Astronomy. — "*On a peculiar anomaly occurring in the transit-observations with the Leiden meridiancircle during the years* 1864—1868. By E. F. VAN DE SANDE BAKHUYZEN and J. E. DE VOS VAN STEENWIJK.

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1. *Introduction.*

Not many years after the mounting of the meridiancircle at the Leiden-observatory and the completion of a number of auxiliary apparatus according to designs by KAISER a beginning was made with the observation for an extensive Fundamental Catalogue. For this purpose a list of 166 stars was drawn up (mainly the list of the Nautical Almanac as far as visible at Leiden) containing 24 circumpolar stars which were to be observed in both culminations. The observations began in February 1864 and were considered as completed in July 1868. The number of observations had been 15870 of which about 12800 pertained to the fundamental stars and 579 to the sun.

All these observations were published — although for the greater part unreduced — in 1868 in the Annals of the observatory, Vol. I. Soon afterwards, however, a beginning was made with the reduction of a limited number of the declination-observations, and this reduction, with a full discussion of the results, which gave rise to important investigations by KAISER and his collaborators, appeared in 1870 in Volume 2. A complete reduction was even then designed, but — apart from an addition to the results of Vol. 2 according to calculations by Dr. VALENTINER — it was not till 1876 that the project, including a complete reduction of all the declination-observations of the fundamental stars and of the sun, was actually taken in hand. In 1879 the work was for the main part completed and a short account of it was given by the first of us in his thesis for the doctorate published in the same year containing a determination of the Obliquity of the Ecliptic according to the Leiden declinations of the sun. The complete reduction of the declination-observations of the stars was then published in 1890 in Vol. 6 of the Annalen, the discussion of the final results, although for the greater part completed even then, has not yet been published.

Whereas the reduction of the declination-observations of the years 1864—1868 had thus been completed within a comparatively short period, the observations of the right-ascensions on the other hand remained for the greater part unreduced. Apart from the reduction

of the observations of planets and comparison-stars, we can from the years following only record a reduction carried out by KAISER and published in Vol. 2 of the Annalen of the transit-observations made in September 1868 for the purpose of the longitude-determination with Brussels In this paper KAISER gave a number of important remarks on the reduction-elements of the meridiancircle, but at the same time he showed that the observations proved an anomalous behaviour of the collimation-constant, which could not be explained at the time.

Some years later the first of us investigated for the Leiden meridiancircle the influence of an eccentric illumination of the field of the telescope, a question which was much discussed at the time and which, although very simple and although the true nature of the disturbance originating from it, when the adjustment of the ocular is incorrect, had been made clear by CARLINI half a century before, had raised a considerable amount of dust. With different positions of the ocular pointings were made on one of the meridian marks, using for the bisections the apparent position of the micrometer-thread outside the mark; for on the mark the central illumination by the mark itself exceeds the eccentric field-illumination and that part of the thread is always seen in its true position. From the variations of the readings for different positions of the ocular it was possible to deduce the position of the mirror in the cube of the telescope which reflected the light for the illumination of the field and, on the instrument being dismounted in 1876, the actual position of the mirror was found to agree exactly with the calculation. In the alterations of our meridiancircle carried out by REPSOLD in 1876—77, amongst other things the field-illumination was modified and made exactly central.

These results naturally suggested to E. F. B., that the disturbance of the collimation-constant occurring in September 1868 would find its explanation in the same phenomenon. The sign of the error arising from it, like the influence of a normal collimation-constant, must change on reversal of the instrument and one would have to suppose, that in the observations for the longitude-determination, especially those of the pole-star, (all transits were at that time observed by the eye-and-ear-method) the ocular had been pushed in too far. This did not appear improbable in itself, as the thread seemed if anything to become even a little finer, when the ocular was pushed in a little too far.

It thus became the question, whether disturbances of that kind had to be suspected during the whole of the period 1864—68. In

that case the computation of the absolute azimuth especially would become much more difficult and the importance of the observations for the determination of absolute Right-Ascensions might perhaps be materially reduced. Fortunately a few years later in 1882, when the first of us at the request of NEWCOMB undertook a revised reduction of the Leiden observations of Mercury, which naturally involved some investigations, albeit provisional ones, on the instrumental constants, it was found that during the whole period of the Fundamental Observations 1864—July 1868 somewhat considerable influences of the eccentric illumination could not be detected.

