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method derived by KAPTEYN from a few particular cases of proper motion, because it seems to me that conclusions deduced from the consideration of only a few proper motions, chosen quite systematically, can hardly serve as criteria of a method which, as a matter of course, presupposes as data a great number of proper motions chosen at random. Finally attention must be drawn to an important point. In this paper (comp. § 1), following the method of KAPTEYN and others, I have considered separately the equations for τ and v.

Also in this modified form, as I have proved, AIRY's method leads to the right result. In AIRY's original method however, the three normal equations are composed from the equations for the two components τ and v. In this case there is but one equation of condition, viz.:

$$[m^2] \text{ or } [\tau^2] + \left[\left(v - \frac{h}{\varrho} \sin\lambda\right)^2\right] = \text{minimum},$$

i. e. "the direction and the amount of the parallactic motion must be chosen so, that *the sum of the squares of the* TOTAL *motus peculiares becomes a minimum.*" If this condition is applied to the instances given by KAPTEYN, it immediately becomes evident, that we arrive at the same Apex as KAPTEYN determines by applying the condition $[\tau = 0]$.

Astronomy. — *Reply to the criticism of* Dr. J. STEIN S.J. *by* J. C. KAPTEIJN.

It appears to be very probable that Dr. STEIN has not completely understood my paper in the proceedings of the February meeting of last year. This fact, and the fear that on the other hand I may also have misunderstood STEIN's reasoning (for one part at least of his paper this is certain) have led me to make my reply more circumstantial and elementary than might otherwise seem necessary.

With a view to the importance of the application of the method of least squares for the whole problem, it seems desirable to recall to mind the following elementary points relating to that method.

a). Let a system of equations of condition be given, thus:

$$\left.\begin{aligned} a_1 x + b_1 y &= n_1 \\ a_2 x + b_2 y &= n_2 \\ a_3 x + b_3 y &= n_3 \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1)$$
$$\ldots\ldots\ldots$$

where the n's represent observed quantities all having the same weight, and let the number of equations exceed the number of unknowns.

If now we take arbitrary values for x and y, and substitute these in the equations (1), there will remain certain residuals (Δ). A second set of values of x and y will be regarded as more probable than the first set, if it gives rise to a smaller value of $\Sigma\,\Delta^2$.

Therefore, in order to find the most probable values of x and y, the adopted values of these quantities must be made to vary until $\Sigma\,\Delta^2$, or, as is commonly said, the sum of the squares of the errors of observation [1]) reaches its minimum value.

b). The equations of condition must *not* be regarded as ordinary algebraical equations. It is not allowed to eliminate unknowns from them, to multiply some of them by a constant factor, etc.

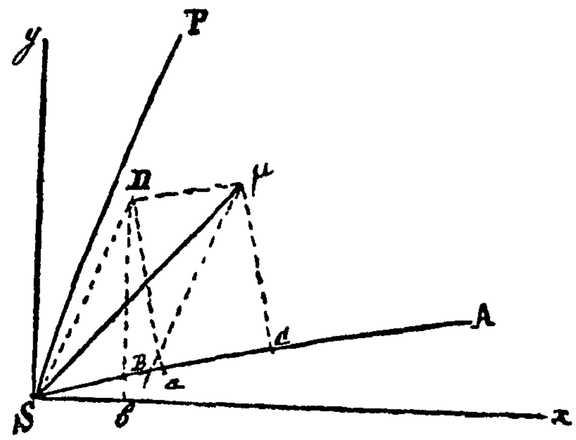

Fig. 1.

Let S be an arbitrarily chosen star, of which the observed proper motion is $S\mu = \mu$;

P the North pole of the heavens;

A (coordinates A and D, at distance λ from the star S) an arbitrarily adopted position of the Antapex;

$\frac{h}{\varrho}\sin\lambda$ an arbitrarily adopted value of the parallactic motion;

The position of A and the value of $\frac{h}{\varrho}$ are variable; they coincide with the *most probable* position of the Antapex and the *most probable* value of the parallactic motion, if certain minimum-conditions are satisfied.

