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THE PERIOD-LUMINOSITY RELATION: A HISTORICAL REVIEW *

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In some ways astronomers are like small children. The high adventure, the furor and excitement they create among themselves is not greatly cared about or understood outside, at least in any immediate way. Of all the discoveries of ground-based astronomy in this century, only the smallest handful can claim to have made the front-page headlines of the world's newspapers. One such instance came in 1952, when the headlines cried the news that the universe was at least twice as big as hitherto believed. To astronomers, the event signaled a turning point in the history of the cepheid period-luminosity relation. It was the revelation of a major error which had gone undetected in forty years' work on this subject. The story is an interesting one, in that it illuminates many facets of the scientist at work, persistently groping his way through research via misapprehensions and downright mistakes.

The First Four Decades

The current fashion for northern astronomers to establish observing stations in South America is not new. At the turn of the century at least two major American observatories maintained South American sites. One of these observatories was Harvard, which operated a 24-inch photographic refractor at Arequipa in Peru. Perhaps the most delectable of the fruits to be harvested by this instrument were contained on direct plates of the Small Magellanic Cloud (SMC). In 1908 Miss Henrietta Leavitt, having examined plates taken be-

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tween 1893 and 1906, produced a catalog of 1777 variable stars in the SMC (Leavitt 1908). Of these, 16 appeared on a sufficient number of plates for their periods to be determined. These ranged from $1^{\rm d}.25$ to $127^{\rm d}$, and Miss Leavitt observed, "It is worthy of notice that . . . the brighter variables have the longer periods." Four years later she had sufficient material to increase the number of known periods to 25, and it was in this paper (Pickering 1912) that she gave the first precise formulation to what later became known as the period-luminosity relation, viz., that the apparent magnitudes of these stars decreased almost linearly with the logarithms of their periods. Miss Leavitt is sometimes unjustly accused of not having appreciated the significance of her discovery, but she says in her paper, "Since the variables are probably at nearly the same distance from the Earth, their periods are apparently associated with their actual emission of light." She went on to note the similarity of the light curves to those of some galactic variables, and added, "It is to be hoped, also, that the parallaxes of some variables of this type may be measured." Miss Leavitt, however, was mainly occupied with determining magnitudes and periods and up to the time of her death in 1921, she does not seem to have returned to this larger problem.

Ejnar Hertzsprung was the first to return to this problem. He had already concluded (Hertzsprung 1907) that the variables like δ Cephei were probably stars of high luminosity, and six years later (Hertzsprung 1913) he carried out a statistical parallax analysis on the 13 cepheids for which proper motions were available. His finding was that cepheids of period $6^{\rm d}.6$ were $7^{\rm m}.0 \pm 0^{\rm m}.5$ brighter than the sun in the visual. In order to use Miss Leavitt's results, which were in photographic magnitudes, he assumed a color index of $1^{\rm m}.5$ for all cepheids. Thus, with his zero point and Miss Leavitt's slope, Hertzsprung arrived at a period-luminosity relation which was in effect

$$\langle M_V \rangle = -0.6 - 2.1 \log P \quad .$$

Apart from being the first calibration of the *P-L* relation, Hertzsprung's paper was noteworthy in several respects. In it the word "cepheid" was used generically for the first time; and, significant in the light of later events, Hertzsprung noted that probably the RR Lyrae stars should not be considered as belonging to the cepheid class. On the other hand, his paper may also have set the trend for ignoring the possible effects of interstellar absorption, although the existence of this had already been postulated. Finally, this paper contained the first real determination of an extragalactic distance. Hertzsprung applied his *P-L* relation to Miss Leavitt's results to derive a distance for the SMC, which unfortunately is either miscalculated or misprinted in the paper as 3000 light-years instead of 30,000 light-years. This, together with the then unknown size of our own Galaxy, may have been why this result never received the acclaim it deserved.

Today it is generally overlooked that Henry Norris Russell was also among the first to carry out an absolute magnitude determination of cepheids (Russell 1913). It was a statistical parallax analysis like Hertzsprung's, but Russell gave few details and did not incorporate Miss Leavitt's work. Russell did, however, write a paper with Shapley (Russell and Shapley 1914), which, had Shapley later followed it up, would have had a vast effect on the subsequent history of the *P-L* relation. It was a paper concerned with the galactic distribution of eclipsing variables and cepheids, and it came to two significant conclusions: (a) that King's suggestion (King 1914) that there exists interstellar absorption of about 2 visual magnitudes per kiloparsec was correct, and (b) that probably the RR Lyrae stars (I use the modern term) were not generically related to the cepheids. Shapley noted in a later paper, however, that Russell had actually written this joint paper, and it seems that Shapley did not altogether agree with the conclusions, for he was later to ignore them.

It is with Shapley's name, of course, that the development of the *P-L* relation is usually associated. His first important paper on the subject came in 1918, and since this laid the groundwork of the matter for the next 40 years, it is worth paying some attention to it. Shapley was aware of Hertzsprung's work, and, indeed, made use of the same basic data in much the same way (Shapley 1918). From those 13 stars, however, he eliminated κ Pavonis and l Carinae on the grounds that they were atypical, especially κ Pav! It is important to note that the remaining eleven stars were (in modern terminology) Population I cepheids. The result that he obtained was very similar to Hertzsprung's, viz., an average absolute visual magnitude of -2.35 ± 0.19 at a period of $5^{\mathrm{d}}.96$. Today we would assign an absolute magnitude of -3.7 to a cepheid of this period; so the famous discrepancy of nearly $1^{\mathrm{m}}.5$ was present right from the earliest calibrations, and at least initially had nothing to do with the incorporation of Population II stars, as is often stated in textbooks! What were the reasons for this error? We can list at least three: (a) neglect of interstellar absorption, (b) poor and insufficient data, and (c) the then unknown effects of galactic rotation on the proper motions and radial velocities. In later work the first and last of these became more serious, since increasing the number of stars generally meant using fainter and more distant objects. Galactic rotation effects on proper motion are independent of distance, but the parallactic motion falls off inversely with distance, so eventually the galactic rotation component becomes dominant and a statistical parallax is then meaningless.