The old series of fundamental R. A. observations thus continued fully to deserve an accurate systematically planned reduction and discussion, but the great extent of the work was gradually giving rise to the fear that it would hardly be undertaken any more, when last year the second of us resolved to undertake the task, at least for an important part, notwithstanding the considerable difficulties arising from the present circumstances which compel him to reside outside Leiden.

The working plan to be followed was then agreed upon by us. The main object would be, assuming the relative R. A. of the fundamental stars as given by AUWERS's New Fund. Cat. or by that of NEWCOMB, to deduce from the observations of the sun a new determination of the Equinox for 1865. The declination-observations of the sun, as recalled above, had already been discussed by the first of us a long time ago, and consequently by this procedure data would be obtained regarding the advantages and disadvantages of the method of separate treatment of the two coordinates of the sun.

The work proper would be preceded by a new and rigid investigation of all the reduction-elements and all errors and peculiarities of the instrument and the observers, for which purpose the investigations formerly instituted at Leiden could serve as a first approximation, and the results of two special investigations regarding the value of the divisions of the level used and the irregularities of the pivots could immediately be utilized.

In the first volume of the Annalen the means of the times of transit reduced to the middle thread are given for all the observations, while in the introduction on page LXXXVIII are mentioned the values of the thread intervals which were used in the reduction, the total period having been divided into six parts, for which different values were assumed. It soon appeared to us, however, that in view of the degree of accuracy now aimed at, and the fact that

the times of transit of each of the sun's limbs are usually based on not more than three threads a closer investigation into this question would also have to be instituted beforehand. During this investigation a peculiar anomaly showed itself, the true nature of which was at first not recognized by us for a long time and, as a similar anomaly may also have occurred in other series of eye-and-ear-observations, it seemed to us of some importance to make a separate short communication on this point.

2. *Deduction of the thread intervals. Peculiar divergences in the observation of the transits.*

The reticule of the meridian-circle at that time contained 7 vertical threads. It is probable, that special not very extensive series of observations were carried out each time to deduce the thread-intervals, as given in volume I, although sometimes the method of deduction followed is not quit clear. The motives for the separation of the periods are not always equally clear either. Sometimes some perturbation had taken place or the reticule had been cleaned, but in one case at least, that of the separation of the 6th from the 5th period, the ground for it cannot be recognized in the least.

However that may be, it seemed to us necessary to investigate the matter more closely by testing the intervals assumed each time on large numbers of transits of the fundamental stars observed, using the results of both observers and also, as much as possible, those obtained in both positions of the instrument designated in Leiden as clamp East and clamp West. In later years it had appeared at Leiden again and again, that small systematic differences may occur here dependent upon observer and position of the instrument.

Using the times of transit reduced with the thread-intervals as assumed before, for each observation the differences middle thread—side thread were formed, which we shall call $[\Delta]$. Further calling the corrections of the distances assumed for the threads I to VII (I is the one nearest the clamp) Δ I, ΔII, etc., if there are no disturbances, we must have

Thread	*Clamp West*	*Clamp East*
I	$[\Delta] = +\Delta$ I	$[\Delta] = -\Delta$ I
II	„ $+\Delta$ II	„ $-\Delta$ II
III	„ $+\Delta$ III	„ $-\Delta$ III
V	„ $-\Delta$ V	„ $+\Delta$ V
VI	„ $-\Delta$ VI	„ $+\Delta$ VI
VII	„ $-\Delta$ VII	„ $+\Delta$ VII

In that case, i e. if the times of transit over all the threads are estimated in the same way, independently of their position and order, the condition for each thread would be:

$$[\Delta] \text{ Cl. West} + [\Delta] \text{ Cl. East} = 0.$$

It was already mentioned, that more than once at Leiden systematic differences were found, so that this relation did not hold accurately Such differences have especially occurred with the extreme threads and are most probably due to the first thread being observed in an abnormal manner (we are referring here to chronographic observations). Hereby the sums for the two extreme threads became different from zero by the amount of the anomaly. As an instance, in Dr. PANNEKOEK's observations of the years 1899—1902 the sums for the extreme threads were -0^s032 and -0^s035 respectively, whereas for the other threads the greatest value of the sum was 0^s025, and as a rule it was much smaller.

