value of T has already surpassed this limit. The fact that such a maximum value exists may be understood, if we consider that a gas under a constant pressure is ever more rarefied, when the temperature rises — so that at $T=$ infinite, a pressure $=\frac{\pi}{76}$ would require an infinite volume, and correction would be unnecessary.

The condition for the observation under a pressure of $\frac{\pi}{76}$, without the vapour being saturated is, that T must not descend below a certain limit, which we shall put at $\frac{T_k}{1,6}$.

For $\frac{T_k}{T}=1,6$ we find $\frac{d_n}{(d_0)'}=1-0,0116$, so that the normal density is more than 1 pCt. smaller than that which is furnished by the observation.

If the assumption of $a_t = a\frac{T_k}{T}$ agrees better with the observations, than the supposition that a is constant, we should have to put:

$$\frac{d_n}{(d_0)'}=1-0,001645\left(\frac{T_k}{T}\right)\quad\left[\frac{27}{8}\left(\frac{T_k}{T}\right)^2-1\right],$$

in which case for $\frac{T_k}{T}=1,6$ the normal density would be more than 2 pCt. smaller than would follow from the observations.

Astronomy. — „*Some remarks upon the* 14*-monthly motion of the Pole of the Earth and upon the length of its period*". By Dr. E. F. VAN DE SANDE BAKHUYZEN (Communicated by Prof. H. G. VAN DE SANDE BAKHUYZEN).

(Read in the Meeting of October 29th 1898).

1. In the recent N°. 446 of the *Astronomical Journal* another essay is given by Dr. CHANDLER on the motion of the Pole of the Earth, in which he discusses the observations performed in the years 1890—1898 and employs the older series to investigate anew the length of the 14-monthly period. On this last point he contends the opinions formerly emitted by H. G. VAN DE SANDE BAKHUYZEN and recently by me (Proceedings of the Royal Academy, Amsterdam. June 1898). To this latter paper he devotes a note running as follows:

„The memoir last referred to did not arrive until the present

„article was written, but I interpolate this statement with regard „to it in order to enable astronomers to decide as to the justness „of the views therein set forth. Both of the gentlemen of the Leiden „observatory strenuously maintain that the mean period is more than „431 days, and that it is invariable. The formula V" (that is the result given by E. F. v. D. S. B.) „is deduced by a peculiar and „arbitrary treatment of the results of observation, its initial epoch „being based on the Leyden observations alone, on the alleged „ground that its errors are far smaller than those of all other series, „which are rejected. I must however deny the propriety of assigning „a weight of zero, relative to Leyden, to the extensive and precise „series at Pulkowa between 1863 and 1882 with the Vertical Circle „and Prime Vertical Transit."

I shall now take the liberty to add on my part some remarks to these opinions of Dr. CHANDLER. At the same time I shall make use of the opportunity to consider the problem of the length of the 14-monthly period somewhat more closely, which consideration will naturally lead to the discussion of the results on this point arrived at by CHANDLER in his last paper.

2. In the first place in regard to the grievances raised by CHANDLER against my manner of treatment I will grant at once that, by not using the results obtained at Pulkowa in the years 1863 to 1882, I would have committed a gross error, if it had been my purpose to include in my investigations, in an independent way, the observations before 1890. This however was in nowise the case. It was simply my intention to submit to a discussion only those obtained in the period 1890 to 1897; but as from these alone the length of the 14-monthly period could naturally be derived with but slight accuracy I had recourse to the results formerly deduced and compiled by H. G. v. D. SANDE BAKHUYZEN. It seemed undesirable however to use all these results. In the first place, for reasons to be stated hereafter, I thought it necessary to exclude those of an epoch before 1860. Further consideration then led me to restrict myself, in the deduction of a *provisional* result, as far as concerns the observations between 1860—1880, wholly to the Leyden results. I determined on this course because these proved to have much smaller mean errors than all others of the same time, *in as far as they had been treated by* H. G. v. D. S. BAKHUYZEN, whilst moreover these Leyden results proved to lie about midway between the others; so that by including these the final result could not be modified to any considerable amount.

I might perhaps have pointed out still somewhat more clearly the

entirely preliminary character of my result for the length of the period, if I had not thought my meaning sufficiently evident. At all events I assuredly think that, in formulating my result, I have not lost sight of the prudence necessary under these circumstances. Thus I give, beside my result of 431.11 days, also the one which would follow if the mean epoch found by H. G. v. D. S. BAKHUYZEN were combined with mine, viz. 430.36, whilst finally I observe that for the last 35 years the length of the period *cannot have differed considerably from* 431 *days* and that *such a great variability as* CHANDLER *assumes*, is now already contradicted by the observations. So I believe I may state that the words of Dr. CHANDLER: „stre- „nuously maintain that the mean period is more than 431 days, „and that it is invariable" show but very inaccurately the standpoint taken up by me [1]).