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[1]) The expression is, of course, not literally correct. The true errors of observation, and consequently also the sum of their squares, are *constant* quantities, which can have neither maximum nor minimum. Exactly in the same way the expression $\Sigma\tau^2 = \text{minimum}$, is not literally correct when τ is defined as the projection of the p. m. on the line perpendicular to the direction of the Antapex. The true meaning of the first expression is explained above. That of the latter is entirely analogous.

v the projection SC of μ on SA;

τ the projection μC of μ perpendicular to the former;

The values of τ and v evidently vary with the position of the point A.

Let further x be an arbitrary *fixed* point on the celestial sphere; Sx the great circle joining S to that point and Sy the great circle perpendicular to Sx and let

$\chi =$ the angle PSA;

$\Gamma =$ „ „ PSx. Then, according to STEIN's notation,

$\varepsilon = \Gamma - \chi$.

$v_0, \tau_0 =$ the projections of μ on Sx and Sy respectively.

These projections do *not* vary with the position of A.

For the present purpose we can confine ourselves to the case that all the stars which are considered are at the same distance from the sun, so that $\frac{h}{\varrho}$ is constant.

We can then say that AIRY's method is based on the hypothesis (*Hyp. A*): that the projections of the *motus peculiares* may be treated as errors of observation, and that the most probable values of A, D, $\frac{h}{\varrho}$ are those for which the sum of the squares of the projections of the motus peculiares on two mutually perpendicular directions is a minimum.

2. This being premised, it is easy to form a judgment about the value of Dr. STEIN's criticism.

Everything depends on the choice of the directions on which the *motus peculiares* are projected.

AIRY takes the parallel and the declinationcircles;

STEIN takes the great circles through the *fixed* point x and the circles perpendicular thereto. He takes the point x in the neighbourhood of the most probable Antapex.

KAPTEYN takes the circles through the point A and those perpendicular to them.

In the first part of STEIN's criticism his own decomposition and mine are confounded. (Further on, e. g. in the enunciation of the problem: "if AIRY had formulated his question thus: . . .", there seems to be no such confusion). He says: "KAPTEYN chooses the directions towards a point near the Antapex and the direction at right angles to it."

The words relating to this point are however (Proceedings 1900, Febr. p. 362). "The direction from the star towards the Antapex and the great circle through the star at right angles to the former" while it is moreover abundantly clear from the contents of that paper generally, what the meaning is.

Reading STEIN's paper one gets the impression, that the author considers my decomposition either as impossible, or as identical to his own, if only care is taken to choose for the point x a point which coincides (or even approximately coincides) with the definitive most probable position of the Antapex. Neither of the two is true.

It might perhaps be considered a sufficient reply to the principal point of STEIN's criticism, to have pointed out this confusion. I think however that by going into somewhat fuller details the question as a whole will be more easily understood.

3. If the *total* sum of the squares of the projections of the motus peculiares on the two directions is considered, then the methods of STEIN and of KAPTEYN lead of course to the same result. For in that case, amongst all the different positions which can be given to the point A, that one will be (according to AIRY) considered as the most probable position of the Antapex for which [1])

according to KAPTEYN $\Sigma\,(\overline{Da}^2 + \overline{Sa}^2)$ minimum . . . (2)

according to STEIN [1]) $\Sigma\,(\overline{Db}^2 + \overline{Sb}^2)$ minimum . . . (3)

or

$$\Sigma \left\{ \tau^2 + \left(v - \frac{h}{\varrho} \sin\lambda\right)^2 \right\} \text{ minimum (KAPTEYN)} \quad . \quad . \quad (4)$$

and

$$\Sigma \left\{ \left(\tau_0 - \frac{h}{\varrho} \sin\lambda \sin(\Gamma - \chi)\right)^2 + \left(v_0 - \frac{h}{\varrho} \sin\lambda \cos(\Gamma - \chi)\right)^2 \right\} \quad . \quad (5)$$

minimum (STEIN)

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[1]) I have supposed that by STEIN's „*real*" Antapex is meant what has been defined above as the point A, and which might be called the *variable* Antapex. If this is *not* the case, then his reasoning seems to me unintelligible

respectively, both of which evidently may be reduced to

$$\Sigma\ \overline{SD}^2 \text{ minimum}^{1)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

However, if it is true that the two components of the *motus peculiaris* may be treated as errors of observation, then there is evidently no reason why it should not be allowed, so far as possible, to base the determination of the elements of the solar motion on *one* of these components only. That this is the generally adopted view, is apparent from the fact that generally a result is derived *both* from the right ascensions and the declinations separately.