In the present investigation of the thread-intervals we confined ourselves to such stars as had been observed on all 7 threads and even with this restriction, at least for the four largest periods I, II, V and VI, abundant material was available. As in this communication it is our object more especially to bring forward some general conclusions, we shall exclusively investigate the results of those 4 periods which are the only ones suitable for that purpose. We shall not communicate the results as originally derived, but immediately apply two special modifications which were found to be advisable to the second of us in the course of a preliminary investigation. These modifications consist in the first place in confining ourselves to such stars as were observed *during the night* and had also been observed *in declination*, and secondly in dividing the V_{th} period into two sub-periods Va and Vb, the division being formed by a cleaning of the reticule on January 29, 1866. Although it appeared later on that Vb and VI could be united, we shall for the moment keep these periods separated.

As mentioned above, for each observation the quantities $[\Delta]$ were formed and subsequently the sums $[\Delta]_W + [\Delta]_E$, when the totally unexpected results were obtained which are contained in the following table. The quantities are expressed in thousandths of a time second, the number of observations used in each position being added each time.

The sums will be seen to reach unexpectedly large values and to be roughly the same for all threads and also for both observers. A considerable part of the oscillations which appear may be ascribed

Sums $[\Delta]_W + [\Delta]_E$

Observer Kam.

	Period I	Period II	Period V*a*	Period V*b*	Period VI	Mean II to VI
Numb. of obs.	94—113	58 42	62 100	270—333	136 132	
Thread I	—49	— 93	— 90	—103	—79	— 91
„ II	—44	— 74	— 102	—102	—78	— 89
„ III	—19	—100	— 76	— 89	—33	— 74
„ V	— 2	—104	—103	— 88	— 99	— 98
„ VI	—40	—116	—129	—116	—91	—113
„ VII	—34	—124	—145	— 92	—95	—114
1st Half	—37	— 89	— 89	— 98	—63	— 85
2nd „	—25	—115	—126	— 99	—95	—108
Together	—31	—102	— 108	— 98	—79	— 97

Observer v. Hennekeler.

	Period I	Period II	Period V*a*	Period V*b*	Period VI	Mean
Numb. of obs.	148—166	58—90	173 - 175	225—187	111—73	
Thread I	— 70	— 54	— 87	— 74	— 46	— 66
„ II	— 71	— 86	—109	— 64	—46	— 75
„ III	— 94	— 72	—101	+ 11	—31	— 57
„ V	— 98	— 50	— 87	— 49	—24	— 62
„ VI	— 86	-103	— 135	—136	—98	—112
„ VII	—104	— 85	—130	— 103	— 73	— 99
1st Half	— 78	— 71	— 99	— 42	— 41	— 66
2nd „	— 96	— 79	—117	— 96	—65	— 91
Together	— 87	— 75	—108	— 69	—53	— 78

to the accidental error of the observations. The investigation of the thread-intervals namely gives $\pm 0^s141$ as mean error of a transit over one thread for Kam and $\pm 0^s131$ for Hennekeler, and thus as that of a thread-interval for K. $\pm 0^s200$ and for H. $\mp 0^s185$,

for a value of $[\Delta]$ deduced from 100 observations the m.e. is thus for K $\pm$ 0·020, for H $\pm$ 0·018 and for a value of $[\Delta]_W + [\Delta]_E$, if in each position of the instrument 100 observations are used, it is for K $\pm$ 0^{s}028 and for H $\pm$ 0^{s}026.

Notwithstanding the pretty considerable m.e. the following facts stand out clearly. For KAM in the first period, i e. in 1864, the sum W + E was relatively small — 0^s.031, whereas in the following years it remained very constant at about — 0^s.097. For HENNEKELER no distinct change can be seen, the mean value for him is 0^s.078 and therefore possibly a little smaller than K.'s value in 1865—68. Moreover for both observers the value may be somewhat larger for the threads V—VII than for the threads I—III.