Let this suffice to answer CHANDLER's observations about the treatment followed by me; his remarks concerning the facts themselves will be presently considered.

3. Before discussing the results furnished by my later computations on the length of the period, I will concisely state the results arrived at by CHANDLER in 1894 (Astr. Journ. N°. 322) and those lately deduced by him. His formula of 1894 gave as *Epochs of minimum* in the 14-monthly motion:

$$\left.\begin{array}{l} T = 2402327^d + 428^d.6\,E + 55^d\ sin\ \Psi \\ \text{in which, with a sufficient approximation} \\ \Psi = (t-1865.25)\,.\,5°.48 = E \times 6°.43 \end{array}\right\} \quad (1)$$

From this there results for the length of the period, osculating for the epoch E:

$$P = 428^d.6 + 6^d.2\ cos\ (E \times 6°.43). \quad . \quad . \quad . \quad . \quad (2)$$

So the length of the period may vary from $434^d.8$ to $422^d.4$ and the cycle of this change embraces 56 periods or 66 years. The maximumlength would have been reached in 1865, the minimumlength would take place in the present year 1898.

In his last paper CHANDLER starts with this formula and tests it by the observations of 1890 to 1897. He does not use the x and y of ALBRECHT, but values derived by himself, which however agree with

[1]) Neither are his words accurate, where they concern H. G. v. D S. BAKHUYZEN, See a. o. Astr. Nachr. N°. 3275, page 163 at the top.

the former in their general course. The length of the period with which he starts thus amounts to about 423 days and from the observations a correction is found for it of + 5 days, which however, as CHANDLER observes, must be quite uncertain, it not being sure that the length of the yearly period is *exactly* a year. Meanwhile, later on, a correction of + 4 days for the length of the period is assumed beside such a one of + 8 days for the mean epoch and, as CHANDLER thinks it proved that the length of the period is variable, he accounts for the correction by a quadratic term added to the formula of the epochs which thus becomes:

$$T = 2412646^d + 427^d.0\,E - 0^d.08\,E^2 \ldots\ldots (3)$$

where the initial epoch is placed 24 periods later than that of the preceding formula.

Tested by the older observations this formula proved to satisfy fairly those since 1835, but not at all those of POND, which leave for the epoch a deviation of 166 days. Although formerly CHANDLER set great store by POND's observations, it yet seems that he desires to have the elements of formula (3) regarded as „the revised elements" he wished to determine. It is true that a doubt about this conclusion arises by reading in the „conclusions" which, in another part of the paper (p. 107), are derived from „substantially all the competent „testimony available" (*b*) „that the mean period since 1825 is 428 days „within a small fraction of a day", whilst formula (3) gives us for this quantity $431^d.6$, and (*d*) that the hypothesis of a change in the period uniform with the time is incompatible with the observations before 1860, whilst in conclusion (*e*) a change per saltum between 1830 and 1860 is called also incompatible with the facts. Leaving this for what it is, I shall in what follows, indicate formula (3) as CHANDLER 1898.

The differences between the epochs computed according to this formula and to that of 1894 are rather small between 1870 and 1894, but increase rapidly beyond these limits. So we find for CH 98—CH 94 in 1830 — 126 d., in 1860 + 38 d., in 1898 + 25 d. and in 1900 + 32 d.

4. In the first place I investigated more closely what the observations from 1890—97, taken by themselves, can teach us about the length of the 14-monthly period. In my former paper I examined the x of the three last years only; now I did the same for the three first years and then I acted in the same way for the y.

I thus obtained the following results for the mean epochs of

maximum, to which I add those for the whole of the period 1890—96.