In order to trace the various contributions to this $1^{\mathrm{m}}.4$ discrepancy I have repeated the statistical parallax analysis of these eleven stars using modern data. I find that about $0^{\mathrm{m}}.7$ was due to neglect of interstellar absorption, $0^{\mathrm{m}}.1$ or $0^{\mathrm{m}}.2$ due to the galactic rotation effect, and most of the remainder due to systematic errors in the proper motions. In particular, the incorporation of Polaris (the derived proper motion of which is very sensitive to precession corrections) had a rather large effect on the result. In addition there are inherent difficulties, such as how best to take the average of the periods of the eleven stars. A logarithmic average would have given a smaller average period and therefore reduced the discrepancy.

Having established a zero point, Shapley then went on to show that the *P-L* effect exists among galactic cepheids. He did this by assigning zero peculiar motion to each of his eleven stars, i.e., by assuming that all of the observed motion was a reflection of the solar motion. This gave him a parallax for each star, which showed that the longer period stars were more luminous than those of shorter period; but the results were too crude to give a meaningful slope to the *P-L* relation. This Shapley arrived at by using Miss Leavitt's results, changing her photographic magnitudes to visual magnitudes by use of a period-color relation of the form

$$\text{C.I.} = -0.55 + 1.5 \log P \quad .$$

It was after this stage, however, that Shapley made what was to be the fateful step of incorporating the variables in globular clusters. He noted that in ω Centauri, M 5, and M 3 there were long-period cepheids as well as the usual RR Lyrae stars, and that these longer period stars, together with the RR Lyrae stars, obeyed a *P-L* relation whose slope was very similar to that obtained via the SMC cepheids. It therefore seemed entirely natural to fit those longer period stars onto the *P-L* curve he had already obtained, and thereby extend the relation to include the RR Lyrae stars. We now know, of course, that these Population II cepheids are about $1^{m}.5$ fainter than their Population I counterparts, but by one of those unlikely coincidences that sometimes occur, this error was almost exactly compensated for by the original $1^{m}.4$ error in Shapley's statistical parallax result. Thus the RR Lyrae stars came out to have absolute magnitudes close to zero, not far from where we would place them today; and when later checks on the field RR Lyrae stars gave similar results, there came to be a growing confidence in Shapley's calibration.

A year later Shapley (1919) strengthened his calibration by approaching it in a semitheoretical way. The well-known $P\sqrt{\rho}$ relation is a functional relation between period, mass, and radius. The mass can be replaced by the luminosity through the mass-luminosity relation, and the radius by luminosity and surface temperature through the $L \sim R^2 T^4$ relation, thus giving a period-luminosity-color relation. The detailed functions available to Shapley gave a result reasonably close to his empirical *P-L* relation. He was later (Shapley 1927) to revise this slightly, and in that same year Russell (1927) returned to the cepheid problem by independently doing the same thing. As always, his paper is astonishing in its perspicacity. He anticipated much of the discussion of 35 years later regarding the inconstancy of the $P\sqrt{\rho}$ 'constant,' and showed that the RR Lyrae stars must differ radically from cepheids in some way, presumably in mass. This semitheoretical approach was also later used by Parenago (1940), and the importance of the color term which results from it was finally popularized by Sandage (1958).

Despite Shapley's seemingly consistent results, the sailing was by no means smooth. Kapteyn and van Rhijn (1922), ironically attacking the correct part of Shapley's results, pointed out that the field RR Lyrae stars are found distributed all over the sky and have large proper motions, which almost always implies nearby objects. Since they are also faint, they must therefore have intrinsically low luminosities, and could not be giants as Shapley's results implied. Using very provisional proper motions for a few RR Lyrae stars, Kapteyn and van Rhijn arrived at a mean parallax almost eight times larger than implied by Shapley's luminosities. In retrospect their result seems to have been due mainly to poor data, and quite possibly to incorrect values of the solar motion and apex (which they do not state). In connection with this problem Luyten (1922) pointed out the sensitivity of the results to the adopted value of the solar apex.

In a rebuttal to Kapteyn and van Rhijn, Shapley (1922) announced what now seems a curious result: the finding in the SMC of 13 RR Lyrae stars of just the right apparent magnitude as required by his *P-L* relation. In fact, the true RR Lyrae variables in the Cloud are several magnitudes fainter and were not to be found for another 30 years. Later work (Payne-Gaposchkin and Gaposchkin 1966) has shown that in every one of these cases the presumed period was spurious. Shapley himself did admit that this might be the case for a few.

