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the modus operandi (concentration of the gelatine and of the salts) greatly different dimensions may be expected. PICTON and LINDNER who among others have proved the extremely small conductivity of the aqueous colloidal solution of arsenious sulphide, admit four different „grades" of division of this substance[1]. It will be found impossible however to make clearly defined distinctions in this respect, a gradual change in the size of the particles being much more probable.

The study of the phenomenon of the internal light-reflexion and fluorescence, that shows itself more or less distincly in several substances formed in gelatine, may perhaps lead to further insight; these optical phenomena are also found in bodies as milky colored glass, that also contains invisible, extremely small particles. Others however of the colloid solutions in gelatine are not only perfectly transparent, but do not show any reflection of the light or fluorescence; it may therefore be admitted that the division of matter in gelatine approaches that of bodies soluted in water. In view of phenomena as those stated here, the idea of being soluted seems to lose its sharpness of outline.

Equally interesting are the chemical reactions, which are the effect of diffusion, on the substances formed in gelatine and in which one may be passed into the other. If a Na Cl-gelatine is surrounded by a $Ag_2 Cr O_4$ gelatine the Na Cl diffuses from the former gelatine into the latter, the colloidal $Ag_2 C_r O_4$ being not (or very little) diffusable, while it is known (f i. by experiments of GRAHAM and HUGO DE VRIES) that the salts dissociated into ions diffuse as quickly in gelatine as in water. Now when the Na Cl ions penetrate in the bright red $Ag_2 Cr O_4$ the latter is converted into Ag Cl which, equally colloidal in solution, remains transparent and spreads in the form of a ring.

Astronomy. — *On the motion of the Pole of the Earth according to the observations of the years* 1890—1896. By Dr. E. F. VAN DE SANDE BAKHUYZEN. (Communicated by Prof. H. G. VAN DE SANDE BAKHUYZEN.)

1. In treating the meridian-observations made at Leiden in the last 20 years, it was necessary to arrive at a knowledge as exact as possible of the change in latitude during that period and, before investigating the Leiden observations in this respect, it seemed de-

[1]) Jou . Chem. Soc. **61**, 137, **67**, 63, **71**, 568.

sirable first to make endeavours to derive information from other sources.

Since in 1891 the 14-monthly period in the motion of the Pole of the earth had been found by CHANDLER, many computations have been made on that subject by him. On the other hand the chief series of observations of the last 50 years were submitted by H. G. v. D. SANDE BAKHUYZEN [1]) in 1894 to a searching and complete examination, in which the result of the Leiden observations of the years 1864—1874 obtained by Mr. WILTERDINK'S [2]) elaborate researches, as compared to those of the other observatories, proved to be entitled to considerable weight.

Meanwhile a beginning had been made with special series of observations having expressly in view the investigation of the problem of the latitude and soon important results were furnished by the voluntary co-operation of a number of astronomers. More than once Prof. ALBRECHT has summarised those results in the reports of the "International Geodetic Survey" and of late an ample report has been given by him about the state of the problem in Dec. 1897 [3]), in which he adjusted the whole series of the obtained results by a continuous curve for the motion of the pole.

But a small part of these results could yet be used by H. G. v. D. S. BAKHUYZEN and though it be true that CHANDLER has already made many computations on this subject, yet often there is a lack of judgment in the treatment of the observations, which in my opinion justifies a new investigation covering now the whole of the period of 1890—1897. The results I found being perhaps of some importance apart from the particular aim for which I undertook my investigation, I take the liberty of making them the subject of a concise communication.

2. ALBRECHT could use the observations of 19 observatories, viz: of *Tokio*, the *Cape of Good Hope*, 10 observatories in *Europe*, 6 in *America* and finally *Honolulu*, a temporary observatory having been erected there in 1891—1892. With a few exceptions the observations have been made according to the Horrebow method. Not in a single place however do they cover the whole period, so ALBRECHT had to undertake extensive computations to derive in successive approxima-

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[1]) Report of the Meeting of the Roy. Acad. Amsterdam, Febr. 1894 and also Astr. Nachr. vol. 136 and 137.

[2]) Report of the Meeting of the Roy. Acad. Amsterdam, Dec. 1892.

[3]) TH. ALBRECHT, Bericht uber den Stand der Erforschung der Breitenvariation im December 1897.

tions the mean latitude for each place and to join all the results to a connected system of co-ordinates, by which the instantaneous positions of the pole in respect to a mean one are represented. ALBRECHT chose as co-ordinate axes the meridian of *Greenwich* and one 90° westward, passing centrally over America, and finally he gives, besides a curve for the motion of the pole, the said co-ordinates x and y for every tenth of a year from 1890.0 till 1897.5.

