KONINKLIJKE AKADEMIE
VAN WETENSCHAPPEN
-:- TE AMSTERDAM -:-

PROCEEDINGS OF THE SECTION OF SCIENCES

VOLUME XX

JOHANNES MÜLLER :—: AMSTERDAM
AUGUST 1918

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(Translated from: Verslagen van de Gewone Vergaderingen der Wis- en Natuurkundige Afdeeling Dl. XXV and XXVI).

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KONINKLIJKE AKADEMIE VAN WETENSCHAPPEN TE AMSTERDAM.

PROCEEDINGS

VOLUME XX

N°. 6.

President: Prof. H. A. LORENTZ.
Secretary: Prof. P. ZEEMAN.

(Translated from: "Verslag van de gewone vergaderingen der Wis- en Natuurkundige Afdeeling," Vol. XXV and XXVI).

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Physics. — "*On the Course of the Values of a and b for Hydrogen at Different Temperatures and Volumes.*" (Continuation of the Paper of April, 8th, 1903). By Dr. J. J. VAN LAAR. (Communicated by Prof. H. A. LORENTZ.)

(Communicated in the meeting of October 27, 1917).

1. Introduction.

In a Paper published in These Proc. Vol. XIX p. 359—367 Prof. VAN DER WAALS advanced the supposition that the quantity a of his equation of state — at least below the critical temperature and for the small volumes — would be dependent on the volume. At the conclusion of his paper he expresses the hope that also other investigators will feel an interest in this question, and that a serious research will be set on foot also *above* the critical temperature.

On the appearance of the said paper I had already worked on this subject for a long time, also in connection with my researches concerning the fundamental values of $\sqrt{a}$ for the different elements of the Periodic System [1]), and I had found that a was chiefly a function of the *temperature*, whereas b appeared to be dependent both on T and on v.

If, however, a were also dependent on v, then in my opinion the equation

$$\frac{a}{a_k}=\sqrt{\frac{v_k}{v}}$$

would at least represent the facts equally well as the equation given by VAN DER WAALS

$$\frac{a}{a_k}=2-\sqrt{\frac{v}{v_k}}\,.$$

For *Fluorbenzene* we find e.g. what follows.

For the lower temperatures, where d_2 has not been measured

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[1]) It is clear that it is of the highest importance in this, to know accurately in how far a and b are variable from $T=\infty$ to $T=0$. The more so as some physicists, among others MATHEWS, assume in their calculations that the values of a at the absolute zero will not differ much from those at the ordinary temperature. We shall see in what follows that this assumption is very far from reality.

t	p mm.	$log^{10} p$	$10^5 \frac{d\,log^{10}\,p}{dT}$	T	$\pi = T\frac{d\,log\,p}{dT} - 1$	d_2	$\frac{p}{d'_2}$	$\pi \times \frac{p}{d'_2}$	d_1	$a' = \pi \times \frac{p}{d_1 d'_2}$	$\sqrt{d_1}$	*Quot.*
0° C.	20,92	1,3206										
5			2405	278,1	14,40		1,017	14,64	1,0406	14,07	1,020	13,8
10	36,40	1,5611										
15			2209	288,1	13,65		1,053	14,37	1,0286	13,97	1,014	13,8
20	60,54	1,7820										
25			2030	298,1	12,93		1,088	14,07	1,0164⁵	13,84	1,008	13,7
30	96,61	1,9850										
35			1899	308,1	12,47		1,123	14,00	1,0045	13,94	1,002	13,9
40	149,6	2,1749										
45			1752	318,1	11,83		1,158	13,70	0,9928	13,80	0,9964	13,9
50	223,9	2,3501										
55			1619	328,1	11,23		1,192	13,39	0,9807	13,65	0,9903	13 8
60	325,1	2,5120										
65			1540	338,1	10,99		1,224	13,45	0,9683	13,89	0,9840	14,1
70	463,5	2,6660				0,0021						
75			1435	348,1	10,50		1,254	13,17	0,9559	13,78	0,9777	14,1
80	644,9	2,8095				0,002884						
85			1365	358,1	10,26		1,277	13,10	0,9431	13,89	0,9711	14,3
90	883,0	2,9460				0,003846						
95			1248	368,1	9,576		1,305	12,50	0,9300	13,44	0,9644	13,9
100	1177	3,0708				0,005035						
110	1542		1139	383,1	9,046	0,006532	1,331	12,04	0,9096	13,24	0,9537	13,9
120	1989	3,2986										
130	2530		1013⁵	403,1	8,406	0,01053	1,355	11,39	0,8811	12,93	0,9387	13,8
140	3172	3,5013										
150	3929		913	423,1	7,895	0,01629	1,360	10,74	0,8519	12,61	0,9230	13,7
160	4829	3,6839										
170	5841		808	443,1	7,243	0,02413	1,365	9,887	0,8203	12,05	0,9057	13,3
180	7006	3,8455										
190	8376		750⁵	463,1	7,004	0,03496	1,351	9,462	0,7857	12,04	0,8864	13,6
200	9899	3,9956										
220	13550		659	493,1	6,483	0,05907	1,293	8,383	0,7265	11,54	0,8523	13,5
240	18161	4,2591										
260	23960		588	533,1	6,219	0,1226	1,102	6,853	0,6163	11,12	0,7850	14,2
280	31217	4,4944										
283,3	32569		550	556,4	6,046							
286,55 (cr)	33912	4,5304				0,3541			0,3541	9,52	0,5951	16,0

