HORIZONTAL BRANCH EVOLUTION

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Abstract. In 1973 the outstanding problems confronting the theory of horizontal branch evolution were the "second parameter" problem and the Oosterhoff Effect. Despite significant progress, particularly in the observations and in the observation/theory interface, they remain as the outstanding problems of 1988. The Oosterhoff Effect is now discussed primarily in the guise of the Sandage Period Shift Effect. The morphology of the HB seems more complicated than ever. E.g., many clusters show bimodal distributions along the HB. Here we will tentatively consider those to be manifestations of the second parameter problem. We will indicate why we feel that all previously suggested solutions have all been chimeras.

1 *A RETROSPECTIVE—RTR*

By the early 1970's the basic theory of horizontal branch (HB) stars seemed on firm ground. Faulkner and Iben had convincingly identified the HB with the core helium burning phase of globular cluster stars. The solution to the "first parameter" problem—the HB gets redder as metalicity increases—followed naturally from this identification and thus never became a problem. The major oversight in the first models, the presence of first overshooting and then semiconvection, at the boundary of the convective core had been demonstrated by Castellani, Giannone, and Renzini. Models incorporating this result had been constructed by, e.g., Demarque, Mengel, and Sweigart. The HB had been shown not to be an evolutionary sequence, but most probably a sequence of stars with equal core masses and slightly different total masses due to some differential mass loss process on the red giant branch. I was gratified when Monte Carlo simulations constructed along these lines bore a striking similarity to observed HBs.

At that time there seemed to two remaining problems. The "second parameter" problem—at a given metalicity some clusters have HBs that are too red or too blue—was particularly intriguing. If, for example, the second parameter could be identified with age, one would have direct observational evidence on the collapse time of the galaxy to a precision of 1 Gyr or perhaps even better. The other was the Oosterhoff effect—on the basis of the properties of their RR Lyrae stars the clusters fell into two distinct groups. For Oo I clusters about 75% of the RR Ly were pulsating in the fundamental mode (Type ab) with mean period 0.55 day. For the Oo II clusters the corresponding numbers were 50% and 0.65 day. The trouble was that the second parameter problem was too easy to solve—variations in age, helium abundance, CNO abundances all worked, with little but aesthetics to chose among them. On the other hand nothing worked for the Oosterhoff effect. (I had started the investigation convinced that solution of the Oosterhoff problem would identify *the true* second parameter.)

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One could go no further at this time. It was obvious that mass loss was extremely important, but there was no mechanism for producing the mass loss, much less knowing how it scaled with parameters like the metal abundance. The abundances themselves were poorly known. The conversions from theoretically determined log L, log $T_{\rm eff}$ to observables were suspect. The photometric samples were too small and often incomplete. There was uncertainty whether the evolutionary history of an HB star could affect whether it pulsated in the first harmonic or fundamental mode (hysteresis). I punted.

Surprisingly, by the end of the 1970's my wish list of 1973 was complete. For mass loss, we had both the model of Fusi Pecci and Renzini (now unfortunately ruled out by X-ray observations) and the empirical Reimer's formula. At the least, these provided a whipping boy against which to test hypotheses. The atmospheres were in much better shape due to the work of Bell, Gustafsson, Kurucz, and others. The work of Stellingwerf provided a concrete way of dealing with the mode of the variables. Many observers (bless them) had increased both the quantity and quality of the photometry and spectroscopy. The first suggestions of variations in [CNO/Fe] and in star-to-star abundance variations were appearing. Photometry extending below the main sequence turnoff point was becoming commonplace. I approached the problem with new optimism. With the help of first Pat Seitzer and then Debe Crocker, the simulation program was updated, expanded, and made fully interactive along the lines of the data reduction programs then becoming available at NRAO. It was clear that the profusion of data required a far more efficient approach.

As with a decade earlier my optimism was misplaced. For the second parameter problem things got worse. Perhaps it was only due to the efficiency that we could now produce models, but the sensitivity of the results to assumptions about mass loss seemed worse than ever. When one finds oneself twiddling with the Reimer's efficiency η_R in the third significant figure, how can much significance possibly be placed on the results? One could argue that η_R was a function of [Fe/H], or not, depending (sensitively) on how [CNO/Fe] varies with [Fe/H]. Peterson found significant rotation in HB stars. The number of possible second parameters was actually increasing with time! The problem with the Oosterhoff effect, likewise, was resistant to solution. At least it acquired a new name—the Sandage Period Shift Effect.

The following sections informally summarize some of the results obtained in the last few years. For a more detailed review and complete references I direct you to the excellent review of Renzini and Fusi Pecci (1988).

2 *THE STANDARD MODEL*

Many lines of evidence suggest that globular cluster red giants lose 0.1–0.2 $M_\odot$ of their mass either in the form of a stellar wind or at helium flash. The standard model of the HB suggests that for some reason there is a dispersion of order 10–20% in the amount of mass lost. Since the core mass at flash is insensitive to total mass, stars begin quiescent core helium burning along a line of constant core mass and varying total mass (the ZAHB). The observed HB is the result of the superposition of evolutionary tracks of stars of differing mass which arrived on the ZAHB at sometime in the past. While this model is widely accepted at the moment, the possibility remains that some parameter other than, or in addition to, total mass varies along the HB. Firmly established are: (1) The lower envelope of the observed HB is close to the ZAHB except possibly for the hottest stars. (2) The HB is not an

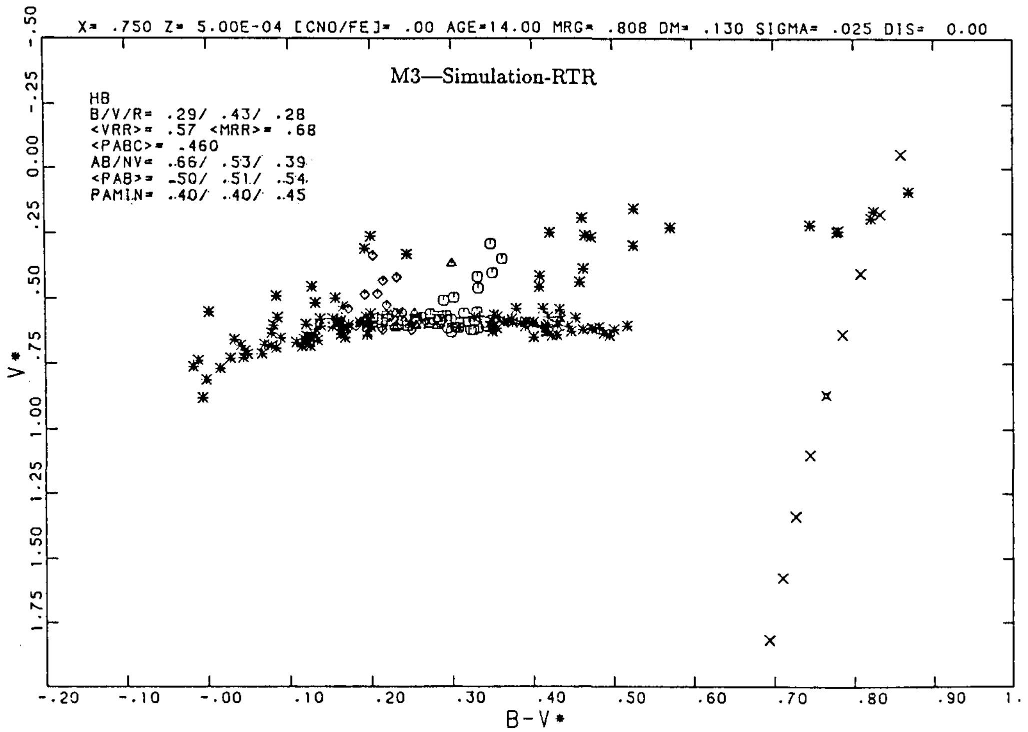

Fig. 1.—Simulation for M3 using the Rood (1973) fits to evolutionary tracks.