The curve of ALBRECHT is rather intricate and so we may take for granted that his adjustment, performed undoubtedly with great care, has strained the observations but slightly. Therefore I thought myself justified in avoiding for my further computations the prolix work of falling back upon the original observations, and in making use of ALBRECHT's x and y co-ordinates. Finally however I have also compared my results with the original observations.

It being tolerably certain that the motion of the pole consists at least in the main of a 14-monthly and a yearly motion, I have examined.

1°. the 14-monthly motion;
2°. the yearly motion;
3°. in how far the observations may be represented by the combination of a *constant* 14-monthly and yearly motion.

3. *The* 14-*monthly motion.*

Leaving the results 1897.0—1897.5 out of consideration for my computation, I had 70 x- and 70 y- co-ordinates at my disposal. Assuming for the present 432 days for the length of the period we find 6 periods = 7 years and 35 days. So the data allow of a good separation of the two motions, but the mutual commensurability is not so approximate that an easy determination of the elements of both at the same time can be founded thereon. So I began by deriving in 1st approximation the yearly motion and proceeded to to use corrected x- and y- co ordinates to determine the 14-monthly motion.

The computation of the x and y was made quite independent of each other and, assuming 432 days for the length of the period, I united in both cases the 70 values to 8 means, which then formed the basis for the computation of periodic formulae.

So I obtained:

$$x = +\,0''.151 \cos 2\pi \frac{t - 2412439}{432}$$

$$y = -0''.143 \sin 2\pi \frac{t-2412438}{432}$$

in which the epochs are expressed in Julian dates.

By these my 8 means are represented as follows:

x obs.	$O-C$	y obs.	$O-C$
+ 0''.127	− 0''.017	− 0''.046	− 0''.003
+ 076	+ 7	− 115	+ 13
− 040	+ 7	− 118	+ 18
− 145	− 10	− 085	− 22
− 164	− 21	+ 036	− 12
− 056	+ 10	+ 157	+ 28
+ 059	+ 7	+ 141	+ 7
+ 120	− 15	+ 042	− 21

and

$$\sqrt{\frac{\Sigma \Delta x^2}{n}} = \pm 0''.013 \qquad \sqrt{\frac{\Sigma \Delta y^2}{n}} = \pm 0''.017$$

ALBRECHT assumes 0''.04 as mean error of his co-ordinates, according to which that of the mean of 9 values, with which we operated here, would be 0''.013.

We thus see that the amplitudes found for x and y are very nearly equal and that the difference of phase amounts to 90° with the difference of only a single day. The motion thus proves to be, with considerable approximation, *circular* and to be *direct*, that is with the rotation of the earth from west to east.

To determine the length of the period these results must be united with the results of anterior observations. For that purpose I did not retain the whole of the data used by H. G. v. D. SANDE BAKHUYZEN, but chose only the most certain results, namely those from Leiden 1864—1874 and those from Pulkowa 1882—1892, for which the mean errors are by far the smallest. So I obtained as epochs of maximum of x, = epochs of maximum of the latitude for *Greenwich*:

				O—C	O—Ch
Leiden	1864—68	Fundam. Stars	2403394	+ 2	— 19
»	1864—74	Polaris	2403386 [1])	— 6	— 27
		Mean	2403390	— 2	— 23
Pulkowa	1882—92	Vert. Circle	2410298	+ 8	— 7
Observ.	1890—96	Summ. of Albrecht	2412439	— 6	+ 13

Giving equal weight to the three results, we get:

Epoch of maximum	2408565
Period	431.11 days

and the differences Obs.—Comp. contained in the last columer but one.

This epoch of maximum falls in 4 days before the one according to the results of H. G. v. d. S. Bakhuyzen, whilst the length of the period found by him was first 431.22, later 431.55. Had I combined his mean epoch of maximum with my result then 430.36 would have been found for the length of the period.

This result again agrees well with the supposition that the length of the period has remained unchanged for the last 35 years. In that entire period it cannot have differed much from 431 days and such a great variability as Chandler assumes is now already contradicted by the observations. This appears, first in the differences: Obs.—Ch. which I inserted in the last column of the above table. In the second place however the consideration that just in the very last years, according to both suppositions, the epochs must begin to differ widely led me to investigate separately the three last years of Albrecht's summary. I operated only with the x co-ordinates and determined from these the epoch in the same way as before. This result, though necessarily less certain, an error in the assumed yearly motion being now of influence, agrees however almost perfectly with that of the 7 years together.