Ralph Wilson (1923) attempted to improve the situation by providing more extensive basic data. In an analysis of the proper motions and radial velocities of 74 cepheids and ten RR Lyrae stars, he came to the conclusion that Shapley's zero-point should perhaps be about $0^{m}5$ *fainter*, but the results were not very precise. This was largely due to the increasing effects of the neglected interstellar absorption when incorporating more distant stars. Again, with hindsight, we can see the effect of another unfortunate coincidence which was to bedevil this and several subsequent analyses. Due to the first coincidence described above, Shapley's original calibration was just about right for Population II stars. When Wilson and others came to do statistical parallax analyses on field RR Lyrae stars, their neglect of interstellar absorption corrections mattered little because most of the RR Lyrae stars are at high galactic latitudes. But in the case of the cepheids, which are at low latitudes, the effect was large and gave results consistently too faint. Thus the overall effect was always to get results not very different from Shapley's.

In 1925 the *P-L* relation came to its full importance as the basis of the cosmological distance scale. For years arguments had raged over the nature of the spiral nebulae: Were they true full-fledged galaxies, or were they merely distant star clouds associated with our own Galaxy? A high point of the controversy had been the famous Shapley-Curtis debate in 1920, entertainingly described by Struve

(1960). The conclusive answer was provided on New Year's Day of 1925 in a paper by Edwin Hubble (read by Russell) to the American Astronomical Society meeting in Washington, D. C. It announced the discovery of cepheids in M 31 and M 33 which, when Shapley's *P-L* relation was applied to them, placed these systems at immense distances and showed them to be galaxies in their own right. The excitement of this AAS meeting is described in *Popular Astronomy*, (33, 158, 1925). The lengthy abstract of Hubble's paper is given elsewhere (Hubble 1925*a*). This paper and others (Hubble 1925*b*; 1926*a, b*; 1929) laid the groundwork for Hubble's remarkable researches in observational cosmology.

Shortly after this episode, and all the more surprising for it, a regrettable red herring came to be laid across the trail by Schilt (1926, 1928). On the principal basis of spectroscopic parallaxes and other more indirect arguments (in which he became the victim of the fact that the average period of cepheids is a function of galactic longitude), Schilt concluded that, in the Galaxy, the *P-L* relation applied only to cepheids of period less than ten days. Those of longer period not only failed to obey the *P-L* relation, but were all of much lower luminosity. It took Shapley and Miss Payne (1930), as well as Adams, Joy, and Humason (1929), some time to prove the fallacy of this, and it was still a matter for debate 20 years later (Melnikov 1947). In the main, it proved to be a simple selection effect.

If some of these controversies seem faintly ridiculous in the light of today's knowledge, one must bear in mind the theoretical background of the subject at that time. Despite strong arguments against it, the original binary hypothesis of cepheids was by no means dead (see, for example, Plummer 1920; Hagen 1921). Eddington (1926) was developing the pulsation theory, while Jeans (1928) was developing a theory based on a rotating liquid star nearing the point of fission. The fierce debates between these two over this subject (and others) became famous, and make entertaining reading in the volumes of *The Observatory* reporting the meetings of the Royal Astronomical Society during the 1920's. Even as late as 1949 we find Hoyle and Lyttleton (1943, 1949; Code 1949) ferociously denouncing the pulsation theory and instead advocating an ingenious theory based on binary components rotating inside a common envelope. A beautiful example of how one's beliefs can color one's conclusions was provided by Perrine. Perrine believed in the binary theory of cepheids (which required them to be small) and also admitted in a footnote that he found great difficulty in accepting that late-type supergiants and dwarfs, which would be so enormously different in size and luminosity, could possibly have such similar spectra. In a remarkable examination of the data he concluded (Perrine 1927) that cepheids were no more luminous than ordinary dwarfs, and that the SMC was therefore no more distant than about 1 kpc. When Doig (1927) remonstrated that this led to easily disproved consequences, Perrine remained quite unmoved, and a brisk correspondence on the matter ensued (Perrine 1928; Doig 1928). In the light of these and similar discussions, it is not surprising that the path to progress was by no means always clear.

New methods for checking the zero point of the P-L relation came to be developed. After his pioneering work on galactic rotation, Oort (1927) checked Shapley's distance scale by an inverse use of the equation

$$\text{Rad. vel.} = rA \sin 2(\ell - \ell_0) \quad ,$$

A being derived from other stars. Again no allowance for interstellar absorption was made, and Oort concluded that Shapley's distances were essentially correct. Luplau Janssen (1929), with the advent of Hubble's data on cepheids in external galaxies, suggested comparing the distance of NGC 224 derived from cepheids with that derived from novae, but there were insufficient data on the absolute magnitudes of novae to give any convincing result. Kipper (1931) attempted to derive absolute magnitudes of cepheids by a method similar to that later developed by Wesselink. He concluded that Shapley's zero point should be made fainter by about 1^m0, but again it is now easy to see the technical failings of his method (such as identifying the observed radial velocity with the star's surface velocity) which gave him this result.

A return to statistical parallaxes was made by Gerasimovic (1931), who provided fresh data for a new calculation. It is an interesting paper, since in it allowance is made for galactic rotation effects, and the term "classical cepheid" is introduced to distinguish ordinary cepheids from "atypical" cepheids such as W Virginis, RU Camelopardalis, and VX Cygni, which Gerasimovic specifically excluded from his calculation. (He had excluded RR Lyrae stars from the beginning.) His conclusion that Shapley's zero point should be made fainter by 1^m0 must again be considered largely due to the neglect of interstellar absorption. This result was later questioned by Nassau (1934) on the basis of an ingenious technique involving the trigonometric parallaxes of cepheids. At the time formal trigonometric parallaxes had been derived for 34 cepheids, although none was large enough to be significant in itself. Nassau attempted a statistical treatment which involved calculating the parallax π_c for each of these cepheids from Shapley's *P-L* relation, and forming the quantity $(\pi - \pi_c)$, where π was the trigonometric parallax. Nassau then adjusted the zero point of the *P-L* relation until the distribution of $(\pi - \pi_c)$ was symmetrical about zero. He concluded that Shapley's zero point was more nearly correct than Gerasimovic's. The latter, however, (Gerasimovic 1935) was quick to find technical reasons in the handling of the statistics which vitiated this approach.