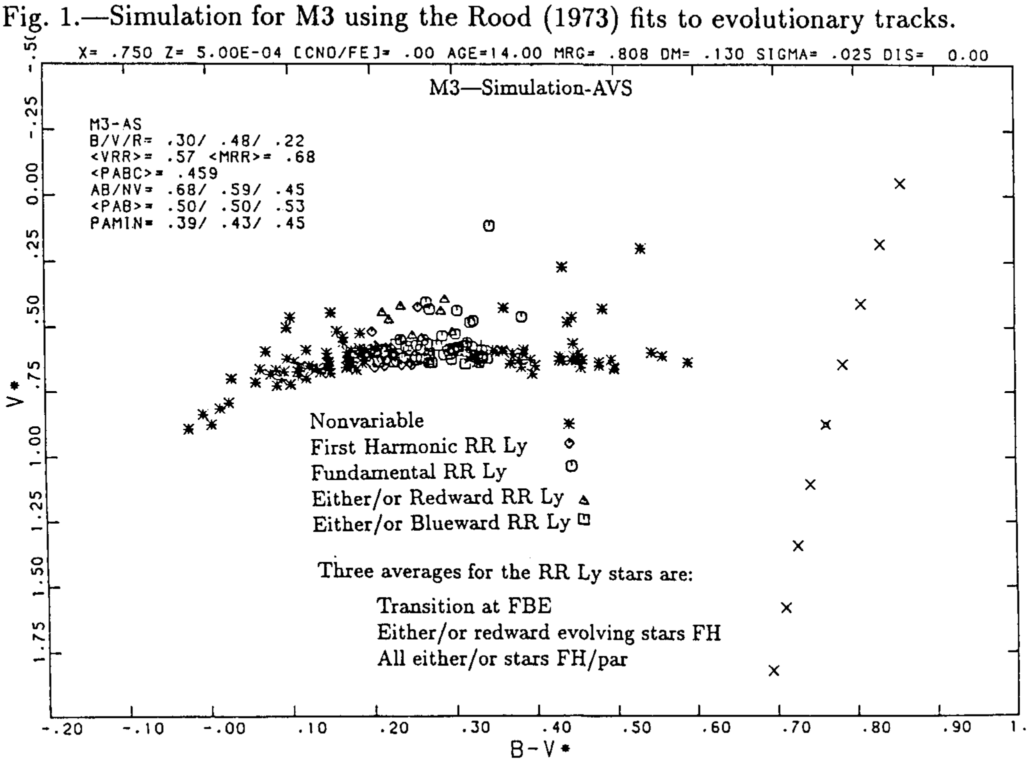

Fig. 2.—Simulation for M3 using Sweigart (1987) tracks

evolutionary sequence like the RGB. I.e., something besides age varies along the HB. Otherwise the HB would have "sharp" ends rather than the observed trickle out. The true skeptic should bear these points in mind.

Because the HB is a superposition of evolutionary tracks most of the details of the tracks are lost. For this reason we have not worried too much about our representation of the evolution away from the ZAHB. To illustrate this point we compare a simulation for M3 using the old Rood (1973) approximation (Figure 1) to a fit (admittedly crude) to the much newer Sweigart (1987) tracks (Figure 2. (Note that an "*" on the axis labels as in $B - V*$ denotes that " observational errors" have been added.) As expected, the results are indistinguishable. We anticipate the same for the ongoing calculations of Van den Berg which use updated input physics. The observed HB for M3 (Buonanno *et al.* 1988) is shown in Figure 3. The agreement is fairly good except for the observed blue extension. The adopted mass distribution, which is purely *ad hoc*, could easily be fudged to give such a tail in the simulations.

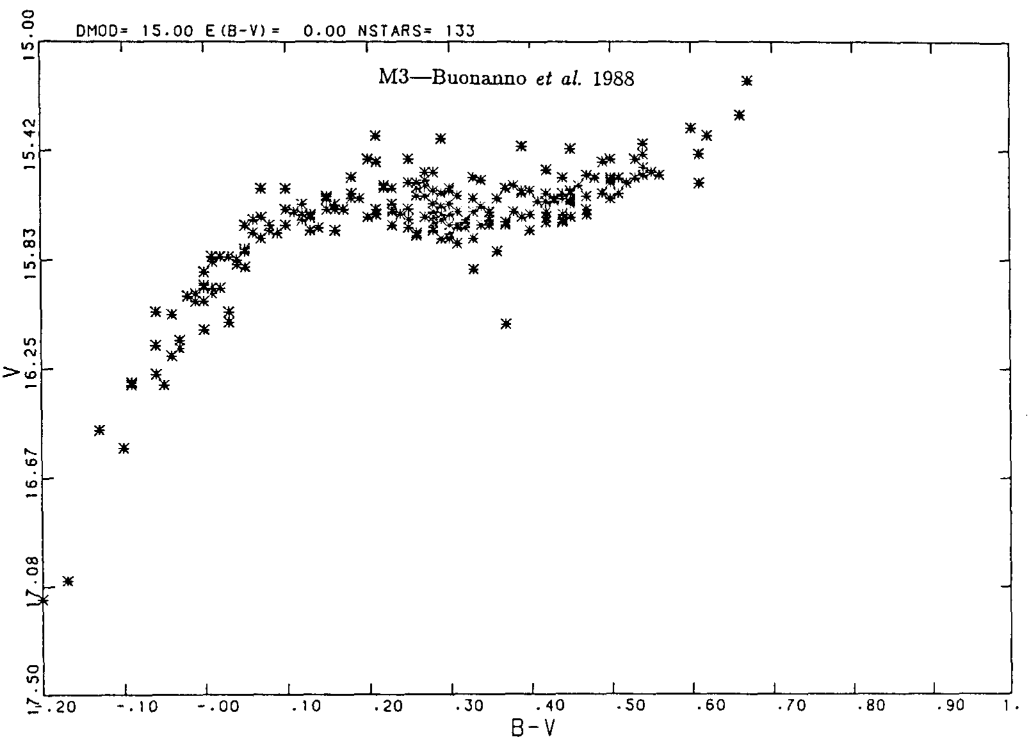

Fig 3.—M3 HB for stars with $B - V \geq -0.20$ as observed by Buonanno *et al.* 1988.

