Our result 0''.26 larger, is thus in moderate accordance with the theory.

As regards the periodic terms, it is very satisfactory that BROWN's inequalities are so well confirmed by the observations. It is, however, remarkable that after BROWN's thorough investigations the observations still betray an inequality with a coefficient of 0'' 66, which is theoretically unexplained.

Still I think that we are driven to this conclusion, and that the supposition of E. F. BAKHUYZEN (Proc. Akad. Amsterdam **6** 1903, 417), when he could find no trace of the existence of BROWN's term I (= Radau I) in the observations, that in the years considered another term must have neutralized its effect, is fully confirmed. For we now find that with equal coefficients their arguments in 1863 differed by 180°.

Astronomy. — *Investigation of the inequalities of approximately monthly period in the longitude of the moon, according to the meridian observations at Greenwich.* 2nd part. By J. E. DE VOS VAN STEENWIJK. (Communicated by Prof. E. F. VAN DE SANDE BAKHUYZEN).

In connection with my previous paper on the inequalities in the longitude of the moon, the period of which differs little from the anomalistic period of revolution, I have made some further calculations.

Even after applying the corrections which we have discussed and all the new inequalities determined by BROWN, a discordance still remained between observation and theory, which could be expressed by the following empirical term to be added to the theoretical longitude:

$$+ 0''.66 \; sin. \{g + 244°.4 + 40°.35 \; (t - 1900.0)\}$$

I have already pointed out that it is striking that an inequality with such a comparatively large coefficient should exist, which is not explained by theory, and on this account I have tried to establish the reality of this term with greater certainty.

For this purpose first the value of this term and its influence upon h and k were calculated for each year, and my $-h_v$ and $-k_v$ were corrected for this. After this the mean residual discordance in h_v and k_v was determined, and in the second place for comparison the same was done, when h_v and k_v were not corrected for the empirical term. In the third place the same calculation was made again, when h_ι and k_v were neither corrected for my empirical term nor for the inequality BROWN I (the period of which differs little from that of the empirical term). In making the three calculations weight $^1/_2$ was given to the h and k of NEWCOMB's first series, and the mean discordances given below refer to an h and k with weight unity. We found:

	h			k		
	I	II	III	I	II	III
1848—1875	±0″377	±0″649	±0″400	±0″416	±0″666	±0″440
1890—1910	±0.368	±0.543	±0.879	±0.392	±0.534	±0.842
Together .	±0.373	±0.602	±0.667	±0.405	±0.608	±0.659

We see in the first place that the mean residual discordances in h and k agree in the three cases very well with each other, and it is shown clearly that in the period 1848—74 the term BROWN I and my empirical term counteract each other to such an extent that they could both be omitted without the mean discordance being perceptibly increased, and that therefore apparently the non-existence of BROWN I had to be inferred from the observations of these years, while in the period 1890—1910 the relation is just the opposite.

Further it is seen that the mean discordances I, remaining after the empirical term was also applied, are considerably smaller than the values II. If the former are to be attributed to accidental errors alone, they must be about equal to the mean errors in h and k deduced from the equations for each year

$$r = c + h \sin g + k \cos g$$

These mean errors were calculated for the three years 1893, 1901, and 1908, and we found:

	μ_h	μ_k
1893	± 0″272	± 0″274
1901	± 0 321	± 0.330
1908	± 0.274	± 0 291

We may therefore take for this mean error on the average the value ± 0″30, while for the mean residual discordance in h and k for the years 1890—1910 we find ± 0″38, which agreement may be considered satisfactory.

We may therefore conclude:

1. That the reality of our empirical term is established;
2. That, when its influence, together with that of all the theoretical terms, is applied to the results for h and k, deduced from the observations, the residuals may probably be ascribed to accidental errors.

Finally I wish to make a few remarks which refer to another form in which our empirical term can be expressed.