I found:

Ep. of Max.	Obs.—C.	Obs.—Ch.
2413299	— 8	+ 27

1) These numbers deviate 3 and 4 days from those in the summary of H. G. v. d. S. Bakhuyzen, as I interpreted the results of Wilterdink somewhat differently.

The error of the hypothesis of CHANDLER is thus already rather considerable in 1895.

Let us now consider the amplitude. Uniting according to the weights my results obtained by the x and by the y and adding to them some values, formerly found, we obtain

			Amplitude
Leiden	1864—68	Fundam. stars.	0″.156
»	1864—74	Polaris	0 .158
Pulkowa	1882—92		0 .139
Final result of H. G. B.	1860—92		0 .168
Result	1890—96		0 .148

We have to remark here, that the result of Pulkowa is probably too small on account of the unequal distribution of the observations of the individual stars, for which WILTERDINK applied a correction in dealing with the Leiden observations.

So a change in the amplitude since 1860 is no more probable now than it appeared before to H. G. v. D. S. BAKHUYZEN. On the other hand we may not, as he already remarked, combine the observations before 1860 with the later ones, certainly not so far as the amplitude and perhaps not so far as the phase is concerned. This might be reconciled with the conception of the 431 day-period as a time of oscillation proper to the earth, when we assume [1]) that from time to time sudden causes may change the mutual position of the axis of rotation and the axis of inertia.

Finally I should like to lay down as most probable elements of the motion of 431 days since 1860

Time of transit through the posit. axis of x	2408565
Period	431.1 days
Amplitude	0″.155

consequently

$$x = +\ 0''.155 \cos 2\pi \frac{t-2408565}{431.1}$$

$$y = -\ 0''.155 \sin 2\pi \frac{t-2408565}{431.1}$$

[1]) See also GYLDEN Astr. Nachr. Vol. 132. N⁰. 3157.

4. *Yearly motion.*

To the original x's and y's of ALBRECHT the values according to the 431 day-motion were applied, in order to derive from the residuals the yearly motion in 2nd approximation. I employed for that purpose however values deviating slightly from those found above as the most probable. For the amplitude I adopted 0″.151, for the period 431.0 and for the epoch 2412439 (or 2408560), which is that found by me from ALBRECHT's summary only.

Taking the means of the corresponding values for the different years, I obtained for x and y 10 mean values, from which periodic formulae, depending only on the *sine* and *cosine* of the single angle, were derived, just as had been done in the 1st approximation.

The results found for the 7 years together deviated but slightly from those which the 1st approximation had given, but I had now also the opportunity of dividing the period into two and of deriving the yearly motion from each half separately.

This seemed desirable, it being *a priori* very well possible that this motion, probably caused by meteorological influences, might differ considerably in the different years.

So I found

	from x	from y
1890—96	$x = +\,0''.116 \cos 2\pi \frac{t-261}{365}$	$y = +\,0''.067 \cos 2\pi \frac{t-143}{365}$
1890—92	$x = +\,0''.123 \cos 2\pi \frac{t-252}{365}$	$y = +\,0''.087 \cos 2\pi \frac{t-126}{365}$
1893—96	$x = +\,0''.113 \cos 2\pi \frac{t-269}{365}$	$y = +\,0''.058 \cos 2\pi \frac{t-163}{365}$

The epochs of maximum are expressed here in days from the beginning of the year; if they are expressed in dates they are:

$t_0 =$ Sept. 18	$t_0 =$ May 23
Sept. 9	May 6
Sept. 26	June 12

By the formula found for the whole of the period the means employed are represented as follows:

x obs.	$O-C$	y obs.	$O-C$
− 0″.034	− 0″.009	− 0″.060	− 0″.008
− 085	+ 2	− 011	+ 6
− 117	− 2	+ 032	+ 8
− 107	− 7	+ 060	+ 4
− 061	− 15	+ 054	− 13
+ 025	0	+ 051	− 1
+ 089	+ 2	+ 024	+ 7
+ 109	− 6	− 017	+ 7
+ 089	− 11	− 058	− 2
+ 045	− 1	− 067	0

$$\sqrt{\frac{\Sigma \Delta x^2}{n}} = \pm 0''.007 \qquad \sqrt{\frac{\Sigma \Delta y^2}{n}} = \pm 0''.007$$

The mean residuals found by testing the mean values for the half-periods have about the same magnitude.