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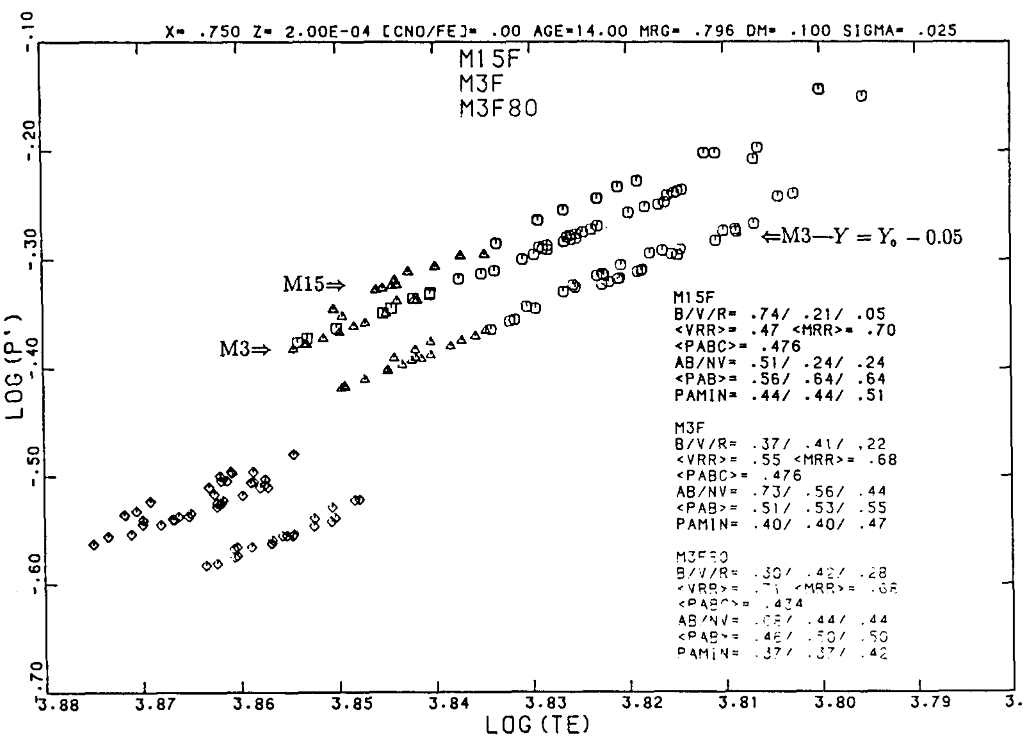

Fig. 4.—Model period shifts for a standard M3, M15, and helium depleted M3.

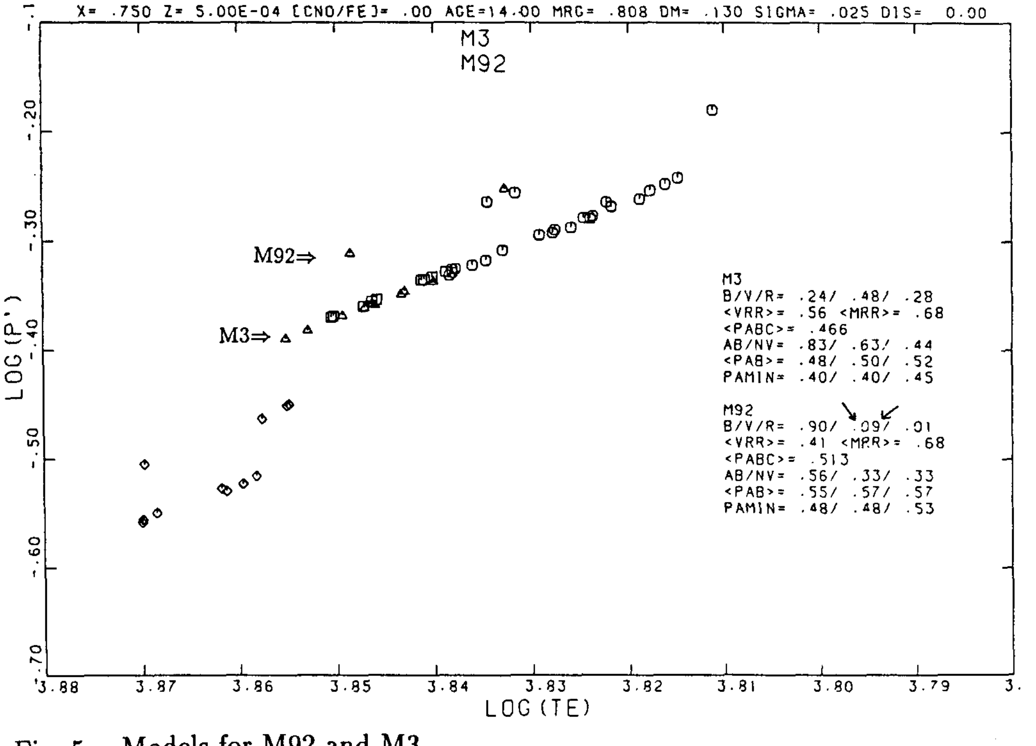

Fig. 5.—Models for M92 and M3.

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One particular improvement we can point to in our understanding of the standard model is the solid evidence that has been obtained on HB lifetimes and thus the degree of mixing during core helium burning. Buzzoni *et al.* (1983) and Buonnano *et al.* (1985) use the ratio of asymptotic giant branch stars to HB stars to show that the standard treatment of overshooting and semiconvection lead to about the correct amount of mixing.

3 *THE OOSTERHOFF EFFECT OR SANDAGE PERIOD SHIFT*

In the early 1980's Sandage first argued that the periods of RR Lyraes decreased as cluster metalicity increased. In particular, he noted "... at **every temperature** the shifts exist ..." (emphasis ours) The shift was most prominent in the log P'-log $T_{\rm eff}$ diagram. The use of log P' [$= \log P + 0.336(m_{\rm bol} - \langle m_{\rm bol}\rangle)$] removes much of the scatter due to the intrinsic dispersion of luminosity among the variables. This period shift was much larger than predicted by the standard models. The only explanation Sandage could find was an **anti**correlation of helium with metalicity. This was a most unpalatable explanation (we think for Sandage as well). It was, however, most important because it showed that the difference between the periods between the Oosterhoff classes was due to some systematic difference between the clusters. The Oosterhoff dichotomy was not present just in the average properties. It could not be blamed on some quirky distribution or on hysteresis.

While the case for the shift was fairly convincing, there was some cause for concern. In particular, note that in Fig. 11 of Sandage (1982), for M15 the slope in the (log P-log $T_{\rm eff}$)-diagram is much less than that for M3 which agrees quite well with theory. Now if there is any thing we should get right, its this slope. Hence, we from the outset were suspicious of the "observed" temperatures for M15. (Sandage addresses this point in his paper in this volume.) After much maneuvering with many parameters (Rood 1983; Rood and Crocker 1985a) we reached the same conclusion as Sandage—only the anticorrelation of Y and Z worked as shown in Figure 4. A similar conclusion was reached in a far more extensive study by Sweigart, Renzini, and Tormambè (1987). As a simple explanation was not forthcoming, we would have blamed the observations were it not for the paper of Bingham *et al.* (1984) which presented a convincing case that the shift exists between M15 and M3. Since that time we have considered the observational basis of the Sandage period shift to lie primarily in the M15, M3 pair.

Recently Lee, Zinn, and Demarque (1987, 1988) have claimed to have explained the Sandage shift. Lee *et al.* argue that their result arises, in part, from the inclusion of HB evolution all the way to central helium exhaustion and the use of an unusually fine grid of models. We believe these are not important new additions. Instead, their results arise partially from extreme parameter values which exaggerate the theoretical shift (variable strip wider than standard), and partially from "measuring" the shift a single (too cool) temperature in the log P-log $T_{\rm eff}$ plane rather than shift at *every temperature* found by Sandage and Bingham *et al.* in the log P'-log $T_{\rm eff}$ plane. Further, by their own admission, they cannot account for the crucial M15, M3 pair.