Prof. E. VAN DE SANDE BAKHUYZEN called my attention to the remarkable fact that the period of the argument χ which is added to g agrees, within the errors of observation, with the periodic time of the moon's perigee (the difference in annual motion is 0°.33). As the longitude of the perigee for 1900.0 is 334°.3, our term may be put in the form (if l represents the mean longitude of the moon)

$$+ 0''66 \sin (l - 89°.9) = - 0''66 \cos l.$$

Prof. BAKHUYZEN will give a short paper upon the possible signification of our term in connection with this transformation. The new form suggested to me to investigate in how far the term can arise from the circumstance that, following NEWCOMB, my whole investigation was based on the tabular errors in right-ascension instead of on those in longitude.

The great advantage of this method is that by it the errors of observation in right-ascension are not mixed with those in declination, but we must now pay attention to the systematic differences between the deviations $\delta\alpha$ and δl, therefore also to those parts, arising from the tabular errors in latitude. NEWCOMB on pp. 12 and 16 of his "Investigation" carefully considers these differences; he finds approximately

$$\alpha = l - 2°.5 \sin 2l - 1°.1 \sin (2l - \theta) + 1°.1 \sin \theta$$

in which θ represents the longitude of the node and, as

$$d\alpha = \frac{d\alpha}{dl}\,\delta l + \frac{d\alpha}{d\theta}\,\delta\theta + \frac{d\alpha}{di}\,\delta i$$

putting

$$\delta l = \delta\alpha - P$$

we find:

$$\begin{aligned} P = (&+ 0.018\delta\theta - 0.037\,\delta l) \cos (2l - \theta) \\ &- 0.087\delta l \cos 2l \\ &+ 0.018\delta\theta \cos \theta \\ &+ 0.21\delta i \sin \theta \\ &- 0.21\delta i \sin (2l - \theta) \end{aligned}$$

NEWCOMB further points out that P contains no perceptible terms, whose period approaches that of g, so that their influence upon his and also upon our investigation must be small.

The value of the term $0.018\delta\theta$ has increased since NEWCOMB's time, as $\delta\theta$ is now about 10′′, but its influence may certainly still be disregarded in our investigation.

Newcomb did not, however, take into account the possibility of a constant error in the latitude, although he had himself previously called attention to the want of foundation of Hansen's supposition that the centre of gravity and the centre of figure of the moon should not coincide in the direction of the radius vector.

The influence of a constant error in the latitude upon the R.A. is

$$\frac{d\alpha}{d\beta} = -\frac{\sin \varepsilon}{\cos^2 \delta} \cos \lambda$$

or, as the greatest difference of $\cos^2 \delta$ from unity, for $\lambda = 90°$ or $270°$, is 0.16, about

$$\frac{d\alpha}{d\beta} = -0.40 \cos \lambda.$$

We see therefore that, as we may neglect here the difference between mean longitude in the orbit and ecliptical longitude our term could, as regards its form, be completely explained by a constant error in the latitude and that the correction for this would have to be $+1''.65$, more than compensating Hansen's term $-1''.0$. As Bakhuyzen, from the declinations observed at Greenwich, after having freed them as far as possible from systematic errors, and reduced them to Newcomb's fundamental system, deduced a latitude-correction of only $+0''.20$, both from the observations of the limbs and from those of Mösting A, while in using the uncorrected declinations the correction would have been found to be zero or negative, it appears that if I, instead of using differences $\Delta\alpha$, had used the errors in longitude $\Delta\lambda$, calculated at Greenwich, I should have found an empirical term of about the same value.

Astronomy. — *On the significance of the term in the Right Ascension of the moon, found by* J. E. de Vos van Steenwijk. By Prof. E. F. van de Sande Bakhuyzen.

The most important result of the investigation of de Vos van Steenwijk is doubtless the fact that the observations of the moon, besides the inequalities theoretically determined by Brown, betray the existence of a new term, apparently of an analogous form, which was not explained by the theory. The reality of this term might already be considered as established after his previous calculations; its existence and also the approximate accuracy of the co-efficients were put beyond all doubt by the determination of the mean residual discordances remaining in h and k (see his second paper), after they had been corrected on the one hand only for the