from linear theory, and the empirical red edge from observations, respectively. The dashed lines are the nonlinear transition lines between the regions of full amplitude modal behavior. For the region labled F or 1H there is either F mode pulsation or 1H pulsation at limiting amplitude, depending upon initial conditions.

COX:

You are telling me you might believe the strip is even wider?

SPANGENBERG:

No, I'm not going to say that. What I'm going to say is that there was some argument about perhaps the depth of the envelope or the fact that Bob's models had a rough edge here or there as far as things one could do easily to improve them. I think I have reasonable surface boundary conditions: I have a deep envelope; I have a fairly substantial number of zones; I think I have the best opacity that we have available; and I think I am using it in a reasonable method. But I don't think we can reject out of hand, because the calculations are nonsense at this region at present, at least for this afternoon, maybe not tomorrow.

BAKER:

It seems to me that after all, the type of instability that people are calculating in the linear perturbation analysis of the nonlinear motion is, after all, governed by the same type of nonadiabatic behavior that the stability of the equilibrium model itself is.

CHRISTY:

I am a little confused. Is this found for linear and . . . ?

STELLINGWERF:

Yes, solid lines are linear results.

CHRISTY:

Now, what are the amplitudes of the full amplitude nonlinear motion up in this region?

SPANGENBERG:

Which amplitude?

SEVERAL:

Both amplitudes.

CHRISTY:

What I want to know, is, can you get large amplitude motions in this region, or is there somehow or other a nose of the small amplitude region that is not populated by large amplitude motions?

STELLINGWERF:

[To Spangenberg] I showed the amplitudes of my models. You might want to comment on yours.

SPANGENBERG:

Now, which amplitude are you talking about, velocity, kinetic energy, light?

CHRISTY:

Is there any characteristic difference . . . You see, it is this peculiar bend [in the transition line] that puzzles me and I wondered if there is any characteristic difference between models [at the top] and models [at the bottom] in terms of how big an amplitude they will go to.

SPANGENBERG:

The velocity amplitudes increase with increasing luminosity.

CHRISTY:

I see. Right up here you get . . .

SPANGENBERG:

I may not have a model way up there. I have it maybe at 2.

CHRISTY:

I see. [To Stellingwerf] How big are yours?

STELLINGWERF:

There exists sort of a small decrease in the amplitudes as you go to higher motions. The fundamental and the first harmonic curves are both shown, and as you get a higher amplitude you can see that the luminosity amplitude comes down as you go to higher luminosity. So, I would expect the amplitudes to go up there. I don't know what causes this, but I would expect amplitudes to increase, perhaps viscosity has something to do with it. Again, I don't know.

COX:

[To Christy] I don't understand your motivation for pointing it out to us, Bob.

CHRISTY:

My motivation is simple—that usually lines are moderately straight when you draw them over small distances. I don't care where these intersections come, but I feel that if the large amplitude motion is confined by some boundary, maybe then the projection [at the top] is merely a reflection of something that shows up at small amplitudes, but disappears when you try to consider a large amplitude motion.

COX:

But these are limiting amplitude cases . . .

CHRISTY:

Obviously, but my question is how big is the limiting amplitude.

COX:

You mean you are worrying about it not even being observable.

CHRISTY:

Possibly.

[An inaudible statement or question.]

No, I was just wondering whether there was anything in the models that corresponds to this peculiar projection up there.

STELLINGWERF:

My feeling is that everyone expected the transition line to sort of scale up and down at the intersection point once this fact was pointed out. This doesn't seem to happen. I think what we have here is a bunch of straight parallel lines, all parallel to the linear radius, and that the only place you are getting anything funny is when you get very high luminosities at the intersection point and at the very low luminosities, where the first harmonic tends to take over.

KARP:

How many models went into specifying that figure?

STELLINGWERF:

About 50 models.

KARP:

You did 5 sequences of 10 each, or something like that?

STELLINGWERF:

Yes.

KARP:

So you get those edges pretty accurately.

BAKER:

There are also a number at higher luminosity.

KARP:

You wouldn't be thrown off by one or two models that are doing something peculiar?

STELLINGWERF:

There are more models down around where real stars are. I wouldn't bet on the ones way up or the low luminosity ones.

KARP:

One peculiar star wouldn't trip the top of that curve off a good bit?

STELLINGWERF:

The line that whips off to the right of the diagram . . .

SPANGENBERG:

My either/or region is a little bit narrower at the top. I don't remember the numbers.

KARP:

I would be interested in seeing that slide with the actual models plotted . . .

BAKER:

I think that Stellingwerf has tables in his paper. If we can just come back just for a moment to this question that had the Stellingwerf models been deeper, he might have had a different behavior. I find it very unlikely that behavior would have been different. I really think that the things that control the stability are in the outer nonadiabatic layers, and I would just find it very difficult to understand if this type of behavior changed.

* * * * *

KARP:

But the question is, you feel your lines are pretty good even if one or two models are peculiar?

SPANGENBERG:

I agree with that. I don't have any indication that there are individual models sitting somewhere that are peculiar.

KARP:

No, I am just wondering with this [projection] that if you got one model that for some reason is peculiar you get this funny "S" shape.

STELLINGWERF:

I should point out that I don't have nearly that many models. I only did models every 300 K. But for all my models, the switching rates are smooth curves. I have enough models to ensure that they are smooth. So I am fairly confident that those are the proper transition lines for those models.

BAKER:

It is not relevant to the conversation, but since you brought it up—I don't know to what extent these ideas are borne out, but I think that one needn't take the old Population II evolutionary tracks as immutable. I certainly think there is indirect evidence—at least I haven't seen a satisfactory refutation of our point of view—that you cannot explain the Oosterhof groups without some picture like the one we put forward. So I think there is a case for that side, too. Now, Stellingwerf has talked with the model builders more recently, and I think they no longer find it that outrageous.

STELLINGWERF:

When you look at Rood's most recent diagrams—his cluster-synthesis-type situation—you find in M3 for instance, there are a lot of points at the one luminosity, and then just a couple of points scattered above it. I assume that those points sitting at that luminosity are evolving slowly toward the left in the H-R diagram, and the scatter of one or two points above are moving to the right. So that would indicate that at least a factor of 10 more stars in his diagram, using the standard tracks, are going slowly toward the left.

COX:

But the van Altena-Baker theory requires some to spend a lot of time going in the other direction.

STELLINGWERF:

The effect agrees with Rood's results also, because I think ω Centauri, for instance, wouldn't have any stars on the zero age horizontal branch or in the RR Lyrae gap. They would have to go originally toward the left, and then travel toward the right going very fast.

COX:

I was happy until Bob Stellingwerf found a wider type 3 behavior region. I thought maybe the type 3 behavior area could be smaller to settle this question.

BAKER:

Certainly, there's no question that it's dependent upon opacities and there we are. That, I think, we can all agree on. I really feel this question of getting into evolution is not really relevant to the conference. I just felt I ought to defend myself a little bit.

If I do raise this question about opacities again, I think all of us are working with opacity tables, formulas, and so on, of different origins, different vintages, different grid spacings, and all of that. Is there a possibility of standardizing things more if Spangenberg is willing to send out some of his splined tables? Would most people find this too fine to work with? Or isn't this sort of standardization even desirable?

KARP:

I was going to say do you really want to do that? After all, if people are computing the same model at different times with these tables, maybe we can find out what is important and what isn't.

BAKER:

Only if it is done in a systematic way, I suppose.

COX:

But let me give my standard response. All that we talked about didn't make much difference and I was a little surprised, but not too much. That is why I sort of precipitated this discussion on the transition edges, because we want to know where opacities are important, and that is the most important area. Now, I guess the answer to Norm's question is, if you want to work on transition edges and all, you had better do something really good, which is what Bob is doing. If you are going to some standard things that most of us have been doing for other effects, one needn't be so precise.

BAKER:

In addition to that, let me raise another point. Even though the type of opacity table, formula, and so on, that you use, may not have such a big effect on the full amplitude motion, it may have a good deal to do with how easy it is to finally get your model to converge and that type of thing. Now, that may be something that the model makers would like to discuss too. That goes along with this question of zoning, I suppose.

KARP:

My major worry about everybody using one person's opacity table is what happens if there is a mistake, the keypuncher drops a comma or something, or for some reason an opacity source is omitted and we are all using the same thing. So all of our models agree with each other and we have great consistency, but they are all wrong.

Whereas, if everybody is using their own opacity table, eventually, something like this change to the transition line is going to show up.

COX:

I think I can be a bit more philosophical. No matter what we decide here on what we are supposed to do, the next day you will find out that somebody else is doing something else anyway.

So I don't think it is very important. You see, Norm, once again—let me refine or bring up another point about the need for accurate opacity. You are right, the zoning—the coarseness or fineness of zoning that you have—may affect the accuracy of the spacing of your opacity tables. Perhaps an even more important point is the growth rate. The star is going to grow very slowly, and then, if you are there putting in noise due to interpolation of the table, you are going to have troubles. You're spending a lot of time following a lot of noise. However, if the noise is swamped by a very rapid growth rate, then you could use a coarse table.

I think that the exact opacity tables that are needed should be used, depending upon a variety of conditions including other things like your computer, the accessability of opacity tables and all the rest. I guess I would say, let's not try to legislate any particular standard table or mixture or anything.

SPANGENBERG:

I assume that the original King series of data is available to anyone who writes you a letter.

COX:

I am happy to hear that you have got spline fitted data for all five mixtures.

SPANGENBERG:

I may not have done 5a yet. It is not a long process.

BAKER:

Do all of the people who are actually actively working on this have the feeling that they know what is available, or should we, perhaps, make a table of what is available?

KARP:

I have just done my literature search on my thesis and the last consistent set of opacity tables that I found in the literature are Cox and Stewart. Maybe we ought to get some of these new ones out into the literature so there is a place to reference them.

COX:

We have been publishing a batch about every five years. We still have another year to go.

CHRISTY:

I understand Carson is about to put out some opacities.

COX:

I saw him the other day. He did admit that his opacities, which were twice as big as ours at 10^6 K, are no longer that big. Whether he disagrees with our tables anymore or not, I don't know. It is true that there are lots of things wrong with our opacity tables.

BAKER:

What does one do if one wants opacity tables, and if one wants to know what is available? One writes to Dave King or Bill Spangenberg–are those the people who know the opacities now?

KARP:

Could we put in an appendix in the conference report for the people who have opacities, saying what they've got?

BAKER:

I think it would be great. However, the question of publishing is something else, again. Because, of course, we are only a small part of the community that uses opacity tables, so the people who do stellar models and so on would like to know it, too. [We decided not to publish the opacity tables.]

* * * * *

BAKER:

Shall we pass on to this question of archetype model, whether something further should be done on this? What are the feelings on it?

CHRISTY:

I think there is some difficulty there; the actual Cepheids show up at different places in the instability strip. I think that, until we can cover at least a two-dimensional area with models, we are not going to be sure that if we produce a model, say, of a certain period, that it's going to correspond to a Cepheid of that period. Usually, we don't know exactly where it sits in the instability strip, nor do we know its mass or luminosity that well.

BAKER:

That's true, but on the other hand, it is just as a technical matter. I don't know how many more people are going to start doing this kind of thing. Certainly it was true, even up until Stellingwerf's recent work, that one generally started off with a Christy series 5 model, and you didn't feel that you understood your code until you either reproduced that or understood why you couldn't.

CHRISTY:

I thought you meant for the purpose of comparing with observers. Did you mean just to compare with each other?

BAKER:

I think that that was the suggestion here.

KARP:

The idea came up because I had so much trouble checking my code. I didn't have a specific model that I could say "My code reproduces it pretty well, I can have some confidence in it."

KING:

Isn't it true that these results are going to be published, and these curves and so forth will be available? It's not clear that you are going to do very much more that one would need—I mean, the differences the people are going to get when they use a different code are going to be sufficient that this comparison with any of these models perhaps is enough.

BAKER:

In other words, you would feel that there is a certain amount of heterogeneity here, but you would feel the differences that we have seen are not primarily due to the differences in these inputs, but, in fact, are because people do things different ways?

KING:

I doubt that one is going to get better agreement than that. If that's the purpose of it, then why can't this conference report just be used for that purpose, instead of computing a new model?

BAKER:

Does everyone feel that way?

KARP:

You would use this, then, to define–you would just say, "Look at these models. They were all supposed to be the same. That's our archetype. If you can match as well as we match with each other, you're probably all right."

KING:

If you define some other model, I don't think you are going to agree better than we did this time.

COX:

It could be worse.

BAKER:

That's the most optimistic thing I've heard you say. Is there any dissent to this? I think that anyone should feel free at this point to bring up anything that is bothering them.

COX:

There is the question of the transport-type codes.

BAKER:

Yes, I forgot about that. Other than that, we've gone through everything except the question of the form of the report. I think we might save that until the last. Gil, why don't you talk now?

DAVIS:

The model that we presented to this meeting is a full multifrequency transfer model with 11 thin zones outside the ionization region. In an attempt just to study the effects of zoning and the transfer approximation, we ran a couple more. [See Figure W12.]