Lee *et al.* do indeed find a significant shift between M92 and M3. We have never had any difficulty with M92 either as shown in Figure 5. We never considered this significant because of the small number of variables in M92. More than 90% of the M92 HB is blueward of the variable strip. For such a cluster **all** of the RR Lyrae and red HB can be near helium core exhaustion. In Figure 6 we show log L as a

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function of time. Roughly 10% of the HB life is spent at large enough log L to lead to δ log $P' \sim 0.04 - 0.07$. If this works for M92, could "improved" tracks work with M15? Since only about 70% of M15 is blue HB, roughly 30% of the HB lifetime would have to be spent $\delta \log L \sim 0.08$ or ~ 0.2 mag "above" the ZAHB. These stars would not be confined to the variable strip in M15; in other clusters, like M3, they would lie above the HB. There is no evidence for such a large number of pre-exhaustion stars in any metal poor cluster.

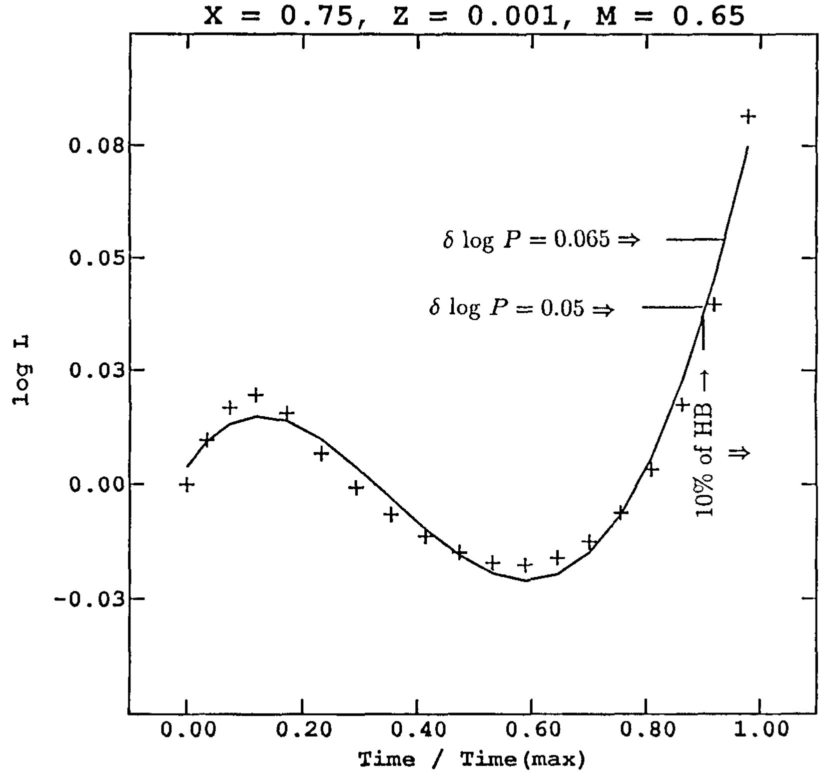

Figure 6. Sweigart's evolution of log L vs. time (crosses) extrapolated to $Y_c = 0$ and our fit (line).

In part, the Lee *et al.* "explanation," as well as that of Caputo (1988), rests on the absence of a shift in ω Cen and its implications for the amplitude and rise time vs. temperature diagrams. Basically, could part of the "shift" be a temperature shift rather than a period shift? Dickens, elsewhere in this volume, presents evidence concerning the role of ω Cen. (He also notes that ω Cen is also more than 90% blue HB. It may well be that ω Cen is like M92—"no problem." Indeed, the standard theory may predict no shift within the cluster.)

We would like here to provide some additional information (confusion?) on the matter of the temperatures of the RR Lyrae. It has always been possible to interpret the shift

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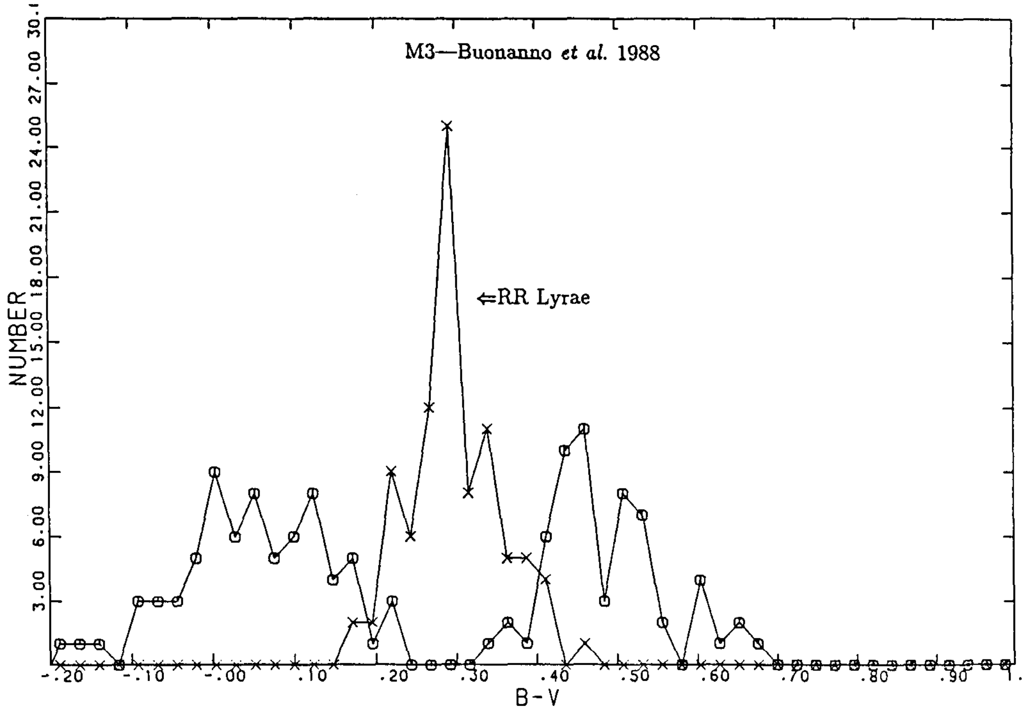

Fig. 7.—The observed distribution in color of the HB stars for M3.

