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The magnitude equation in visual observations of right ascension.

Reply to the article by Dr. Fritz Cohn in A. N. 4119.

By *Arthur R. Hinks.*

In a recent number of the Monthly Notices of the Royal Astronomical Society (1906 June) I published some evidence from a discussion of photographic observations which seemed to me to show that Dr. Cohn's Normalkatalog der Rektaszensionen der Erossterne (A. N. 4060) was affected by a magnitude equation; and that the error was not constant throughout the system.

In A. N. 4119 Dr. Cohn repudiates this conclusion, arguing that, in the first place, it must necessarily on a priori grounds be false; and in the second place, that the arguments which I have brought forward do not prove it to be true.

It is necessary for me to reply briefly to Dr. Cohn's remarks. But before doing so in any detail I should like to say what is, I think, the origin of the principal difference between us. We have adopted different ways of obtaining the best linear representation of a magnitude equation which is not, in reality, linear at all. There is no reason to believe that the magnitude equation is in any case a true linear function of the magnitude; in some cases there is ample proof that it is not (see a paper by Prof. H. H. Turner on magnitude equation at Cambridge, Berlin, and Greenwich, Monthly Notices Vol. LX p. 3). The material is not often sufficient to determine the true shape of the magnitude equation curve, and neither Dr. Cohn nor I have attempted to do so. We have both tried to fit a straight line to the curve as best we could — but with this difference in procedure, that whereas Dr. Cohn gives equal weights to each determined point upon the observed curve, I on the contrary have given weights depending on the number of observations which determined each point. Had the true magnitude equation curve been linear, the difference between the results of these two procedures would have been of an accidental character. As the true curve is not linear, the differences become more or less systematic. And this explains what Dr. Cohn finds inexplicable, that from the same numerical material we derive very different results. We shall find this explanation removes the chief difficulty, but there are other points upon which we disagree, and they may be summarized as follows:

I. As to the freedom from magnitude error of the concluded photographic right ascensions:

I examined the results from eight observatories; found that they had no sensible relative magnitude equation among themselves; and concluded that it was not likely that they had an absolute magnitude equation common to all.

Dr. Cohn endeavours to show that these results do contain a relative magnitude equation, and that it is related to the hour angle of the exposure.

II. As to the alleged difference of magnitude equation in Dr. Cohn's list I and list II:

I derived for a portion of list I a magnitude equation of $0^s\!.017$ per magnitude; for the remainder of list I and the first part of list II an equation of $0^s\!.002$; and I consider that this change of magnitude equation is real.

Dr. Cohn admits that there is an apparent change, but refuses to believe that it is really in the visual observations.

III. As to the alleged difference of magnitude equation in system C (Cohn) and in system T (Tucker):

I concluded that the magnitude equation in system C is more variable than in system T; and that there will be no advantage in adopting either system C or system T, in preference to system L (Loewy) which has been already very largely used.

Dr. Cohn denies that variations of magnitude equation are more sudden in system C than in system T.

I will deal briefly with these three points in order.

I. The alleged magnitude equation in the photographic right ascensions.

In Table I of my paper the results obtained at Paris are compared directly with those obtained at eight other observatories; and practically no trace of magnitude equation is found.

Dr. Cohn obtains an indirect test of the magnitude equation of each photographic series from the comparison of each with his system C given in my table III. The result amounts to this, that Bordeaux, Catania, Greenwich, and Toulouse appear to show that the magnitude equation in system C is small; while Helsingfors, Northfield, Paris, and San Fernando appear to show that it is large. Dr. Cohn interprets this as showing that the four former series have a very different magnitude equation from the four latter.

Against this conclusion we may set the following fact. From the figures given in my table I we may form the mean of the four comparisons of Paris with Bordeaux, Catania, Greenwich, and Toulouse respectively, which will correspond very nearly with the result of a direct comparison of Paris with the mean of these four. Similarly we may yet the result of comparing Paris alone with the mean of Helsingfors, Northfield, Paris, and San Fernando. The results are:

Mag.	Paris minus mean of B, C, G, T	Paris minus mean of H, N, P, SF
– 6.2	$-0^s.013$	$-0^s.017$
6.3 – 6.9	−0.004	−0.017
7.0 – 7.4	0.000	−0.015
7.5 – 7.9	+0.002	−0.009
8.0 – 8.4	+0.007	−0.001
8.5 – 8.8	+0.003	−0.004
8.9 – 9.2	+0.003	+0.002
9.3 –	0.000	−0.004

It is clear that Paris has less linear magnitude equation relative to the mean of the group from which, according to Dr. Cohn it differs, than it has with respect to the group in which he places it. How, then, are we to explain Dr. Cohn's result? It must be due to the fact that he has used system C as an intermediate standard of comparison, instead of comparing the photographic results directly. Such a procedure would be fair only if exactly the same stars entered into the magnitude groups in all series. The effect of any errors in system C would then be automatically eliminated. But if the stars are not exactly the same, as is actually the case, the attempt to compare two photographic series indirectly, by taking the difference between their mean differences from C, must necessarily introduce to some extent the errors of C, and does injustice to the photographs.