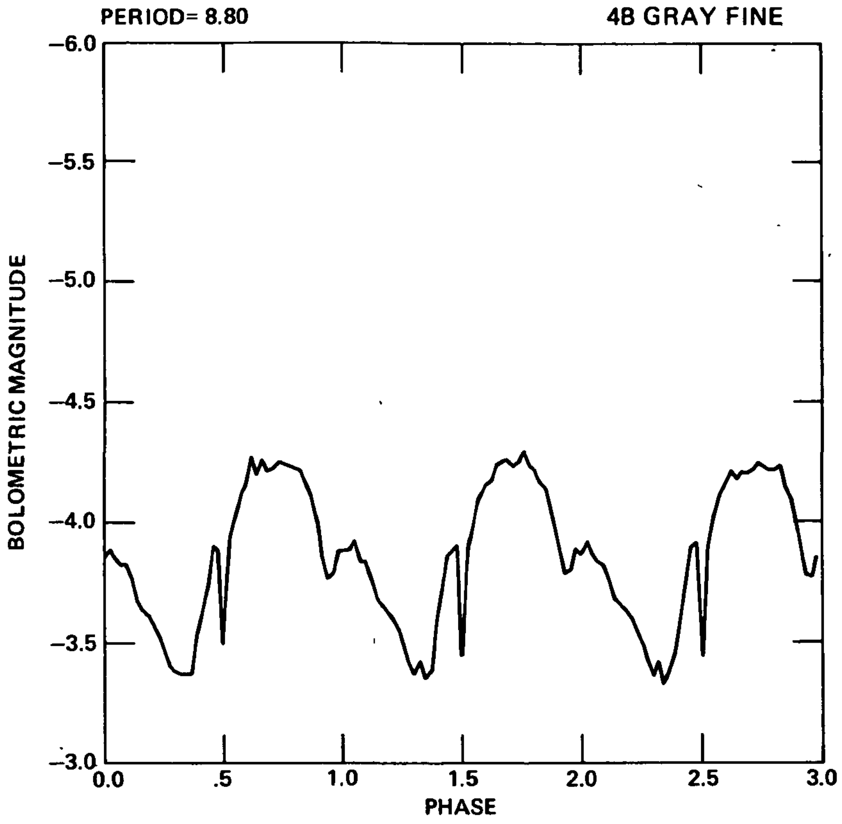

Figure W12. Light curve for CD gray fine-zoned model.

This particular model has the same parameters that we described, but with a coarser zoning in the sense that we have 7 zones outside the ionization region instead of 11. [See Figure W13.] And I compare that with the gray case–these are now gray calculations, but multifrequency. You can't see any real differences.

COX.

This is the Goddard conference model?

DAVIS:

Yes, this is the 4b. This is not the Goddard conference model, but the substitute Goddard conference model. We see the large spike, for the pseudoviscosity and the second bump. Just to check the transport effect, we took the same fine zone model, which is the final model we're presenting here, and did a multifrequency calculation [Figure W14]. The only effects of multifrequency, apparently, are just to decrease the depths of that pseudoviscosity spike. I think that just means we transferred through the toe of the pseudoviscosity. You just see a reduction in the depth of the spike, but the rest of it is almost identical. So

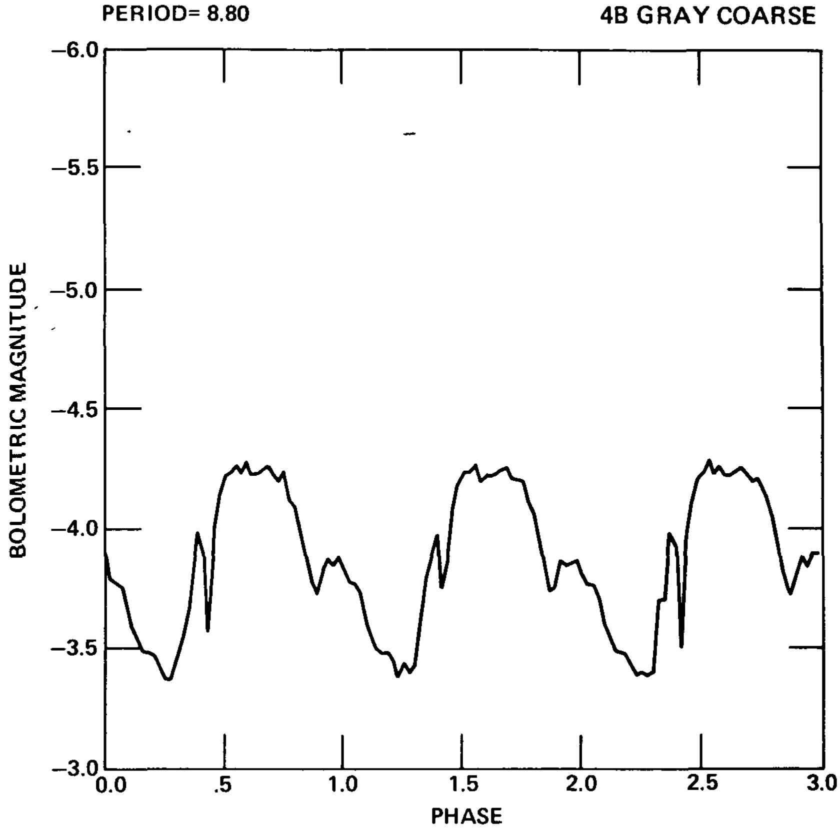

Figure W13. Light curve for CD gray coarse-zoned model.

multifrequency transfer effects are not important. I should say, when we do a gray model, the Eddington factor is one-third, so that's basically a nonequilibrium diffusion model.

VON SENGBUSCH:

You did not particularly put zones into this region?

DAVIS:

They moved through 15 or so zones.

CHRISTY:

The zones are especially thin there. That's just part of the regular structure.

DAVIS:

The static structure has 11 thin zones outside the hydrogen region.

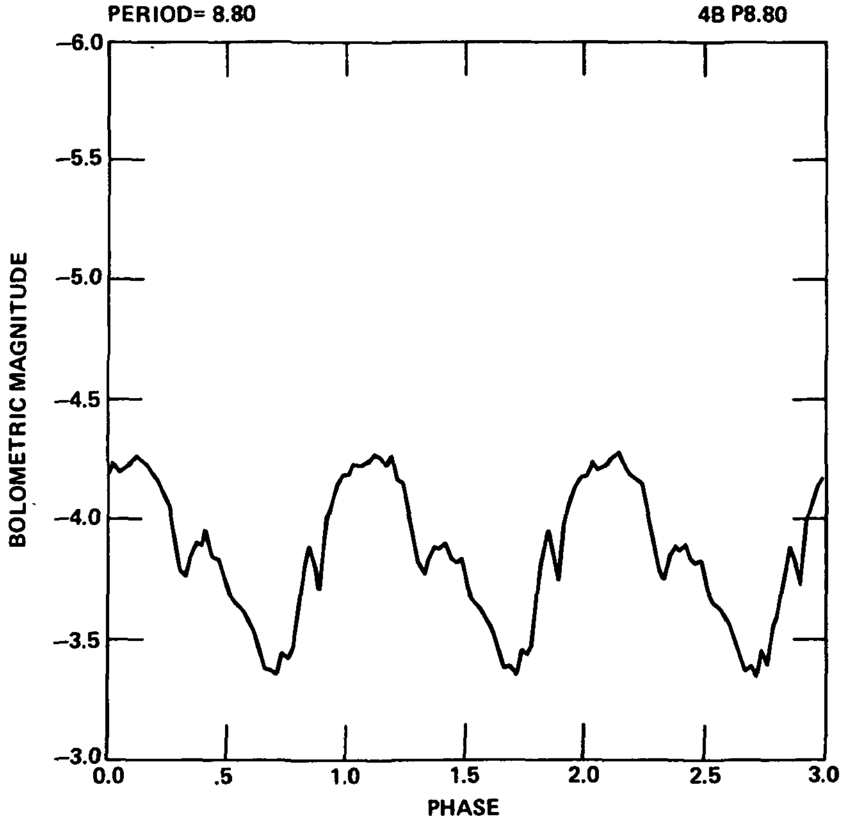

Figure W14. Light curve for CD multigroup radiative transfer: fine-zoned model.

VON SENGBUSCH:

Did you have a constant mass ratio?

DAVIS:

Yes, and the other coarse model had seven zones outside the hydrogen region.

VON SENGBUSCH:

The only way to get more zones inside is to readjust the constant mass ratio.

KING:

That is pretty hard to do.

DAVIS:

But you can see the effects are very small.

VON SENGBUSCH:

It means you have more time steps. That is not a feature of the explicit code. The other time step is really governed by things that are going on. More of the zones are moving between the hydrogen ionization region.

COX:

In our paper yesterday, where you were trying to reproduce the ultraviolet excess that Jim Hutchinson had found, at one pass of the OAO-2, you found, unfortunately, that it occurs in the rising part of the light curve where you have your pseudoviscosity problem. Maybe there is some way to get around that. My question is, from what you just told us, that it didn't matter whether you had transport effects or not . . .

DAVIS:

No. The transfer decreased the bump. It didn't make that dip as deep. It sort of washed out, which you would expect because you are integrating over a frequency group.

COX:

Don't you feel that you had better do some of these fancy transport things anyway, if you are trying to get fancy answers.

DAVIS:

If there are non-LTE effects in the lines, of course. Maybe you should try to get some more information about the shock. That's what I'd like to do.

COX:

Even further along that line, I don't think we should leave the impression that you get right answers with diffusion. A paper Gil published some time ago showed that you really did have to put the right transfer effects in there to explain the shape of the light curve, say, for W Virginis. So, with the Population II Cepheids, it is essential even for the light curves.

DAVIS:

Apparently, even the extended atmosphere is important there.

COX:

So don't sell the transport too short.

KARP:

I also compared the diffusion approximation with the radiative transfer models. The only appreciable difference that I noticed is that the radiative transfer model had slightly lower amplitude–and I haven't been able to come up with any good reason as to why–about 10 percent lower amplitude. Everything else is about the same, except in the outer layer above $\tau = 10^{-2}$ I get temperature inversions for the transfer code that I don't get with the diffusion approximation.

BAKER:

Now, are you talking about the same level of transfer theory?

KARP:

Mine is gray plane parallel calculation.

DAVIS:

This is a full transport calculation with the proper Eddington factor and time dependent equations.

KARP:

I used the Feutrier method to solve the standard parallel transfer equations.

DAVIS:

You can't do much more.

BAKER:

The question is, could you do it cheaper?

DAVIS:

You could include scattering and the angular redistribution, but that's much harder.

BAKER:

Any other points that anyone would like to raise?

COX:

I have a question as to what people's plans are. Maybe nobody wants to tell his competitor what he plans to do.

BAKER:

I thought you had to read all those things anyway.

I certainly would have no feeling for saying that. I suppose that other people would certainly feel the same. Should we go down the list and see? I think it would be quite interesting. Shall we start off with Bednarek. Would you like to say what you are planning to do?

BEDNAREK:

What I'm planning to do is to determine why my period is so much longer than anyone else's. I just noticed that the mass fraction that I used for my static model is only about 10 percent of the total mass of the star. That may explain the discrepancy with everyone else.

KING:

You should get a shorter period and not a longer period, if you don't have enough mass.

BAKER:

Assuming that you clear that up, are you planning to do any more extensive work on it?

BEDNAREK:

I plan to work on Population II Cepheids . . .

BAKER:

Bob, are you going to play hooky from some of your administrative duties occasionally in the future?

CHRISTY:

I hope that I may have some time to do some calculations, but I can't say that I have had much time. I know the things that are on my list to try, but I doubt that I will get to them. I have put in a provision into my code that allows me to calculate to a more or less stable, large amplitude model and then, at that point, rezone the ionization region and put a lot more zones in there as a cheap way of getting more detail in the atmosphere and ionization regions. I have that provision, and I might try using it sometime to see if I can clear up some of these light curves.

The only other thing that I had wanted to do—and it seems very unlikely that I will find time to do it—is to get some more or less reasonable form of convection into these calculations to see if it made a difference. I think it's exceedingly unlikely I will make any progress with that.

BAKER:

That's a big problem.

CHRISTY:

Yes.

BAKER:

Maybe at the end I'll say something about what my own plans are in that direction. Gil, you have already said a bit about . . .

DAVIS:

A little bit, except for other opacities. If Art can produce opacities for the proper molecular bands, the structures might be improved in these calculations. That would be worth waiting for, if it isn't too long.

BAKER:

Do you have plans about this, Art?

COX:

That's right. You will recall in my talk, I felt that we were missing blanketing in our absorption coefficients, and not giving the proper atmospheric structure in deriving colors. Of course, as I also said during my talk, the idea was not to get good absolute colors, but merely to get something reasonable and find how to take means. If you take that attitude, then maybe some of the atmosphere people could do a better job than they've been trying to do for 50 or more years in calibrating the color scale in the terms of temperatures. I spoke to Sid [Parsons] about that at lunch, and he felt that he had already done the definitive work. [Laughter]

PARSONS:

Thanks, Art.

COX:

And I'm not sure that we would delve into that, because the atmosphere people have refined that much better than we can catch up and do. But I do remain puzzled that Parsons' color-temperature scale, Bohm-Vitense's color-temperature scale, Kraft, Schmidt, and what not, are all different, and it makes me somewhat suspicious of the whole thing.

BAKER:

And the fact that the mass discrepancy problem doesn't really seem to be going away.

COX:

I don't know. Let me go off on that tangent and see. All we need to do is change the radius by 10 or so percent, and that would change the mass by 30 percent, and there is maybe a 10-percent wiggle in that number on the board we put up right after lunch. I'm grasping straws, I will admit, but wouldn't it be nice if we could get the color-temperature scale fixed as Dave King wanted. He gets this phasing of the bump a little bit. That would solve two of the mass discrepancy aspects. The third one, which I have no hope for, is the beat Cepheid problem that David King talks about. I don't know what we can do about that.

BAKER:

We can knock our way out of that, though.

COX:

We did?

BAKER:

Don't you agree? At least, there are low mass Cepheids. It may very well be that some of them are in different evolutionary states.

CHRISTY:

There's probably less opportunity for mass losses.

COX:

That's right off the main sequence.