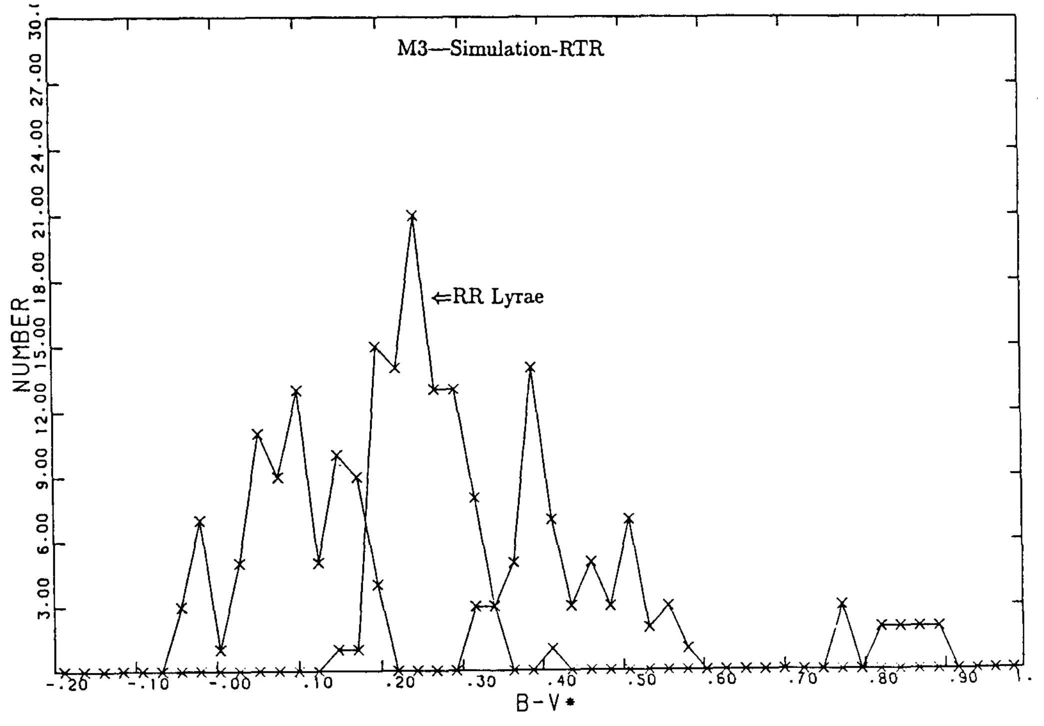

Fig. 8.—Simulation of the color distribution in M3.

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between M15 and M3 *either* as a period shift, $\delta \log P' \approx -0.065$, *or* a temperature shift, $\delta \log T_{\rm eff} \approx -0.02$ ($\delta(B-V) \approx 0.06$). Such an interpretation is not without difficulties, but considering the intractability of the period shift, we feel that it deserves some consideration. Indeed, there is a hint of difficulty with the temperatures which can be inferred from our models.

When comparing HB simulations with the observations, the pertinent temperatures, periods, etc. are those the star would have if it were not pulsating. Unfortunately the observers cannot stop the stars from pulsating, but must use some sort of averaging. We always have been uneasy with this process. In particular, it seems that zero point jumps in "$B-V$" might enter at the boundaries of the variable strip and between the fundamental and first harmonic. There could easily be amplitude related factors (again, see Sandage in this volume). We have often been told that there is no overlap between the variables and non variables. The largest complete sample to quantify such a statement is that of Buonanno *et al.* for M3. The color histogram (Figure 7) shows virtually no overlap. In addition there is no overlap between the RRab's and RRc's for M3 (Bingham *et al.*, 1984). Even if these boundaries are "theoretically sharp," our simulations show that there should be significant overlap observed. Our simulation for M3 is shown in Figure 8. There is significantly more overlap, particularly at the blue edge, than observed. We have taken $\sigma(B-V) = 0.02$, the samples are the same size, and the BHB/RR/RHB ratio is about that observed. Likewise, there is significant overlap between the RRab and RRc in the simulated ($\log P'$, $\log T_{\rm eff}$)-diagram. Although we doubt that the results are statistically significant we feel there is a strong hint at a jump in the temperature scale at both the BHB/RRc boundary and RRc/RRab boundary. These are rather nitpicking points which have until this time been quite justifyably neglected by the observers, but they are of the magnitude that they could be important for the Sandage Effect.

It is possible that assorted temperature errors could explain, or at least make more manageable, the Sandage Effect problem. Before one rejoices too much at this possibility, one should recall that this will still leave us with the Oosterhoff problem almost as it existed in 1973. The difference between the mean periods of Oo I and Oo II clusters will be without explanation. The one definite bit of progress is that the fraction of RR Lyrae in the fundamental mode follows quite naturally from nonuniform distribution of stars along the HB. The blue HBs of Oo II clusters give about 50% RRc so long as the transition takes near the fundamental blue edge. At least hysteresis and a wide either/or strip seemed to have been ruled out.

We should also mention two other failures of the standard theory as it applies to the RR Lyrae. Alert to the possibility of systematic temperature errors, one might be concerned by the extremely peaked observed color distribution for M3 shown in Fig. 7. Could this be an indication of an amplitude related error in averaging $B-V$? Suggesting otherwise, is the period distribution as shown from the catalog of Cacciari and Renzini (1976) in Figure 9. There is a pronounced peak at the blue edge of the RRab's. This cannot be due to some sort of averaging error—the stars know their true colors even if we don't. There appears to be a real pile-up at this point. The same is typical for other clusters. Simulations, as shown in Figure 9 on the right, are always much flatter. If simulations could produce peaked distributions, in other than

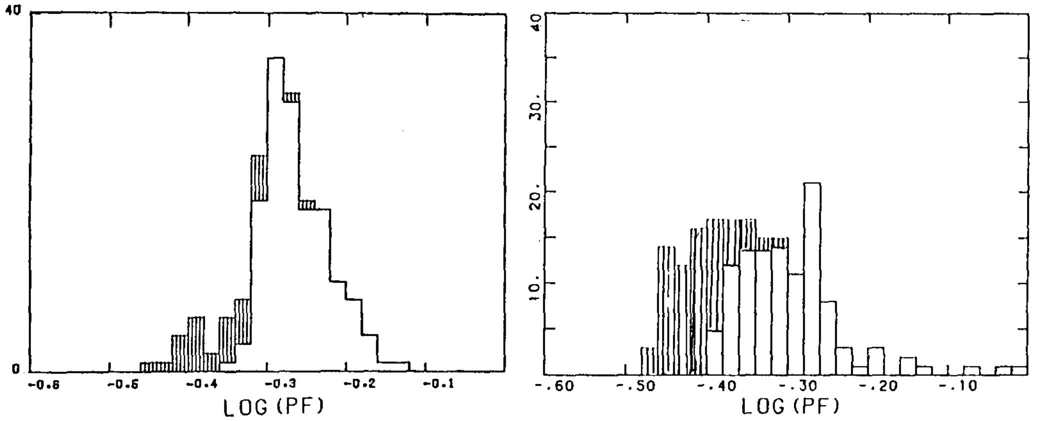

Fig. 7.—On the left is the period distribution for M3 from the catalog of Cacciari and Renzini (1976). On the right is a simulated period distribution for M3.

a purely *ad hoc* way, there could be some relevance for the mean properties of the RR Lyrae and thus the old Oosterhoff problem.

In discussions at this meeting Art Cox and Jim Nemec have again pointed out that the masses derived for double mode RR Lyrae in Oo I and Oo II clusters differ significantly. Again the standard model fails, predicting essentially equal masses. Even the $\Delta Y \propto -\Delta Z$ solution gives essentially the same masses for the two groups. We may well not have given adequate consideration to this point. Perhaps the solution when it is found will give the correct mass differences.

4 *THE SECOND PARAMETER PROBLEM—1988*

For some time the second parameter problem in its most obvious form—the distribution along the HB—has been caught in the quagmire of mass loss. Variations in cluster age, Y, and [CNO/Fe] affect the HB directly. The HB is indirectly affected by anything which can affect mass loss. Rotation delays He flash increasing the duration of the strongest winds; rotation could determine the size of magnetic fields and thus the magnitude of MHD winds. Magnetic fields might vary in some way independent of rotation. The list has only been limited by our imaginations. Various pieces of observational evidence at times seem to favor one or the other. Yet no convincing identification has been made. At the very least, to approach the problem in a brute force way, one has to know how mass loss depends on [Fe/H] to at least two significant figures. In reality perhaps even the sign of this variation is uncertain. One can, of course, parameterize things, e.g., making η_R a function of [Fe/H], but things rapidly degenerate into parameter twiddling. Recently our interest has been directed toward clusters with bimodal HBs (Rood and Crocker 1985b; Crocker and Rood 1985; Crocker, Rood, and O'Connell 1988, hereafter CRO).

