THE DETERMINATION OF ABSOLUTE MAGNITUDES FROM PROPER MOTIONS

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Abstract. A brief review is given of the several ways in which stellar motions may be used statistically. In the case where the motions of all stars shown on plates of certain regions are measured these are generally used to determine kinematical relations in the Galaxy and only secondarily for the estimation of luminosities; when motions are measured for special groups of stars the solar motion is first determined and then used for the calculation of distances and luminosities.

When dealing with the statistical analysis of stellar motions we should never lose sight of the fact that on them depends, in final analysis, the derivation of the scale of the universe.

In actually utilizing proper motions for the estimation of stellar luminosities we must distinguish several different situations. The most general case is that where the motions of *all* stars shown on one or more plates covering certain areas in the sky have been measured. This is the situation dealt with by Vasilevskis and Klemola in the Lick Observatory program of determination of absolute proper motions relative to galaxies. Here, of course, the prime purpose is not so much the determination of stellar luminosities but rather an analysis of the kinematics of the Galaxy, solar motion, galactic rotation, etc., with, as one of the most important by-products, the derivation of the corrections to be applied to convert relative to absolute proper motions. In general, in these cases, there is no dearth of material, thousands of stars are normally available, and the procedures employed are straightforward. But, as usual, there are pitfalls, such as, e.g. whether to include or reject, or give only partial weight to any, or all stars of large proper motion, since even one star with a very large motion can dominate the solution for a fairly substantial area in the sky. Another one is that, if one wishes to use such data and analyses for the derivation of luminosities, one must make a guess as to the value of the solar velocity – usually from the declination of the apex determined – for, with the very faint stars considered in these investigations, there never are radial velocities available.

Perhaps the most satisfactory situation in the statistical utilization of proper motions occurs in the application to moving clusters. The classical case is that of the Hyades cluster where we have available substantial numbers of very accurate absolute proper motions as well as radial velocities, and the solar motion as such is not involved. It has now become almost possible to determine accurate individual distances and luminosities for the stars in the cluster, but this is obviously a very special case.

In another, somewhat more general case, we deal with groups of stars which, at best, appear to belong to a fairly homogeneous class, astrophysically speaking, either from their spectra, or from other properties, such as Cepheids, Long-period variables, N or R stars, Planetary Nebulae etc. Here the usual approach is, first, to try and deter-

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mine the solar apex from the motions alone, then select a value for the solar velocity and proceed from there. Sometimes there are radial velocities available, which can give at least an approximate solution for the value of the solar velocity, but if there are very few, or no radial velocities available, one must, again, do the best one can and estimate the solar velocity from the declination of the apex found. But, one must be very careful and watchful for surprises such as discovering that what at first sight appeared to be a homogeneous group, later turns out to be a mixture of two or more subgroups with different kinematical properties – witness what happened in the case of the W Virginis type of Cepheids.

Also, one must be very careful that pre-conceived astrophysical beliefs do not force the proper motions into a straight-jacket. I am thinking of such situations as with the Faint Blue Stars where the spectroscopists identified large numbers of White Dwarfs among them, even though many of these had been known before, or were found later to possess virtually no proper motions. This was especially glaring since most of these Faint Blue Stars were in high galactic latitudes where we would expect the stars to have rather larger tangential velocities than usual, and this is, indeed, borne out by or proper motion surveys. When a group of tenth-magnitude blue stars near the North Galactic Pole, selected only from their colours, all turn out to have proper motions of 0″.1 annually, or less, they are not likely to be, *all* of them, White Dwarfs with absolute magnitudes around +11.

In the spectroscopic estimation of absolute magnitude there are many hidden unknowns and uncertainties. We know there are many large uncertainties in results derived from proper motions but I think it is fair to say that most of these are very visible and on the surface since they result from the kinematic distribution – and hence one can make fairly reliable estimates of them.