BAKER:

Maybe I'm too willing to sweep that under the rug. I think that's probably the worst of them all. But anyway, I guess you're right. The mass discrepancy problem remains with us, unfortunately, and we have to really scrape to solve the pulsation masses or the Christy bump phasing masses to match the evolutionary people. I have been leaning heavily on them to straighten up that mess.

ALEXANDER:

I just want to comment, relative to the molecular opacities, that I am taking the molecular subroutines out of my atmosphere code for cool super giants. I'm in the process of calculating the Rosseland mean opacities for the temperature range 10^3 to 10^4 K. Anybody who wants some of those can write to me.

ROSENDHAL:

Isn't it true when you get to the temperature regime where you worry about the molecular opacities, that's where convection is important? It may have a more dominant role in the models.

ALEXANDER:

That's not necessarily true. The molecules can become important in the opacity at much higher temperatures than one might expect.

SPANGENBERG:

If you want to do shocks, you will always have low temperature zones.

KARP:

Thin-zone models very often show low temperature regions when you get very far out, where the molecules would limit the temperature minimum.

BAKER:

I think as far as FSK, perhaps a report from two different sides on what will go forward.

SPARKS:

For myself, I plan to return to that nova problem. There's only one group working on that, fortunately.

KARP:

I'm interested in these bumps. I'm going to go out and compute another model so I can draw the straight line between my two points.

BAKER:

You are planning, though, to continue with these?

KARP:

Yes.

BAKER:

Now, we have von Sengbusch, who has been doing systems programming lately. I don't know if he's planning to stop.

VON SENGBUSCH:

I thought always that it should be worthwhile that we should look a little bit into the low amplitude case.

BAKER:

At this point, I would think you would be interested first, in trying to compare some of your results with Stellingwerf.

VON SENGBUSCH:

That is, of course, something I definitely - - I was disappointed in the results because we didn't get any overlaps for the models. Our stability region moves a little bit if we make a different treatment of Q. I don't think it will change the amplitude so much, but we can check that.

SPANGENBERG:

I guess I will try to write up my results on RR Lyrae stars before John Cox and John Caster run out of patience. I think it would be interesting to look at some of the hydrodynamic phenomena that is occurring in the RR Lyraes with the mixed Euler-Lagrangian hydrodynamic scheme.

BAKER:

I'm not sure I understand what you have in mind.

SPANGENBERG:

One would like to zone up certain regions—the hydrogen region, for instance, or regions of the atmosphere—but the Lagrangian approach runs out of zones. I want to go to various schemes that exist in the one dimensional problems whereby you would, in effect, have continuous rezoning capability to look at some fine details.

COX:

I don't know the status, but John Castor has been with us for a couple of summers to try to learn how to slide zones in a Lagrangian mesh back and forth to keep a set of zones in the hydrogen ionization region. He has had some results.

SPANGENBERG:

But he has been unable to pursue it.

COX:

Yes. He's only worked on it for a total of about four or five weeks, and what he has wouldn't be in any suitable form for us to use. But it's a really nifty idea, and he has done some preliminary calculations where the light curves really come out quite differently. No more of these jags that we have been seeing all day.

SPANGENBERG:

It will take a little bit more work to put in some radiation transport, if you want to put on some sort of atmosphere.

COX:

You probably know more of his status than I do. It is done only with diffusion, isn't it?

SPANGENBERG:

Yes, I think so.

COX:

I don't know. I could once more be optimistic, but when you really get those Christy bumps a little better defined, those numbers go back to 0.22 or whatever.

VON SENGBUSCH:

That's really a feature that is not possible in the eigenvalue problem, because we are not really restricted to the same zoning through the whole period.

SPANGENBERG:

If you put on 25 optically thin zones, there is no basic problem?

VON SENGBUSCH:

There's no basic problem.

BAKER:

In connection with this zoning, I just might mention for those of you who might find it useful, either for this or other problems, that a certain amount of work has been done, mostly by people doing hydrodynamics, of what you might call automatic zoning schemes. The one that I am familiar with is one that was developed by Gough, Toomer, and Spiegel and has been discussed at a recent meeting on numerical methods in hydrodynamics, I believe, in Boulder. It was discussed by Gough there.

This was really developed to use with boundary layers where you have something happening rather violently in a thin layer. The procedure, which was developed by Gough, is to define an artificial independent variable, which is always evenly spaced in the mesh. If you have 100 points, then this variable takes up one-hundredth in each step.

Then the normal independent variable becomes a dependent variable, which depends on the new independent variable in certain ways, according to a certain function. That function is determined by variational principles, which can be written in different ways. I think the usual way is to somehow minimize the gradients of all of the quantities in a Henyey-type code. This is done particularly in Henyey-type codes, so the net result is that it increases the order of the system that you are iterating by 1 or 2, depending on whether you use a linear or a quadratic type of variable.

But it does mean that you automatically put the zones where they are the most necessary. Whether this would be useful to the Cepheid problem or not is certainly not clear to me, because the difficulty you get into is really somewhat different than it is in an ordinary boundary layer problem. It might be that the criteria you would want to apply for the variational principle would be different.

But I mention it, and it is available in the recent literature in case anyone would be interested in trying it for this problem. For some types of problems, it works very well. For others, it doesn't seem to be particularly useful.

COX:

I don't really know, but I imagine that Castor's idea was to slide these zones around and keep them at about the right temperature.

BAKER:

If you can do it in an automatic way, of course, it's nice.

COX:

I don't know. Maybe Bill [Spangenberg] or Bob [Stellingwerf] remembers what he had in mind, but the idea is to slide them back and forth and really follow in great detail the hydrogen ionization region.

I believe, and I expect that everybody would agree, that we have enough zoning in the helium ionization region. So it's merely a problem of having maybe a dozen more zones sliding back and forth with the . . .

VON SENGBUSCH:

You can only have a small number of points; in the whole helium ionization zone there are only four to five points.

COX:

But it moves around a little bit.

VON SENGBUSCH:

Sure, but it may move a whole mass layer. You have to describe the helium ionization region by at least three mass shells.

COX:

But all I'm saying is, I think the specifications are to move these layers around. The Cepheid problem is considerably less complicated than what you indicated.

BAKER:

It may well be. On the other hand, if you thought about it you might find an easy way to adapt already existing machinery. I guess we have come to Stellingwerf. What plans do you have?

STELLINGWERF:

I agree with Kurt that the eigenvalue tempts us to look at low luminosity cases. I've been thinking about that. Maybe we can get together on that.

CHRISTY:

People occasionally remember that these programs indicate instability in the sun. Has anybody ever tried that with a good eigenvalue method?

STELLINGWERF:

Bob Stein spoke to me about that, about trying it. What type instability do you use?

CHRISTY:

I believe that some people used to claim they get just the ordinary linear Cepheid instability; radial pulsations of the sun.

COX:

For the sun?

CHRISTY:

They didn't say it very loudly. It wasn't a very obvious pulsator, so they had to say that under their breath.

COX:

What they are saying, Bob, is that you can get it for red dwarfs or something.

BAKER:

Yes. You can certainly get it a little bit above the main sequence. I don't know if it's true with modern opacities and all of the rest, but I think that perhaps the linear stuff should be redone. The fact was that if you extrapolated the Cepheid instability strip down to the main sequence, then the sun was somewhere in the middle of it. This is not true.

COX:

A stars.

BAKER:

But, again, as far as that's concerned, it is a problem that is very much involved with the convection problem.

CHRISTY:

It would be interesting to get the Cepheid instability strip down to lower luminosities, however.

BAKER:

It certainly would. And there is abundant evidence from the linear stuff—I don't remember who has done it lately, but I think there has been an extensive survey. There certainly are some interesting indications that, for example, some of the higher modes are certainly excited in the linear case. I remember doing some myself a few years ago. It was perfectly clear that the fundamental, and sometimes even the first harmonic, were stable, and then you get into instability when you get into the second and third harmonics, and so on, and finally went stable again somewhere in the fifth harmonic. Other people have found that type of behavior, too.

Also, I think there are interesting questions there about nonradial oscillations, and there certainly is some evidence from the observations that there may be some nonradial stuff. Has there not been some work reported lately on some of these stars, the AI Velorum stars, I believe? The difficulty has always been to get the period ratios right for these double period Cepheids. And I think that it is still a problem.

KING:

Yes, it's Dziembowski. He talks about this.

BAKER:

Yes, Dziembowski has, I believe, a new preprint out, and it is still having trouble. I got it just before I came, so I didn't read it.

COX:

I would say this is a problem where the periodic people haven't quite used the word I've been waiting to hear them say, and that is the slow growth rate, not necessarily low amplitude or low luminosity.

BAKER:

Those certainly are low growth rate cases.

COX:

In particular, I wanted to say that it would be nice if one could really bear down on the mixed mode behavior and try to calibrate to what extent you can decompose those light curves as the observers have done into individual harmonics. I suspect that when you get something going really strong, you are going to distort each mode badly enough so that the linear superposition of the modes is inaccurate, even though the first case we did showed that it apparently was.

STELLINGWERF:

That would have to be checked. I have a suspicion, though, that some sort of power spectral analysis would still give the proper periods.

BAKER:

Which is, of course, easier to do with the models than it is with the observations.

STELLINGWERF:

The one I checked wasn't a perfect decomposition. If you just added up the two limit cycles, they looked vaguely like the actual limiting amplitude. The basic thing is, you certainly have some bumps in the velocity curves where the two modes are interacting. The periods are still pretty good.

COX:

Not only do you get the periods, but you get the sinusoidal variations for each of the periods.

BAKER:

Are you planning to look at the bump problem some more, probably go a little deeper?

STELLINGWERF:

Tempting!

BAKER:

I think the thing is that I'm more or less certain that you're right about it not affecting the growth rates and the periods, or probably even the amplitude of the final motion, but perhaps one would have more confidence if you did a few models that went deeper.

CHRISTY:

I think it does affect the amplitude of the final motion in the Cepheid region. When the bump is able to come back in phase with the next period, I find it corresponds to a very considerable increase in amplitude. That happens around a 20-day period in the sequence of the models I was doing. But when that happens, the amplitude of the final motion goes way up.

STELLINGWERF:

True. And that could affect stability also, I would imagine.

CHRISTY:

I'm not so sure about that.

STELLINGWERF:

I found that in the original models I had a pretty good way to vary the amplitude in that if I did it in the C_Q, I could get just about any amplitude I wanted. I found that the lines did tend to move around and it would change the amplitude. If you had a resonance effect, that could change things.

BAKER:

It might be worth investigating if only for your own peace of mind. Dave King, how about you?

KING:

I am interested in looking some more at these double-mode-type Cepheids. John Cox and I have discussed trying to combine our present code with a code similar to the one Bob Stellingwerf is using, because—well, it's not clear that it is needed for these double-mode

Cepheids, because they have fairly fast growth rates. They really have a low mass. But we feel this is a way to investigate many problems, and if you have a hybrid-type code, you can always run it the other way.

Another thing I am doing—actually, a student is trying to do—is a two-dimensional problem and putting in rotation in some sense. Although this is probably not important for Cepheids, that is what we can first start with. The spectrum is very important there. And then perhaps look at some of these other things where it would actually be important, where rotation might have an effect, such as for these short-period stars. I don't know what progress we will make with that.

COX:

Certainly, David, you are going to need Martin Schwarzschild to worry about convection.

KING:

I understand Martin is going to do it himself.

COX:

If you don't do it next time, he's going to hit you over the head. (Laughter)

BAKER:

I do think, as far as these rotation problems go, that this has not been done properly in the linear case, either. I think we should do linear, both radial and nonradial, pulsations and also include a small amount of rotation. The splitting of modes may well cause some resonances and so on. I think that would be a worthwhile thing to do.

COX:

Smeyers at Louvain has done something. He talked about it in Australia. I can't tell you about all these problems.

BAKER:

I wasn't aware of their work on rotation. I certainly would like to know about that. Certainly I think one would want to be a bit clearer on the linear picture before sinking a lot of work in the nonlinear. At least that would be my feeling.

COX:

I'd like to ask Bob Stellingwerf a question. Apparently the situation is that, if you have a rapidly growing pulsation, the classical methods of merely having an initial value problem and letting it go are the most efficient way of getting the answer. However, when you get to the slower growth rate, then, obviously, if it took a million periods to e-fold, your method is much better than to follow the thing for 50 years on the computer.

Now, do you estimate what kind of growth rate would be the one where we would switch from the classical methods to yours? I ask you rather than Kurt, because you did the whole thing in eight iterations, whereas Kurt took almost twice that number.

CHRISTY:

It may just be a question of the criterion for terminating the iterations that was different.

BAKER:

Also, he has fewer zones, of course.

COX:

But in any case, I am interested in the answer from both.

STELLINGWERF:

I ran 30 zones, but let's say I ran 60 zones. That would increase my time by a factor of 4, and the total calculation would then take about 12 minutes on the 7600. That's for two convergences. So you can compare that to the standard code. How long would it take?

KING:

Depends on how clever you are.

BAKER:

The periodic people could do something that you really can't do otherwise, that is to test its stability, because no matter how long you integrate, you can't get a solution that's sufficiently periodic to do this type of testing.

COX:

Would you advocate, Norm, that everybody switch over to this technique?

BAKER:

No, by no means.

CHRISTY:

I might say you car do some testing of the stability, although it's not always satisfactory. Namely, if you are doing it by the time-forward method and you always have some mixture of modes, then you can find out whether that mixture appears to be dying away or growing when it is a small, small level. This is some indication of stability.

KING:

What do you look for, precisely?

CHRISTY:

The variation in amplitude essentially. That is, the variations in amplitude are a measure of the mixture, and if the variations in amplitude are diminishing, then one asserts it is dying away.

STELLINGWERF:

I found that you can follow growth pretty well by looking at kinetic energy. Maybe the method Kurt described, of the Fourier Analysis, may be a little more accurate.