However that may be, the chief result is undoubtedly the constant amount for all the threads. The cause of this had to be inquired into, in order thereby to deduce the influence on the determination of the thread-intervals. The value being as large as it is, it was not allowable without further investigation to take for the correction of the thread-intervals the mean of the results in the two positions. Before proceeding, however, the meaning of the results obtained may first be established. They depend upon the corrections which the intervals previously assumed require, and therefore also upon those former values themselves The question might therefore arise, whether the anomalies found above, i.e. the values of the sums as differing from zero, may not have their origin in the results of the original investigation and therefore in the nature of the material used at the time. But it will be seen at once that this cannot be the case, e.g. thus: whereas before the reduction was made with one definite set of distances, errors in it cannot give rise to the differences between the corrections now found in the positions Cl. E. and Cl. W.

At the same time, although no conclusion may be drawn from the fact that at present stars were used which were also observed in declination and formerly stars, observed in R. A. alone, still it could not but appear at once probable, that the anomaly found would be connected with the bisection by the horizontal thread (the material originally used by the second of us also consisted, for the great majority, of stars observed in decl.) and it was then natural to look for the cause in a peculiar personal error of the kind as was previously shown to exist in the observations of the first of us, namely that after bisecting the star, probably owing to the diminished brightness, the times of transit were observed later. The retardation in his case amounted to about 0^s.03.

If we now assume, that the bisection, which with a very few

exceptions took place in the immediate neighbourhood of the middle thread, always took place beyond it, calling the change in the personal error by the bisection Δ', such that a positive value means an accelerated observation, we shall have:

Thread	*Clamp West*	*Clamp East*	$W+E$	$\pm\frac{W-E}{2}$
I $[\Delta]=$	$+\Delta$I	$-\Delta$I $+\Delta'$	$+\Delta'$	ΔI $-\frac{1}{2}\Delta'$
II	$+\Delta$II	$-\Delta$II $+\Delta'$	„	ΔII $-\frac{1}{2}\Delta'$
III	$+\Delta$III	$-\Delta$III $+\Delta'$	„	ΔIII $-\frac{1}{2}\Delta'$
V	$-\Delta$V $+\Delta'$	$+\Delta$V	„	ΔV $-\frac{1}{2}\Delta'$
VI	$-\Delta$VI $+\Delta'$	$+\Delta$VI	„	ΔVI $-\frac{1}{2}\Delta'$
VII	$-\Delta$VII $+\Delta'$	$+\Delta$VII	„	ΔVII $-\frac{1}{2}\Delta'$

By a change in the personal error after the bisection the sum W + E thus obtains a constant value differing from zero for all the threads and our result might be explained by assuming that after the bisection H. estimated the times of transit $0^s.08$ too late, K at first in 1864 $0^s.03$, later on $0^s.10$ too late. The signs as found are such as might be expected according to the suggested explanation, whereas, if we assume that the bisection was made before the middle thread, our result (see the discussion further down) would mean an accelerated observation in consequence of the bisection To begin with, therefore, the above supposition appeared to us a very probable one, but on further consideration we hesitated to adopt it definitely, especially in view of the relatively large retardation which would have to be assumed and the great influence which the retardation, as shown by the last column of the table, exercises on the derivation of the intervals themselves. For the quantities $\pm\frac{1}{2}$(W—E) (+ for threads I—III and — for V—VII), which in normal observations immediately give us the corrections of the thread-intervals, must now be corrected by half the retardation and, by working out the more general supposition that in p cases the bisection was made before and $1-p$ cases after the middle thread, it appears that the value which will be found for the retardation itself entirely depends on the supposition made as to the moment of the bisection. The result from the general supposition is given in the following table:

Thread	*Clamp West*	*Clamp East*	$W+E$	$\pm\frac{W-E}{2}$
I $[\Delta]=$	$+\Delta\mathrm{I}-p\Delta'$	$-\Delta\mathrm{I}+(1-p)\Delta'$	$(1-2p)\Delta'$	$+\Delta\mathrm{I}-\frac{1}{2}\Delta'$
II	$+\Delta\mathrm{II}$ „	$-\Delta\mathrm{II}$ „	„	$+\Delta\mathrm{II}$ „
III	$+\Delta\mathrm{III}$ „	$-\Delta\mathrm{III}$ „	„	$+\Delta\mathrm{III}$ „
V	$-\Delta\mathrm{V}+(1-p)\Delta'$	$+\Delta\mathrm{V}-p\Delta'$	„	$+\Delta\mathrm{V}$ „
VI	$-\Delta\mathrm{VI}$ „	$+\Delta\mathrm{VI}$ „	„	$+\Delta\mathrm{VI}$ „
VII	$-\Delta\mathrm{VII}$ „	$+\Delta\mathrm{VII}$ „	„	$+\Delta\mathrm{VII}$ „

The thread-intervals must therefore in each case be corrected by half the amount of the retardation (or acceleration), but the retardation itself cannot be determined, not even as to its sign, without making arbitrary suppositions, and, if in half the cases the bisection was made before m and in the other half after m, the retardation has no influence on W + E and remains completely indeterminate.