About this time Robert J. Trumpler (1930) finally established beyond doubt the existence of interstellar absorption. In what now reads as a fascinating paper, Lundmark (1931) examined the effect of this on Shapley's zero point, and found that the rates of absorption being quoted (Bottlinger and Schneller 1930) would imply a brightening of Shapley's zero point by as much as 1^m2 (!). This, however, seemed so excessive that Lundmark preferred a more moderate correction of -0^m85. He then noted that Gerasimovic had recently called for a downward revision of the zero point by about this amount, so his final conclusion was that Shapley's original result must have been about correct!

Confidence in Shapley's zero point was further strengthened by new proper motions and an attendent statistical parallax calculation by Mrs. Bok and Miss Boyd (1933). This was concerned solely with RR Lyrae stars, and showed them to have absolute magnitudes close to 0.0. Although this figure was to persist in the literature for another 30 years or so, it is interesting to note that Fletcher (1934) pointed out that the weighting system used by Bok and Boyd should be altered somewhat, and that the mean absolute magnitude then came out as $+0^m44$, which is about the figure we have come back to today.

Gerasimovic (1934), in considering the discrepancy between his own result and that of Bok and Boyd, noted that his own work had been on the low latitude classical cepheids, while that of Bok and Boyd concerned the high latitude RR Lyrae stars, so that the discrepancy might well be due to the effects of interstellar absorption. He thus hit the nail right on the head, and it is unfortunate that he did not then go back and correct his own result. It is also rather curious that the following year he reverted to his original conclusion when replying to Nassau.

It was in this year also that Shapley came on a problem that has not been fully settled even yet. Throughout these decades, in a long series of bulletins and circulars, the Harvard workers were steadily improving and extending the data on the cepheids in the Magellanic Clouds. In a review of the situation, Shapley (1934) observed that the slope of the *P-L* relation in the Large Cloud seemed to be steeper than that in the Small Cloud. He offered what may still be the best explanation: that the difficulty lay in determining the photographic magnitude scale.

In the late 1930's a number of Russian astronomers began to take an interest in the problem of the *P-L* relation. Kukarkin (1937) initially was concerned with determining the shape of the *P-L* relation. From an examination of cepheids in external galaxies he concluded that the relation was definitely nonlinear. It could be approximated by a quadratic, but the best fit was by two straight lines of different slope intersecting at a period of ten days. Significantly, he assigned a zero point about $0^{m}5$ brighter than Shapley's. Kukarkin (1949) was later to return to the problem, and in this second paper he explicitly showed that the classical cepheids and the RR Lyrae stars do not lie on the same *P-L* curve. There was a break between them, with the classical cepheids having a significantly brighter zero point. Kukarkin has generally not received the recognition he deserved for this. Reference has already been made to the work of Parenago (1940) and Melnikov (1945, 1947).

Meanwhile, Ralph Wilson (1939) was undertaking a grand revision of the calibration by the usual method of statistical parallaxes. There were by now data available for 67 RR Lyrae stars and 157 cepheids. Allowance was made for both galactic rotation effects and interstellar absorption, which at first sight makes it seem astonishing that Wilson's overall conclusion should be that Shapley's result was still correct. The paper is a perfect illustration of how most of the astronomers of the day simply could not bring themselves to believe that interstellar absorption played any important role. Today such a paper would be largely concerned with a discussion of how best to correct for interstellar absorption. In Wilson's paper, 25 pages long, the matter is dismissed in a single sentence! That sentence indicates that Joy had suggested a correction of 0.85 mag./kpc, and that Wilson was applying this value. Reference to Joy's paper (Joy 1939) then reveals that he had obtained that value by *assuming* Shapley's zero point to be correct. In short, Wilson's conclusion that Shapley's zero point was correct was due to a completely circular argument! Camm (1944) also criticized Wilson's paper on a number of other details in which Wilson had completely failed to appreciate the enormity of interstellar absorption effects. (Although, ironically, Camm himself came to the conclusion that Shapley's zero point was correct after all.)

A year later Shapley (1940) undertook what was presumably his final detailed revision of the *P-L* relation. Here he was concerned not so much with the zero point, which appeared to be well-established, but with applying the results of the long series of Harvard investigations to determine the best shape of the relation. He arrived at a nonlinear curve which for $P > 1^d$ I find can be well approximated by the quadratic

$$\langle M_{pg} \rangle = -0.36 - 1.38 \log P - 0.28(\log P)^2 \quad .$$

It seems almost incredible that yet further confirmation of Shapley's zero point came from a pulsation parallax calculation by Becker (1940). He used only a few stars, however, and it seems that the temperatures he adopted were affected by interstellar reddening. There were also errors in some of the radii he derived.

