THE ROLE OF PROPER MOTIONS IN DETERMINING THE LUMINOSITY AND DENSITY FUNCTIONS

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The papers presented this morning form a good illustration of the old adage that you should never publish a paper that is completely correct, for then you can write only one paper. But if you publish a paper with a lot of errors, you are sure to be criticized, and you can write a second paper to answer your critics, and if the heat proves to be too much you can write a third paper withdrawing everything.

First a bit of history. It is difficult to pin-point when exactly this question of the star density in high latitudes first came up, but I feel sure that it is implied, to say the least, in the early work on faint blue stars by Malmquist (1927, 1938) and by Humason and Zwicky (1947). This morning Ivan King showed us a table of the changes in the frequency of occurrence among the stars in high galactic latitude as one goes to fainter and fainter magnitudes. This is exactly what I did in 1960 when I published a color-magnitude diagram for 4000 stars down to 19th photographic magnitude near the South Galactic Pole (Luyten 1960). These data had been obtained with the Palomar 48-inch telescope using Haro's three-image method. I also made up a similar diagram calculated from what I thought were then the best available data on the luminosity and density functions. The conclusion of my analysis was that there seemed to be rather fewer M stars than had been expected, and a great many more stars with the color of F and G stars than expected. This idea was not popular at the time; hence this paper has been carefully ignored until these ideas were used by others twelve years later without reference to the 1960 paper.

Later, Klare and Schaifers (1966) published results of an objective prism survey giving data for 1571 stars at high galactic latitudes. They made two fatal errors, however; one, they not only mentioned but actually used proper motions in order to separate giants and dwarfs, and two, they concluded that their results agreed with my luminosity function. Hence this paper too has been carefully ignored. It is surprising that in 1968 and again in 1976 other similar surveys described themselves as a first unbiased survey of faint red stars.

Edith A. Müller (ed.), Highlights of Astronomy, Vol. 4, Part II, 89-93.

That word "unbiased" appears to be the key word in many of the recent papers. Spectroscopists and photometrists appear to be firmly agreed that anything done with or derived from proper motions is extremely biased. Now, what are the facts? Photoelectric observers are apt to claim that their observations of magnitudes and colors are subject to errors of $\pm 0^{\mathrm{m}}\!.005$ (or even less). Hence if systematic errors of $0^{\mathrm{m}}\!.2$ turn up, these amount to 40 times the mean error. When I did the Bruce Proper Motion Survey and the Palomar Proper Motion Survey (by hand), my motions had errors of $0''\!.025$ annually. Forty times that would be a proper motion of one arc second per year, and I believe that anyone who has even heard of proper motions would know that such systematic errors are out of the question. Another salient comparison may be made as follows. If we should make a systematic error of, say, ten per cent, in the size of a proper motion, this eventually comes through as a thirty-three per cent error in the star density derived. But if, as in the present topic of discussion, a systematic error of ten per cent, i.e. $0^{\mathrm{m}}\!.1$, is made in the (*B-V*) of an M dwarf, the estimate of the absolute magnitude will be off by nearly $1^{\mathrm{m}}\!.5$, the distance by a factor of nearly 2 and the resulting star density by a factor of 8.

Spectroscopists also have their troubles. You all remember the case of T202, which was first classified as a white dwarf and later, by the same person, as a quasar, which means that by his own figures he first underestimated the luminosity by a factor of 10^{14}. A second case is AT Cancri, first announced as a cepheid because it is variable in light, then classified spectroscopically as a white dwarf. I measured the proper motion - but it doesn't have any, and a 12th-magnitude white-dwarf variable would be very interesting; so I strongly suspect this too to be a quasar. The third one is CD -42° 14462, announced as the brightest single white dwarf at the St. Andrews Conference. A telephone call to Cape Town brought the detailed photometry, and hence at the Brighton IAU meeting it was announced again as a definite 9th-magnitude white dwarf. More recently a parallax of $0''\!.001$ was determined for it; so again it is definitely *not* a white dwarf.

The proper way to conduct these investigations, apparently, is to take two or three papers with miniscule samples of stars, and one paper with a good solid systematic error in the colors, while ignoring all the basic data, especially those on proper motions, and then call this the *total* evidence. Next, as one of our columnists put it, "with the unerring academic eye for the false solution," you derive a new luminosity function. In this case it demands 29 stars nearer than *two* parsecs - we know four: the three components of Alpha Centauri and Barnard's star. Hence there must be twenty-five more, all brighter than $m = 12$! Does anyone believe such nonsense?