AIRY himself says expressly: (*Mem. of the Roy. Astr. Soc. XXVIII)* „we must consider *the elements of motion* of the different stars as „being, to all intents, chance quantities, to be treated in the same „way as chance errors of observation," and a little earlier (which also shows clearly which meaming is attached by AIRY to the words *elements of motion*): „in the instances in which evidence as to proper „motion in one element fails, it enables us to take account of . . . . the „evidence derived from the proper motion in the other element alone."

4. To this, however, STEIN does not object. The real point at issue is this, that, according to my contention, such a determination from *one* component alonc may lead to an unacceptable solution, a solution which, for the particular case of stars situated at

[1]) To avoid all mistakes, even at the risk of falling into repetitions I give here *in extenso* the reasoning by which the formulae (2) and (3) are derived. The reasoning is essentially the same as that which was used in § 1 (*a*). Suppose for a moment that the Antapex is at the arbitrarily chosen point A (fig, 1), at a distance λ from S, and take the arbitrary quantity $\frac{h}{\rho}\ sin\ \lambda = \overline{SB}$ for the parallactic motion. In the supposition that the Antapex is at A, this parallactic motion is in the direction $\overline{SA}$ and the motus peculiaris $\overline{SD}$ is such that the resultant of $\overline{SD}$ and $\overline{SB}$ is μ. The projections of this motus peculiaris are, always in the same supposition,

for my decomposition Da and Sa,

for STEIN's decomposition Db and Sb.

If now we successively take other points for the Antapex and other values for $\frac{h}{\rho}$, then also the direction of the parallactic motion and the amount of the motus peculiaris and its components will change. According to AIRY that point A will be regarded as the most probable position of the Antapex, for which the minimum conditions (2) and (3) respectively are fulfilled.

one and the same point of the sky, does not generally fulfill the condition

$$\Sigma \tau = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

while STEIN asserts that this condition will be satisfied.[1]).

To refute my contention he shows that a determination from the components y, i. e. from the condition

$$\Sigma \left(\tau_0 - \frac{h}{\varrho} \sin \lambda \sin (\Gamma - \chi)\right)^2 \text{ minimum} \quad . \quad . \quad . \quad . \quad (8)$$

for stars at one and the same part of the heavens, does indeed lead to the condition (7).

I never denied this.

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[1]) The determination from both components *together* does satisfy the condition (7). For if the derivatives of the expression (4) with respect to $\frac{h}{\rho}$, A and D are equalled to zero, and if the values

$$\frac{\partial \tau}{\partial A} = v \frac{\partial \chi}{\partial A}, \quad \frac{\partial \tau}{\partial D} = v \frac{\partial \chi}{\partial D}, \quad \frac{\partial v}{\partial A} = -\tau \frac{\partial \chi}{\partial A}, \quad \frac{\partial v}{\partial D} = -\tau \frac{\partial \chi}{\partial D}$$

are introduced, we have for the determination of the three unknowns the three normal equations

$$\left[\left(\frac{h}{\varrho} \sin \lambda - v\right) \sin \lambda\right] = 0$$

$$\left[\tau v \frac{\partial \chi}{\partial A} + \left(\frac{h}{\varrho} \sin \lambda - v\right)\left(\frac{h}{\varrho} \cos \lambda \frac{\partial \lambda}{\partial A} + \tau \frac{\partial \chi}{\partial A}\right)\right] = 0$$

$$\left[\tau v \frac{\partial \chi}{\partial D} + \left(\frac{h}{\varrho} \sin \lambda - v\right)\left(\frac{h}{\varrho} \cos \lambda \frac{\partial \lambda}{\partial D} + \tau \frac{\partial \chi}{\partial D}\right)\right] = 0.$$

For a group of stars, situated at one and the same point of the heavens, these are reduced to the following:

$$\frac{h}{\varrho} \sin \lambda = \bar{v} \quad (\bar{v} = \text{mean value of } v)$$

$$[\tau] = 0.$$

I only showed that a determination from the components τ, i. e. from the condition

$$\Sigma\,\tau^2 \text{ minimum}, \quad \ldots\ldots\ldots\ldots \quad (9)$$

does *not* fulfill this condition.