Summarizing I would say that in those cases, where one hopes one is dealing with a single astrophysical group, presumably with a well-defined mean luminosity, and not too large a dispersion around it, about the only way to determine this mean luminosity from the proper motions is to make a solution for solar motion and infer the solar velocity from the declination of the apex found. But, if the stars are not sufficiently well distributed to yield a reliable solar apex, and one must use parallactic motions for single areas, from an assumed apex, then, obviously, the resultant uncertainties in the luminosities become very much larger.

The situation with which I personally am mostly familiar is that of the determination of luminosities for stars which had been pre-selected according to size of proper motion. This problem is basic to the derivation of the function describing the distribution of stellar luminosities and involves the estimation af absolute magnitudes in individual cases as well as the determination of mean absolute magnitudes and their dispersion for different groups of stars of known proper motion and apparent magnitude.

There are two main approaches toward a solution: the use of mean-parallax formula in individual cases, which was pioneered by Kapteyn, and, again, the determination of the solar motion, and subsequent evaluation of the parallactic motion.

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Kapteyn's formula was a simple one:

$$\log p = a + bm + c \log \mu$$

and one determines the constants from data for the stars of known, and reliable trigonometric parallaxes, When one is dealing with the proper motions of the very faint stars involved in the derivation of the luminosity function there are actually few, if any stars with known trigonometric parallaxes, and one must extrapolate from data for brighter stars. It is for this reason that I have tried a different formula, viz.

$$M = a + bH \quad \text{where} \quad H = m + 5 + 5 \log \mu,$$

the so-called 'reduced proper motion', first used by Hertzsprung. This being a two-constant formula it cannot, of course, be as accurate as Kapteyn's which uses three constants, but because of the large extrapolation and inherent uncertainties involved I have felt that, because it is simpler, it has an advantage in usage.

The real difficulty in the application of either formula is that there are *no* parallaxes or radial velocities for these very faint stars – fainter than the eighteenth apparent magnitude, and hence it is not now possible to make a reliable determination of the real dispersion in the absolute magnitudes derived, as due to the dispersion in tangential velocities and the only thing one can do is, obviously, to extrapolate from the known data for brighter stars.

Since there have been several recent attempts to criticize my derivation of the luminosity function from the data in the Bruce and Palomar Proper Motion Surveys in 1938 and 1968, I may perhaps digress here a little and deal with those criticisms.

Wanner has, for the third time now, claimed that the mean absolute magnitudes I derived for the proper-motion stars used were too bright and repeatedly has derived new reduction formulae which would indicate that my absolute magnitudes are too bright by about $1^{m}.2$ in the region where it counts most, between $M = +13$ and $M = +16$, near the maximum of my luminosity function. Yet when he is finally through with his own derivation of the luminosity function, he ends up with a maximum for this function which is more than *three* magnitudes *brighter* than mine (+12.3 as against +15.7).

On the other hand, Murray and Sanduleak have, on the basis of an analysis of the proper motions of all of 21 stars near the North Galactic Pole, reached the conclusion that the frequencies in the region of the maximum of my luminosity function must be multiplied by a factor of *five* – then they also obtain agreement with Oort's mass-density per cubic parsec. Now the 21 stars they used were found on objective-prism plates, and classified as M. Since they are of the apparent magnitude 15 vis, the argument is used that they cannot be giants, hence they must be M-dwarfs, with a mean absolute magnitude of around +11. Now of the 21 stars they used, seven had been found before, in my proper motion survey, to have proper motions averaging about 0″.10 annually. Seven more were found later to have small proper motions, averaging around 0″.06 annually, and the last seven appear to have proper motions so small that I would call them marginal. Yet, according to the spectroscopists they must all be M-dwarfs and should have parallaxes around 0.″02 on the average, which would imply that their average tangential velocity must be around 14 km s^{-1} – and this at the North Galactic Pole.