COX:

But that is applicable to either technique.

CHRISTY:

May I ask, is there any inherent difficulty in the eigenvalue method? If you are looking for a very slow growth rate, does this imply that your eigenvalues are getting so near 1, or so close together, that it is getting hard there, too? Or is there no problem at all?

STELLINGWERF:

It is always a little uncertain in the eigenvalue. It just depends on how good your mathematics are. But the way to get around that is to have smooth variations. If you are careful enough, if all your derivatives are good enough, you can draw a smooth line through the points and get it just as accurate as you like. It is just the point where you get zero, where it is pretty uncertain.

BAKER:

Also, you can do what Kurt does, if necessary, to iterate on the eigenvalue. He simply uses the matrix solving program. For finding the eigenvalues of your matrix, when you want to find the one that is nearest to 1, you actually have to use a special iteration procedure. If you use a program you take off the shelf, you don't get sufficient accuracy; you have to use a special iterative procedure. Then you can get very high accuracy. So that in the marginal cases, if it were really very close, you might have to go to that kind of iterative procedure, too.

VON SENGBUSCH:

But I think what you really meant was, to what extent would the number of iterations that we would have to use to get to the final solution depend upon the excitation rate?

COX:

No, I think that's what Bob [Christy] asked.

VON SENGBUSCH:

I think I have no feeling for that, but I would imagine it would not be very much dependent on that.

BAKER:

I would think quite the contrary. That it would be the other way around because, if the growth rate is slow, it means that the departures from adiabaticity are small. That should simplify it.

STELLINGWERF:

From my experience, there does not seem to be any particular dependence on growth rate. The number of iterations or the accuracy of the eigenvalues seems to be determined mainly by the accuracy of the derivatives.

COX:

Let me ask another question. What is the largest number of periods for an e-folding that you have run into? Have you done a thousand, ten thousand periods per e-folding time?

STELLINGWERF:

One thousand.

COX:

That's almost what's been done already by the initial value approach.

Where is the breakover point? There are some of these Population II Cepheids that grow up, you know, like a rocket. We could use the classical methods; but, at some point—I'm curious to know if we can define where it is—one should really go to this new Baker-von Sengbusch technique or the Stellingwerf technique.

STELLINGWERF:

I'm just trying to see where the computer times would be the same.

COX:

You do a lot more computing per iteration.

KARP:

Would that depend on the number of zones, too? I mean, do you do 100 zones or 200 zones?

STELLINGWERF:

As I said, it goes as the square of the number of zones. For a 60-zone model, it would take 12 minutes for the normal calculation. You might be able to do four times as many periods as I can do iterations in the same amount of time.

COX:

Beyond 100, one should never use the initial value technique?

CHRISTY:

Even with e-folding times of 100 periods, you can get to the final amplitude in 30 periods by accelerations.

COX:

If one has the code, one might as well use it. But beyond that, only rarely, I think, has anybody had the patience to fight it.

BAKER:

The other problem you have there, is that if your growth rates are extremely small, then you may very well have transients that die out very slowly.

STELLINGWERF:

That's what I was going to say. You not only want to look at the growth rate, you also have to know what the switching rates are if you want to do the mode problem. You may have a big growth rate with small switching rates.

CHRISTY:

That's right. Sometimes the worst problem is to eliminate the unwanted modes when you are near final amplitude.

COX:

I thought they sort of run together. If they grow faster, they probably switch faster, too.

STELLINGWERF:

No. Whenever you are near one of these transition lines, it can be very slow.

BAKER:

That is clear from the topology of your diagram.

COX:

Perhaps one should then say that more than just David [King] and John Cox ought to think about switching to these newer techniques.

BAKER:

Depends on what they want to do.

VON SENGBUSCH:

It is obvious that only a little more work and you will get a good result.

BAKER:

And, of course, anytime that there is something that you want the time integration for, then I take it that in both of these methods, you just flip a switch.

COX:

One of the things Norm and I talked about in Toronto three years ago—and Norm admitted as you did, too—was that there was a great deal of difficulty in the outer layers where you had to do something special, which added a lot to the computing time.

At that time, it looked as though, if you weren't careful, it took about as long to do the model the new way as it did the old way. Now, apparently it has been established that if the growth rate's e-folding time is about 30 periods, either way is all right.

BAKER:

I guess since everybody has discussed what he is going to do, I can say a word, because it may be of some interest to people.

For several years, we have been working on the convection-pulsation problem. This is work I have been doing with Douglas Gough. The technique used is a generalization of mixing length theory to a time-dependent case, and Gough developed it some time ago. It is coded for linear pulsations and essentially runs. There are numerical difficulties—it is exceedingly complex—and the numerical difficulties that we have been working on for a while now are connected with the turbulent pressure. From our preliminary work, which we talked about some years ago, it became clear that the turbulent pressure, even though it is never decisive in static models, can be quite important for the pulsation convection interaction, and it may well be more important than the way in which the pulsation modulates the convective flux.

The reason is, of course, that it is part of the pressure. It is the component of the pressure that is out of phase with the radius variation, and therefore in phase with the velocity, that provides the work. The ordinary gas pressure is never more than a little out of phase with the radius fluctuation. However, for a turbulent pressure, there is actually no requirement that it be anything like the density. So this is rather important.

On the other hand, it seriously complicates the computing, and in particular, we have a lot of trouble even getting static models. We've been working very hard, and have succeeded in getting a few models, but they are still so hard to get that we are not satisfied. But I think in the course of the next year, we are certainly going to solve this.

The problems are numerical ones. It is a very hard numerical problem, but I think there are no problems in principle. What we will do, of course, will be just linear work.

The theory is, in principle, not too difficult to put into a nonlinear code of the sort von Sengbusch and Stellingwerf use. It would be quite difficult to put it into a marching-type time integration, because it involves integration over the lifetimes of the convective elements. So you'd have to be computing convolution-type quantities all the time, whereas if you know the motion is periodic, you can make that assumption to begin with.

I think that if the linear case is successful, and if we believe in it, and we can sort out all the effects, then, of course, we will be strongly tempted to try to put it into this type of a nonlinear code. But how long this will take, I don't know. It is an awful lot of hard work.

COX:

I guess I am not aware of the basic physics involved.

ALEXANDER:

Is this related at all to the work Castor has been doing?

BAKER:

There is some similarity to Castor's work–it is more similar to Unno's theory. The most important difference is that Unno did not include the turbulent pressure aspect. The other differences are probably not too significant, although we have not sorted all of them out.

Just to answer Art's question, the physics of the thing is really that one takes the usual picture of the mixing length theory, namely rising and falling elements. At any level, you have a certain prototype rising and prototype falling element that transports the heat convectively across any horizontal surface in the star. In terms of an element that is born with, maybe a slight density deficiency, say, relative to the ambient medium, then it rises and gains kinetic energy from the buoyancy field. Then after a certain distance, the mixing length, it dissipates and gives up all its excess heat.

If you want to be fancy, a la Böhm-Vitense, you can put in the radiative losses of the element as it rises. If you take that picture, then what you have to do is ask what are the dynamics as the ambient medium is changing. In other words, as it rises, it is feeling a different acceleration.

Furthermore, if you take any horizontal plane in the medium, the heat flux across the plane at any time is due to elements, born at different times and places with different histories. Then you have various trajectories in space and time. At any given time, what is crossing a certain height may come from several different trajectories; so, you have to find some way of integrating over these trajectories. That is the basic idea.

As I said, the fact that you have to include in this both the flux and the turbulent pressure makes this system exceedingly complicated. It is a horrendously complicated thing. But I think in principle, in the linear case, there are no real difficulties; however, we are persisting with this.

* * * * *

COX:

I'm always interested in mass discrepancy. It is important that, in spite of scale problems and all the rest, everybody does seem to feel that Christy's formula is correct, and a radius measurement by the phasing of the bumps is an accurate one within 10 percent or so.

CHRISTY:

I do not believe it is fair to say that everyone has had a chance to test that formula for themselves. This is an indication that it may be reasonably right, but before you say everyone agrees to it, they might want to go home and check it out for themselves.

COX:

For this model, they did not seem to express any hesitancy when they wrote those numbers down, or did they?

CHRISTY:

I don't know. I was trying to give them an out.

* * * * *

COX:

It would be nice, Warren—I get my ear bent from time to time by Martin Schwarzschild, who always says that at the end of a conference one should list problems remaining to be solved. Now I guess they have been scattered throughout the two days of talk; and I guess I'm not proposing that we sit down and write them right now, but the editors of the proceedings might find it useful for us to list them.

CHRISTY:

Some of the discussion about what people plan to do represents a listing of problems to be solved. It doesn't mean those are the only problems, but those are the problems people thought they might be able to do something about.

[We followed Christy's suggestion.]

COX:

Certainly there is the mass discrepancy and the convection. There is a conversion of some people to the periodic solution techniques. I guess that is not a problem, but more of a technical decision. However, it would be useful for a conference, if one could outline the existing problems.

Another one that I thought of which people might think is a little off the subject is calibrate the temperature scale. We don't do it, but somebody should.

ROSENDHAL:

Also, what about the problem of boundary conditions, whether the bump is due to the inner boundary or to the outer boundary?

CHRISTY:

I think people have varied the outer boundary quite a lot. Some of these have very many thin zones, and others have very few, without affecting the bump predicted. Exactly what its origin is can be debatable until one can trace it in detail. But I believe it is reasonably clear the outer boundary can be moved around a lot without affecting the bump.

COX:

That is a problem I don't think we do have. The problem is the visibility of the bump.

CHRISTY:

It is clearly visible in the optically thin regions, but if you have a not too extensive optically thin region, it still seems to be fairly visible.

I would like to be able to, somehow or other, extract the bump in the region just below the photosphere where it seems to vanish totally. If I get a chance to look for it in the temperature or the internal energy, I will try to do it.

For an observer, I would like to suggest examining some Cepheids with bumps to see what the phase relation is between the bump on the velocity curve and in the light curve. If that can be clearly established, then one can look at quite a number of Cepheids where only light curves are available.

The light curve may have a relative minimum at the phase of the bump, as suggested by some of the calculations. It would be nice to be sure, from looking at observed light curves, exactly what feature on the light curve can be identified with the bump.

ROSENDHAL:

You would have to do the measurements simultaneously.

CHRISTY:

That is probably wise to do, or to have enough familiarity with which measurements in the literature are good. I think it needs a little bit more work from the point of observed curves.

STELLINGWERF:

I wonder whether or not this would have some bearing on the second bump discrepancy, because that minimum will appear before the bump or after the bump, depending on where the bump appears, with respect to the maximum velocity.

CHRISTY:

In terms of the calculations I tried to make, I usually try to take a velocity curve identified there. There are not very many of those.

COX:

You mean in the literature?

CHRISTY:

Yes. I invite anyone to try to make his own calculations of observed curves and conclude what mass will make it agree with the model calculations.

STELLINGWERF:

I am referring to taking the second bump rather than what seems to be the minor bump.

CHRISTY:

Would you define which is the second and which is the first bump? I'm confused.

STELLINGWERF:

I'm just saying your relation, really, is to take the second bump. I think Stobie pointed that out. The relationship between a period and the second bump is a linear relationship; isn't that correct?

CHRISTY:

I say that the time delay is linearly related to the radius.

STELLINGWERF:

Suppose you have an observation of a 10-day Cepheid in which you have two equal bumps at the top?

CHRISTY:

Yes, that's the problem. Until you also get a velocity curve and can see both of them in the velocity curve and then identify them in the light curve, you are going to have a problem.

STELLINGWERF:

Do we have some sort of linear relationship now between the phase of the second bump and some period?

CHRISTY:

But the calibration constant in that relation comes out of the theory, the models, and the models do not give the evolutionary masses.

If you use this same calibration constant around 0.23 or 0.24, then you will get, certainly, the same masses as in the models. The question is, can you find any basis for changing that calibration constant? I don't know.

STELLINGWERF:

Maybe I'm not getting across what I am trying to say. Isn't there some kind of funny discrepancy there, where the second bump is not the one you expected?

CHRISTY:

When the bump moves to approximate coincidence with the peak, then it becomes a debatable question as to which one is which.

KARP:

Very often the observers only take the one that has the maximum light. If you do that, you find that it is about half a period from minimum light. That means that the light curve is symmetric, if that is indeed the main pulsation, so that is probably the second bump. You get roughly the right asymmetry for the amplitude if you pick the first bump, even though it is smaller, to be the primary pulsation, and the other one would be the secondary bump, even though it's bigger.

The zero phase is always defined at maximum light no matter whether that bump is the first one or the second. You get the wrong asymmetry for the amplitude if you do that.

* * * * *

COX:

If I understand what happened today, you [Christy] were the only one who alleged any bump on the rising part.

KING:

But there are observed curves like that.

CHRISTY:

I think it is possible, and we all have calculations that show a rising branch with the bump. We will sometimes find that the bump is coincident with the peak.

COX:

And Gil keeps claiming that that is an artificial viscosity effect.

CHRISTY:

I am not absolutely convinced that it may not show up in real light curves. Let me give an example. In type c RR Lyrae variables and the harmonic type RR Lyrae variables, most of the light curves that I calculated tend to look like sinusoidal, but with a notch in them.

If you look at an observed light curve for the same kind of variables, they characteristically have a little notch in them too, but much, much smaller. Now, I think it is quite possible that this, which is an observed dip in a light curve, may in fact be the same phenomenon we see in a much more enhanced form. So I am not convinced this is totally artificial.

COX:

I guess this is the first time I have ever heard about bumps for RR Lyrae stars.

SPANGENBERG:

It may not be—it looks like a bump, but it may actually be a turnback.

CHRISTY:

This is a well observed thing in type RR Lyrae stars.