To Mineur (1944) must go the credit for firmly pointing out that the value of the zero point determined by statistical parallaxes is inseparably tied to the interstellar absorption correction. He attempted to determine both by use of galactic structure techniques, such as the requirement that the z distribution of cepheids not correlate with distance from the sun. This is a rather insensitive technique, and one fraught with selection effects, but Mineur did conclude that Shapley's zero point should be brightened by about 0^m75. The analysis was repeated by Berthod-Zaborowski (1946) and the result found to be the same. Their work does not seem to have made much impression at the time, however, presumably because of the world situation then prevailing.

It was the advent of the 200-inch telescope at the close of the 1940's that finally revealed the error in the zero point. The discovery received the widest possible circulation when it was announced by Baade at a meeting of Commission 28 during the 1952 IAU meetings in Rome (*Trans., I.A.U.*, 8, 397, 1952). In outline, the discovery came about in the following way. With the 100-inch telescope, hitherto the largest available telescope, the limiting photographic magnitude was about 21 (Baade 1944). Fitting Shapley's *P-L* relation to the longer period cepheids visible in M 31 (a ten-day cepheid appeared slightly fainter than $m_{pg} = 20$) implied that the RR Lyrae stars in M 31 should appear at $m_{pg} = 22.4$. They were therefore beyond the reach of the 100-inch telescope. Tests on the new 200-inch, however, had shown that this magnitude could be reached in a mere half-hour exposure. One can imagine the surprise, therefore, when such exposures on M 31 failed to reveal a single RR Lyrae star. In fact, only the brightest Population II stars were resolved at this magnitude. At the same time, Sandage's color-magnitude diagram for the globular cluster M 3 (Sandage 1953) had shown that the assignment of absolute magnitude zero to the RR Lyrae stars was probably about correct, but the brightest Population II stars were some $1^{m}5$ brighter than this. Hence in M 31 the RR Lyrae stars must be at $m_{pg} \simeq 23.9$. It followed that the zero point of the classical cepheid *P-L* relation must be about $1^{m}5$ brighter than had been thought. Baade (1956) later enlarged on some of these arguments.

By superb coincidence, the same conclusion had just been reached from observations of the SMC by the Radcliffe Observatory astronomers in South Africa. Thackeray reported at the same meeting the discovery of three RR Lyrae variables in the SMC. (See also Dartayet and Dessy 1952.) Instead of being at $m_{pg} = 17.5$ as predicted by Shapley's *P-L* applied to the SMC cepheids, they were at $m_{pg} = 19.0$, thus giving even more direct evidence than Baade's of the needed $1^{m}5$ brightening in the cepheid zero point. Thackeray and Wesselink (1953) soon after discovered RR Lyrae stars in the Large Cloud as well.

Mineur (1952) also pointed out that his work had indicated the need for assigning higher luminosities to the cepheids.

The effects of this revision were profound. Not only was the entire cosmological distance scale increased by a factor of two, but so also (through the inverse Hubble constant) was the cosmological time scale. The latter had previously appeared as less than two billion years, which had been in conflict (or near conflict) with the developing geological and other evidence on the age of the solar system. A variety of other difficulties in astrophysics were also relaxed by the revision.

To summarize this earlier history of the subject then, we find that the basic error arose in the very first calibrations of the *P-L* relation by Hertzsprung and Shapley. This initial error came not through the inclusion of Population II variables, but partly through the neglect of interstellar absorption and partly through having to use poor and insufficient data. Shapley then inadvertently compounded the error by placing the long-period cepheids in globular clusters on the *P-L* relation and thereby extended the latter to include the RR Lyrae stars. By coincidence, the true difference of $1\overset{m}{.}5$ between the Population I and II variables was just cancelled by the initial error of the same amount in the Population I calibration, so the net result was to assign an almost correct absolute magnitude to the RR Lyrae stars. Later statistical parallax analyses on the field RR Lyrae stars then tended to corroborate this result because, being at high galactic latitudes, the interstellar absorption corrections for these stars were small, and this led to a false confidence in the calibration of the *P-L* relation as a whole. Similar analyses on the low-latitude cepheids, however, continued to be plagued by a persistent failure to realize the extent of the interstellar absorption corrections, and this led to zero points which in some cases were even fainter than Shapley's. Although in the 1930's and early 1940's a number of people, such as Lundmark, Gerasimovic, and Mineur, came tantalizingly close to the truth, none quite reached it. Most unfortunate of all was the 1939 paper of Wilson's with its circularity of argument. There must be few instances in the history of science where so large and basic an error has persisted through the work of so many people over the course of nearly 40 years without being discovered.

The Last Two Decades

The definitive study of the herd instincts of astronomers has yet to be written, but there are times when we resemble nothing so much as a herd of antelope, heads down in tight parallel formation, thundering with firm determination in a particular direction across the plain. At a given signal from the leader we whirl about, and, with equally firm determination, thunder off in quite a different direction, still in tight parallel formation. For years the literature had abounded with confirmations of Shapley's zero point. Now, the revelation of its error by Baade triggered off a tremendous spate of papers all eager to confirm or refine the new zero point. The following is only a partial listing of these papers: Savedoff (1953), Stebbins (1953), Blaauw and Morgan (1954), Cholopov (1954), Filin (1954), Kukarkin (1954), Jaschek (1954), Shapley and McKibben Nail (1954), Wallenquist (1954), Weaver (1954), Kopylov and Kumajgorodskaja (1955), Parenago (1955), Whitney (1955), Badaljan (1956), Pskovskii (1957), Zonn (1957), Becker (1958). Some of these were more detailed than others. Probably the best known of them is the study by Blaauw and H. R. Morgan, which showed that the method of statistical parallaxes is quite capable of correct results when a good system of proper motions is used and when close attention is paid to interstellar absorption. Most of the other papers, however, involved either indirect arguments or else rather rough methods. The situation, therefore, was still not in an entirely satisfactory state.