As an example of the dangers of this indirect comparison we may cite Dr. Cohn's results for list II (p. 235), where he obtains magnitude equations of $0^s.010$ per magnitude for Bordeaux minus San Fernando, and $0^s.015$ for Bordeaux minus Helsingfors. I have counted the stars which must have been employed by Dr. Cohn in this comparison. There are 91 stars in the Bordeaux list, all of which, with 250 others, are observed at San Fernando. Hence the comparison Cohn minus Bordeaux depends upon 91 stars, making little more than one fourth part of the whole 341 stars upon which the comparison Cohn minus San Fernando depends.

Again, there are 32 stars common to Bordeaux and Helsingfors; 75 observed at Helsingfors and not at Bordeaux; 59 observed at Bordeaux and not at Helsingfors. It follows that the comparison of Cohn with Bordeaux depends on 91 stars; that of Cohn with Helsingfors upon 107; but only 32 stars are common to both sets.

We are not entitled, in such a case, to take the difference of the two comparisons

Cohn minus Bordeaux
Cohn minus Helsingfors

and to call the result

Bordeaux minus Helsingfors.

The intermediate standard of comparison, Cohn, will not be eliminated unless it is quite smooth and homogeneous; and this is the very matter we are trying to decide. I do not think, therefore, that Dr. Cohn's conclusions, based upon indirect comparisons similar to the above, can be maintained. I prefer to take the verdict of the direct comparisons made in my paper, that the photographic right ascensions of the repère stars contain no sensible magnitude equation.

If we conclude that there is no magnitude equation, we need not examine at length into a suggested explanation of such an error. But it will be necessary to refer very shortly to Dr. Cohn's belief that there is a magnitude equation which is a function of the hour angle in which the plate is taken. He gives the result of comparing the places of faint comparison stars derived from five pairs of San Fernando plates. A remarkable case is the pair 3234-3231. More than 40 stars are common to the two plates, and the mean difference between the right ascensions derived from the two plates is $0^s.035$. This difference certainly requires explanation, but it is easy to show that the error is not due to magnitude equation depending upon hour angle. In the first place, if we divide the comparison stars into three magnitude groups we have the following result:

Mag.	No. of stars	Mean $\Delta\alpha$ (3234—3231)
9.9 – 10.5	3	$+0^s.037$
10.6 – 11.2	21	+0.030
11.3 – 12.1	17	+0.039

There is no evidence of magnitude equation here.

Further, if we examine the individual repère star places derived from these two plates (communicated to me in manuscript by the Director of the San Fernando Observatory) we find

Mag.	No. of stars	Mean $\Delta\alpha$ (3234—3231)
– 7.0	3	$-0^s.020$
7.1 – 8.0	3	−0.020
8.1 – 9.0	7	+0.021
9.1 –	3	−0.010

Again there is no evidence of magnitude equation.

Finally, both the plates of this particular pair were taken on the same side of the meridian, one about $3^1/_2$ hours east, the other 20^m east. Yet the discordance of this one pair is nearly twice as great as the mean discordance of the four others, whose difference of hour angle averages $9^1/_2$ hours.

It does not seem to me that these figures, when examined thus, give the slightest support to Dr. Cohn's allegation, that the photographic places have in them an error which is a function of the magnitude and the hour angle. And I see no reason to modify my original conclusion, that the eight photographic series which I have used are sensibly free from magnitude equation down to the limit of magnitude of the repère stars.

II. The magnitude equation between part of system C and the photographic series.

The only difficulty in this section arises from the fact, to which reference has already been made, that Dr. Cohn and I do not agree in the methods which we use respectively for the evaluation of our magnitude equations. He uses a numerical method which gives equal weight to the mean discordance for each magnitude group, irrespective of the number of individual stars contributing to that mean.

The method that I have used is thus described (p. 485 of my paper): »All the numerical values given in this paper have been obtained by plotting the tabular results, and finding the general slope of the curve by laying a ruler on it, having regard to the number of stars which contribute to the different parts.«

A good example of the way in which these two methods lead to divergent results is to be found in discussing part of my Table IV which reads thus:

System C minus mean photographic RA.