Other recent determinations of the luminosity function find, in one case, a constant frequency from $M = +8$ to $M = +16$, and, in another case, a continuously increasing frequency from supergiants all the way to, I believe, micrometeorites.

Since all these recent analyses appear to pull my luminosity function in several different directions at once, and since, as of now, there is no material inexistence even faintly comparable to that from the Palomar Survey – which most of these researches ignore – I see no reason to change the present luminosity function. I want to emphasize again that it is necessarily a very preliminary one. When the Palomar Survey is at least half completed we shall have a lot more, and better data on the frequency of proper-motion stars. But beyond that we shall need at least hundred trigonometric parallaxes for very faint stars down to at least the twentieth photographic magnitude in order to determine both the mean absolute magnitude and the dispersion in tangential velocities for these stars. Until we have these data I believe it is useless to talk about a determination of the luminosity function other than an extreme preliminary and cursory evaluation of it, and, equally, there is no point in trying to find any second order undulation in the shape of the curve: a smooth curve is all that the present data warrant.

Finally, there is the problem that sometimes comes up with an attempt to find a rough luminosity for a single object of a new astrophysical type, perhaps, when no spectroscopic data are available and only a very rough preliminary proper motion is known. If it is a star found from its large proper motion then, perhaps $p = \mu/12$ is as good a guess as any, but if the proper motion is determined after the object had been found then $p = \mu/10$ is perhaps better. The direction of the motion is also a good indication, for one must remember that no self-respecting high-velocity star goes north, or toward the solar apex.

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DISCUSSION

Strand: Since Dr Luyten has referred so frequently to Naval Observatory parallaxes, I like to show a diagram showing the present status of this program, as far as data now completed, and now in press in the Observatory's publications. In this M_v vs $B-V$ diagram it is noted that there is a clear separation between the main sequence and the degenerate branch, with no white dwarfs beyond $B-V$ larger than approximately one. It is noted *for* the stars of immediate colour that those with *small* parallaxes lie approximately on the average one magnitude *below* the main sequence. These are high velocity sub-dwarfs.

Pecker: Do you have some T-Tauri and UV Ceti or at least stars with IR excesses in your survey? Where do they fall on your diagram?

Strand: There are no T-Tauri stars on our program at this time.

Murray: I would like to congratulate Strand on the results he has shown for his 61-in. reflector. However, he is fortunate in having a dedicated telescope for parallax work. It is desirable for others to observe faint stars. This means using large telescopes and taking what time one can get. Hence, as in our programme with the Isaac Newton Telescope, it is essential to use the time available, whatever the hour angles involved.

Jaschek: Is any effort being made to take spectra of the stars you are observing for parallax?

Strand: No. Only Greenstein is observing the brighter ones, many of them being faint.

Luyten: In connection with determining parallaxes in declination I should like to point out that not only are the amounts much smaller that in RA but furthermore, while one can get the extremes of the parallax ellipse in right ascension it is in the nature of things impossible to get one of the extremes in declination, because then the star is in conjunction with the sun and inobservable. Only if the star is very close to the pole could one observe it, but then it will be below the pole and in that case other troubles appear.

Van Altena: In an attempt to increase the accuracy of trigonometric parallaxes several changes in the observation and reduction methods have been made at Lick and Yerkes Observatories. Observations are made at large hour angles to increase the parallax factor. Traditional observations near the meridian give average parallax factors of 0.6–0.7, while at Yerkes an average value of 0.95 is obtained. The effects of differential colour refraction can be accurately determined observationally and theoretically and agree very well. The use of a large number of reference stars (~ 24) allows one to: (1) solve explicitly for the magnitude equation; (2) compensate for quadratic effects in the coordinates; (3) minimize the importance of a single reference star in the position of the parallax star; (4) obtain the correction to absolute parallax from the proper motion dispersion and the expected velocity dispersion. The results of these modified methods give increased accuracy of parallaxes and parallaxes in agreement with their expected position in the colour-magnitude diagram.