	Observ.	Obs.—E. B.	Obs.—Ch.94	Obs.—Ch 98
x				
1890—1896	2412439	— 6	+ 13	+ 6
1890—1892	2412006	— 7	+ 3	0
1894—1896	2413300	— 7	+ 28	+ 13
y				
1890—1896	2412438	— 7	+ 12	+ 5
1890—1892	2412007	— 6	+ 4	+ 1
1894—1896	2413298	— 9	+ 26	+ 11

If we derive the length of the period from those couples of partial results, lying three periods apart, we shall find:

from x 431^d.3

„ y 430 .3

The surprising agreement with the results obtained from great intervals of time had of course to be regarded as partly accidental. Now in order to investigate more closely what accuracy might be arrived at, I fell back on the original values for the coordinates x and y as they have been derived by ALBRECHT. In my preceding paper I gave on page (53) 12 a comparison of these values with those computed by my formula. In entirely the same way I now made comparisons with formulae in which 423 and 428 days were successively assumed for the length of the 14-monthly period, but which agreed for the rest, mean epoch included, (which mean epoch coincides approximately with 1893.0) with those employed for the former comparison. These lengths of the period were taken from CHANDLER's two formulae.

I shall not here communicate these comparisons themselves, but shall give only the sums of the squares of the deviations and the mean values of the latter, repeating also the values formerly found with the length of the period 431^d :

Period	$\Sigma \Delta x^2$	M. dev.	$\Sigma \Delta y^2$	M. dev.
431	1207	± 0".040	1582	± 0".046
428	1322	± 0 .042	1651	± 0 .047
423	1699	± 0 .047	2083	± 0 .052
„	1708	± 0 .047	1976	± 0.051

We see that a 428 days' period satisfies the original observations almost as well as a 431 days' period.

On the other hand a 423 days'period leaves considerably greater errors, which are but slightly diminished by deducing anew also the yearly motion, as is shown by the numbers given in the last place.

So our result is, that the observations from 1890—97 prove in themselves a 423 days' period to be improbable; but much farther than that we cannot go.

5. In the second place I had recourse again to the older series of observations, but, before discussing this investigation, I will state that, now again, it lays no claim to completeness. I have only again combined the results of observations treated already by others, with each other and with my results for 1890—1897 and from these I have drawn such conclusions as seemed most probable to me. The observations of Pulkowa 1863—1875 only make an exception, as for these I made a computation myself founded on the results arrived at by IVANOFF in two important papers. [1])

First came the question how far we may go back in the employment of older observations and this again depends upon that other question, whether we assume in the 14 monthly motion a lasting continuity, or whether we do not exclude the possibility that more or less sudden changes may take place.

As is already remarked, CHANDLER includes amongst the conclusions formulated in his last paper, also this one, „that a change per saltum „between 1830 and 1860 is incompatible with the facts". To me on the contrary it seems that there is every reason to assume the possibility of such a change between 1840 and 1860.

This statement is based in the first place on the values for the amplitudes as they have been found before 1860 and after that

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[1]) A. IVANOFF, Variations de la latitude de Poulkovo déduites des observations 1863—75. (Mélanges math. et astr. T. VII.) St. Pétersbourg 1894.

A. IVANOF, Recherches définitives sur les variations de la latitude de Poulkovo (Bull. Acad. Pétersb. Serie V. T. II). St. Pétersbourg 1895.

time, and which follow below. Here and there I have inserted the results of two different treatments of the same series of observations.

Series of Observations.	Amplitude.	Authority.
Greenwich Mural C .. 1825—1836	0″.126	Ch. A. J 315
Greenwich Mural C .. 1836—1850	0 .060	Ch. A. J. 320
Pulkowa Prime Vert. . 1840—1855	0 .035	" " " 296
Pulkowa Vert. C. .... 1840—1849	0 .056	H. G B A N. 3275
" " " " "	0 .08	Ivanof Rech déf.
Greenwich Tr C .... 1851—1858	0 .069	H G. B. A. N. 3261
Greenwich Tr. C. .... 1858—1865	0 .175	H. G. B. A. N. 3261
Washington Prime Vert 1862—1867	0 .126	" "
Leiden Fund. Stars... 1864—1868	0 .156	Result Wilterdink.
" Polaris..... 1864—1874	0 .158 [1]	" "
Greenwich Tr. C. .... 1865—1872	0 .233	H. G. B A N 3261
Pulkowa Vert. C. Pol. 1863—1870	0 226	" "
" " " 1871—1875	0 .179	" "
" " All the St. 1863—1875	0 .127	Ivanof Rech déf, E F B
" Prime Vert . 1875—1882	0 .236 [2]	Ch. A. J 297
" Vert. C .... 1882—1891	0 .145 [3]	Nyrén Bull. Pét T. 35
" " " " "	0 .139	H. G. B. A N. 3261
Greenwich Tr. C .... 1880—1891	0 .141	Ch. M. N. 53 119
Madison ..... 1883—1890	0 .152	Ch. A J. 307
Lyon..... 1885—1893	0 .175	" " 334
Summary Albrecht ... 1890—1896	0 .148	E. F. B. Ac Amst. 1898.
" 1890—1892	0 .167	Result E. F. B.
" 1894—1896	0 .131 [4]	" "

[1]) These results deviate slightly from those communicated by H. G v. d. S. B. in A. N. 3261. The investigation of WILTERDINK will be shortly published by him in detail.