A major breakthrough came quite by accident in 1955. John Irwin, then of Indiana University, was on sabbatical leave in South Africa carrying out a program of photoelectric photometry of southern cepheids. One night he came to observe the star S Normae, and, having set the telescope, he was immediately struck by the view in the finder telescope. S Nor, easily distinguished by its reddish color, was surrounded by a close grouping of blue stars. In short, it appeared to be in a galactic cluster. The cluster, in fact, was NGC 6087. Since accurate photometric methods for determining the reddening and distances of galactic clusters had recently been developed, here was a prime tool for establishing the zero point of the *P-L* relation.

To interject a personal note, it was this event that allows me to date the beginnings of my own interest in cepheids literally to the hour. I was a graduate student working at the Royal Cape Observatory at the time. Since this is a British institution, the ritual of morning tea was closely observed by its senior officials (and visitors), who foregathered at 11 A.M. alternately in the homes of Her Majesty's Astronomer and the Chief Assistant. On this particular morning, before the crumpets were half consumed, Irwin burst in, bleary-eyed and full of excitement over his discovery of the previous night. Even British reserve permitted a ripple of interest around the table.

This piece of serendipity was quickly followed by another: the discovery by Irwin in the same way of U Sagittarii in the cluster M 25 (Irwin 1955). A search of the literature, however, soon revealed that his discoveries were really rediscoveries. The presence of these two cepheids in galactic clusters had been known to Doig (1925, 1926) and probably others. In fact, at a time when the *P-L* relation was considered well-determined and methods for determining the distances of clusters were not, Doig (1925) had used U Sgr to check his distance for M 25 determined by spectroscopic parallax. Also a well-known Harvard astronomer had complained in the literature that the measurement of photographic magnitudes for S Nor was made difficult by the crowded star field in its vicinity. Nothing is as dead as yesteryear's literature, however, and these facts had long been forgotten.

These casual discoveries prompted systematic searches for other cases of cepheids in galactic clusters (Kraft 1957; van den Bergh 1957). A number of possibilities were found, and detailed investigations of these over the next few years gave about five useful cases. More recently, cepheids in associations and wide binaries have also been investigated. Detailed references to the original work on all these cases may be traced through references in Fernie (1967*a*) and Sandage and Tammann (1968, 1969).

Sandage (1958) was the first to apply the early results from cepheids in galactic clusters to the calibration of the *P-L* relation. Although, as has been described, Shapley, Russell, and others before Sandage had developed a semitheoretical *P-L* relation with the $P\sqrt{\rho}$ relation as its basis, Sandage's independent work greatly improved the end result. He allowed for such matters as the effect of evolution on the mass-luminosity law, and through the cluster cepheids was able to tie down the constant in his equation with much better accuracy. In particular, he greatly broadened the scope of the investigation by pointing up the importance of the color term, and since this time the calibration of the so-called *P-L* relation has really been the calibration of the period-luminosity-color (*P-L-C*) relation.

The need for further revision, however, soon became apparent. More and better data on the cluster cepheids became available, and an error in Sandage's work was discovered (Reddish 1959). Also, Sandage had based his work on a suggestion of Eggen's (Eggen 1951) that, in analogy with the Bailey type *a*, *b*, and *c* RR Lyrae stars, the classical cepheids showed divisions into types *A*, *B*, and *C*; the latter perhaps obeying a different *P-L-C* relation. Later work (e.g., Kraft 1960*a,b*) now indicated that this division was unnecessary.

A major analysis of the whole problem was undertaken by Kraft. In a series of papers (Kraft 1960*a,b*; 1961*a,b,c*) he systematically discussed the determination of cepheid colors, and then used this to calibrate the *P-L-C* relation through the use of cluster cepheids. A drawback to the cluster cepheids (as available at that time) was that they were all of rather similar period, and therefore gave no hold on the problem of the slope of the *P-L* relation. In order to overcome this, Kraft made use of the extensive study of the SMC cepheids which had recently been carried out by Arp (1960). A detailed critique of Kraft's work has been given elsewhere (Fernie 1967*a*), but basically he adopted Arp's slope for the *P-L* relation and fixed the zero point from the cluster cepheids. The color term was introduced as usual through the $P\sqrt{\rho}$ relation. Kraft also discussed at some length the most meaningful way in which averages of magnitude and color should be taken over a cepheid's cycle. Previously, it had been the median magnitude which had been generally accepted, but Kraft suggested that the best way would be to use the average power output of the star. This is achieved by converting the bolometric light curve of the star to one of intensity versus time, numerically integrating this to determine the average intensity, and then converting this average intensity back to a visual magnitude. The average color, on the other hand, was determined by integrating the color curve directly, i.e., without any conversion to intensity. In retrospect, it probably does not matter much in practice how these averages are defined, provided consistency is maintained. It should be borne in mind that the various definitions do sometimes differ slightly but systematically from each other. For instance, the average color index defined as $\langle B\rangle - \langle V\rangle$ is systematically different from the quantity $\langle B-V\rangle$.