We had thus come to the conclusion, that it would be hardly possible, even from the large material of fundamental stars, to derive accurate values for the reduction to the middle thread, when we discovered that in the last two periods a considerable number of transit-observations were available, which were not combined with observations of declination and which might throw light on the problem before us A separate investigation of these observations not only revealed the true nature of the anomaly, which appeared to be entirely different from our former supposition, but at the same time showed that its influence could also be completely, or at least for by far the greater part, eliminated in the remaining periods. This investigation may be now detailed

As already mentioned, it had been found that neither a priori nor a posteriori any ground existed for separating the two periods Vb and VI For the period Vb the corrections to be applied to the provisional intervals V had been calculated, but naturally from these could be derived those other ones which would have been found, if the preliminary intervals VI had been used as the basis.

The observations of non-bisected stars in the now extended period VI, Jan 1866—1868, may be divided into 4 classes· (1) fundamental stars 1866—April 1867, (2) fundamental stars April 1867—1868, (3) stars observed in 1867 which had been used in the longitude-operations with Gottingen, (4) observations for the determination of the longitude Leiden—Brussels. Each class comprises between 50 and 100 observations in each position of the instrument for each of the two observers. The longitude-determination with Brussels was,

however, carried out by KAM alone and the Göttingen-stars were only observed in the position clamp West. For this reason it was ultimately considered advisable not to use the last-mentioned set. We begin by giving the values of the half sums $\frac{1}{2}([\Delta]_W + [\Delta]_E)$· it will appear immediately, why we now divide by two.

$\frac{1}{2}([\Delta]_W + [\Delta]_E)$. Non-bisected stars.

Thread	Observer KAM				Observer HENNEKELER		
	1866-67	1867—68	L. Brussels	Mean	1866—67	1867—68	Mean
I	—14	—22	+ 6	—10	+10	—10	0
II	+ 2	— 8	—24	—10	— 2	+ 8	+ 3
III	+ 2	—12	—20	—10	+ 8	+22	+15
V	—20	—15	—28	—21	0	+12	+ 6
VI	— 9	—14	—18	—14	+ 4	—22	— 9
VII	—16	—11	—33	—20	+ 4	—19	— 8
Mean	— 9	—14	—20	—14	+ 4	— 2	+ 1

It appears, that with HENNEKELER the observation of all the threads was accomplished without any abnormality; with KAM an anomaly seems to show itself, particularly in the later observations, in the same sense as for the bisected stars, but of a much smaller amount. It is therefore very probable, that in this case the combination of the two positions of the instrument will yield practically correct thread-intervals. By taking the mean of the results Cl. W. and Cl. E., i.e. by forming the half-differences $\pm \frac{1}{2}([\Delta]_W - [\Delta]_E)$, the following results are obtained:

Corrections to the preliminary thread-intervals VI according to the non-bisected stars.

Thread	Observer KAM				Observer HENNEKELER			$\frac{K+H}{2}$
	1866—67	1867—68	L. Brussels	Mean	1866 67	1867—68	Mean	
I	+36	+24	+28	+29	+ 9	+ 6	+ 8	+18
II	—24	— 2	—12	—13	—46	—26	—36	—24
III	—10	—34	—50	—31	—24	—20	—22	—26
V	— 4	+26	+19	+14	+19	+ 5	+12	+13
VI	+12	+40	+24	+25	+38	+38	+38	+32
VII	0	+29	+10	+13	+38	+14	+26	+20

As the table shows, the final results for the two observers agree mutually within the limits of the errors of observation; this proves again that the results obtained by this method must be fairly accurate.