For the entire period, as well as for both halves, the amplitudes in x and in y prove to be considerably different. At the same time the difference in phase clearly deviates from $90° = 91$ days; it amounts in the three cases to 117°, 124° and 105°.

So the orbit of the yearly motion of the pole is a rather excentric ellipse, whose principal axes are inclined to the meridian of Greenwich.

The motion of the pole in this orbit is very approximately a simply harmonic one. The mean deviations of the observations are here even considerably slighter than in the case of the 431-day motion.

In order to investigate this elliptic motion more closely, I have in the 3 cases turned the co-ordinate axes through an angle such that they coincided with the principal axes of the ellipses. Doing this it appeared in the first place that the major axes of the ellipses fall *east* of the meridian of Greenwich and form with it the following angles:

ellipse	1890—96	19°	East.
»	1890—92	29°	»
»	1893—96	10°	»

Further we find for the components of the motion in the direction of the principal axes:

$$1890-96 \quad x = +0''.121 \cos 2\pi \frac{t\text{-Sept. }28}{365} \quad y = -0''057 \sin 2\pi \frac{t\text{-Sept. }28}{365}$$

$$1890-92 \quad x = +0''.136 \cos 2\pi \frac{t\text{-Sept. }23}{365} \quad y = -0''.065 \sin 2\pi \frac{t\text{-Sept. }23}{365}$$

$$1893-96 \quad x = +0''.114 \cos 2\pi \frac{t\text{-Oct. }1}{365} \quad y = -0''.055 \sin 2\pi \frac{t\text{-Oct. }1}{365}$$

So the motion is *direct*, like that of 431 days, and the times t_0 express the times of transit through the positive halves of the major axes.

The motions found for the two half-periods do not differ so much that there is any guarantee for a real difference existing. It is my opinion therefore, that for the present we must assume as the most probable orbit for the yearly motion of the pole an ellipse, whose major axis lies 19° east of the meridian of Greenwich and whose semi-axes amount to 0'.12 and 0''.06.

As early as 1894 CHANDLER had found [1]) an excentric orbit for the yearly component; so on the whole I can fully confirm his result. His ellipse however has a greater inclination (45° east of the meridian of *Greenwich*) and also a somewhat greater excentricity (semi-axes 0''.16 and 0''.05) than mine.

The results of absolute determinations of zenith distances are so liable to systematic perturbations of a yearly period, that to my idea they cannot contribute to a more accurate knowledge of the yearly term of the motion of the pole.

The results obtained by the Horrebow-method too are certainly not quite free from these perturbations and particularly there would be reason to fear for them the influence of the inclination of the strata of air either in or outside the observing room. (see a. o. ALBRECHT

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[1]) Astron. Journal, No. 323, 329 and 402.

„Report etc." page 11—12). Yet I believe, especially on account of the good mutual agreement of the results 1890—92 and 1893—96, obtained by the cooperation of observatories partly different for the two periods, that the results obtained are on the whole trustworthy.

To derive the yearly term H G. v. D. S. BAKHUYZEN also exclusively made use of the results then known of the Horrebow-observations, namely the series of observations of Berlin, Potsdam, Prague and Strasburg made in 1889—1892. Of course only the x co-ordinate can be derived from these and, computing the corrections for the small differences in longitude by considering the motion as circular, the result is:

$$x = +0''.112 \cos 2\pi \frac{t - \text{Sept. } 12}{365}$$

nearly coinciding with that which has now been derived from so much fuller data.

Let us finally consider what may be said about the meaning of the results obtained. In 1890 RADAU [1]) for the first time drew attention to the fact that by cooperation of periodic displacements of mass, depending on the season, with the period proper of the axis of the earth, then evaluated at about 304 days, motions of the axis of inertia, caused by the above mentioned displacements, may be transmitted greatly magnified to the axis of rotation. By the relatively small difference between the period of the perturbing action and the period proper, we have to do with phenomena which can be regarded as resonance and consequently the possibility arises that the relatively small displacements of mass which may be brought about by meteorological influences, may cause a rather considerable motion of the axis of rotation. Shortly afterwards the matter was more fully investigated by HELMERT [2]) who showed that the general elliptic motion of the pole of inertia causes motions of the pole of rotation in orbits, which in the various cases may have every shape from the circle with direct motion through more and more excentric ellipses and the straight line, to ellipses with retrograde motion and finally the retrograde circle. In the most favourable case (a direct circular motion of the pole of inertia) the motion is transmitted magnified about 6 times, in the most unfavourable case (a retrograde

[1]) Bulletin astron. T. VII, page 352.