Mag.	List I No. of stars	List I Mean discordance	List II No. of stars	List II Mean discordance
– 6.2	9	$-0^s.045$	8	$-0^s.028$
6.3 – 6.9	16	−0.043	6	−0.035
7.0 – 7.4	11	−0.044	9	−0.039
7.5 – 7.9	34	−0.047	28	−0.036
8.0 – 8.4	65	−0.047	30	−0.039
8.5 – 8.8	96	−0.052	32	−0.044
8.9 – 9.2	57	−0.060	15	−0.042
9.3 –	14	−0.060	—	—

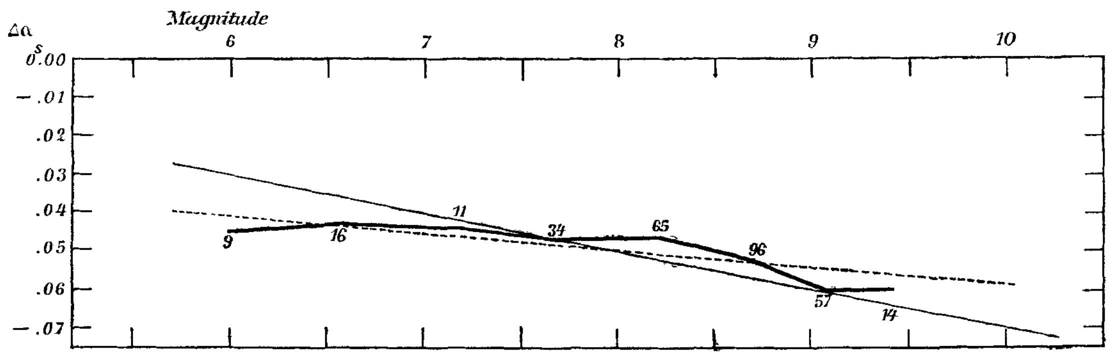
Upon this evidence I wrote: »The magnitude equation in system C for list I is confirmed.« Dr. Cohn remarks: »Es ist mir völlig unerfindlich, wie Herr Hinks angesichts dieser Daten seinen Schluß aufrecht erhalten kann;« and he finds for the two series the respective equations

$$-0^s.050 - 0^s.0045\,(m - 8.0)$$

and

$$-0.039 - 0.004\,(m - 8.0)$$

There is, however, nothing inexplicable in the difference between our conclusions, as will be seen if the values are plotted.



System C minus mean photographic RA.

I should agree with Dr. Cohn that for list II the equation is about $0^s.004$ per magnitude, but for list I I differ from him. His determination of the magnitude equation is represented by the broken line; and it will be noticed that the most strongly determined points, with 65, 96, and 57 stars respectively, lie on a line which crosses his at a considerable angle. In my graphical solution I give more weight to these points than to the badly determined ones at the beginning; and I give to the line representing the supposed linear magnitude equation a slope at least as great as that shown by the continuous line in the figure, which corresponds to an equation of $0^s.010$ per magnitude.

In all cases where we differ as to the numerical value of a magnitude equation, the explanation is to be found in this way. We have each tried, in different ways, to fit a straight line to a series of points which does not approximate, in reality, to a straight line but to a curve.

Fortunately, however, there is little difference of opinion between us on the fact most important of all, that if we divide list I into two groups, whose limits are defined on page 488 of my paper, the magnitude equation of is very different in the two groups. My determination makes it $-0^s.017$ per magnitude in the first, and $-0^s.002$ in the second. Dr. Cohn (p. 232 of his paper) makes it $-0^s.013$ and $-0^s.002$. The difference between our results for the first group is due to the difference of our methods. But Dr. Cohn is in substantial agreement with me, that there is in this group a remarkable difference of magnitude equation between system C and the photographic right ascensions.

I feel compelled, for the reasons given above, to attribute this error to system C and not to the photographs. It seems unlikely that the error is dependent upon the declination, and I have not ventured to attempt to assign a cause to it. I have, however, pointed out that it seems to be rather greater along one stretch of the planet's orbit than along another of equal declination. Upon this Dr. Cohn remarks: »Was in aller Welt haben die Meridianbeobachtungen damit zu tun, ob der Planet Eros sich in diesem Teile seiner Bahn aufhielt oder in jenem?« To this I can reply only that Dr. Cohn has not appreciated my meaning. Is is true that the passage of the planet through the region cannot have been responsible for the error. But it is equally true that had the planet never passed that way the meridian observations would never have been made. The meridian observations were made to provide a system of standard stars, uniform and homogeneous, along the track of the planet. If it is found that along a certain portion of the track the system is erroneous, then the object of the meridian work has not been fulfilled. One may be justified in saying that the system fails in a certain locality, without being held to imply anything at all — passage of planet or anything else — as to the cause of that failure. The important thing is that the system does appear to fail at a critical point. I do not find that the evidence brought forward to prove this proposition has been weakened by Dr. Cohn's examination of it.