We may thus use these results for the purpose of subtracting from the total values of $[\Delta]$ found for the "bisected night-stars" those parts $[\Delta]_1$ which depend upon the thread-intervals and thus obtaining *for the two positions of the instrument separately* the portions $[\Delta]_2$ which are the consequences of the disturbance. The results are given in the table below. The results for the threads I to III and V to VII respectively have been combined, but the two sub-periods Jan. 1866—April 1867 and April 1867—July 1868 have still been kept separate.

	Values of $[\Delta]_2$					
	Clamp East			Clamp West		
	66—67	67—68	Together	66—67	67—68	Together
	Observer Kam					
Threads I—III	—33	—34	—34	—65	—29	—47
„ V—VII	—25	—35	—30	—74	—60	—67
Together	—29	—34	—32	—70	—45	—57
	Observer Hennekeler.					
Threads I—III	—19	—31	—25	—23	—10	—17
„ V—VII	—63	—41	—52	—33	—24	—28
Together	—41	—36	—38	—28	—17	—22

The table shows that, contrary to what was originally supposed, it is not the times of transit over the second half of the reticule only that are abnormal, but that for *all the side threads* the distances from the middle thread show a deviation of approximately the same amount in the same sense, i.e. so that the side-threads appear all shifted to the same side. If this be the case, it is undoubtedly simplest to suppose, that the observed time of transit over *the middle thread itself was disturbed.*

This thread would have been observed

by Kam	in 1866—67	0s050	too early
	in 1867—68	.040	„ „
	On the average	0s045	„ „
by Hennekeler	in 1866—67	0s034	„ „
	in 1867—68	.026	„ „
	On the average	0s030	„ „

Independently of whether the amounts of the disturbances for all the threads be equal or not, they are now given by $\frac{1}{2}([\Delta]_W + [\Delta]_E)$. and not as in the previous supposition by the sums themselves, so that smaller, i.e. less improbable values may now be ascribed to them, but we may go farther and conclude that it is most probable, that an abnormality in the observation of the middle thread has been the main cause of the anomaly found This we may perhaps imagine as having occurred in the following manner. With eye- and ear-observations the observer forms a mental image of the position occupied by the star at the last preceding second. While his attention is now partly occupied by the bisection, it is possible that this image is derived from too late a moment and this would lead to too early an estimate of the time of transit.

If the abnormal observation of the transit over the middle thread is the only source of disturbance, the expression $\pm \frac{[\Delta]_W - [\Delta]_E}{2}$ must give us. the true values of the thread-intervals with the bisected stars also This may first be tested with the stars in the same period VI, which were also observed in declination.

Corrections to the preliminary thread-intervals VI according to the bisected stars.

Thread	Kam			v. Hennekeler			$\frac{K+H}{2}$
	1866–67	1867–68	Mean	1866–67	1867–68	Mean	
I	+ 6	+26	+16	+24	+23	+24	+20
II	−30	−24	−27	−17	−11	− 14	−21
III	−56	−26	−41	−44	−14	−29	−35
V	+28	+24	+26	−24	− 8	−16	+ 5
VI	+53	+38	+46	+ 2	+27	+14	+30
VII	+58	+40	+49	+42	+20	+31	+40

Here and there (with threads V and VI) it might look as if systematic differences exist between K and H, but on comparing the results obtained with those derived from the non-bisected stars, this becomes very doubtful and the final results from the two series are in very good agreement with each other.

This is highly important, as we may now expect, that in the other periods which contain but few non-bisected stars the values of $\frac{[\Delta]_W + [\Delta]_E}{2}$ for the bisected stars will represent the deviation in observing the middle thread, and further, that other disturbances are small and that the half-differences $\pm \frac{[\Delta]_W - [\Delta]_E}{2}$ will give us the true thread-intervals. The values of the deviations for the periods I, II and V*a* are found immediately by taking half of the values given before (page 350). As regards the first period we may, however, utilize another important series of observations, which has not been discussed so far, namely those of stars *observed in the day time and also observed in declination*; observations of that kind occur in fairly considerable number during this period (K. E. 79, K. W. 61, H. E. 82, H W. 103). They give the following results, where for the sake of comparison those according to the night-observations have been added.

Period I. Deviation of the Middle thread.

	Kam.		Hennekeler.	
Thread.	Day	Night	Day	Night
I to III	— 9	— 19	— 31	— 39
V to VII	— 19	— 13	— 46	— 48
Together	— 14	— 16	— 39	— 44

Corrections to the thread-intervals.