STEIN's reasoning thus misses its aim.

As has been remarked above it seems to me that, in STEIN's opinion, the conditions (8) and (9) are identical, if only care is taken to choose the point x near enough to the position which will ultimately be found to be the most probable position of the Antapex. Perhaps the great difference between the two conditions is best seen by comparing the results which they give for the position of the Apex in the example which I used formerly to show the inadequacy of AIRY's principle.

Suppose, therefore, two stars at one and the same point of the sky, having equal proper motions $S\mu$ and $S\mu'$ forming an obtuse angle. We suppose (which can safely be admitted for the present purpose) that the magnitude of the parallactic motion is equal for the two stars (say $= SA = SA'$).

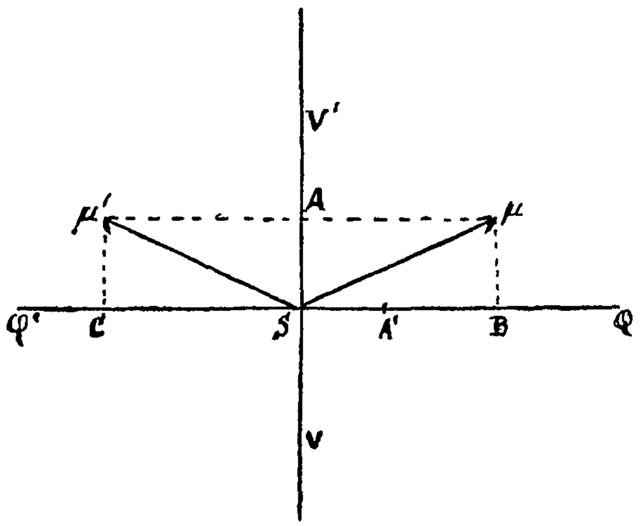

Fig. 2.

It is easily seen that in consequence of the conditions imposed the Apex must lie either on the line QQ' (in which case the parallactic motion is SA') or on the line VV' (in which case the parallactic motion is SA). The question is only on which of these two lines the Apex must be sought.

Now according to my contention, the condition (9) gives the line QQ', because:

$\Sigma\,\tau^2$ for Apex at $Q' < \Sigma\,\tau^2$ for Apex at V, i. e. because:

$$\overline{\mu B}^2 + \overline{\mu' C}^2 < \overline{\mu A}^2 + \overline{\mu' A}^2.$$

According to STEIN's contention the condition (8) gives the line

VV', because, all the projections being taken on the *same* line QQ', Σ proj.² mot. pec. for Apex at $V < \Sigma$ proj.² mot. pec. for Apex at Q', i. e. because

$$\overline{\mu A}^2 + \overline{\mu' A}^2 < (\overline{\mu A} - \overline{SA'})^2 + (\overline{\mu' A} + \overline{SA'})^2$$

The two contentions are nowise contradictory. They are in fact *both* true. They have nothing whatever to do with each other.

5. STEIN does not however confine himself to the assertion that for stars at one and the same point of the heavens (8) leads to (7), he also tries to prove directly, in two different ways, that (9) gives a determination which is identical to that by (8). In fact he tries to prove that both of these conditions lead to his conditions C.

It must already be evident from the above, that this proof must be impossible, and that consequently there must be an error in STEIN's argument.

The condition (9) evidently gives rise to the two equations

$$\left[\frac{\partial \tau}{\partial A}\, dA\right] = 0 \qquad \left[\frac{\partial \tau}{\partial D}\, dD\right] = 0.$$

In STEIN's *second* proof he simply derives the values of the derivatives $\frac{\partial \tau}{\partial D}$ and $\frac{\partial \tau}{\partial A}$ from the *equations of condition*

$$\tau_0 = \frac{h}{\varrho} \sin \lambda \sin \varepsilon \qquad v_0 = \frac{h}{\varrho} \sin \lambda \cos \varepsilon.$$

Here, therefore, he falls into the error, against which was warned in § 1 (*b*).