COX:

Would you ascribe it to any echoing like you do in the classical Cepheids?

CHRISTY:

No. It seems to coincide with the calculations with this rapid acceleration phase, which I don't identify as any echoing phenomena.

STELLINGWERF:

For longer period Cepheids, periods greater than 10 days, there is a bump . . .

COX:

Yes, there is a bump moving across the peak and it comes on the front side.

CHRISTY:

Yes, there is. I am not prepared to identify it. I know that it appears there, but I am not sure that I would want to label it as fitting any formula that I have given.

COX:

I thought that 0.24 applied no matter on which side of the peak the bump came?

CHRISTY:

In the calculations that I have, only when the bump occurs on the falling branch of the velocity curve does it fit that formula. At other times I have not been able to obtain any clear fitting formula.

KING:

I recently was looking at some light curves that a couple of South Africans had prepared, both on Population I Cepheids and RR Lyrae stars. They get some rather interesting things. They frequently find wiggles in the bottom of a light curve on RR Lyrae stars. How big they are and so forth seems to depend on the period, too. I do not remember how it goes, but maybe someone here does.

CHRISTY:

There are some RR Lyrae stars that have characteristic wiggle at minimum light.

KING:

Sometimes they found even more than one little bump.

CHRISTY:

There are some calculations that will show that in a consistent way, too. But I have never been able to be sure exactly what feature of the calculation does it or exactly how–it sometimes shows up in a calculation, and it certainly shows up in observation.

BAKER:

Is this photoelectric photometry?

KING:

Not just UBV, they used a number of filters. They had some 70 RR Lyrae stars, and maybe 40 Cepheids, with periods ranging over most of the entire range of periods.

CHRISTY:

If someone was fooling around with RR Lyrae stars, it might be worthwhile to try to pin that down in models, because any specific feature that you can finally identify in models and show exactly how it varies with various parameters, you may be able to use to measure things in observed stars.

STELLINGWERF:

Actually, when Steve Hill did his extended atmosphere, he got that accentuated . . .

HUTCHINSON:

Bumps at both phases have been observed by OAO-2 on RR Lyrae stars, right at the bottom (minimum light).

COX:

I have a question I am a little concerned about. Did we see this morning, or did we not see this morning, that the velocity curve bump did shift a little bit with optical depth?

CHRISTY:

I think it was clear, in the cases where there were several optical depths, that it shifted to later times farther out in the photosphere.

COX:

Which surprises me, because those numbers certainly seem to be all the same.

CHRISTY:

No. First of all, I commented that my number is 0.23 something, if I look at the photosphere. And I think most—well, some of them—are quoted for optical depth around 0.1 and some around 1. It does vary about 0.23 and 0.24. If you go to an optical depth of 10^{-3}, then it becomes considerably delayed.

COX:

But now we have got an observer sitting right in the front row here. We have to be careful that we come up with the curve with the right set of lines that corresponds to the right optical depth.

ROSENDHAL:

I think this also involves somebody sitting down in a realistic case, realizing that the observers measure only the strongest lines, because the weakest lines, which would be correspondent to the photosphere, are the ones that get buried in blends. Somebody has to sit down and actually calculate the depth of formation of these lines, and then you can specify at which depths the observers are measuring them and how high you make the comparison.

PARSONS:

I did that for a few representative lines that have been measured for radial velocities, and I got an average depth formation of about 0.16. The range was from 0.05 to 0.3.

COX:

This is optical depth at 5000 Å?

PARSONS:

Essentially 5000 Å. Rosseland mean.

KARP:

I computed velocity curves from line profiles that I computed in my hydrodynamic models. I find that, except for the very strongest lines, if you measure at the half intensity point of the profile, you see almost no velocity difference between different strength lines, a very slight effect, but less than you would be able to observe.

So what I am saying is, measure all your velocities at the half intensity point of the profile. You should get just about the same result for any strength line. It is a matter of calibrating the part of the star that you are referring to, so you can compare the models. I have not done that. But it does not make much difference if you use a strong line or a weak line, as long as it is neither extreme.

PARSONS:

The measures—FeI for the radial velocity curve—should be fairly representative of optical depth 0.1 or so.

A couple of years ago I had a student compile radial velocity curves for classical Cepheids . . .

ROSENDHAL:

I think some time ago, de Jager calculated depths of formation, and my impression always was that it was about 0.1.

COX:

It seems we are getting a reasonable reading from the observers. What Bob Christy is saying is, look at those 0.24 numbers a little bit more at the 0.1 level.

SPARKS:

Which ones are calculated at the 0.1 level?

DAVIS:

I should redo mine.

SPANGENBERG:

I don't have a table with me to use to interpolate.

ROSENDHAL:

Al, this half intensity depth is probably the principle, but that, of course, is not the way you measure photographic spectra. If you have an oscilloscope comparator you can fit the whole profile. You can sort of wiggle around to get the wings to overlap each other; but, among other things for good velocity measurements, you tend to overexpose spectra. In order to get the lines well defined, it may be around the part of the photographic characteristic curve where the thing sort of blows up anyway. You cannot really define the half intensity point well.

KARP:

But you have got to stay clear of the minimum because of line splitting that you might not see, because of the slit or macroturbulence is smearing it out. That splitting will foul up your velocity measurements. You have to be very careful with that.

ROSENDHAL:

I think, for most of the iron lines, you rarely see any asymmetries. So you get a fairly good fit on these relatively sharp line stars for the profile as a whole.

SPARKS:

I hate to cut off the discussion, but we only have 10 minutes.

Thank you, everybody, for coming and participating. I would like to thank the students who have been helping with slides and running errands.

BAKER:

I would like to say that I was rather skeptical about the whole idea of comparing these things, and I really didn't think it would work at all. But I was pleasantly surprised, and I certainly think that you [Sparks] and Dave Fischel have done a very fine job of organizing. I think it worked out very well, and so I think we also ought to thank you and the authorities at NASA for letting us have the conference.

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APPENDIX
STANDARD MODELS

This Appendix contains the tabular data of the standard models as submitted by the various contributors. The phases in the tables are measured from maximum light and are not shifted. For the shifts relative to FSK and Ki see Table W1 in the Workshop. The Conference participants were sent a description of the standard model and the graphs to be used in the Workshop. Portions of this letter are excerpted below.

Dear Colleague:

*****

Presentations will be limited to 30 minutes maximum. We would also request that all manuscripts be brought with you or mailed in advance so that the proceedings of this workshop can be published within 3 or 4 months. Without a manuscript, we will have to depend upon tape recordings.

For the people doing the standard model we have some special requests. We will have viewgraphs made for all the various standard models in order to facilitate the comparison. Therefore, we would ask for the following graphs and tables to be sent by July 15: bolometric magnitude vs phase, velocity vs phase, effective temperature vs phase, temperature at maximum and minimum light vs mass, density at maximum and minimum light vs mass, and pressure at maximum and minimum light vs mass. The following table gives the range and scale for each variable.

Variable	Range	Scale
M_{bol}	±0.9 mag from equilibrium	0.1^m/cm
Vel	±27 km/s	3 km/s/cm
T_{eff}	4 to 7 X 10^3K	200 K/cm
ϕ	0 to 1.25	0.05/cm
$\log_{10}T$	3.4 tc 6	0.15/cm
$\log_{10}P$	0 to 11	1/cm
$\log_{10}\rho$	-13 to -1	1/cm
$\log_{10}Q$	-11 to -1	0.5/cm

ϕ is the phase which is defined to be 0 at maximum light and Q is the exterior mass fraction $1 - (M_r/M_*)$.

The graphs should all be plotted on 10X10 to the cm, 18X25 on graph paper (like K&E 46 1510, a sample of which is included). The graphs for the depth dependent variables should contain one curve (solid line) for conditions at maximum light and one curve (dashed line) for conditions at minimum light. The tables need not be more than fifty entries. These graphs and tables will be published in the workshop session. Please do not feel you must limit yourself to just these graphs and tables. Detailed computer printout will also be welcomed. The luminosity (1.243×10^{37} erg/s) of the standard model given in our previous letter is not found in the *Astrophysical Journal* paper (182, p. 859) which discusses the model; but it is from a private communication from D. King.

*****

Sincerely,

Warren M. Sparks and David Fischel
Laboratory for Optical Astronomy

THE BEDNAREK (Be and BeII) MODELS

Theodore Andrew Bednarek

The unusual behavior of my first model, Be, relative to my linear, nonadiabatic results and the other models presented at the Conference, prompted additional calculations. Model Be had a long period of 10.7 days and did not have a " Christy bump" on the velocity curve. The cause of this behavior was traced to the treatment of the equation of state in the core zones of the model. In model Be, the equation of state was obtained from tables for all zones with $T < 10^5$ K in the static model, and an analytic expression for $T \geqslant 10^5$ K. The principal motivation for using this technique was to reduce the effects of truncation error in the energy equation in the core zones. This procedure caused problems because interpolation in a table overestimated the pressure in the core zones by less than 0.5 percent relative to the analytic expression. The zoning of the table was 0.05 in $\log_{10} T$ and 0.3 in $\log_{10} \rho$, and 4-point interpolation was used.

The deficiency of pressure in the core zones of model Be slowed the expansion of the envelope following maximum compression and resulted in the lengthened period. The retarded maximum in the outward radial velocity also tended to mask the Christy bump.

A new model, Be II (see list of parameters on following page), was calculated using a tabular equation of state throughout the entire model. Apart from this difference, the same mass zoning and calculation procedures were used for both models. The use of the tabular equation of state in the core zones caused the internal energy conservation in model Be II to deteriorate to 1 part in 10^3 compared to 1 part in 10^4 in model Be. Some of the characteristic differences between the two models are listed below.

Model	Be	Be II
KE (peak)	3.0×10^{42} erg	1.2×10^{42} erg
$(\delta R/R)_{\text{semiamplitude}}$	0.09	0.06
ΔV	54 km s^{-1}	38 km s^{-1}
ΔM_{bol}	1.15	0.97
$(\delta KE/KE)_{\text{peak}}$ at KE = 5×10^{41} erg	*4.5%	6.6%
Period	10.7 days	9.87 days
Christy bump	not present	present

* The value of the growth rate stated at the Conference was incorrect.

The growth rate and period of the linear, nonadiabatic model were 8.8 percent and 9.72 days, respectively.

In model Be II, the radial velocity and bolometric magnitude were both sampled at a fixed zone corresponding to optical depth 0.3 in the static model. The effective temperature was obtained by interpolation at optical depth 2/3. There are differences in the plots of $\log_{10} T$, $\log_{10} P$, and $\log_{10} \delta$ between the two models Be and Be II. In particular, the behavior of $\log_{10} T$ was similar in the two models at minimum light. However, the surface zones of model Be II were cooler than those of model Be at maximum light.

In conclusion, these calculations demonstrate that the input physics should be consistent throughout the model. It should be emphasized that the differences between models Be and Be II were not due to any fundamental differences in the equation of state itself, but rather, in the form of interpolation used to obtain the thermodynamic variables. The importance of high accuracy tabular interpolation is evident.

Be II Model Parameters

Zones	36/4
Opacity	Russian tables, X = 0.70, Y = 0.28
Program	Christy type
Viscosity	Q 1
No. of periods	92
Period	9.87 days
L	1.243×10^{37} erg s^{-1}
T_{eff}	5700 K
Fractional core radius	0.15
Boundary condition	P = 0 at surface
Christy phase delay of bump	0.24

Be Model

Phase	M_{bol}	T_e	Vel km/s
0.000	-4.78	6144	+20.75
0.016	-4.66	6407	+10.23
0.035	-4.64	6081	+7.70
0.053	-4.43	6558	+18.68
0.073	-4.30	5394	+26.79
0.093	-4.22	6170	+26.44
0.112	-4.27	6027	+27.32
0.130	-4.35	5846	+26.77
0.148	-4.40	5811	+26.20
0.167	-4.28	5980	+23.60
0.186	-4.23	5720	+24.57
0.206	-4.28	5809	+20.53
0.224	-4.30	5687	+16.57
0.243	-4.18	5905	+15.32
0.261	-4.11	5588	+14.49
0.282	-4.13	5646	+11.50
0.301	-4.18	5648	+7.63
0.321	-4.20	5539	+4.55
0.339	-4.07	5116	+2.97
0.360	-3.98	4922	+0.88
0.383	-3.93	4884	-0.82
0.407	-3.94	5584	-4.75
0.431	-3.91	5456	-7.13
0.454	-3.87	5450	-8.90
0.478	-3.82	5445	-11.96
0.501	-3.76	4801	-15.09
0.524	-3.68	4779	-17.52
0.547	-3.63	4775	-19.34
0.569	-3.64	4984	-20.14
0.588	-3.62	5200	-22.03
0.608	-3.59	5337	-22.49
0.626	-3.61	5383	-23.89
0.644	-3.63	5403	-25.40
0.663	-3.59	5357	-25.98
0.681	-3.50	4706	-26.30
0.692	-3.43	4714	-25.69
0.700	-3.46	4895	-24.10
0.718	-3.47	5351	-22.02

Be Model (continued)

Phase	M_{bol}	T_e	Vel km/s
0.738	-3.59	5389	-21.55
0.756	-3.61	5562	-16.79
0.773	-3.65	5360	-12.21
0.792	-3.76	5464	-9.46
0.811	-3.78	5471	-4.45
0.831	-3.79	5488	-0.68
0.850	-3.89	5524	+1.16
0.872	-3.90	5513	+2.26
0.892	-3.90	5567	+2.79
0.910	-3.90	5657	+3.98
0.928	-4.11	5616	+10.23
0.945	-4.45	5832	+15.92
0.963	-4.59	6077	+22.18
0.982	-4.74	6267	+22.87