[2]) The result 0″.33 given by NYRÉN for 1875—1878 in Bull. Pétersb. Vol. 35 is certainly too great, as it is influenced by the yearly motion.

[3]) Result for the entire motion, upon which however the yearly motion seems to have had but slight influence.

[4]) Results now deduced by me for the partial groups.

This summary shows pretty clearly that the amplitude was found to be considerably smaller in the years 1836—1858 than in the following period. For a number of series the mean errors have been deduced (see A. N. 3261) and the consideration of these strengthens the conclusion which admits as probable the reality of the observed difference. On the other hand no variability of the amplitude is to be found after 1860 and we may conclude at least that the observations make a more or less sudden change between 1850 and 1860 much more probable than a periodic or continually increasing one. Now the dynamical theory of the rotation of a sphere not absolutely solid, either as a whole or in some of its parts, leads to the same result. It teaches us,[1]) that with slow secular displacements of mass the axis of the greatest moment of inertia is entirely followed in its motions by the axis of rotation; that with periodic displacements the axis of rotation will get a motion of the same period as that of the axis of inertia, which is added to its own motion, but that in the case of sudden displacements of mass the axis of inertia is the only one to shift its position, so that the opening of the cone, described by the axis of rotation around it, changes, introducing thereby a discontinuity in the motion of the latter. The amplitude changes and in general also the phase, but after that the motion continues in its old period.

May we however be led in this problem by a dynamical theory? CHANDLER denies this strenuously. He thinks it has proved itself a blind guide in this case, and that he who would follow it would betray reprehensible conservatism.

It is a fact that misplaced conservatism has frequently delayed the development of science and, if it were still necessary, the beautiful discovery of CHANDLER himself of the motion of the Pole named after him, would prove once again that an unprejudiced investigation of the observations, without being guided by any theory, can lead a problem in the right paths and render an important service to science.

But on the other hand we are justified I think in not granting the conclusion that the most simple theory is erroneous or incomplete, before such a theory is shown to be decidedly incompatible with the observations.

At the same time a theory, even a somewhat imperfect one, if it be only based in general on correct foundations, is certainly entitled

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1) See a. o. HELMERT. Die math. und phys. Theorieën der höheren geodasie. Vol. II page 417.

to some consideration in those cases, where the observations cannot as yet furnish the necessary information. Such a case being before us we are justified in not wholly disregarding what it teaches.

6. According both to observation and theory therefore a more or less sudden displacement of the axis of rotation between 1850 and 1860 must be regarded as possible and so I think that for the present only observations after that time may be employed to deduce the length of the period.

In the following table all the epochs of maximum after 1858, that have been determined, are brought together, at least those which were accessible to me and which seemed more or less trustworthy. In the first place all the results of H. G. v. D. S. BAKHUYZEN have been inserted, together with those of WILTERDINK for Leyden; further several ones deduced by CHANDLER, then my result from the observations 1890—1896 and finally an epoch of maximum deduced by me from all the observations with the vertical circle at Pulkowa 1863—1875, as they have been treated by IVANOFF.

Series of observations.	E.	Epoch.	Weights.	O.—E.B.I.	Auth.
Greenwich Tr. C. ...1858—65	—18	2400745	1	— 60	H. G. B.
Washington Pr. Vert. 1862—67	—14	2506 [1]	2	— 24	"
Pulkowa V. C. Pol. .1863—70	—13	3035 [1]	2	+ 74	"
Leyden Fund. stars..1864—68	—12	3394	2	+ 2	Wilt.
" Polaris .....1864—74	—12	3386	2	— 6	"
Greenwich Tr. C. ...1865—72	—12	3435	1	+ 43	H. G. B.
Pulkowa Vert. C. ...1863—75	—10	4277	4	+ 23	Iv., E. F. B.
" V. C. Pol. ..1871—75	— 8	5146 [1]	2	+ 30	H. G. B.
" Prime Vert. 1875—82	— 3	7290	2	+ 18	Ch.
" Vert. C.....1882—91	+ 3	9867	4	+ 8	H. G. B.
Greenwich Tr. C. ...1880—91	+ 3	9870	1	+ 11	Ch.
Madison .....1883—90	+ 5	2410704	1	— 16	"
Lyon .....1885—93	+ 6	1151 [2]	2	0	"
Summary Albrecht. ..1890—96	+ 9	2439	6	— 6	E. F. B.

