Citation:

Sande Bakhuyzen, E.F. van de, On the significance of the term in the Right Ascension of the moon, found by J.E. de Vos van Steenwijk, in:
KNAW, Proceedings, 16 I, 1913, Amsterdam, 1913, pp. 144-148

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 constant or slowly varying parts and for the theoretical perturbations and on the other hand for the empirical term also. The mean discordance decreased, in the mean for h and k, from $\pm 0''605$ to $\pm 0''389$, and the decrease was about the same for h and k, and about equally great for the earlier and the later years. It did not appear, however, to be so certain that the term deduced from the investigation of the tabular errors in *Right-ascension* really represented an inequality in the *longitude* of the moon.

Originally DE VOS VAN STEENWIJK found the term in the form

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

It soon struck me that the annual variation of the argument χ is almost equal to the annual motion of the perigee 40°.68, so that, as the argument is found most accurately for the mean epoch of the observations used, 1886, the term can be written:

$$+ 0''.66 \sin\{g + 249^\circ.0 + 40^\circ.68\,(t - 1900.0)\}.$$
$$= + 0''\,66 \sin\{l - 85^\circ.3\}$$

in which l represents the mean longitude in the orbit or approximately the ecliptical longitude.

Now it is possible:

1. That the approximate agreement of the two rates of motion is merely accidental, and that the original form found for the term is the true one, so that we might probably have to deal with a still unknown inequality caused by the planets.

2. That the transformation gives us the true formula.

Taking the first supposition, the difficulty remains, which DE VOS pointed out in his first paper, that such a considerable term should have escaped both BROWN and RADAU, and that while all terms with at all considerable co-efficients have been found nearly equal by both.

Taking the second supposition, if we look upon the term in its altered form as a perturbation-term, this would lead to a very improbable form for such a term, as it would depend upon the absolute longitude of the moon, i.e. of a difference in longitude with a fixed direction in the sidereal system or with the aequinox.

There is however a third possibility, viz. that the second form is the true one, but that we are not dealing with an inequality in the ongitude, but with one in the right ascension, proceeding from the lparticular parts of the limb, which are used in the transit-observations, and their different distances from the centre of gravity of the moon. In DE VOS's researches, following NEWCOMB, the immediately observed errors in R.A. were used, and in the last part of his second paper he discusses the influence of this method. He shows

10

there that a constant error in the latitude must lead to a term in R.A. of just the form that he finds, as with fairly close approximation

$$d\alpha = -0.40 \ \cos \lambda d\beta$$

while the new term is pretty nearly

$$-0''.66 \cos \lambda.$$

The special form of the deviations of the limb, which would account for the result now found, would be such that the centre of figure would lie 1″.65 more to the north than according to our ephemerides.

If we assume, disregarding the inclination of the lunar orbit and the influence of the librations, that the moon's equator always coincides with the ecliptic, then in the transit-observations parts of the limb are used, which vary with the longitude of the moon over arcs lying between points 23°5 on each side of the moon's equator. These arcs would then belong to a centre with a latitude 1″.65 larger than according to the ephemerides, therefore, as these include HANSEN's constant term —1″.0 a latitude 0″.65 larger than that according to pure theory. As further, in the course of one year, each value of the moon's longitude successively co-incides with each value of the elongation D and therefore the same number of times with observations of Limb I and Limb II, the two symmetrically lying parts of the limb must each time co-operate.

Last year, I deduced from an investigation of the declinations of the moon observed at Greenwich during the period 1883—1909, that these, after they had been freed from systematic errors as far as possible, and reduced to the fundamental system of NEWCOMB, point to a centre 0″.8 to the south, of the centre of gravity, that is nearly to HANSEN's centre, while the uncorrected declinations would place the centre of figure even more to the south. I found further that the observations of the declination of Mösting A lead to precisely the same results as those of the limbs, or that, when reduced to the centre of the moon with the existing data, they placed it also 0″.8 to the south. De Vos's results now show that the southerly centre, which satisfies arcs of 47° lying symmetrically with respect to the north and south pole, certainly does not satisfy the easterly and westerly arcs which are used in the observations of the rightascensions.

A centre of figure coinciding with the centre of gravity certainly brings about a better agreement. This would correspond with a correction-term —0″.40 *cos* λ, and the question therefore arises whether this smaller co-efficient sufficiently satisfies the observations.