	At Maximum Light				At Minimum Light		
$Log_{10}Q$	$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$		$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$
-1.028	5.986	10.528	-3.611		5.986	10.528	-3.611
-1.173	5.888	10.106	-3.939		5.888	10.105	-3.939
-1.318	5.799	9.718	-4.240		5.799	9.717	-4.240
-1.463	5.717	9.357	-4.522		5.717	9.356	-4.522
-1.608	5.643	9.019	-4.790		5.643	9.017	-4.791
-1.753	5.574	8.701	-5.042		5.573	8.699	-5.044
-1.898	5.507	8.402	-5.278		5.506	8.399	-5.280
-2.043	5.444	8.122	-5.498		5.442	8.116	-5.502
-2.188	5.382	7.857	-5.703		5.380	7.848	-5.710
-2.333	5.324	7.609	-5.896		5.320	7.593	-5.907
-2.478	5.268	7.373	-6.078		5.262	7.349	-6.096
-2.623	5.216	7.147	-6.254		5.207	7.114	-6.278
-2.768	5.165	6.928	-6.424		5.154	6.885	-6.456
-2.912	5.117	6.719	-6.587		5.102	6.661	-6.631
-3.058	5.070	6.516	-6.747		5.051	6.441	-6.803
-3.203	5.028	6.318	-6.909		5.002	6.221	-6.977

Be Model (continued)

	At Maximum Light				At Minimum Light		
$Log_{10}Q$	$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$		$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$
-3.348	4.985	6.127	-7.064		4.949	6.001	-7.151
-3.493	4.944	5.939	-7.215		4.900	5.788	-7.315
-3.639	4.901	5.758	-7.355		4.855	5.592	-7.473
-3.784	4.863	5.584	-7.497		4.819	5.415	-7.617
-3.930	4.824	5.414	-7.629		4.781	5.253	-7.748
-4.076	4.786	5.248	-7.766		4.745	5.106	-7.854
-4.223	4.750	5.089	-7.884		4.701	4.962	-7.947
-4.370	4.712	4.941	-7.997		4.647	4.807	-8.034
-4.518	4.672	4.803	-8.089		4.593	4.639	-8.133
-4.667	4.631	4.674	-8.163		4.542	4.456	-8.254
-4.819	4.586	4.537	-8.238		4.486	4.283	-8.365
-4.973	4.537	4.375	-8.343		4.419	4.129	-8.437
-5.131	4.486	4.222	-8.433		4.265	3.992	-8.385
-5.294	4.428	4.017	-8.578		3.743	3.856	-7.692
-5.467	4.364	3.707	-8.830		3.673	3.656	-7.821
-5.654	4.278	3.423	-9.019		3.644	3.397	-8.050
-5.865	3.958	2.621	-9.298		3.631	2.973	-8.462
-6.124	3.788	2.595	-9.003		3.626	2.577	-8.853
-6.503	3.761	2.161	-9.414		3.624	2.172	-9.260

Be II Model

Phase	M_{bol}	T_e	Vel km/s
0.000	-4.44	6383	+5.29
0.021	-4.32	6053	+1.97
0.042	-4.13	5715	+0.82
0.064	-3.99	5623	+7.87
0.085	-3.95	5445	+11.66
0.108	-4.03	5546	+11.49
0.129	-4.15	5689	+12.35
0.151	-4.21	5781	+11.00
0.171	-4.21	5808	+8.71
0.191	-4.04	5636	+6.94
0.214	-3.99	5559	+4.03
0.238	-4.00	5508	+2.83
0.266	-4.01	5470	+0.54
0.287	-3.99	5458	-3.64
0.308	-3.98	5455	-6.30
0.329	-3.95	5420	-7.96
0.353	-3.86	5309	-10.36
0.373	-3.82	5129	-12.38
0.394	-3.78	5070	-14.95
0.423	-3.77	5236	-16.71
0.444	-3.75	5272	-16.96
0.464	-3.70	5309	-21.08
0.491	-3.58	5117	-23.13
0.512	-3.46	5012	-23.58
0.533	-3.60	5188	-21.79
0.546	-3.55	5152	-19.92
0.554	-3.61	5236	-19.84
0.575	-3.76	5408	-20.22
0.599	-3.89	5675	-14.97
0.619	-3.91	5689	-13.60
0.640	-3.82	5559	-4.55
0.661	-4.01	5875	+2.81
0.682	-4.12	6053	+7.91
0.704	-4.14	6026	+11.02
0.727	-4.13	5902	+13.70
0.747	-4.13	5916	+14.82
0.769	-4.14	5920	+14.39
0.791	-4.17	5943	+13.47

Be II Model (continued)

Phase	M_{bol}	T_e	Vel km/s
0.819	-4.19	5984	+12.34
0.846	-4.20	5940	+11.72
0.871	-4.21	5957	+11.36
0.894	-4.28	5950	+12.54
0.914	-4.40	6095	+12.97
0.943	-4.43	6166	+10.76
0.963	-4.42	6138	+9.66
0.985	-4.42	6237	+7.34

	At Maximum Light				At Minimum Light		
$Log_{10}Q$	$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$		$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$
-1.028	5.989	10.542	-3.602		5.989	10.543	-3.602
-1.173	5.891	10.117	-3.932		5.891	10.118	-3.931
-1.318	5.801	9.727	-4.234		5.801	9.728	-4.233
-1.463	5.719	9.364	-4.517		5.719	9.365	-4.516
-1.608	5.644	9.023	-4.788		5.645	9.025	-4.787
-1.753	5.574	8.703	-5.044		5.574	8.704	-5.042
-1.898	5.506	8.401	-5.281		5.507	8.403	-5.280
-2.043	5.442	8.116	-5.504		5.443	8.119	-5.502
-2.188	5.379	7.847	-5.715		5.380	7.849	-5.713
-2.333	5.318	7.589	-5.914		5.319	7.592	-5.911
-2.478	5.260	7.341	-6.101		5.261	7.347	-6.098
-2.623	5.203	7.102	-6.289		5.205	7.109	-6.284
-2.768	5.149	6.869	-6.470		5.152	6.877	-6.464
-2.912	5.097	6.644	-6.645		5.100	6.653	-6.639
-3.058	5.045	6.425	-6.816		5.050	6.438	-6.808
-3.203	4.997	6.210	-6.982		5.003	6.234	-6.965
-3.348	4.946	6.001	-7.146		4.958	6.038	-7.124
-3.493	4.900	5.796	-7.305		4.915	5.847	-7.272
-3.639	4.853	5.595	-7.464		4.873	5.656	-7.429
-3.784	4.809	5.401	-7.616		4.830	5.460	-7.583
-3.930	4.766	5.211	-7.770		4.785	5.263	-7.742
-4.076	4.724	5.031	-7.907		4.737	5.074	-7.879
-4.223	4.681	4.857	-8.040		4.687	4.895	-8.006
-4.370	4.636	4.691	-8.153		4.635	4.727	-8.107

Be II Model (continued)

	At Maximum Light				At Minimum Light		
$Log_{10}Q$	$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$		$Log_{10}T$	$Log_{10}P$	$Log_{10}\rho$
-4.518	4.593	4.536	-8.255		4.585	4.551	-8.219
-4.667	4.548	4.396	-8.338		4.532	4.362	-8.349
-4.819	4.500	4.259	-8.413		4.475	4.170	-8.473
-4.973	4.446	4.093	-8.514		4.403	3.994	-8.564
-5.131	4.384	3.870	-8.675		4.229	3.927	-8.405
-5.294	4.298	3.568	-8.886		3.741	3.751	-7.794
-5.467	4.005	3.137	-8.858		3.679	3.577	-7.905
-5.654	3.771	2.998	-8.578		3.650	3.394	-8.059
-5.865	3.734	2.800	-8.739		3.635	3.153	-8.286
-6.124	3.719	2.520	-9.006		3.629	2.748	-8.685
-6.503	3.714	1.993	-9.534		3.627	1.960	-9.476

The Fischel, Sparks, and Karp (FSK) Model

David Fischel, Warren M. Sparks, and Alan H. Karp

Phase	M_{bol}	T_{eff}	Vel km/s
0.00	-4.555	6419.5	19.00
0.02	-4.538	6357.7	16.83
0.04	-4.508	6279.1	14.4
0.06	-4.471	6194.4	13.9
0.08	-4.412	6082.8	13.33
0.10	-4.346	5969.0	12.62
0.12	-4.253	5826.4	11.1
0.14	-4.105	5618.2	10.3
0.16	-3.961	5427.5	10.45
0.18	-3.935	5390.1	11.25
0.20	-3.980	5446.3	11.00
0.22	-4.011	5490.4	11.15
0.24	-4.044	5539.9	9.90
0.26	-4.030	5520.8	10.0
0.28	-4.042	5525.9	8.5
0.30	-4.006	5466.4	6.00
0.32	-3.998	5445.0	4.05
0.34	-3.988	5422.5	2.2
0.36	-3.934	5348.1	-0.2
0.38	-3.920	5326.0	-1.74
0.40	-3.905	5307.6	-4.13
0.42	-3.837	5225.2	-6.48
0.44	-3.790	5172.5	-8.41
0.46	-3.752	5135.7	-10.35
0.48	-3.710	5095.7	-12.35
0.50	-3.664	5055.9	-14.35
0.52	-3.617	5018.8	-15.85
0.54	-3.568	4985.4	-18.29
0.56	-3.538	4976.2	-20.05
0.58	-3.523	4988.8	-21.6
0.60	-3.433	4919.3	-21.9
0.62	-3.471	5001.5	-24.29
0.64	-3.496	5067.6	-23.8
0.66	-3.557	5165.4	-23.38
0.68	-3.690	5344.4	-22.82
0.70	-3.799	5499.2	-21.45

FSK Model (continued)

Phase	M_{bol}	T_{eff}	Vel km/s
0.72	-3.892	5644.2	-20.10
0.74	-3.968	5762.4	-19.68
0.76	-4.055	5925.2	-20.38
0.78	-3.958	5822.4	-10.45
0.80	-3.955	5838.5	4.4
0.82	-4.170	6123.5	13.3
0.84	-4.232	6173.1	16.36
0.86	-4.223	6133.4	17.8
0.88	-4.215	6087.0	18.16
0.90	-4.223	6082.8	19.2
0.92	-4.276	6133.4	20.60
0.94	-4.320	6174.5	21.44
0.96	-4.408	6277.7	22.2
0.98	-4.505	6384.1	21.1
1.00	-4.555	6419.5	19.00

	At Maximum Light				At Minimum Light		
$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$		$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
-1.117	5.986	-3.653	10.491		5.986	-3.653	10.491
-1.238	5.905	-3.026	10.139		5.905	-3.926	10.139
-1.360	5.827	-4.176	9.812		5.827	-4.176	9.812
-1.481	5.754	-4.410	9.506		5.754	-4.410	9.506
-1.542	5.689	-4.637	9.217		5.689	-4.637	9.217
-1.724	5.628	-4.854	8.942		5.628	-4.854	8.942
-1.846	5.568	-5.060	8.679		5.568	-5.060	8.679
-1.967	5.511	-5.257	8.428		5.511	-5.257	8.428
-2.088	5.460	-5.451	8.187		5.460	-5.451	8.187
-2.210	5.411	-5.638	7.955		5.411	-5.640	7.954
-2.331	5.362	-5.815	7.734		5.361	-5.817	7.730
-2.452	5.314	-5.981	7.522		5.312	-5.985	7.515
-2.574	5.268	-6.140	7.320		5.264	-6.148	7.307
-2.695	5.223	-6.290	7.127		5.216	-6.303	7.106
-2.817	5.179	-6.434	6.941		5.170	-6.454	6.910
-2.938	5.136	-6.573	6.761		5.124	-6.600	6.720
-3.059	5.093	-6.706	6.586		5.078	-6.738	6.536

FSK Model (continued)

	At Maximum Light				At Minimum Light		
$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$		$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
-3.181	5.051	-6.835	6.415		5.034	-6.871	6.360
-3.302	5.011	-6.902	6.248		4.994	-7.000	6.190
-3.429	4.973	-7.088	6.085		4.955	-7.131	6.023
-3.545	4.936	-7.213	5.927		4.916	-7.262	5.856
-3.667	4.901	-7.336	5.772		4.879	-7.395	5.689
-3.788	4.867	-7.456	5.620		4.843	-7.530	5.523
-3.909	4.834	-7.577	5.471		4.807	-7.660	5.360
-4.031	4.800	-7.695	5.322		4.769	-7.786	5.198
-4.152	4.766	-7.812	5.172		4.728	-7.911	5.033
-4.274	4.731	-7.928	4.931		4.684	-8.028	4.868
-4.395	4.697	-8.039	4.879		4.633	-8.135	4.698
-4.517	4.660	-8.145	4.735		4.581	-8.246	4.522
-4.639	4.621	-8.241	4.592		4.536	-8.371	4.437
-4.760	4.578	-8.333	4.445		4.497	-8.494	4.183
-4.882	4.536	-8.434	4.294		4.458	-8.599	4.039
-5.005	4.498	-8.547	4.141		4.420	-8.679	3.914
-5.127	4.462	-8.660	3.994		4.371	-8.688	3.838
-5.249	4.426	-8.755	3.860		4.022	-8.275	3.702
-5.373	4.389	-8.855	3.721		3.734	-7.897	3.641
-5.496	4.350	-8.960	3.574		3.690	-7.943	3.552
-5.620	4.306	-9.083	3.405		3.663	-8.043	3.424
-5.745	4.251	-9.174	3.246		3.646	-8.182	3.268
-5.872	4.075	-9.236	2.938		3.635	-8.318	3.121
-6.000	3.811	-8.711	2.909		3.628	-8.438	2.994
-6.131	3.771	-8.787	2.790		3.624	-8.553	2.875
-6.265	3.754	-8.883	2.678		3.621	-8.668	2.757
-6.403	3.745	-8.994	2.558		3.619	-8.779	2.644
-6.548	3.740	-9.124	2.424		3.618	-8.883	2.540
-6.703	3.737	-9.251	2.296		3.617	-9.021	2.401
-6.873	3.735	-9.353	2.192		3.617	-9.299	2.124
-7.069	3.733	-9.486	2.061		3.617	-9.660	1.767
-7.312	3.733	-9.651	1.898		3.617	-10.051	1.383
-7.678	3.733	-9.821	1.734		3.617	-10.558	0.905
-8.011	3.732	-9.991	1.572		3.617	-10.809	0.684