In the *first* proof of STEIN I cannot point out the main error, as I have been unable to follow the author's reasoning. [1]).

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[1]) Still I will remark that if, as is done by STEIN, in the equation

$$\left[\tau \frac{m}{\varrho} \cos q \frac{\partial \chi}{\partial A} + \tau \frac{h}{\varrho} \sin \lambda \frac{\partial \chi}{\partial A}\right] = 0$$

the first term may simply be left out of account, because "the motus peculiaris may be considered as an error of observation", then with the same right the other component $v - \frac{h}{\varrho} \sin \lambda$ of the motus peculiaris may be neglected, so that in the second term we may write v for $\frac{h}{\varrho} \sin \lambda$. The equation thus becomes no other than the *contested* one:

$$\left[\tau v \frac{\partial \chi}{\partial A}\right] = 0.$$

6. The above will be a sufficient refutation of STEIN's criticism. I think I have shown:

a. That STEIN's equation B cannot prove anything against my contentions, because *it is not to the point.*

b. That his direct criticism of my treatment of the question is erroneous.

I may however be permitted to make the following remark, which will perhaps explain how STEIN was led to his erroneous views in the matter. He asserts: the second component (i. e. in his decomposition the component v_0, in my decomposition the component v) "does not teach us anything about the position of the Apex", or again "The two other equations" (the normal equation by which the position of the Antapex would be determined) "are left out of consideration on account of their small weight."

If this was really true, the determination of the position of the Apex would, as well for my treatment of the problem as for his, depend *solely* on the components τ and $\tau_0 - \frac{h}{\varrho} \sin\lambda \sin(\Gamma - \chi)$ respectively and it would thus be possible, merely by a different way of decomposing the *motus peculiares*, to derive from *the same* equation (6) (for this is, as already remarked, equivalent to both (4) and (5)) *different* results for the position of the Apex, which is absurd. From this point of view there was thus every reason for the belief that either my treatment or his own was affected by some error, and it is explicable that the author, not finding this error in his own work, looked for it in mine.

As a matter of fact however the error is neither in STEIN's determination based on (8), nor in my own based on (9) but only in his contention about what can be derived from the second component. This component does actually give a determination of the Apex of which the weight, compared to that of the determination from the first component, is generally not at all small.

To show this it is sufficient to write down the neglected normal equations. They are:

$$\left[\sin\lambda_0 \cos\lambda_0 \left(\frac{\partial\lambda}{\partial A}\right)_0\right]\frac{h}{\varrho} + \left[\cos^2\lambda_0 \left(\frac{\partial\lambda}{\partial A}\right)_0^2\right]\frac{h}{\varrho}\,dA + \left[\cos^2\lambda_0 \left(\frac{\partial\lambda}{\partial A}\right)_0\left(\frac{\partial\lambda}{\partial D}\right)_0\right]\frac{h}{\varrho}\,dD = \left[v_0 \cos\lambda_0 \left(\frac{\partial\lambda}{\partial A}\right)_0\right]$$

$$\left[\sin\lambda_0 \cos\lambda_0 \left(\frac{\partial\lambda}{\partial D}\right)_0\right]\frac{h}{\varrho} + \left[\cos^2\lambda_0 \left(\frac{\partial\lambda}{\partial A}\right)_0\left(\frac{\partial\lambda}{\partial D}\right)_0\right]\frac{h}{\varrho}\,dA + \left[\cos^2\lambda_0 \left(\frac{\partial\lambda}{\partial D}\right)_0^2\right]\frac{h}{\varrho}\,dD = \left[v_0 \cos\lambda_0 \left(\frac{\partial\lambda}{\partial D}\right)_0\right].$$

For brevity's sake I will suppose that we consider a group of stars distributed uniformly over the whole sky. In that case we find at once that $\frac{h}{\varrho} dA$ and $\frac{h}{\varrho} dD$ are determined with the weights

$$\left[\cos^2 \lambda_0 \left(\frac{\partial \lambda}{\partial A}\right)_0^2\right] \quad \text{and} \quad \left[\cos^2 \lambda_0 \left(\frac{\partial \lambda}{\partial D}\right)_0^2\right]$$