Before investigating this we must note another circumstance. For the years 1905—1909 the observations of Mosting A in right-ascension were also used and it is impossible to say what influence this has had. Observations of the limbs and of the crater are intermixed in each of the 20 equations deduced from each year's observations and cannot be easily separated.

I have, therefore, made a new calculation of the empirical term and simply left out the six last years. Of the seven normal places (DE Vos p. 139) I have left out the last and I took for the last but one the mean results of only 4 years 1901—1904. A new solution then gave

$$\chi = 253^\circ.7 + 40^\circ.67\,(t\text{—}1900.0)$$

and we thus find an annual variation exactly equal to the annual motion of the perigee.

A new calculation of the co-efficient then gave (calculation B see DE Vos p. 139)

$$\beta = -\,0''.44 \qquad \beta' = +\,0''.67$$
$$\gamma = +\,0''.66 \qquad \gamma' = +\,0''.26$$

from which follows for the co-efficient itself $\alpha = +\,0''.75$, a value even greater than before. As argument for 1900.0 we now get $\chi_0 = 251^\circ.6$, thus as the mean from the two calculations $252^\circ.7$, and the term becomes

$$+\,0''.75\,sin\,\{252^\circ.7 + 40^\circ.67\,(t\text{—}1900.0)\}$$
$$= +\,0''.75\,sin\,(l\text{—}81^\circ.6)$$

now deviating slightly more from the form $-\alpha\,cos\,l$.

The empirical term was now again subtracted from the $-h_v$ and $-k_v$: I with the co-efficient $0''.75$ found here, II with the co-efficient $0''.40$. Calculating in both cases the mean discordance we found.

	I		II	
	h	k	h	k
1848—1874	± 0″.367	± 0″.406	± 0″.463	± 0″.490
1890—1904	± 0 .215	± 0 .341	± 0 .301	± 0 .374
Together	± 0 .318	± 0 .382	± 0 .410	± 0 .450

From these results it is clear in the first place, that for the period since 1890 the mean residual discordance is distinctly smaller than before, so that the results from the observations of the limbs and from those of the crater appear not to make a completely homogeneous whole. DE Vos himself had already observed that exactly the last years, since the crater-observations were added, gave less regular

10*

results. In the second place the results show distinctly that the smaller co-efficient satisfies the observations less well. From all this it seems to me that the supposition that the new term is due to a deviating form of the limb gains in probability, and we must then conclude that, while the polar arcs require a centre of figure about 0″.8 south of the centre of gravity, the aequatorial arcs deviate in the opposite sense and require a centre of figure about 0″.9 to the north of the centre of gravity. This is identical with saying that the northern extremities of these arcs lie 0″.35 further outside, and the southern 0″.35 further inside relatively to the centre of gravity. If we take into account that the term has not exactly the form $a \cos l$, the conclusion is but little altered.

These conclusions now agree with the results found by BATTERMANN from his occultations, who deduced from them on the average a centre of figure coinciding with the centre of gravity.

Our results can be further tested by the results which HAYN, in his "*Selenographische Koordinaten*" deduced for the form of the lunar limb from his measurements in Leipzig and HARTWIG's in Strassburg and also by the results obtained by PRZYBYLLOK in his "*Das Profil der Randpartien des Mondes*". (Mitteilungen der Gr. Sternwarte zu Heidelberg, XI).

HAYN gives in his 3rd paper on p. 77, for a mean libration, the mean radii for arcs of 10° and of 30° counted from the North pole along the limb of the moon (Argument P), and I deduced analogous results from PRZYBYLLOK's *Tafel der Randkorrektionen*. In this way I found

P	$\Delta r\ H.$	$\Delta r\ P.$
60°— 90°	+ 0″.03	+ 0″.32
90 —120	— 0 .27	— 0 .22
240 —270	+ 0 .09	+ 0 .05
270 —300	— 0 .31	— 0 .20

Thus for parts of the limb diametrically opposed to each other HAYN finds deviations in the same sense, which does not agree with the results obtained by DE VOS. The agreement with PRZYBYLLOK is better, but not yet satisfactory.

Still I think that the explanation of the results by the form of the lunar limb is the most probable, or, at any rate, the least improbable, and certainly the investigation of DE VOS in connection with mine in 1912 confirms HAYN's remarks (l.c. p. 75) with regard to the great importance of the study of the deviations of the moon's limb, also for the determination of the moon's position.