Recently Graham Hill processed a pair of Palomar plates on the North Galactic Pole with a 26-year interval and found 3000 motions of stars down to $m_{pg} = 21$ with $\mu \geq 0''\!.035$/yr. Previously I had done a plate at R. A. $13^{h}36^{m}$ and dec. +6° with an interval of 23 years, on which some 2000 similar stars were found. Data for more than 5000 stars are shown

https://doi.org/10.1017/S1539299600003154 Published online by Cambridge University Press

in the table, where the abscissae indicate my very rough colors and where the ordinates express the quantity $\underline{H} = \underline{m} + 5 + 5 \log \mu$, which can also be written $\underline{H} = \underline{M} + 5 \log \underline{T}$, where $\underline{T}$ is expressed in a.u./yr. Since we have no parallaxes for these stars, $\underline{H}$ is the best statistical approach to absolute magnitudes; and for these proper-motion stars we have, roughly, $\underline{H} = \underline{M} + 5 \pm 2$. If the people who see the sky filled with nearby

Color index	0.0	+0.5	+1.0	+1.5	
$\underline{H}_{pg}$ = 7.0		2	2	2	1
8.5		6	10	3	
10.0	3	66	87	16	
11.5	19	121	246	77	5
13.0	36	113	263	155	38
14.5	43	99	215	274	118
16.0	42	86	216	467	320
17.5	14	28	88	308	613
19.0	4	5	8	50	599
20.5	1		1	5	220
22.0				1	46
23.5			1	1	4
25.0					1
	P321	12^h34^m	+ 30°		5149 stars
	P558	13^h36^m	+ 6°		

M dwarfs with very small proper motions were correct, then the three circled squares should contain by far the largest numbers. There is no evidence for this, and the two most heavily populated squares lie about 3 magnitudes further down; i.e., they represent M dwarfs with reasonable motions.

I have now analysed proper motions in more than 800 Palomar Survey fields; one thing which emerges from this is that the number of stars per unit area with proper motions larger than a given amount definitely decreases with galactic latitude. The simplest explanation of this, I believe, is that stars in high galactic latitude have larger tangential velocities because the main motion is parallel to the galactic plane. If now one wants to add large numbers of stars in high galactic latitude with small tangential velocities, then the end result will be equidensity surfaces in the shape of prolate ellipsoids with the long axis perpendicular to the galactic plane.

Recently I completed and published a new catalogue of stars with proper motions larger than 0$''$.5 annually. It contains 3600 entries, 2000 of which have come from the Bruce and Palomar Surveys. I do not now intend to derive a new luminosity function, because as of now the number of new accurate parallaxes being determined for very faint stars, especially at the U.S. Naval Observatory in Flagstaff, Arizona, is so large that in five or ten years we shall be in a much better position to make such a solution. In fact, in about ten years we should be able to get an almost definitive determination for the luminosity function in the solar neighborhood.

By now I have published about 30,000 proper motions for stars near the North and South Galactic Poles, with estimated colors for 27,000. In addition I have data on magnetic tape and computer printouts for another 60,000 stars, which I shall be glad to make available.

Recently an article appeared which gave a rehash of the colors of all of nine stars. It took up 4 pages and had 28 references. On that basis I should be entitled to ask the same journal to publish 3000 4-page papers on my proper motions and another 3000 4-page papers on my colors, for a total of 24,000 pages, including 168,000 references, i.e., if I followed the mutual-admiration society of these Messiahs of the Missing Mass in copiously referring to each other but never to the basic data. Is this what our science has come to? This is not astronomy or astrophysics, it is astrofantasy.

Ours is the age of automation and the computer. Since I had nothing to do either with the design of or the fabrication of my machine, I am not boasting when I say that the engineers at Control Data Corporation have achieved success brilliantly and superlatively in automating the proper-motion survey. The best we ever did was to process a pair of plates on each of which the computer counted 585,000 star images. The scanning took three hours, the computerizing took 2^h23^m of central-processor time and 15 minutes for the peripherals. Thus in less than 6 hours we determined $\underline{x}$, $\underline{y}$, and diameter to one micron for 1,170,000 stars, which works out as better than 50 stars per second; this is 200 times faster than GALAXY. In addition, we made a least-squares solution and the printout showed right ascension to $0\overset{s}{.}1$, declination to 1", red magnitude to $0\overset{m}{.}1$, size of proper motion to $0\overset{''}{.}001$/yr and direction to 1° for 400 stars.

What we need is automation of the photometry for these 30,000 stars near the galactic poles, and this should not be too difficult. I know whereof I speak, for we have several times processed a pair of blue and red Palomar plates and determined colors. And this goes so fast that we could easily repeat it five or six times and thus virtually eliminate any machine-introduced errors and end up with only the plate error. So I suggest that the photometrists get busy - but no systematic errors, please.

Astronomy, like all other fields of human endeavour, has had its funny theories and observations. My generation especially will remember the rotation of what were then known as "spiral nebulae". But how many remember the new theory of gravitation proposed to explain these motions? I think it was due to J. H. Jeans and E. W. Brown, and their force of gravitation was dependent not only upon the distance but also on the angle of direction. Then there was the theory for the origin of the Solar System by Lyttleton and Hoyle, which ended up in a game of cosmic billiards. Here the Sun was supposed to form a binary; and a third star came in, first collided with the other component, glanced off, then hit the Sun, which at that precise moment exploded as a nova. I took a dim view of this.

Some of you may remember, some ten or fifteen years ago, an advertisement in The New Yorker for the Taittinger Champagne Co. It was a very striking one and showed only the black silhouette of the rather distinctively shaped bottle. Then, at the top, it said: "This is the finest champagne in the world.*" The asterisk refered the reader to a footnote which said: "This is probably an understatement."

In my fifty-five years of being in astronomical research this enterprise of the plethora of nearby M dwarfs with no proper motions is the most absurd I have ever experienced. And this is certainly an understatement.

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https://doi.org/10.1017/S1539299600003154 Published online by Cambridge University Press