directly by Young, $RT - \frac{a - RTb}{v_2}$ was substituted for $p : d_2' = pv_2$. In this $b = 0{,}005741$, $a = 0{,}04080$ has been put, found from $b_k = \frac{RT_k}{8p_k}$ and $a_k = RT_k \times b_k \times \frac{27}{8\lambda}$, with $R = 0{,}0036618$, $T_k =$

$273{,}09 + 286{,}55 = 559{,}64$, $p_k = 44{,}62$ atm., $\lambda = 0{,}9732$. (The value of λ follows from our formula $\lambda = \frac{27}{8\gamma - 1}\left(\frac{\gamma}{\gamma+1}\right)^2$, in which $\gamma = 0{,}933$ was taken). On account of this the values of RT at $t = 5°$ C. up to 65° C. (this last inclusive), viz. 1,0184, 1,0550, 1,0916, 1,1282, 1,1648, 1,2015, 1,2381, were resp. diminished by 13, 20, 32, 48, 68, 99, and 142 units of the last decimal. As it concerns only a slight correction, the values of b_k and a_k were taken for b and a. The value of v_2 in the correction term was calculated from $v_2 = \frac{1}{d'_2} = \frac{96{,}04}{22412} \times \frac{1}{d_2}$ (96,04 is the molecular weight of $C_6\,H_5\,F$), while for d_2 the values of the vapour density interpolated from YOUNG's table (calculated at these low temperatures by YOUNG on the supposition that BOYLE's law still holds) were substituted.

For the temperatures from 75° C. the values of p (reduced to atm.) and d_2 measured directly by YOUNG, were used. At 75°, 85°, and 95° the values of $p : d'_2$ were obtained by taking every time the middle value of the corresponding quotients at 70° and 80°, 80° and 90° 90° and 100°.

The values of a might be calculated from $\pi \times \frac{p}{d_1 d'_2}$, by expressing also d_1 in normal units; i.e. all the values from 14,07 to 9,52 (inclusive) must for this purpose still be multiplied by $96{,}04 : 22412 = 0{,}004285$. We have, however, not done so, and determined the limiting value of $\pi \times p : d_1 d'_2$ for $T = T_k$ from a_k. Then a_k is namely $= 0{,}0408$ (see above), hence the limiting value in question is $= 0{,}0408 : 0{,}004285 = 9{,}52$.