III. The alleged discontinuity between system C and system T.

Dr. Cohn lays great stress upon the contention that it is a priori impossible that there should be a sudden change in magnitude equation between system T and system C; and I agree with him that it is hard to see how such a change could come about. That such did occur, however, seemed to me to be indicated by the magnitude equations derived graphically, as explained above from the separate comparisons of the two visual systems with the photographic system. A further study of the figures, and of Dr. Cohn's criticisms, shows me that I ought to modify my conclusions on this point. I am able to agree with him to this extent, that the last columns of his Tabelle III make it clear that C—T has not changed between Group I and Group II by a linear function of the magnitude; and that the apparent linear change in the magnitude equation of C—T, which I derived by taking differences between my determinations for system T and system C, cannot be maintained.

How, then, did this misapprehension arise? The explanation seems to me to be a simple one — the same, in fact, as that which I put forward in section II. The magnitude equation in visual right ascensions is not a linear function of the magnitude; neither is the difference in the magnitude equation of Groups I and II of list I. That there has been a non-linear change is indicated by the last column of Dr. Cohn's Tabelle III, which shows, as he himself remarks, »eine kleine Kurve«. And we have already remarked that curvature in the magnitude equation will have different effects upon determinations made by his arithmetical and my graphical methods both assuming a linear equation.

I conclude, therefore, that there is a decided change in the magnitude equation of C—T as we pass from Group I to Group II; but that the change is not a linear function of the magnitude, and that consequently my numerical determination of it was wrong. And I trust that this explanation will be agreeable to Dr. Cohn, to whom I am indebted for the criticisms which led me to arrive at it.

To sum up: I am acutely sensible of the number of ways in which published photographic star places may be systematically wrong, for I have been working for many months at the star places obtained from the Eros plates. But this experience gives me the more confidence in maintaining that there is no evidence that the photographic right ascensions of the repère stars are affected by magnitude equation to any sensible degree; and I have given my reasons for thinking that Dr. Cohn's results in his tables V and VI may be explained otherwise.

I am therefore compelled to believe that the magnitude equation in C—Ph, for group I, is due to C and not to Ph. We are agreed about its existence, though not about its exact numerical amount. The mean of our results puts it at $0^{s}.015$ per magnitude.

Now this is a very considerable magnitude equation, not very different in size from that affecting system T or system L, but of opposite sign. The question that I had to answer before I could proceed with my Eros reductions was this: Is Dr. Cohn's system C so completely freed from magnitude equation that it will be worth while to undertake the labour of reducing to conformity with it all the plates that have already been reduced to other systems, and published. In the face of the results which I obtained for group I, it seemed to me that there could be no great advantage in adopting system C as the standard system. I was therefore compelled, with much regret, to abandon for the present the idea of utilizing the new fundamental system to which Dr. Cohn has devoted so much energy and skill.

Cambridge Observatory, 1906 Dec. 21.

Arthur R. Hinks.

Gelegentliche Beobachtungen auf der Kgl. Sternwarte in Kiel

mit dem 8-zölligen Refraktor angestellt von Dr. *E. Strömgren.*

I. Kleine Planeten.

Datum	M. Z. Kiel	$\Delta\alpha$	$\Delta\delta$	Vgl.	α app.	Parall.	δ app.	Parall.	Red. ad l. app.		*
					(2) Pallas.						
1905											
Juli 28	$12^h 55^m 25^s$	$+1^m\ 5^s.15$	—	17	$20^h 25^m 17^s.88$	$+0^s.03$	—	—	$+2^s.65$	—	1
28	12 54 23	+0 25.80	—	17	20 25 17.98	+0.03	—	—	+2.65	—	2
31	12 31 14	−1 53.22	—	18	20 22 56.16	+0.03	—	—	+2.67	—	3
Aug. 25	9 55 4	—	+0′ 6″.8	5	—	—	+11° 57′ 34″.6	+2″.3	—	+20″.7	4
25	10 29 26	+1 9.41	—	15	20 5 32.86	+0.02	—	—	+2.59	—	4
27	10 53 46	—	+7 52.2	1	—	—	+11 33 53.3	+2.3	—	+20.8	5