Hardly had Kraft completed this work, however, than new difficulties began to appear. One involved his scale of $(B-V)$ color indices. This had been set up through the cluster cepheids, by applying a color excess as determined from the B-type stars in the cluster to the cepheid. Schmidt-Kaler (1961), however, showed that as a consequence of broad-band photometry, an early-type star and a late-type star, seen through the same amount of interstellar dust, do not show the same degree of reddening. Thus a direct application of the B-star excesses to the cepheids led to a systematic error. Allowance for this effect was contained in a recalibration of the cepheid colors (Fernie 1967*b*), but minor difficulties remain (Johnson 1964; Nikolov 1967; Sandage and Tammann 1968). In fact, Kraft's colors are close to the average of these other determinations. As discussed below, it is particularly unpleasant that the cepheids in the Galaxy should appear to have colors considerably different from those of the Small Magellanic Cloud cepheids.

Another difficulty was Kraft's adoption of the slope of the *P-L* relation from Arp's work on the SMC cepheids. Woolley, Sandage, Eggen, Alexander, Mather, Epps, and Jones (1962) and Hodge and Wright (1969) found that the cepheids in the Large Cloud appeared to obey a *P-L* relation of considerably steeper slope, while Gascoigne and Kron (1965) and Gascoigne (1969), using direct photoelectric photometry, derived a slope steeper than Arp's for the SMC cepheids. Thus it was possible that Kraft's adopted slope was wrong, and perhaps worse, that the slope varied from galaxy to galaxy. In any case, it became well established (Dickens 1966) that there exists a distinct difference in the period-color relations between the galactic cepheids and the SMC cepheids. This implies that even if the two groups of cepheids obey the same *P-L* relation in terms of visual magnitudes, they cannot then also obey the same *P-L* relation in terms of blue magnitudes (or vice versa). This difference in color between the two groups remains a source of uneasiness. Bell and Rodgers (1969) have shown that if it is real it can only be accounted for by extreme effects of metal abundance, microturbulent velocity, and electron pressure all acting together in the same direction. One is led to suspect that it is more likely that there is some as yet undetected difficulty in the reddening corrections which have been applied.

Clearly, further revision of the *P-L* relation was called for. An attempt was made (Fernie 1967*a*) to obtain the calibration of both slope and zero point without recourse to either extragalactic cepheids, the $P\sqrt{\rho}$ relation, or even the cluster cepheids. The basis was the establishment of a period-radius relation through the application of Wesselink's method to selected cepheids, and substitution of this into the $L \sim R^2 T_e^4$ relation. This gave a L-P-T_e relation. To convert this into a M_V-P-$(B-V)$ relation, functions of T_e and bolometric correction versus $(B-V)$ were required. These are not very well known, and in order to overcome this difficulty a new innovation was introduced. At constant P, the M_V-P-$(B-V)$ relation reduces to a line or curve on the H-R diagram. Also, an individual cepheid in the course of its cycle traces out a narrow loop on the H-R diagram. It had earlier been concluded (Fernie 1964*a*; 1965*a*) that the central line of this loop can be identified with the line given by the M_V-P-$(B-V)$ relation at constant P. Thus the coefficient of the $(B-V)$ term in the M_V-P-$(B-V)$ relation can be obtained from a study of cepheids' loops in the V - $(B-V)$ diagram. This approach has been criticized by Sandage and Tammann (1969), and there are some difficulties over its internal consistency (van Genderen and Jansen, private communication), but it does appear to give nearly the right results, at least so far as the P-L relation is concerned. This can be judged from all the cluster, binary, and association cepheids which, since they are not needed in the calibration, can be used as tests. An outcome of this investigation was the finding of a second-order term in $(B-V)$ in the P-L-C relation. Thus, substitution of the linear period-color relation into the P-L-C relation produced a nonlinear P-L relation:

$$M_V = -1.99 - 1.89 \log P - 0.38(\log P)^2 \quad ,$$

or

$$M_B = -1.75 - 1.40 \log P - 0.38(\log P)^2 \quad .$$

In the earlier work of Sandage; Arp; Kraft; Woolley *et al.*; Gascoigne and Kron; and the others mentioned above, a linear P-L relation had always been assumed, but the existence of the second-order term seems reasonably well established. (See also Payne-Gaposchkin and Gaposchkin 1966.) In fact, of course, the pre-1952 work of Shapley and others had always shown a nonlinear relation, and if the M_B equation above is compared with Shapley's 1940 equation given earlier, it is seen that the coefficients in these equations are quite similar. The zero points of course, differ by 1^m4.

This nonlinearity also allowed at least a partial explanation of the discrepancy between Arp and Gascoigne-Kron concerning the slope of the *P-L* relation in the SMC. Arp had used somewhat shorter-period stars than Gascoigne and Kron, and the slope of the relation is less at shorter periods.

The most recent calibration of the *P-L* relation has been given by Sandage and Tammann (1968). The approach used by these writers is quite direct. The shape of the *P-L* relation is determined by a composition of the apparent *P-L* relations observed in the LMC, SMC, M 31, and NGC 6822. The zero point is then fixed by the cluster cepheids in the Galaxy. Again a distinctly nonlinear relation is derived, and with this allowance, Sandage and Tammann find no evidence that a single *P-L* relation does not apply to all these galaxies. Very recent results by Gascoigne (1969), however, continue to show a difference between the two Clouds. Sandage and Tammann give "universal" *P-L* relations for both blue and visual magnitudes. Strictly speaking, this is at variance with the fact that there is a difference between the period-color relations of at least the SMC and the Galaxy. As stated above, this implies that there cannot exist a unique blue *P-L* relation *and* a unique yellow *P-L* relation. However, the difference between the two period-color relations becomes a relatively minor effect when translated to the *P-L* relations, so in a practical sense the conclusion of Sandage and Tammann is reasonable. These writers also calibrated the *P-L* relation in terms of the absolute magnitude at maximum light, which is particularly useful when dealing with very faint cepheids in external galaxies.