The HB of NGC 6752 was rather a shock when it was first shown by Cannon and Lee at a Frascati workshop in 1973. It wasn't horizontal, but rather a blue droop stretching down to magnitudes as faint as the turnoff. Even worse—right in the middle was a big ugly gap. Since that time many other clusters have been found with HBs with gaps or somewhat less pronounced bimodal distribution. Even such familiar clusters as M15 have turned out to have a gap in the BHB. Suggestions of less extreme bimodality are commonplace. We have adopted as a working hypothesis, that bimodal HBs are a special case of, or extreme example of, the second parameter problem. The bimodality could be the result of some second parameter operating within a single cluster. In some cases like NGC 6752, this parameter might be taking on an extreme value and thus be easier to identify.

One must be careful in defining bimodality. The distribution in $B-V$ is inappropriate. An clump is produced in the blue as $B-V$ saturates as a temperature measure. A clump is produced in the red HB as the mass dependence of ZAHB $\log T_{\rm eff}$ decreases with increasing mass. Neither of these clumps are in some sense "real." We feel HB structure is best examined by "straightening out" the HB. To do so we define one coordinate X_{HB} measured "along" the HB and another Y_{HB} perpendicular to the HB (Figure 10). The (X_{HB} , Y_{HB})-diagram depends on many factors both observational and theoretical—the photometry, $E(B-V)$, $(m-M)_V$, the theoretical ZAHB and thus assumed composition, the color temperature relation and bolometric corrections, etc. Despite the complexity things are not as bad as one might fear. Some things are not particularly important, e.g., the assumed composition of the ZAHB. Others produce characteristic and easily recognized distortions. E.g., based on a downward bend at the red end of the HB (Figure 11), we conclude that for M92 $(m-M)_V = 14.8$ rather than the 14.5 reported by Harris and Racine (1979). This is in accord with our discussion above concerning the period shift for M92. Other uncertainties are reduced by considering only differential studies, i.e., one cluster as compared to another.

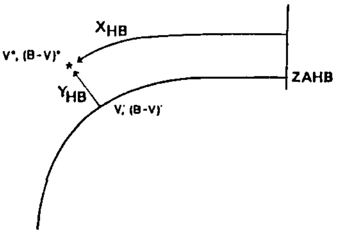

Fig. 10.—Definition of X_{HB} and Y_{HB} .

Because X_{HB} is defined for each star relative to the ZAHB it leads directly to a "temperature" $\log T_{\rm eff}(X_{HB})$ and "mass" $M(X_{HB})$. $M(X_{HB})$($MASSD$ in the figures) differs slightly from the true mass because of evolution away from the ZAHB. As shown in Figure 12 the difference between the "real" and $M(X_{HB})$ distributions in a simulation for M3 is small. The observed $M(X_{HB})$ distribution for M3 is shown in Figure 13. One is able to define HB morphology in a much more quantitative way using the $M(X_{HB})$ distribution. The bimodality of clusters such as NGC 6752 and M15 stands out (CRO). Bimodality appears in less obvious cases like M92. Clusters, such as M5, which superficially appear bimodal in $B-V$, prove to unimodal.

The distribution in $M(X_{HB})$ will of course be just as subject to the problems of mass loss as is the morpholgy in the color-magnitude diagram. *Ad hoc* bimodal distributions in your favorite second parameter will produce bimodal $M(X_{HB})$ distributions. Maybe thats all there is to clusters like NGC 6752. Nature has provided precedence for such a solution in dromedaries and Bactrian camels. We hope this isn't the case—we certainly don't need more free parameters.

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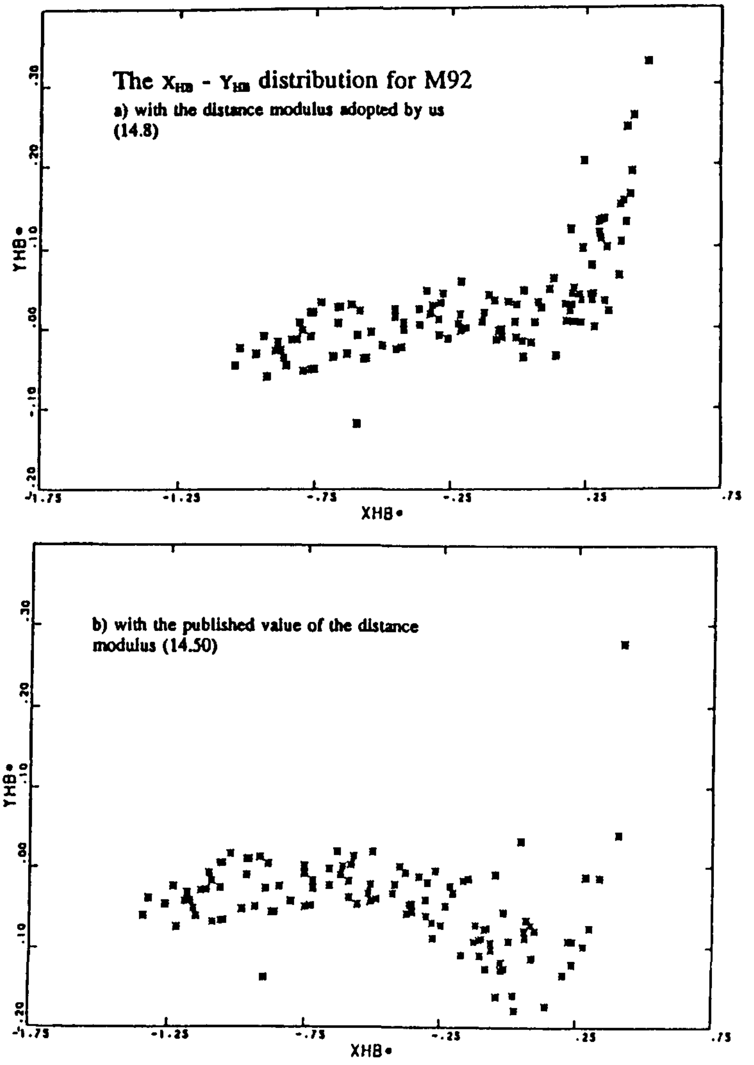

Fig. 11.—The (X_{HB} , Y_{HB})-diagrams for M92 with different assumptions for $(m - M)_V$.