Van de Kamp: I would like to ask Dr Strand at what hour angles he makes his observations?

Strand: Less than 15 min.

Van de Kamp: It is an interesting idea to use a large number of reference stars in order to obtain the correction to absolute parallax. Also, by using a large number you obtain 'luxury parallaxes'; most of us determine 'economy parallaxes'. I would like to ask Van Altena why only quadratic terms are considered; why not also other powers? Also, if you observe at large hour angles, you are playing with fire.

Van Altena: Automatic measuring machines make it possible to measure many reference stars nearly as easy as a few reference stars. In my opinion, the advantage of many reference stars outweigh the slight economy of a few. Plots of the residuals versus the coordinates occasionally show quadratic terms, but not higher power terms. Since we have many reference stars distributed nearly uniformly over the inner 150 mm of the plate, it should be possible to eliminate any systematic errors introduced by observing at large hour angles. (Differential colour refraction is eliminated from the measured coordinates before further proceeding.) The lack of any systematic trends in the residuals supports this contention.

Murray: Concerning the question of selection of reference stars and the reduction to absolute, at Herstmonceux we normally use six reference stars. In the first 20 fields, which have recently been submitted for publication, we found two reference stars, or about 2% which showed significant parallax.

Strand: Dr van de Kamp mentioned that the *p.e.* of unit weight decreases from 1.4 μ to 1 μ when the Sproul plates measured manually were measured with the U.S. Naval Observatory machine. A recent Sproul Series measured with this machine actually decreased to 0.8 μ; of the same size as with the material from the 61-in. astronomic reflector.

Pecker: The number of people working on parallaxes is decreasing, whereas the needs are still large. Could a list of essential programs of parallaxes be done for such instruments as the Nice 76-cm refractor, where some scientists, although not experienced, express a definite interest for that kind of observation? These programs could be done according to the interest of the stars from a purely astrophysical point of view. What is your opinion?

Van de Kamp: Yes. The need for more parallax determinations is as great or greater than ever before. Astronomers at active parallax observatories would be glad to assist and give advice in the formulation of observing programs.

Pecker: Are the masses of the perturbators in the perturbed cases you have shown (ROSS 614, etc.) known? Wat are they, if the reply is yes?

Van de Kamp: Yes and no. For ROSS 614, which is now a resolved binary the latest study (Lippincott and Herskey) yields a mass of 0.06 solar mass for the companion. For other perturbations, where the companions have not yet been seen the estimated masses range from approximately 0.008 to close to one solar mass, except for the possible planetary companion or companions of Barnard's Star.

Jaschek: Is any effort being made to re-observe nearby stars with large parallax errors?

Van de Kamp: Yes.

Blaauw: Already more than 20 yr ago we noted that there are large systematic differences between parallax catalogues, f.i. 0ʺ005 between Allegheny and Cape. Have these systematic differences been re-analyzed, and has more recent work thrown some light on where in between these catalogues (or outside this range) the true parallax system lies?

Van de Kamp: We need more parallax Observatories, particularly in the Southern Hemisphere. How can we improve on systematic differences between different observatories, such as Allegheny and Cape? Do the telescopes remain the same and what about decreased activity at some observatories?

Gliese: I refer to a paper 'Errors in Trigonometric Parallaxes' which I have presented at Herstmonceux 1971 and which is now published in *Quart. J. Astron. Soc.* **13**. The inclusion of the modern observations does not change the systematic difference $\pi_{\text{Cape,Yale}} - \pi_{\text{Alleghyeny}} = +0''005$. Moreover, I have combined (1) a comparison of trigonometric parallaxes with the moving cluster parallaxes of the Hyades by Upton, (2) a very preliminary comparison between first 61-in. results and the Jenkins' system, and (3) a comparison between luminosity calibrations based on large Allegheny parallaxes and calibrations based on small Allegheny parallaxes. The resulting 'true system' of trigonometric parallaxes is: $\pi_{\text{Allegheny}} + 0''002 \pm 0''0014$ (s.d.) which corresponds to $\pi_{\text{Cape,Yale}} - 0''003$.