Sandage and Tammann (1969) have now made a small revision to their first paper. There is, however, some inconsistency in this second paper, in that the writers arrive at a linear *P-L-C* relation and a linear period-color relation. These yield a linear *P-L* relation, which is at variance with the nonlinear form given in their diagrams and earlier paper. The matter is hardly of importance, however, since the curvature is slight and there is good agreement among almost all recent calibrations of the *P-L* relation. The relations given by Sandage and Tammann (1969) and by Fernie (1967*a*), for instance, differ nowhere in their predictions of M_V by as much as $0^{\rm m}2$ over the whole period range from $2^{\rm d}.3$ to $87^{\rm d}.$

Two further difficulties have appeared in recent years. One concerns the evolutionary tracks of cepheids on the H-R diagram, and the other the possibility of overtone pulsation in some cepheids.

Continuing studies of stellar evolution by Hofmeister, Kippenhahn, Weigert, Thomas, Iben, and others in the mid-1960's revealed that the evolutionary tracks of early-B stars on the H-R diagram are considerably more complicated than had been suspected. Detailed references and useful summaries can be found in Kraft (1966) and Hofmeister (1967). An early-B star not only evolves from left to right across the diagram, but then continues to evolve back and forth across the cepheid instability strip, the number of crossings being dependent on a variety of factors, particularly mass and chemical composition. The significance of this for the *P-L* relation is that these multiple crossings by each star are made at different levels of luminosity. If we presume that a star does not change its mass significantly during this phase, this means that stars of different mass can occupy the same position in the instablility strip. (At a given position the star might be making its first crossing, or it might be a star of lower mass making its second or third crossing.) Since the period of the star is governed by its mass and radius, this means that two stars occupying the same position in the instability strip (and therefore having the same luminosity) can have different periods. Hence a dispersion is introduced into the *P-L* relation.

Fortunately, there are two effects which reduce this phenomenon to relative unimportance. The first is that the period is much more dependent on the star's radius than on its mass, as can be seen from the $P\sqrt{\rho}$ or similar relation (Fernie 1965*b*). Thus a small range in mass among stars at the same position in the instability strip makes for an even smaller range in period. The second effect is that the rates at which these crossings are made differ greatly from one another. The second crossing takes a time one or two orders of magnitude longer than the others. Thus at a given epoch the great majority of cepheids must all be making their second crossing, so in effect, the old single-crossing supposition remains. A disturbing factor in this, however, may be the seemingly great sensitivity of these tracks to chemical composition (higher order crossings fail to happen (Hofmeister 1967; Schlesinger 1969)). The details from the standpoint of the cepheids remain to be evaluated.

Any problem of overtone pulsations among cepheids is much less certain. A cepheid pulsating in the first overtone would have a period only 0.7 that of its counterpart pulsating in the fundamental. Given the slope of the *P-L* relation, and supposing the latter to be based on fundamental pulsators, one finds that the absolute magnitude predicted for the first overtone pulsator will be about $0^m\!.4$ too faint. The discrepancy, of course, becomes greater for higher order overtones.

In the case of a few short-period classical cepheids the presence of both fundamental and first-overtone pulsations is well established (Oosterhoff 1964). Whether there is other direct evidence of overtone pulsation among classical cepheids is uncertain. Christy's (1966) suggestion that η Aquilae is a first-overtone pulsator was subsequently withdrawn by him (private communication). Radius determinations for 15 cepheids by Wesselink's method (Fernie 1968) gave what appeared to be rather convincing evidence that as many as one-third of this sample were overtone pulsators. However, there is other evidence which contradicts this. Among the dozen or so cepheids which are members of galactic clusters, binaries, or associations, not one appears to be an overtone pulsator, although one would have expected about four on the previous evidence. Again, if overtone pulsators were common, the apparent *P-L* relations observed in external galaxies should show two parallel sequences, which, in general they do not (although there is some evidence of this among short-period, low-amplitude cepheids in the SMC). Thus most of the evidence is against overtone pulsators, but the matter cannot be considered as completely settled yet.

By comparison with the classical cepheids, the Population II cepheids (W Virginis stars) have been somewhat neglected. This is not entirely by oversight. Being of lower luminosity they are less important as distance indicators, and not many of them are bright enough for any very detailed study to be done of them. *P-L* relations which have been derived for them (Sawyer 1931, 1935, 1942; Arp 1955; Fernie 1964*b*; Demers 1966; Dickens and Carey 1967; Baade and Swope 1965; Sawyer Hogg and Wehlau 1968) rely entirely on the cepheids in globular clusters (except for Baade and Swope, whose work is concerned with the cepheids in M 31) and the distances of these clusters are still not certain. There seems general agreement, however, that the slope of the *P-L* relation for Population II cepheids is somewhat less than for their Population I counterparts, and the zero point, of course, is fainter by about $1^m\!.5$. A definitive study remains to be done.

In conclusion, then, although there are still minor matters to be considered, it seems that after more than half a century of work, the *P-L* relation is in reasonably satisfactory condition at last. No sooner has one said that, however, than there comes back the echo of Edwin Hubble from the distant past. Speaking of Shapley's *P-L* relation during the Silliman Memorial Lectures at Yale in the autumn of 1935, he said (Hubble 1936), "Further revision is expected to be of minor importance."

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