THE COX AND DAVIS (CD) MODEL

A.H. Cox and C.G. Davis

Phase	M_{bol}	Velocity	T_{eff}
0.014	-4.2676E+00	1.9214E+01	6.1448E+03
0.038	-4.2505E+00	1.6263E+01	6.0974E+03
0.060	-4.2219E+00	1.3640E+01	6.0392E+03
0.083	-4.2596E+00	1.1402E+01	6.0758E+03
0.105	-4.1671E+00	8.4876E+00	5.9373E+03
0.128	-4.1489E+00	5.2230E+00	5.9040E+03
0.151	-4.0277E+00	3.5926E+00	5.7344E+03
0.174	-3.9156E+00	2.9900E+00	5.5821E+03
0.196	-3.8138E+00	4.2970E+00	5.4456E+03
0.219	-3.7682E+00	6.8967E+00	5.3812E+03
0.242	-3.8423E+00	7.8401E+00	5.4664E+03
0.264	-3.8889E+00	7.3832E+00	5.5187E+03
0.288	-3.8790E+00	6.2385E+00	5.4998E+03
0.310	-3.8997E+00	5.0351E+00	5.5214E+03
0.333	-3.8421E+00	2.4274E+00	5.4469E+03
0.356	-3.8223E+00	-1.2360E-01	5.4212E+03
0.378	-3.8340E+00	-1.7813E+00	5.4375E+03
0.401	-3.7150E+00	-4.2729E+00	5.2956E+03
0.424	-3.6618E+00	-7.6648E+00	5.2380E+03
0.447	-3.6404E+00	-1.0254E+01	5.2211E+03
0.469	-3.6136E+00	-1.2508E+01	5.2011E+03
0.492	-3.5704E+00	-1.5229E+01	5.1644E+03
0.515	-3.5103E+00	-1.8520E+01	5.1119E+03
0.538	-3.4412E+00	-2.0958E+01	5.0513E+03
0.560	-3.3738E+00	-2.3248E+01	4.9960E+03
0.583	-3.3863E+00	-2.5354E+01	5.0360E+03
0.606	-3.3502E+00	-2.6986E+01	5.0219E+03
0.628	-3.4513E+00	-2.7183E+01	5.1702E+03
0.652	-3.4307E+00	-2.9178E+01	5.1794E+03
0.673	-3.4709E+00	-2.8103E+01	5.2602E+03
0.697	-3.7308E+00	-2.7581E+01	5.6230E+03
0.720	-3.8566E+00	-2.7109E+01	5.8252E+03
0.743	-3.9502E+00	-2.7395E+01	5.9913E+03
0.764	-3.8477E+00	-2.5430E+01	5.8871E+03
0.787	-3.7329E+00	8.6730E+00	5.7428E+03
0.810	-3.9898E+00	2.0400E+01	6.0717E+03

CD Model (continued)

Phase	M_{bol}	Velocity	T_{eff}
0.832	-4.0767E+00	2.6424E+01	6.1593E+03
0.856	-4.1428E+00	2.9336E+01	6.2106E+03
0.879	-4.1783E+00	3.0012E+01	6.2199E+03
0.901	-4.1824E+00	2.8508E+01	6.1862E+03
0.924	-4.2364E+00	2.6793E+01	6.2250E+03
0.947	-4.2056E+00	2.5023E+01	6.1444E+03
0.970	-4.2182E+00	2.2517E+01	6.1311E+03
0.992	-4.2384E+00	2.1178E+01	6.1301E+03
1.014	-4.2646E+00	1.9005E+01	6.1407E+03
1.037	-4.2796E+00	1.6365E+01	6.1391E+03
1.060	-4.2237E+00	1.3618E+01	6.0418E+03
1.083	-4.1929E+00	1.1248E+01	5.9834E+03
1.105	-4.1728E+00	8.3957E+00	5.9458E+03
1.129	-4.1566E+00	4.8413E+00	5.9154E+03
1.152	-4.0443E+00	3.1483E+00	5.7579E+03
1.174	-3.9110E+00	2.5793E+00	5.5779E+03
1.196	-3.8019E+00	4.0528E+00	5.4328E+03
1.219	-3.7561E+00	6.9960E+00	5.3686E+03
1.242	-3.8386E+00	7.9555E+00	5.4638E+03

Maximum Light Phase = 0.0			
$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
-1.1309E+00	6.0054E+00	-3.5786E+00	1.0550E+01
-1.2062E+00	5.9508E+00	-3.7365E+00	1.0338E+01
-1.2816E+00	5.9037E+00	-3.8976E+00	1.0130E+01
-1.3569E+00	5.8647E+00	-4.0620E+00	9.9312E+00
-1.4322E+00	5.8242E+00	-4.2179E+00	9.7372E+00
-1.5076E+00	5.7821E+00	-4.3639E+00	9.5508E+00
-1.5829E+00	5.7405E+00	-4.5043E+00	9.3704E+00
-1.6582E+00	5.7016E+00	-4.6426E+00	9.1952E+00
-1.7335E+00	5.6650E+00	-4.7789E+00	9.0250E+00
-1.8089E+00	5.6301E+00	-4.9123E+00	8.8599E+00
-1.8842E+00	5.5966E+00	-5.0431E+00	8.6992E+00
-1.9595E+00	5.5635E+00	-5.1687E+00	8.5437E+00
-2.0349E+00	5.5296E+00	-5.2877E+00	8.3930E+00
-2.1102E+00	5.4951E+00	-5.4007E+00	8.2469E+00

CD Model (continued)

Maximum Light Phase = 0.0			
$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
-2.1855E+00	5.4606E+00	-5.5078E+00	8.1057E+00
-2.2608E+00	5.4262E+00	-5.6110E+00	7.9681E+00
-2.3362E+00	5.3931E+00	-5.7112E+00	7.8349E+00
-2.4115E+00	5.3620E+00	-5.8114E+00	7.7048E+00
-2.4868E+00	5.3332E+00	-5.9108E+00	7.5783E+00
-2.5622E+00	5.3059E+00	-6.0091E+00	7.4551E+00
-2.6375E+00	5.2794E+00	-6.1050E+00	7.3351E+00
-2.7128E+00	5.2525E+00	-6.1963E+00	7.2185E+00
-2.7882E+00	5.2254E+00	-6.2833E+00	7.1052E+00
-2.8635E+00	5.1982E+00	-6.3673E+00	6.9944E+00
-2.9389E+00	5.1712E+00	-6.4489E+00	6.8858E+00
-3.0142E+00	5.1444E+00	-6.5286E+00	6.7792E+00
-3.0896E+00	5.1177E+00	-6.6068E+00	6.6741E+00
-3.1649E+00	5.0912E+00	-6.6838E+00	6.5702E+00
-3.2403E+00	5.0653E+00	-6.7595E+00	6.4683E+00
-3.3157E+00	5.0401E+00	-6.8344E+00	6.3680E+00
-3.3911E+00	5.0153E+00	-6.9098E+00	6.2679E+00
-3.4665E+00	4.9915E+00	-6.9851E+00	6.1694E+00
-3.5419E+00	4.9688E+00	-7.0601E+00	6.0729E+00
-3.6173E+00	4.9464E+00	-7.1345E+00	5.9772E+00
-3.6927E+00	4.9240E+00	-7.2087E+00	5.8816E+00
-3.7682E+00	4.9017E+00	-7.2832E+00	5.7860E+00
-3.8437E+00	4.8795E+00	-7.3580E+00	5.6904E+00
-3.9192E+00	4.8572E+00	-7.4332E+00	5.5944E+00
-3.9947E+00	4.8352E+00	-7.5097E+00	5.4976E+00
-4.0703E+00	4.8133E+00	-7.5864E+00	5.4010E+00
-4.1460E+00	4.7915E+00	-7.6619E+00	5.3053E+00
-4.2217E+00	4.7696E+00	-7.7366E+00	5.2102E+00
-4.2975E+00	4.7475E+00	-7.8104E+00	5.1154E+00
-4.3733E+00	4.7251E+00	-7.8838E+00	5.0205E+00
-4.4493E+00	4.7024E+00	-7.9563E+00	4.9258E+00
-4.5254E+00	4.6791E+00	-8.0270E+00	4.8309E+00
-4.6017E+00	4.6546E+00	-8.0950E+00	4.7355E+00
-4.6781E+00	4.6284E+00	-8.1593E+00	4.6391E+00
-4.7547E+00	4.6005E+00	-8.2198E+00	4.5418E+00
-4.8316E+00	4.5718E+00	-8.2805E+00	4.4427E+00
-4.9088E+00	4.5430E+00	-8.3468E+00	4.3404E+00
-4.9863E+00	4.5151E+00	-8.4199E+00	4.2363E+00
-5.0643E+00	4.4886E+00	-8.4980E+00	4.1308E+00

CD Model (continued)

Maximum Light Phase = 0.0			
$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
-5.1428E+00	4.4625E+00	-8.5790E+00	4.0242E+00
-5.2219E+00	4.4365E+00	-8.6616E+00	3.9163E+00
-5.3018E+00	4.4105E+00	-8.7463E+00	3.8067E+00
-5.3827E+00	4.3839E+00	8.8316E+00	3.6961E+00
-5.4646E+00	4.3563E+00	-8.9115E+00	3.5880E+00
-5.5480E+00	4.3265E+00	-8.9858E+00	3.4806E+00
-5.6331E+00	4.2912E+00	-9.0381E+00	3.3825E+00
-5.7203E+00	4.2381E+00	-9.0411E+00	3.3017E+00
-5.8102E+00	4.0614E+00	-8.8834E+00	3.2182E+00
-5.9035E+00	3.8107E+00	-8.4477E+00	3.1270E+00
-6.0012E+00	3.7649E+00	-8.4838E+00	3.0441E+00
-6.1048E+00	3.7416E+00	-8.5550E+00	2.9474E+00
-6.2164E+00	3.7279E+00	-8.6517E+00	2.8353E+00
-6.3391E+00	3.7194E+00	-8.7766E+00	2.7023E+00
-6.4785E+00	3.7145E+00	-8.9602E+00	2.5206E+00
-6.6449E+00	3.7108E+00	-9.1537E+00	2.3202E+00
-6.8614E+00	3.7081E+00	-9.3165E+00	2.1602E+00
-7.2017E+00	3.7084E+00	-9.6498E+00	1.8565E+00
-1.4148E+01	3.7035E+00	-1.0272E+01	1.2368E+00

Minimum Light Phase = 0.6			
$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
-1.1309E+00	6.0050E+00	-3.5797E+00	1.0549E+01
-1.2062E+00	5.9516E+00	-3.7338E+00	1.0340E+01
-1.2816E+00	5.9030E+00	-3.8997E+00	1.0128E+01
-1.3569E+00	5.8651E+00	-4.0606E+00	9.9324E+00
-1.4322E+00	5.8242E+00	-4.2182E+00	9.7370E+00
-1.5076E+00	5.7822E+00	-4.3636E+00	9.5513E+00
-1.5829E+00	5.7406E+00	-4.5043E+00	9.3704E+00
-1.6582E+00	5.7016E+00	-4.6424E+00	9.1955E+00
-1.7335E+00	5.6651E+00	-4.7786E+00	9.0254E+00
-1.8089E+00	5.6302E+00	-4.9122E+00	8.8601E+00
-1.8842E+00	5.5968E+00	-5.0427E+00	8.6998E+00
-1.9595E+00	5.5636E+00	-5.1683E+00	8.5442E+00
-2.0349E+00	5.5297E+00	-5.2874E+00	8.3935E+00

CD Model (continued)