Murray: Just to keep the record straight, according to my information parallax working is still going on at the Cape Observatory.

Maeder: In connection with the problem of parallaxes, I would like to make a brief comment on the accuracy of apparent magnitudes, which may in fact also limit the accuracy of the absolute M_v. For example, in the Catalogue of Bright Stars, careful comparisons have shown that more than 40% of the stars have appreciable *systematic* errors of about 0^m10, and we shall return in a following session (with Dr Rufener) to this point.

Jaschek: Is somebody making a critical revision of the parallax catalogue in order to eliminate determinations with small weights, which are still quoted, although improved values exist?

Gliese: One should reject all stars with large errors, whatsoever the value of the parallax is.

Lippincott: I wish to say a few more words on the accuracy of trigonometric parallax determination. At the Sproul Observatory, the long range astrometric program initiated by Peter van de Kamp, now yields series of plates on many nearby stars covering an interval of over thirty years. Those series may be considered to be made up to 5 or 6 individual parallax series of 6 or 5 yr each. Thus considered, there appears a far larger range in parallax values than would be generated by purely accidental errors for some series. This has occurred despite the extreme effort to obtain a homogenous material. It seems likely that these systematic errors are due to a lack of proper collimation over a period of time. Therefore it is not surprising to find that there are significant systematic differences between parallaxes determined at different observatories. Also since one and the same telescope performs differently over different time intervals it makes it even more difficult to evaluate the differences between observatories.

The second point I would like to stress which is reassuring, is the determination at the Sproul Observatory of the difference between parallax in right ascension and declination for the average of 124 stars already quoted by Dr van de Kamp as $-0''0018 \pm 0''0011$ (*p.e.*) Here the systematic errors are for the most part independent.

Gliese: On the basis of reliable trigonometric parallaxes the following mean relations for main-sequence stars from dFS to dM have been derived and published 1971 in *Veröff, astr. Rechen-Inst. Heidelberg, No. 24*:

M_v, MK type	M_v, $B-V$
M_v, Mt. Wilson type	M_v, $U-B$ (from F8 to K7)
M_v, Kuiper type	M_v, $(U-B)$ Cape (from F8 to K7)
	M_v, $R-I$.

For late type main-sequence stars the latter relation, obviously, is the most reliable one. But since the number of stars with U, B, V photometry is still larger than the number of stars with $R-I$ data, I have tried to improve the $(M_v, B-V)$ relation by a simple formula:

$$M_v = M(B-V) + b \cdot \delta(U-B)$$

where $\delta(U-B)$ is the UV – excess. In the region $B-V > +0.45$ the derived values of b are always positive. In H. L. Johnson's colour system b is about $+7$ for F dwarfs; it decreases to $+4$ in the K dwarf region and it is $+8$ for $B-V > +1.36$. Eggen's system give a similar run of b but somewhat smaller values $(+2 < b < +6)$. The correlation coefficient r between ($M_{\text{trigonometric}}$ minus M_{B-V}) and $\delta(U-B)$ decreases from $+0.69$ (dF) to $+0.51$ (dK) and increases to $+0.89$ for M dwarfs. These variations of b and r probably are caused by the effects of the observational errors in $\delta(U-B)$ which are largest in the K dwarf region and which diminish b and r.

I summarize: Obviously there is a correlation between the ultraviolet excess of late-type main-sequence stars and their luminosity. But the evaluation of additive corrections as a function of $\delta(U-B)$, depends on the colour system and seems to be so sensitive to observational errors that an effective application of corrections is not yet recommended.

Strand: We at the Naval Observatory are now determining $(V-I)$ for all red dwarfs on the parallax program.