[2]) Astron. Nachr. Vol. 126, N°. 3014.

circle) reduced to about one half. If the motion of the pole of inertia is not simply elliptic, but, more generally, expressible by a series of periodic terms, these are transmitted more and more reduced to the pole of rotation the higher the terms we reach. So in general to irregularities in the motion of the former will correspond much smaller ones in that of the latter.

All this remains not only true in principle, when we take the period proper of the axis of the earth to amount to 431 days, but also the numerical values remain about the same [1]. So it is possible and important to consider which motion of the pole of inertia must be assumed to explain the yearly motion found for the pole of rotation. Substituting my final results for 1890—96 in the general formulae, we find as co-ordinates for the *pole of inertia* with respect to the axes finally adopted:

$$x = + 0''.055 \cos 2\pi \frac{t - \text{Sept. } 28}{365}$$

$$y = + 0''.084 \sin 2\pi \frac{t - \text{Sept. } 28}{365}$$

that is, the motion must be *retrograde*, the principal axes have the same direction as those of the pole of rotation, but the major and minor axes have changed places, and the motion of the pole of inertia is but slightly smaller than that of the pole of rotation. We would have found a more considerable proportion had the ellipse of the latter been assumed less excentric.

So after all little has been gained for the explanation of the phenomenon. Only it is perhaps more intelligible that the yearly motion of the pole of rotation may be pretty regular, though *a priori* the reverse be probable for the pole of inertia.

5. *Comparison of the observed motion with the sum of the two adopted terms.*

In the first place the x and y of ALBRECHT have been compared with the computed values. I adopted for the motion of 431 days the same elements which have served for the derivation of the yearly motion, and for the yearly motion my final results for the whole period. So I found the following differences between observation and computation expressed in hundredths of seconds.

[1] See also NEWCOMB Monthl. Not. 1892, March.

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x Albrecht — x computed.

	.0	.1	.2	.3	.4	.5	.6	.7	.8	.9
1890	−10	−6	−2	+1	+2	+2	+1	−1	−6	−7
91	−8	−4	−1	0	+3	+3	+1	0	+2	+5
92	+6	+6	+1	−3	−5	0	+3	+2	−1	−2
93	−1	+1	+2	+2	0	+4	+1	−1	−2	0
94	+2	+4	+6	+7	+4	+2	0	−2	0	+3
95	+4	0	−7	−10	−10	−8	−3	−2	−2	0
96	+1	+1	−1	−3	−4	−3	−2	+1	+2	+3
97	+6	+5	+6	+3	−2	−6				

y Albrecht — y computed.

	.0	.1	.2	.3	4	.5	.6	.7	.8	.9
1890	−4	+5	+9	+8	+4	+2	−1	−2	−3	−6
91	−7	−3	+1	+7	+4	−3	−9	−11	−6	+1
92	+7	+8	+2	−5	−8	−1	+4	+6	+1	0
93	0	+1	0	−2	−4	−1	−2	−1	0	−1
94	0	0	+3	+5	+1	−1	−2	−2	−1	0
95	−1	−5	−6	−7	−8	−6	+6	+6	+5	+1
96	−1	−2	−3	−2	+2	+8	+9	+9	+2	+4
97	+1	−4	−1	−3	−4	−1				

$$\sqrt{\frac{\Sigma\,\Delta\,x^2}{n}} = \pm\,0''.040 \qquad \sqrt{\frac{\Sigma\,\Delta\,y^2}{n}} = \pm\,0''.045\;,$$

whilst ALBRECHT assumes as mean error of his co-ordinates the value 0''.04, differing but slightly from the former. The residuals however show more than once a systematic character.

In the second place the original results of observations — usually monthly means — as they are communicated by ALBRECHT, were compared with my formula, and for each observatory [1]) the mean devia-

[1]) If there exist for *one* observatory various separate series of observations, I reckon them each separately as ALBRECHT does; besides as he I use only the observations till 1896.5. So, if we leave out 4 very short ones, we arrive at 27 series. Neither did I take into consideration the observations which ALBRECHT laid aside on account of clearly appearing systematic error.