Minimum Light Phase = 0.6			
$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
-2.1102E+00	5.4954E+00	-5.4001E+00	8.2477E+00
-2.1855E+00	5.4607E+00	-5.5074E+00	8.1061E+00
-2.2608E+00	5.4264E+00	-5.6105E+00	7.9687E+00
-2.3362E+00	5.3930E+00	-5.7113E+00	7.8346E+00
-2.4115E+00	5.3619E+00	-5.8117E+00	7.7041E+00
-2.4868E+00	5.3323E+00	-5.9123E+00	7.5756E+00
-2.5622E+00	5.3045E+00	-6.0119E+00	7.4506E+00
-2.6375E+00	5.2771E+00	-6.1098E+00	7.3274E+00
-2.7128E+00	5.2491E+00	-6.2035E+00	7.2072E+00
-2.7882E+00	5.2206E+00	-6.2934E+00	7.0894E+00
-2.8635E+00	5.1923E+00	-6.3799E+00	6.9748E+00
-2.9389E+00	5.1643E+00	-6.4637E+00	6.8629E+00
-3.0142E+00	5.1369E+00	-6.5447E+00	6.7542E+00
-3.0896E+00	5.1097E+00	-6.6239E+00	6.6474E+00
-3.1649E+00	5.0833E+00	-6.7008E+00	6.5437E+00
-3.2403E+00	5.0576E+00	-6.7759E+00	6.4425E+00
-3.3157E+00	5.0322E+00	-6.8517E+00	6.3410E+00
-3.3911E+00	5.0079E+00	-6.9275E+00	6.2413E+00
-3.4665E+00	4.9840E+00	-7.0042E+00	6.1420E+00
-3.5419E+00	4.9607E+00	-7.0807E+00	6.0434E+00
-3.6173E+00	4.9378E+00	-7.1577E+00	5.9448E+00
-3.6927E+00	4.9153E+00	-7.2356E+00	5.8464E+00
-3.7682E+00	4.8932E+00	-7.3140E+00	5.7478E+00
-3.8437E+00	4.8711E+00	-7.3926E+00	5.6493E+00
-3.9192E+00	4.8493E+00	-7.4716E+00	5.5507E+00
-3.9947E+00	4.8273E+00	-7.5499E+00	5.4523E+00
-4.0703E+00	4.8044E+00	-7.6267E+00	5.3540E+00
-4.1460E+00	4.7805E+00	-7.7022E+00	5.2553E+00
-4.2217E+00	4.7554E+00	-7.7763E+00	5.1560E+00
-4.2975E+00	4.7285E+00	-7.8478E+00	5.0559E+00
-4.3733E+00	4.6988E+00	-7.9146E+00	4.9551E+00
-4.4493E+00	4.6652E+00	-7.9752E+00	4.8529E+00
-4.5254E+00	4.6294E+00	-8.0325E+00	4.7486E+00
-4.6017E+00	4.5956E+00	-8.0969E+00	4.6418E+00
-4.6781E+00	4.5654E+00	-8.1730E+00	4.5330E+00
-4.7547E+00	4.5380E+00	-8.2576E+00	4.4232E+00
-4.8316E+00	4.5127E+00	-8.3459E+00	4.3141E+00

CD Model (continued)

Minimum Light Phase = 0.6			
$Log_{10} Q$	$Log_{10} T$	$Log_{10} \rho$	$Log_{10} P$
-4.9088E+00	4.4887E+00	-8.4316E+00	4.2078E+00
-4.9863E+00	4.4650E+00	-8.5085E+00	4.1075E+00
-5.0643E+00	4.4409E+00	-8.5725E+00	4.0162E+00
-5.1428E+00	4.4151E+00	-8.6201E+00	3.9343E+00
-5.2219E+00	4.3856E+00	-8.6523E+00	3.8551E+00
-5.3018E+00	4.3483E+00	-8.6941E+00	3.7624E+00
-5.3827E+00	4.2889E+00	-8.7036E+00	3.6792E+00
-5.4646E+00	4.0407E+00	-8.3722E+00	3.6300E+00
-5.5480E+00	3.7970E+00	-7.9900E+00	3.5677E+00
-5.6331E+00	3.7364E+00	-7.9898E+00	3.5066E+00
-5.7203E+00	3.7035E+00	-8.0146E+00	3.4481E+00
-5.8102E+00	3.6803E+00	-8.0616E+00	3.3762E+00
-5.9035E+00	3.6627E+00	-8.1338E+00	3.2890E+00
-6.0012E+00	3.6497E+00	-8.2284E+00	3.1861E+00
-6.1048E+00	3.6402E+00	-8.3392E+00	3.0687E+00
-6.2164E+00	3.6333E+00	-8.4588E+00	2.9408E+00
-6.3391E+00	3.6283E+00	-8.5830E+00	2.8092E+00
-6.4785E+00	3.6252E+00	-8.7176E+00	2.6741E+00
-6.6449E+00	3.6239E+00	-8.8861E+00	2.5196E+00
-6.8614E+00	3.6239E+00	-9.1165E+00	2.3200E+00
-7.2017E+00	3.6250E+00	-9.4845E+00	2.0330E+00
-1.4148E+00	3.6266E+00	-1.0438E+01	1.4652E+00

THE KING (Ki) MODEL

D.S. King

$4M_\odot$	Kippenhahn Mixture (X = 0.602, Z = 0.044)	
Phase	M_{bol}	r_{49} (km/s)
0.00	-4.260	+23.48
0.02	-4.227	+21.03
0.04	-4.103	+18.21
0.06	-4.057	+15.91
0.08	-4.048	+14.86
0.10	-4.063	+14.43
0.12	-4.077	+14.25
0.14	-4.104	+13.56
0.16	-4.134	+12.58
0.18	-4.151	+11.37
0.20	-4.157	+9.643
0.22	-4.162	+7.333
0.24	-4.165	+4.627
0.26	-4.122	+1.682
0.28	-3.928	- 1.370
0.30	-3.776	- 3.470
0.32	-3.655	- 1.023
0.34	-3.651	+3.968
0.36	-3.693	+6.684
0.38	-3.743	+6.305
0.40	-3.751	+4.829
0.42	-3.751	+2.821
0.44	-3.767	+0.484
0.46	-3.786	- 2.033
0.48	-3.782	- 4.671
0.50	-3.739	- 7.402
0.52	-3.615	- 10.21
0.54	-3.542	- 13.10
0.56	-3.484	- 15.84
0.58	-3.457	- 18.46
0.60	-3.461	- 20.45
0.62	-3.461	- 22.03
0.64	-3.430	- 23.74
0.66	-3.391	- 25.83
0.68	-3.353	- 28.79

Ki Model (continued)

4 $M_\odot$	Kippenhahn Mixture (X = 0.0602, Z = 0.44)	
Phase	M_{bol}	r_{49} (km/s)
0.70	-3.325	- 30.12
0.72	-3.352	- 32.26
0.74	-3.371	- 33.06
0.76	-3.324	- 29.34
0.78	-3.799	- 27.13
0.80	-3.732	- 21.73
0.82	-3.100	- 9.026
0.84	-3.760	+ 7.183
0.86	-3.935	+14.09
0.88	-4.097	+19.40
0.90	-4.195	+23.08
0.92	-4.229	+25.01
0.94	-4.223	+25.70
0.96	-4.211	+26.03
0.98	-4.202	+25.71
1.00	-4.231	+24.37
1.02	-4.264	+22.23
1.04	-4.127	+19.57
1.06	-4.084	+16.85
1.08	-4.049	+15.19
1.10	-4.055	+14.60
1.12	-4.080	+14.31
1.14	-4.109	+13.70
1.16	-4.136	+12.72
1.18	-4.155	+11.48
1.20	-4.160	+ 9.737
1.22	-4.156	+ 7.437
1.24	-4.160	+ 4.760
1.26	-4.135	+ 1.871
1.28	-3.938	- 1.081
1.30	-3.788	- 3.052

Ki 4b Model (continued)

4 $M_\odot$	Kippenhahn Mixture (X = 0.602, Z = 0.044)			
Light Maximum (continued)				
Zone No.	$Log_{10}Q$	$Log_{10}T$	$Log_{10}\rho$	$Log_{10}P$
14	-1.982	5.537	-5.191	8.489
15	-2.080	5.497	-5.345	8.298
16	-2.179	5.455	-5.490	8.114
17	-2.277	5.413	-5.627	7.937
18	-2.376	5.371	-5.760	7.765
19	-2.475	5.334	-5.890	7.598
20	-2.573	5.298	-6.019	7.435
21	-2.672	5.262	-6.144	7.277
22	-2.771	5.225	-6.261	7.124
23	-2.869	5.188	-6.373	6.975
24	-2.968	5.156	-6.480	6.833
25	-3.067	5.119	-6.584	6.695
26	-3.166	5.085	-6.685	6.561
27	-3.265	5.053	-6.783	6.430
28	-3.364	5.022	-6.882	6.302
29	-3.463	4.993	-6.981	6.174
30	-3.563	4.964	-7.081	6.047
31	-3.663	4.935	-7.180	5.921
32	-3.763	4.907	-7.280	5.796
33	-3.863	4.879	-7.380	5.670
34	-3.964	4.851	-7.482	5.544
35	-4.066	4.823	-7.584	5.417
36	-4.168	4.793	-7.684	5.282
37	-4.272	4.762	-7.779	5.164
38	-4.376	4.727	-7.865	5.040
39	-4.483	4.686	-7.937	4.917
40	-4.592	4.642	-8.002	4.793
41	-4.703	4.598	-8.078	4.664
42	-4.819	4.556	-8.174	4.523
43	-4.940	4.513	-8.295	4.359
44	-5.068	4.466	-8.424	4.184
45	-5.207	4.408	-8.508	4.029
46	-5.362	4.278	-8.465	3.883
47	-5.543	3.762	-7.830	3.705
48	-5.777	3.676	-8.046	3.415
49	-6.130	3.666	-8.375	3.027
50	——	3.634	-9.577	2.094

The Stellingwerf (St) Model

R. F. Stellingwerf

Phase	M_{bol}	Vel	T_{eff}	Phase	M_{bol}	Vel	T_{eff}
0.00	-4.71	15.97	6886	0.64	-3.52	-22.89	4932
0.02	-4.67	19.37	6799	0.66	-3.43	-25.90	4848
0.04	-4.66	23.71	6752	0.68	-3.36	-28.62	4798
0.06	-4.61	25.24	6636	0.70	-3.33	-30.74	4790
0.08	-4.49	26.88	6418	0.72	-3.34	-30.04	4835
0.10	-4.43	27.84	6303	0.74	-3.19	-28.63	4704
0.12	-4.47	29.59	6320	0.76	-3.49	-26.04	5069
0.14	-4.39	28.84	6161	0.78	-3.56	-25.94	5184
0.16	-4.40	27.44	6133	0.80	-3.84	-26.82	5552
0.18	-4.30	26.03	5968	0.82	-3.78	-26.88	5507
0.20	-4.28	24.71	5902	0.84	-3.67	-28.34	5403
0.22	-4.30	23.36	5907	0.86	-3.11	-17.94	4776
0.24	-4.22	22.04	5778	0.88	-3.58	- 8.72	5339
0.26	-4.20	19.88	5720	0.90	-3.68	- 5.61	5473
0.28	-4.23	17.24	5736	0.92	-3.75	- 1.86	5568
0.30	-4.16	15.06	5634	0.94	-3.99	3.22	5879
0.32	-4.12	13.35	5560	0.96	-4.49	9.71	6589
0.34	-4.09	11.16	5513	0.98	-4.64	13.32	6804
0.36	-4.08	8.28	5493	1.00	-4.71	15.97	6886
0.38	-4.04	5.50	5430	1.02	-4.67	19.37	6799
0.40	-3.96	3.51	5324	1.04	-4.66	23.71	6752
0.42	-3.91	1.84	5268	1.06	-4.61	25.24	6636
0.44	-3.89	-0.53	5237	1.08	-4.49	26.88	6418
0.46	-3.85	-3.39	5196	1.10	-4.43	27.84	6303
0.48	-3.81	-6.05	5153	1.12	-4.47	29.59	6320
0.50	-3.78	-8.19	5122	1.14	-4.39	28.84	6161
0.52	-3.76	-10.00	5111	1.16	-4.40	27.44	6133
0.54	-3.72	-12.16	5075	1.18	-4.30	26.03	5968
0.56	-3.68	-14.37	5042	1.20	-4.28	24.71	5902
0.58	-3.66	-15.85	5025	1.22	-4.30	23.36	5907
0.60	-3.62	-17.83	5001	1.24	-4.22	22.04	5778
0.62	-3.59	-20.24	4979	1.26	-4.20	19.88	5720

St Model (continued)

$Log_{10}Q$	$Log_{10}T$		$Log_{10}\rho$		$Log_{10}P$	
	(L_{max})	(L_{min})	(L_{max})	(L_{min})	(L_{max})	(L_{min})
-5.87	3.75	3.59	-8.10	-7.35	3.45	4.04
-5.78	4.08	3.62	-8.58	-7.29	3.57	4.13
-5.68	4.33	3.67	-8.81	-7.30	3.68	4.18
-5.59	4.38	3.74	-8.68	-7.33	3.87	4.21
-5.48	4.42	4.10	-8.65	-7.88	3.94	4.24
-5.38	4.46	4.39	-8.54	-8.23	4.09	4.29
-5.27	4.50	4.44	-8.45	-8.29	4.23	4.28
-5.17	4.54	4.47	-8.37	-8.31	4.34	4.30
-5.06	4.58	4.50	-8.29	-8.28	4.48	4.37
-4.95	4.62	4.53	-8.24	-8.21	4.59	4.48
-4.83	4.65	4.57	-8.17	-8.14	4.70	4.60
-4.72	4.69	4.62	-8.1C	-8.08	4.81	4.72
-4.61	4.72	4.67	-8.03	-8.02	4.92	4.84
-4.50	4.74	4.71	-7.94	-7.96	5.04	4.97
-4.38	4.77	4.75	-7.84	-7.88	5.16	5.09
-4.27	4.80	4.79	-7.74	-7.80	5.29	5.21
-4.16	4.83	4.82	-7.65	-7.71	5.41	5.34
-4.04	4.86	4.85	-7.55	-7.61	5.53	5.46
-3.93	4.88	4.87	-7.46	-7.51	5.65	5.59
-3.82	4.91	4.90	-7.35	-7.40	5.77	5.72
-3.70	4.94	4.93	-7.25	-7.29	5.90	5.85
-3.59	4.97	4.95	-7.14	-7.18	6.04	5.98
-3.48	5.00	4.99	-7.03	-7.06	6.18	6.12
-3.36	5.03	5.02	-6.92	-6.95	6.31	6.27
-3.25	5.07	5.06	-6.81	-6.84	6.46	6.42
-3.13	5.10	5.10	-6.70	-6.72	6.60	6.58
-3.02	5.14	5.14	-6.59	-6.60	6.75	6.74
-2.91	5.18	5.18	-6.47	-6.47	6.92	6.91
-2.79	5.22	5.22	-6.34	-6.34	7.09	7.08

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