According to VAN DER WAALS's expression the values of a must be proportional to $2 - \sqrt{\frac{d_k}{d_1}}$. That this is not accurately fulfilled appears e. g. from the following calculation for two arbitrary temperatures. At 5° C. $d_k : d_1 = 0{,}3403$, hence $2 - \sqrt{(d_k : d_1)} = 1{,}4166$. For 130° C. is $d_k : d_1 = 0{,}4019$, $2 - \sqrt{\ } = 1{,}3660$. Hence the ratio is $= 1{,}037$, while the corresponding ratio of the values of a, viz. 14,07 : 12,93, has the value 1,088. On the assumption of simple proportionality with $\sqrt{d_1}$ we find *the same* value 13,8 at the two temperatures for the "quotient" given in the above table. The last assumption has, moreover, the great advantage that the critical temperature does not appear there, as it does with VAN DER WAALS, as a very special temperature. The product $a_k \sqrt{v_k}$ can, namely, be considered as a simple *factor of proportionality*, whereas VAN DER WAALS's v_k cannot be eliminated in the relation between a and v.

However, notwithstanding the constancy of the quotient $a : \sqrt{d_1}$ is remarkable (disregarding the value at T_k, on an average 13,84), all this is nevertheless very little convincing. Really, from

$$\frac{dp_{12}}{dt} = \frac{1}{v_2 - v_1} \int_{v_1}^{v_2} \left(\frac{dp}{dt}\right)_v dv = \frac{1}{T(v_2 - v_1)} \int_{v_1}^{v_2} \left(p + \frac{a}{v^2}\right) dv \,,$$

holding when a and b are assumed to be independent of T, hence when $\left(\frac{dp}{dt}\right)_v = \frac{R}{v-b} = \frac{1}{T}\left(p + \frac{a}{v^2}\right)$, follows:

$$T\frac{dp_{12}}{dT} = \frac{1}{v_2 - v_1}\int_{v_1}^{v_2} pdv + \frac{1}{v_2 - v_1}\int_{v_1}^{v_2} \frac{a}{v^2} dv = p_{12} + \frac{1}{v_2 - v_1}\int_{v_1}^{v_2} \frac{a}{v^2} dv \,.$$

And in this $\int_{v_1}^{v_2} \frac{a}{v^2} dv = \frac{a}{v_1} - \frac{a}{v_2} = \frac{a(v_2 - v_1)}{v_1 v_2}$ is only valid when a is assumed to be *independent of* v. And only then we have:

$$a = \left(T\frac{dp_{12}}{dT} - p_{12}\right) v_1 v_2 = \left(T\frac{d \log p}{dT} - 1\right) \times \frac{p}{d_1 d_2} \,,$$

when for the pressure of coexistence p_{12} we simply write again p.

And as VAN DER WAALS, just as we in our table, used for the calculation of a a formula for which the constancy of a with respect to v was assumed [1]), it follows in my opinion *by no means* from the fact that after all the *thus found values* of a appear to be proportional with $\sqrt{d_1}$ (or with another function of d_1) that the *real* values of a are proportional with it. For this we should have to carry out the integration $\int \frac{a}{v^2} dv$ on the supposition of an assumed dependence of a with respect to v. And at any rate — at least for higher temperatures — the *vapour* density d_2 would have to occur in the result [2]), not only d_1. From the fact, established by VAN DER WAALS and by me, follows only that a *can* be dependent on v.

But it is, of course, just as well possible that — v_1 being a function of T along the boundary line of the region of saturation — the quantity a is *not directly* dependent on v, but that it is a *pure function of the temperature*, and only *indirectly* dependent on v_1.

If however a is dependent on T, the above formula must be

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[1]) He puts namely the internal latent heat proportional with $a(d_1 - d_2)$.

[2]) The calculation of the *critical* quantities would also have to undergo a far reaching modification. Much of what has been pretty well established now, would then have become entirely uncertain again.

modified, as $\left(\frac{dp}{dt}\right)_v$ will then get a more intricate form — the more so as also b will appear to be a function of T [1]).

I do not consider the theoretical grounds, on which v. D. W. bases the dependence of a on v_1 convincing enough; with his considerations (especially those on p. 365 and 366), for so far as I am able to follow them, I cannot entirely concur [2]).