The solution we suspect lies in another direction—Y_{HB} . The distribution in Y_{HB} should be much less affected by the details of mass loss. One requires quite good photometry to do this in the (X_{HB} , Y_{HB})-diagram. A simulation using Sweigart's new tracks is show in Figure 14. The real data is shown in Figure 15. We have not analyzed this result in detail, but there is a suggestion that the simulation errors ($\sigma(V) = 0.01$; $\sigma(B - V) = 0.02$ are too small in particular for the variables. However the general agreement is not bad. It is quite obvious that far less than 30% of the HB

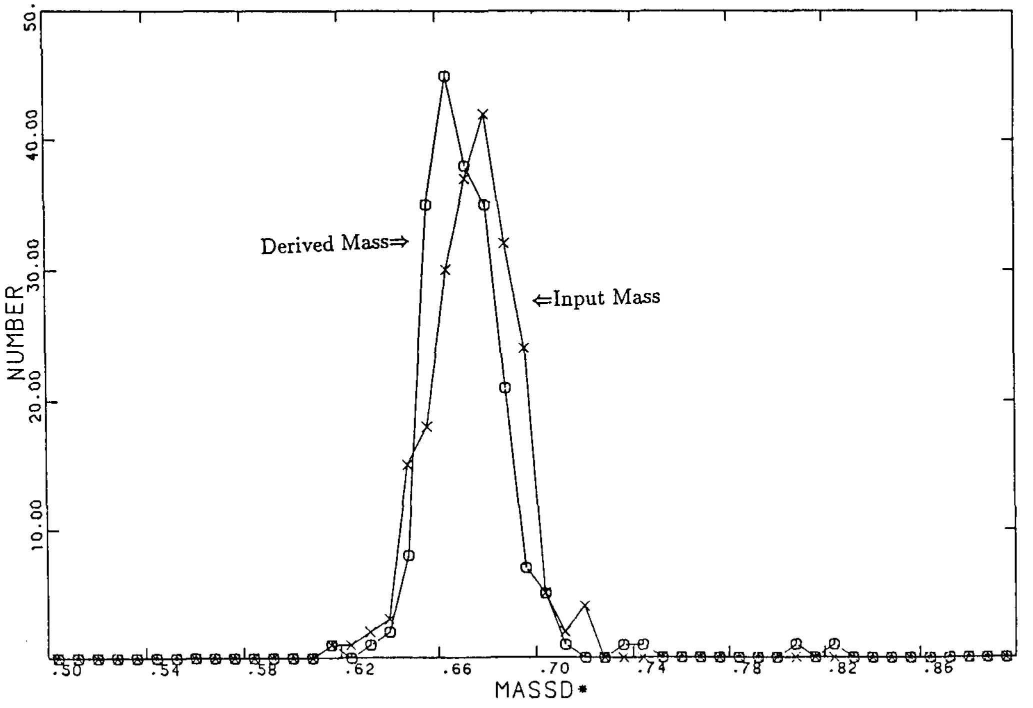

Fig. 12.—Distribution in $M(X_{HB})$ as compared to input mass distribution.

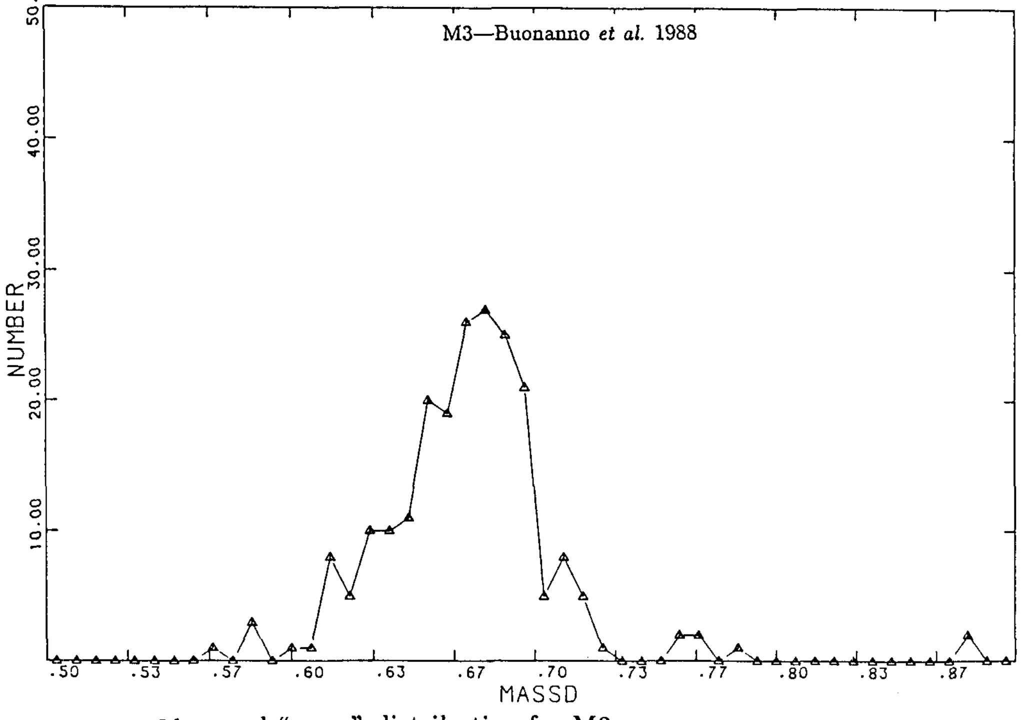

Fig. 13.—Observed "mass" distribution for M3.

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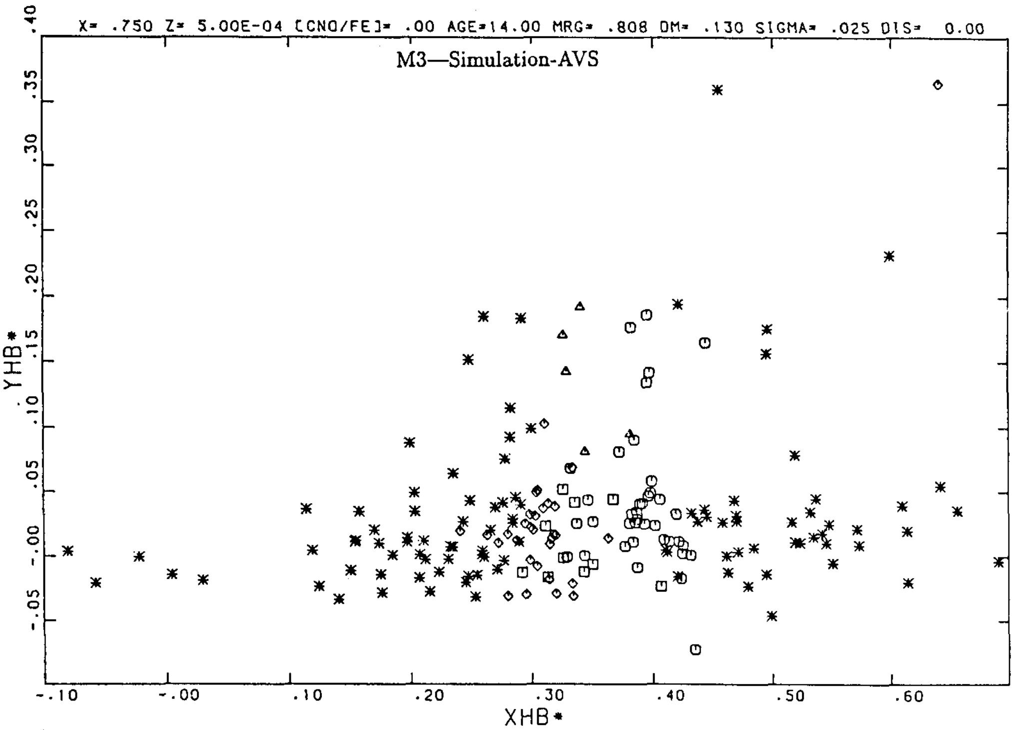

Fig. 14.—Simulated (X_{HB} , Y_{HB})-diagram for M3.