[1]) CHANDLER also discussed these series of observations; his results deviate resp. only + 4, + 5 and — 5 days.

[2]) GONNESSIAT, whose observations of 15 polar stars have been employed here, found himself an epoch 3 days later. Bull. Astr. Vol. XI. Afterwards a formula with 4 terms has been deduced by him. C. R. T. 124. page 930.

For that purpose I employed his table on page 269 of the „Recherches définitives" and resolved the 14 equations founded thereon without paying regard to the weights assigned. The epoch obtained by me agrees entirely with the epoch deduced from a curve by IVANOFF himself.

The column E contains the rotation-numbers of the maxima; for the initial epoch was taken the mean maximum epoch of my preceding paper. The following column contains the epochs of maximum reduced to Greenwich and against these the weights have been inserted which I assigned to those results. It was difficult to determine these weights accurately on account of the evidently considerable systematic errors. It was not allowed to take as their exclusive measure the mean errors derived from the agreement of the observations of a single observatory *inter se*; so they have been determined according to a rough estimation. I adopted the values assumed by H. G. v. d. S. BAKHUYZEN, and for the remaining series I acted in an analogous manner. The column Obs.—E.B.I. contains the deviations from my formula deduced in the preceding paper and the last contains the authorities from which the several results were borrowed.

I at once omitted the series of Greenwich finally not included by H. G. v. d. S. BAKHUYZEN in his computation, their results being already contained in those of the other series. On the other hand I have inserted, besides the epoch deduced from IVANOFF's results for all the observations with the Vertical Circle at Pulkowa 1863—1875, also those deduced by H. G. v. d. S. BAKHUYZEN, from Polaris only, as observed resp. by GYLDÉN and NYRÉN. True, the former result is founded on a much greater number of observations, but it is possible that the mixing up of the results of both observers has done more or less harm, a point which IVANOFF himself also discusses in his first paper page 516.

I have now tried to correct my first formula with the aid of the results compiled in this way, and have rigorously resolved for that purpose all the equations they furnished, having due regard to their weights. At first sight the differences Obs—E.B.I seem to betray a non-linear course, but on closer examination this proves to be only apparent, at least for the greater part, and on account of the occasionally considerable differences between close-lying epochs I thought I was not allowed to depart even now from the simple supposition of a constant length of the period. I made two solutions: including the first time the result according to IVANOF and omitting those of

the Polaris-observations of GYLDÉN and NYRÉN and the second time including the two latter results instead of the former [1]).

So I obtained:

1st *solution:*	Δ epoch	+ 0d.1
	Δ length of the period	+ 0 .06
2nd *solution:*	Δ epoch	+ 4d.7
	Δ length of the period	— 0 .45

We see that it makes rather a considerable difference whether we

		Obs—E.B.IIa	Obs—E B IIb	Obs—Ch.94	Obs—Ch.98
Greenwich Mural C.....	1825—1836[2])	+142 d.	+116 d	+ 44 d.	+163 d.
" "	1836—1850[2])	+ 22	0	+ 2	+ 7
Pulkowa Prime Vert....	1840—1855[2])	— 9	— 30	— 23	— 27
" Vert. C.....	1840—1849[3])	— 50	— 71	— 59	— 70
Greenwich Tr. C.....	1851—1858[3])	— 92	—108	— 82	—127
Greenwich Tr. C.....	1858—1865	— 59	— 73	— 61	— 98
Washington Prime Vert.	1862—1867	— 23	— 35	— 38	— 61
Pulkowa Vert. C. Pol..	1863—1870	+ 75	+ 63	+ 56	+ 37
Leyden Fund. Stars.....	1864—1868	+ 3	— 8	— 19	— 34
" Polaris.....	1864—1874	— 5	— 16	— 27	— 42
Greenwich Tr. C.....	1865—1872	+ 44	+ 33	+ 22	+ 7
Pulk. Vert. C. All the St.	1863—1875	+ 23	+ 14	— 5	— 13
" " Pol.	1871—1875	+ 30	+ 22	— 3	— 4
" Prime Vert...	1875—1882	+ 18	+ 12	— 18	— 7
" Vert. C.....	1882—1891	+ 8	+ 5	— 12	0
Greenwich Tr. C.....	1880—1891	+ 11	+ 8	— 9	+ 3
Madison.....	1883—1890	— 16	— 18	— 25	— 19
Lyon.....	1885—1893	0	— 2	— 3	0
Summary Albrecht.....	1890—1896	— 7	— 7	+ 13	+ 6

[1]) I also made a solution in which the length of the period was assumed as uniformly variable, but I do not mention it here, as the result seemed wholly illusory.

