from which $C = 1.635$ and $H = 0.32$ is calculated. The value of H is of the former order of magnitude. But C has decreased to 0.8 of its former value. This appears still more clearly when benzene is compared with hexamethylene or cyclohexane. With this latter substance we have also the cyclic binding of 6 carbon atoms; only the coincidence of two valencies for carbon has disappeared. The formula is C_6H_{12} and $\frac{T_k}{p_k} = 13.9$. Hence the comparison with benzene gives the two following equations:

$$6\,C + 12\,H = 13.9$$

and $$6\,C + 6\,H = 11.73$$

or $$H = 0.3616$$

and $$C = 1.593$$

I will still give a few values calculated in the meantime, viz. propylbenzene, calculated with benzene and $CH_2 = 2.76$, equal to 20.01, $\frac{T_k}{p_k}$ being equal to 19.772, and chlorobenzene with Cl—H = 2.185 calculated at 13.915 and found 14.18.

But all the nitriles appear to give much too high values of $\frac{T_k}{p_k}$, and so for *b*, and are associating in a high degree. Even benzonitril, but this nitril in a less degree than the others.

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

Professor BATTERMANN and Prof. ERNEST BROWN have both been so kind as to point out to me, in letters to Prof. BAKHUYZEN, that BROWN's theoretical value, quoted by me, for the motion of the moon's perigee (p. 140), which was taken from *Monthl. Not.* **64** 532, does not quite agree with his final result, which was published by him in *Memoirs R. A. S.* **59**, 94 (comp. also *Monthl. Not.* **70**, 3). If we use the value assumed by me for the ellipticity of the earth 1 : 297.5, then the theoretical result for the sidereal motion in a Julian year for 1850 becomes **146435″16**, so that my result from the observations **146435″31** is now only 0″15 greater, against 0″26 formerly.

We approach, therefore, the limits within which this difference might be ascribed to the errors of observation. However, I now think," that the difference which was found, small as it is, still deserves closer consideration, and this especially with regard to the value which NEWCOMB has deduced for this motion from the long series of observed occultations discussed by him in his lately published posthumous paper *Researches on the motion of the moon.* He found (p. 225) **146435″29 ± 0″02**, a result which appears to be very accurate and which agrees almost exactly with mine.

This induced me to consider in how far the small difference might be ascribed to inaccuracies in the values, deduced from observations, on which the theoretical calculations are founded. Such inaccuracies might occur in those parts of the motion of the perigee which depend upon the figure of the earth and of the moon. The latter part is very small, but probably also very uncertain. It must be calculated from the libration-phenomena and BROWN deduced for it, from HAYN's results, 0″03.

Much greater (6″4) is the influence of the ellipticity of the earth, or more exactly of the difference between its polar and equatorial moment of inertia, which can be deduced both from the results of gravity determinations and from measured terrestrial arcs, by means of relations that are connected with CLAIRAUT's theorem. However, these deductions are open to criticism, as BATTERMANN also pointed out. Still we see that, when the ellipticity of the earth is calculated from the most reliable results, recently deduced from both classes of observations the results agree well with each other, and this makes it appear probable, that also the values deduced for the difference of the moments of inertia and thereby for the constant of the lunar perturbations would be fairly accurate.

From the gravity determinations HELMERT deduced 1 : 298.3 a few years ago, and recently HAYFORD and BOWIE deduced from determinations in the United States 1 : 298.4 [1]). On the other hand, HAYFORD, from his discussion of all the measured arcs in the United States found 1 : 297.0, while in Europe, from the Russio-Scandinavian arc of meridian, 1 : 298.6 was deduced. In the American calculations reductions for isostatic compensation were applied.

According to these results the value adopted by me 1 : 297.5 would appear to be too large rather than too small. But now it is remarkable, that all lunar perturbations which are caused by the

[1]) A division of the 89 stations into 2 groups, an eastern and a western, led to 1 : 297.8 and 1 : 299.6 respectively; the addition to the 89 stations of 10 stations in Alaska gave, however, as the result from all 1 : 300.4.

figure of the earth would indicate a greater value for the ellipticity. Amongst these perturbations there are four which have a somewhat considerable coefficient:

1. a motion of the perigee;
2. a motion of the nodes;
3. a periodic inequality in the longitude;
4. a periodic inequality in the latitude.

The first of these, according to our results, would lead to 1 : 294.3, according to NEWCOMB's to 1 : 294.6; the second, according to NEWCOMB's results, would yield 1 : 294.3 and the 4th, according to NEWCOMB, 1 : 293.7, while the 3rd which has a period of 18 years cannot be used for our purpose on account of the unexplained inequalities of long period in the mean longitude. Are these differences to be regarded as real and would therefore the measurements made on the surface of the earth not lead to an accurate determination of the difference in the moments of inertia?

On account of the possibility that other circumstances may exercise an influence upon the motions of the perigee and node, the periodic inequality in the latitude, which has a monthly period, would certainly be the most likely to yield a decisive answer to this question, if it were not that an error in the assumed obliquity of the ecliptic has precisely the same influence upon the declination of the moon as the inequality in the latitude. (See also NEWCOMB's very interesting Addendum to Chapter XI, p. 226).

Physics. — "*Magnetic researches.* XI. *Modification in the cryomagnetic apparatus of* KAMERLINGH ONNES *and* PERRIER." By Dr. E. OOSTERHUIS. Communication N°. 139*b* from the Physical Laboratory at Leiden. (Communicated by Prof. H. KAMERLINGH ONNES).

(Communicated in the meeting of January 31, 1914.)

In the researches on paramagnetism at low temperatures, described in N^{os}. VI, VII, and VIII of this series (Comm. N°. 129*b*, 132*e*, 134*d*), an apparatus was used, in the main the same as that constructed by KAMERLINGH ONNES and PERRIER, of which a complete description is found in Comm. N°. 139*a*.

In one particular, however, a change was made in the apparatus. The apparatus so changed, which was briefly indicated in § 1 of Comm. N°. 129*b*, is here more fully described. The force acting