Thread.	Kam	Hennekeler	½ (K + H)	Night observ.
I	+ 37	+ 8	+ 22	+ 28
II	+ 2	— 21	— 10	+ 3
III	+ 13	— 8	+ 3	+ 6
V	— 44	— 38	— 41	— 34
VI	+ 10	+ 24	+ 17	+ 14
VII	+ 16	+ 31	+ 24	+ 30

It appears from the tables, that the agreement between the results from day- and night-observations is in every way satisfactory. Both as regards the deviation and the thread-intervals it is closer than might have been expected. Where before for Kam the deviation was

found much smaller in the first period than later on, the day-observations give again the smaller value, and we may thus assume with great probability, that in 1864 this deviation must have been smaller for him than in the later periods.

The very close agreement between the results of the day- and night-observations is of great importance from another point of view, as it proves, that an influence of the *eccentric field illumination* cannot be present to an appreciable amount in these observations. In examining this effect it is found, that it cannot be exactly the same for all the threads. Towards the side of the illuminating-mirror in the cube the effect becomes smaller, to the other side it increases, and the thread-intervals must therefore be found *too small* or *too large* on *both* sides of the middle. The former will occur with the ocular pushed in too far and this independently of the side from which the field-light comes. In the original arrangement of the Leiden-instrument the direction of incidence of the field-light, which came from the side of thread VII, made in the middle an angle of 2° 20′ with the optical axis. At the extreme threads on both sides this angle was 14′ smaller or larger and here the relative effect was therefore 10 % of the total effect at the middle thread.

If the absolute effect for the middle tread was 2″.0 (corresponding to the ocular being pushed in too far by 0.6 mm.), as must have been the case in the longitude-determination with Brussels, the relative effect for the extreme threads is found to be 0″.2=0^s.013, which is just observable. By a comparison of the results *Cl.E.* and *Cl.W.* the effect cannot be revealed; that of the results of day-and-night-observations in period I shows, that, in accordance with what was derived from other facts, it was probably inappreciable.

Our investigation thus makes it extremely probable, that the observers KAM and HENNEKELER in observing the stars which they observed also in declination, *made an abnormal estimate of the time of transit over the middle thread*, in exactly the same way in observations in the day-time as at night. Taking this into account very accurate values for the thread-intervals may be derived from their observations. The changes which have occurred in these intervals in the course of 5 years 1864—68 are found to be small as a rule, particularly in the case of the middle thread, notwithstanding frequent catching of the movable thread, by which the point of coincidence sometimes changed considerably.

We subjoin the amounts found for the deviation in the transit over the middle thread for the various periods.

Deviation of the Middle thread.

	KAM	HENNEKELER
Period I	— 15	— 42
II	— 51	— 38
Va	— 54	— 54
VI	— 45	— 30

For KAM the value is distinctly smaller in the first period, whereas in the case of HENNEKELER there is no distinct evidence of a change.

Physics. — *"The Increase of the Quantity a of the Equation of State for Densities Greater than the Critical Density"*. By Prof. J. D. VAN DER WAALS.

(Communicated in the meeting of June 24, 1916).

Already in 1873 when drawing up the equation of state I realised that it must follow from the derivation from the kinetic theory that the quantity b would have to decrease with diminishing volume. Accordingly I stated explicitly already then that b, which represents 4-times the molecular volume at infinite volume, would have to decrease. Afterwards I came to the opinion that it would have to diminish to twice that volume, or even to a still smaller value. And that b decreases is pretty generally accepted at present. The cause of this decrease is, however, in my opinion, often sought in a wrong direction, namely in the real diminution of the molecule. I will not return to this point at present. Not until 1910 did I express my doubt of the invariability of the quantity a (These Proc. XIII p. 107). I ascribed the variation of this quantity to what I called then: "quasi association or molecule complexes". That I looked for the cause for this increase of a in what acts as an enlargement of the molecule, as is the case for real association, I still consider correct. But I treated these possible complexes approximately as if we had to do with real association, this I should certainly not do now. In subsequent calculations in the course taken then I became more and more convinced that the result could not be valid even as an approximation. And in the following pages I will unfold the idea, which has more and more forced itself upon me, according to which the cause for this increase of a must be sought without molecule complexes being necessary, but as a consequence of the ordinary regular molecular movement of molecules which have