[2]) According to CHANDLER.

[3]) According to H. G. VAN DE SANDE BAKHUIZEN.

follow one way or the other in reference to the observations of Pulkowa.

For the initial epoch and the length of the period itself we obtain in the two cases:

II*a*	2408565	431.17 days.
II*b*	2408570	430.66 „

Although after all the first solution seems preferable, I have given below the deviations of the observations from both, besides those from CHANDLER's two formulae of 1894 and of 1898. In order to show in what relation the results of the observations before 1858 stand to those of later years I also include the former.

The consideration of the deviations for the observations 1858—1896 shows that the agreement for CHANDLER's formulae, notwithstanding their greater intricacy, is not better than that for mine. If, in order to compare in this respect E.B. IIa with Ch.94 and Ch.98, we omit, as is only just, the two Polaris-series of Pulkowa, we shall find that the sum of the squares of the residuals multiplied by the weights is even smallest for E.B.IIa. The distribution of weights, however is of very great influence on these results.

With regard to this period (1858—1896) therefore I should like to give as the results of my investigation:

1°. For the present there is no sufficient reason to assume in the 14-monthly motion since 1860 a non-uniform velocity.

2°. The length of the period in these years has not deviated much from 431 days.

These results clash entirely with those of CHANDLER's last paper and little change has been brought about in the conclusions, at which I arrived in my previous communication agreeing in the main with the anterior results of H. G. v. D. S. BAKHUYZEN.

The epochs according to both solutions II*a* and II*b* coinciding about 1893 and no reason existing not to adopt for the length of the period the round number of 431.0 days, lying between both solutions, I assume for the present as final result:

Elements II of the 14-*monthly motion since* 1860.

Epoch of maximum for Greenwich . . .	2412446
Length of the period	431^d.0
Amplitude	0."156 [1]

[1] Mean value deduced from the previous summary.

In the second place with regard to the period before 1858, I think as yet little can be said about it. Whilst the much smaller amplitude found in this period makes it fully justifiable in my opinion, not to connect the results for that period with the later ones, I dare not deduce anything from the observed epochs themselves. The results 1836—1858 are very uncertain on account of the small amplitude and I cannot give an opinion about the certainty of the results of the observations of POND.

Physics. — Communication No. 44 from the Physical Laboratory at Leiden by Dr. H. KAMERLINGH ONNES. „*A standard open manometer of reduced height with transference of pressure by means of compressed gas.*"

(Read in the meeting of October 29th 1898)

§ 1. *The Principle.* In order to make accurate determinations of high pressures to about 100 atmospheres, open mercury-manometers are indispensable. If we deduce the pressure from the compression of any kind of gas in a closed manometer by making use of the equation of condition of this gas, determinations with open manometers form the basis of the measurements and in making accurate measurements it will prove desirable to test if possible the indications of the closed manometer by comparing them with those of the open manometer. But wherever we want to determine the pressure with greater accuracy than is secured by the equation of condition of the gas with which the closed manometer is filled, there is no other way than making the measurements by means of an open manometer, and that with an apparatus which admits of a high degree of accuracy.

The frequent use made of closed manometers [1]) for the experiments in the Leiden laboratory and the necessity to measure the pressure with great accuracy in the case of some determinations (especially

[1]) If we can measure a range of pressures in a comparatively short time with great accuracy the graduating of closed manometers after we have filled them becomes so simple that we may omit the measurements from which in other cases the value of the scale is deduced. In principle a closed manometer graduated in this way, as a measuring-apparatus is equal to the metal-manometer, but it is preferable to the latter in so far as its indications when the necessary corrections are applied, are perfectly reliable and probably much more sensitive. The graduating of the closed manometer after its construction relieves the observer from those determinations that take up much time and are very uncertain. The accuracy which can be attained in closed manometers to 100 atmospheres is sufficient for the gauging of ordinary metal-manometers, which in order to be reliable must be tested repeatedly and which are specially used as indicators of operations when employed in accurate measurements.