If the same quantities are determined from the first component (see STEIN's equations *B*) they have, in the case here considered, the weights

$$\left[\sin^2 \lambda_0 \left(\frac{\partial \chi}{\partial A}\right)_0^2\right] \quad \text{and} \quad \left[\sin^2 \lambda_0 \left(\frac{\partial \chi}{\partial D}\right)_0^2\right].$$

Now

$$\frac{\partial \lambda}{\partial A} = -\sin \lambda \cos D \frac{\partial \chi}{\partial D} \qquad \frac{\partial \lambda}{\partial D} = \frac{\sin \lambda}{\cos D} \frac{\partial \chi}{\partial A}$$

It is thus clear at once that the weights of the two determinations are entirely of the same order of magnitude.

The same thing is true of the analogous component in my treatment. A determination of the position of the Apex actually derived from this component was published by me some time ago. (*Astr. Nachr.* N°. 3721 *Meth. IV*).

7. It may be urged that the objections made by me would not apply to AIRY's method if it were understood as is implied by the equation (4), i. e. if the sum of the squares of *both* components of the *motus peculiares together* were made minimum.

This is quite true. I now [1]) go even further, and express as my opinion that the equations, which would be derived in this way for the determination of the Apex, must be considered as very acceptable. The confidence which they would deserve could however *not* be derived from the hypothesis (*Hyp. A*) of AIRY. For, if it is admitted that STEIN's criticism is erroneous, it was shown by me that a legitimate application of this hypothesis may lead to unacceptable results.

This confidence must therefore rest on quite another basis. For me this basis is the following: the equations which are derived

[1]) *Now*, because at the time when I wrote my communication of Febr. 1900, my attention had not yet been drawn te BRAVAIS' method.

from (4) are identically the same as those which BRAVAIS derived, long before AIRY, from a quite different, mechanical, principle. (*Journal de Liouville* 8; 1843, p. 435).

8. With a view to the two last breaks of STEIN's communication, the wording of § 3 and the note to § 4 may seem somethat strange. The reason of this is that these two paragraphs failed in the M. S. which the author kindly permitted me to use.

The reply to the *last* part of this addition is however already contained in the above. The *first* part strikes us as very peculiar. The author thinks "it is superfluous to refute at large the objections against AIRY's method derived by KAPTEYN from a few particular cases of proper motion," *not*, as might be expected, because he has proved before that the treatment of these cases is *erroneous* [1]), but because "conclusions deduced from the consideration of only a few proper motions chosen quite systematically can hardly serve as criteria of a method . . .".

It looks as if the author is not very strongly convinced of the stringency of his own proof. Moreover the facts are not quite fairly represented.

The special cases to which the author refers were treated, not as *proof* of any particular thesis, but only as *examples* to illustrate the different conclusions to which the conditions $[\tau] = 0$ and $[\tau v] = 0$ can lead. As such I do not think they are badly chosen. That, in my opinion, especially the first example incidentally has some considerable direct power of proof, I will certainly not deny. The absurdity to which a treatment of this example by AIRY's method leads is very much of the same sort as that which would be involved [2]), if from the indication of two clocks showing 8 minutes to and 8 past twelve respectively it was concluded that most probably the time is either a quarter to or a quarter past twelve, but on no account twelve o'clock.

Does Dr. STEIN really mean to say that there is nothing in such a result that throws any doubt on the method by which it was obtained?

1) For STEIN thinks he has shown that the condition $[\tau^2] = 0$ for stars at one and the same part of the sky is equivalent, *not* to $[\tau v] = 0$, but to $[\tau] = 0$, which both according to *his* and to *my* opinion, is the correct solution.

2) In order that my reply might still be printed in the Proceedings of October, the dutch text had to be written within a few hours after I got sight of the last paragraphs in STEIN's criticism. Owing to this haste the illustration contained in the last lines was not so well chosen as I could wish. I have taken the liberty to remedy to this defect in the translation.