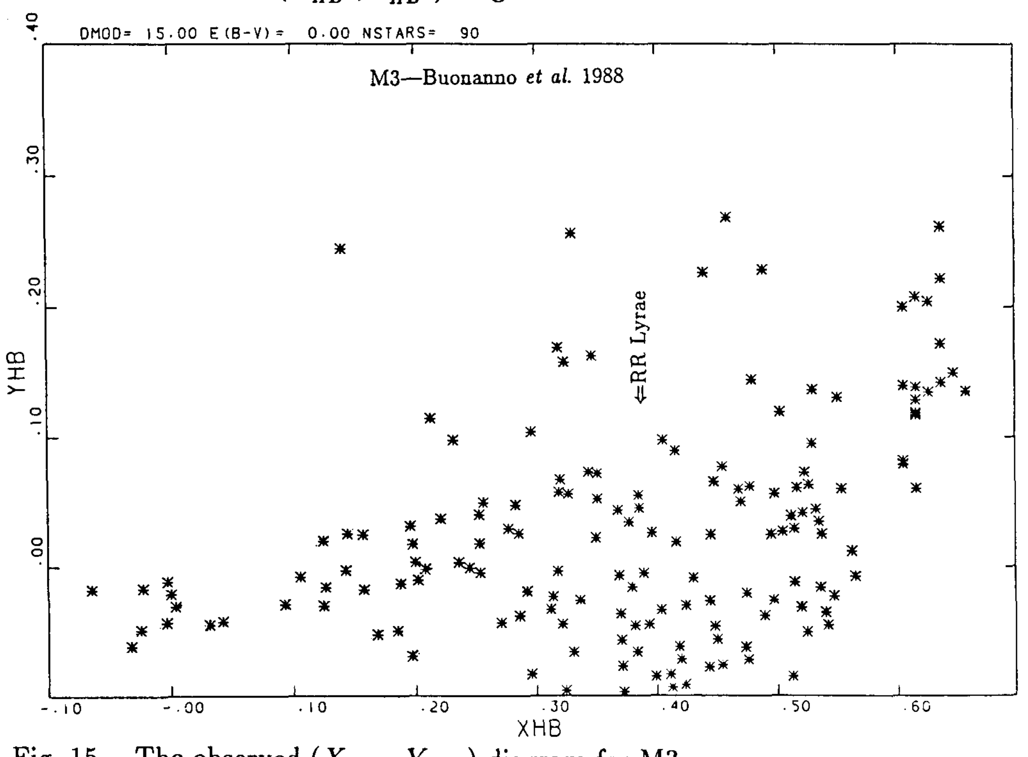

Fig. 15.—The observed (X_{HB} , Y_{HB})-diagram for M3.

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lies at $Y_{HB} \gtrsim 0.20$, which would be the case if the solution of the M15, M3 Sandage shift were the inclusion of the helium core exhaustion phase of the HB.

To this point our investigation of the Y_{HB} distribution has been primarily confined to the blue HB stars in the (log g, log $T_{\rm eff}$)-diagrams of gap clusters. The possibilities we were searching for are summarized in Figure 16. Our results which have just appeared in CRO do not appear to be consistent with any of our hypotheses. Qualitatively the stars blueward of the gaps are displaced as they would be if rapidly rotating. However, the displacement appears much larger than we would expect. Such nice solutions as [O/Fe] (Rood and Crocker 1985b) appear to be ruled out.

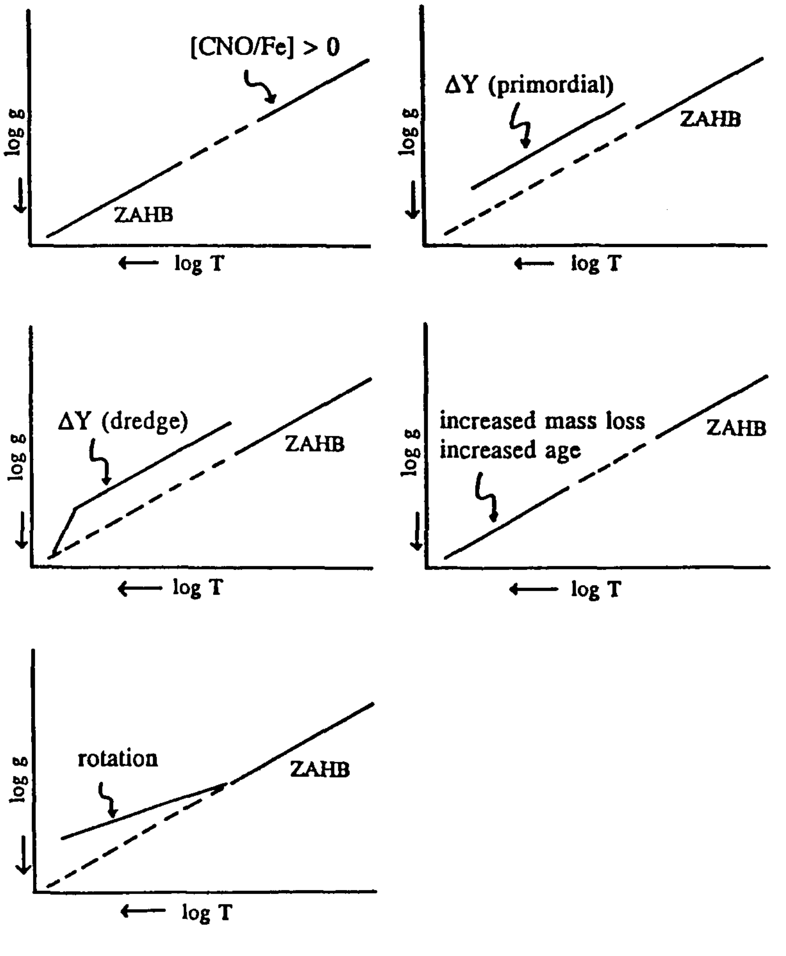

Figure 16. (log g, log $T_{\rm eff}$)-diagrams for assorted "solutions" to BHB gap clusters.

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The first detailed investigation of Y_{HB} distributions was, in fact, by Sandage—the period shift can be mapped into Y_{HB}. The ($\log P$, $\log T_{\rm eff}$)-diagram is equivalent to the ($\log g$, $\log T_{\rm eff}$)-diagram. The expected shift is $\delta \log g \approx 1.19\delta \log P$. The observations in CRO were designed to detect this "Sandage Shift" in the nonvariable stars. Unfortunately, what appears to be an intrinsic dispersion in our results hides any shift if present.

5 *CONCLUSIONS*

The main difficulties facing the theory of HB stars are the second parameter problem and the Sandage Period Shift. There appears to be a slight chance that the Sandage Period Shift will "go away," becoming a temperature shift. Then we are still left with the Oosterhoff Effect and we are back to 1973 (theoretically). We suspect that the solutions to these problems will arise from new kinds of observations and new ways of "looking at" the old. The observational status of HB stars is much better than in 1973 and promises in many ways to get much better. Unfortunately, some kinds of crucial observations do not appear to be stylish. It sure would be nice to have some more old fashioned RR Lyrae work like that of Bingham *et al.* (1984) for M15 for **large** samples in other clusters.

Its hard to know whether to be optimistic or not. Still we must keep chipping away. The solutions to these problems have interest far beyond those of the globular cluster and variable star aficionados and stellar structure theorists who wish to check off another obscure success. In understanding these stars we have probably our most direct evidence of what was going on at the formation of the Milky Way and one ofour most important probes of the early universe.

We wish to thank Flavio Fusio Pecci for providing the data M3 data in machine readable form.

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