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tion of such a result was computed; by ALBRECHT himself the comparison of the observations with his curve had already been made. Moreover for every observatory ALBRECHT's residuals as well as mine were united to half yearly means,. that is to say means of these residuals were taken for each summer- and each winter-half year, and finally from these, mean deviations for those half-yearly periods were computed. [1])

It would take too much space to communicate these comparisons *in extenso*, so I shall restrict myself to a few results derived from them.

If we call Δ_A and Δ_B the mean residuals of the single results of observation as found by ALBRECHT and by myself, computed for each observatory; Δ'_A and Δ'_B the corresponding quantities for the half-yearly periods, I find, taking the means of the 27 values of each of these quantities:

$$\Delta_A \text{ (mean)} = \pm\ 0''.061 \qquad \Delta'_A \text{ (mean)} = \pm\ 0''.033$$
$$\Delta_B \text{ (mean)} = \pm\ 0\ .071 \qquad \Delta'_B \text{ (mean)} = \pm\ 0\ .048$$

Among the 27 series Δ_B is smaller than Δ_A in 5 cases. (Pulkowa 2nd series, Berlin 1st s., Cape of Good Hope, Potsdam 2nd s. and Lyons) and larger in 22 cases; for the Δ' the corresponding numbers of cases are also 5 and 22.

If I use only the 18 series with the smallest mean deviations from ALBRECHT. I find

$$\Delta_A \text{ (mean)} = \pm\ 0''.051 \qquad \Delta'_A \text{ (mean)} = \pm\ 0''.029$$
$$\Delta_B \text{ (mean)} = \pm\ 0\ .061 \qquad \Delta'_B \text{ (mean)} = \pm\ 0\ .041$$

The fact that the Δ_B are in general larger than the Δ_A may be attributed to real deviations from my formula and to systematic errors of the observations, which for a part will have been transmitted to the curve of ALBRECHT. As mean values for the sum of those influences (deviation from my formula and partial influence of the systematic errors), we find for all observatories together:

$$\sqrt{\frac{\Sigma(\Delta_B^2 - \Delta_A^2)}{27}} = \pm\ 0''.037 \qquad \sqrt{\frac{\Sigma(\Delta'^2_B - \Delta'^2_A)}{27}} = \pm\ 0''.036$$

and for the eighteen most accurate series only:

$$\sqrt{\frac{\Sigma(\Delta_B^2 - \Delta_A^2)}{18}} = \pm\ 0''.033 \qquad \sqrt{\frac{\Sigma(\Delta'^2_B - \Delta'^2_A)}{18}} = \pm\ 0''.033.$$

So it appears: 1° that the combined influence of both circumstances is very appreciable; 2° that influences are at work which

[1]) Means depending only on a single monthly result were neglected.

operate in a constant direction for rather long periods, 3°. that in the most accurate series of observations they appear in a less degree. So it appears certain that systematic errors have been at work, as indeed had already been found before (see above).

How great however is the share of those systematic errors? In this respect only the deviations of the various observatories compared mutually for the same period can give us some indications and we can use for this end the half-yearly means. If we look into these more closely we often find considerable differences between observatories situated close to each other, which must thus be due to systematic error, but side by side with these we sometimes also find criteria that real deviations from my formula may exist.

To show the first I have derived for the 4 observatories which have furnished observations during 6 successive half-years (Potsdam, Strasburg, Carlsruhe and Lyons), by comparing the result of each one with the mean of the 4, the mean value of the total error of observation in the half-yearly mean of an observatory. The result found is $\pm$ 0″.049, that is a considerable amount and which, as it agrees exactly with the value of Δ'_B obtained above, would as far as it goes tend to show that in this case [1]) systematic errors of the observations alone may account for the deviations from my formula.

On the other hand a case pointing to a real deviation from my formula is offered by the summer of 1895. Here follow for the European observatories the deviations from ALBRECHT as well as from me:

	O—A	*O—B*
Kasan	+ 0″.025	— 0″.022
Prague	+ 10	— 48
Potsdam	+ 33	— 30
Carlsruhe	— 104	— 170
Strasburg	— 26	— 90
Lyons	+ 17	— 37

If a real deviation from my formula is indicated by this, it would prove that the combination of a constant motion of 431 days and a *constant* yearly motion does still somewhat fall short of the reality. That the latter may be different in the various years is *a priori* not improbable. It must be left to further observations, which will have to be kept still more carefully free from systematic error, to furnish more certainty upon this point.

[1]) The same criterion is not so easily applicable to other observatories and in other years.