II. Investigation for hydrogen.

We shall not discuss this at any length, but communicate the result of the investigation desired by v. D. W. concerning the region *above* the critical temperature. No other substance is so well adapted for this as *hydrogen*, of which we possess determinations of the isotherms from — 257° C. = 16° abs. to 100° C. = 373° abs. inclusive through the exceedingly accurate investigations of KAMERLINGH ONNES and his collaborators. Further the very accurate experiments by SCHALKWIJK at 20° C. and those of AMAGAT from 0° to 200° C. The critical temperature of H_2 lies at 33° abs., so that the examined region extends from $^1/_2\,T_k$ (below which H_2 becomes solid, viz. at 14° abs.) to more than 14 T_k; hence it also includes the BOYLE-point at $3^1/_4\,T_k$.

Already in 1903 I was occupied with the course of the values

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[1]) Cf. also my paper: "On the Values of Some Differential Quotients etc." in These Proc. of April 1912 especially p. 1099—1101. There also the temperature function $1+\sqrt{(1-m)}-{}^1/_2(1-m)$, in which $m = T : T_k$, proposed *then* by v. D. WAALS, which is in good harmony with the results, was discussed. But also in this formula the critical temperature plays a very special part, and for values of $T > T_k$ $(m > 1)$ it yields moreover imaginary values.

[2]) In passing I draw attention with what was said on p. 360, where in the transition case $l = 2r$ $(l + 2r =$ distance of the centres, $2r$ diameter molecule) the available space for the movement of a molecule is assumed to be $= 8m$ $(m =$ the volume of a molecule). It is easy to see that this must be about $16m$. For in the case in question the lineary distance of two centres is just twice as great as at contact, hence the volume 8 times as great. But the volume at contact is not $= m$, but $= \frac{6}{\pi} \times m = 1{,}91\,m$, so that the available volume becomes $= 15{,}3\,m$. And as v_k is assumed to be $= 2b_g = 8m$ by v. D. W. [this is however only the case for substances with comparatively high critical temperature, see "New Relations" I (These Proc. of Febr. 1914), where $v_k : b_k$ in general was found $= 2\gamma$], the volume in question is not $= v_k$, but almost $2v_k$. For $v = v_k$, $(l + 2r) : 2r$ is therefore not $= 2$, but about $1^3/_4$, so that the conclusions drawn by v. D. W. with respect to v_k are untenable.

of b for H_2 [1]). But then only AMAGAT's experiments were known, and I calculated the b-values at 0°, 100°, and 200° C. on the supposition that a was constant $= 300 . 10^{-6}$. The thus calculated values were compared with those which would follow from the "equation of state of the molecule", published at the time by V. D. W.; and for greater pressures, from 500 atm. to 1000 or 2800 atm. a good agreement was found.

But later on I saw that not only this formula of VAN DER WAALS, but *every* interpolation formula — both with ascending powers of p and with those of v^{-1} — gave equally good agreement. The objections to VAN DER WAALS's formula (besides theoretical difficulties, about which in the conclusion) lie chiefly in the too small values of b_0, yielded by it. Thus for 0° was found $b_0 = 463 . 10^{-6}$, which is still possible with respect to $b_g = 917 . 10^{-6}$. But for 100° C. b_0 was calculated $= 386$ (b_g likewise $= 917$), and for 200° C. even $b_0 = 306$ ($b_g = 910$).

Besides I came afterwards to the conclusion that the supposition a independent of T could no longer be maintained, as for T_k the value of a_k had to be considerably higher than $300 . 10^{-6}$. But only from the recent determinations of KAMERLINGH ONNES c. s. it appeared very clearly that the value of a also below T_k became greater and greater, and even gets into the neighbourhood of $700 . 10^{-6}$ for — 257°, a_k being only $= 486 . 10^{-6}$.

The very recently published *direct* determination of the critical point by K. ONNES, DORSMAN and HOLST enabled me to revise my calculations and to supplement them. As the above mentioned isotherm determinations of K. O. c. s. all refer to relatively *great* volumes, only about the limiting values b_g there is something to be derived with certainty. But these values, and also those of the corresponding values of a (how the *separation* is possible, will appear presently) appeared to be *dependent on T to such a high degree* that the dependence on v suggested by V. D. W. becomes quite unnecessary — at least below T_k and for the great volumes. Thus e. g. at H_2 the ratio of the value of a at 16° abs. to that of a at T_k is already about 1,4, whereas the ratio of the corresponding values of a for Fluorbenzene amounted to about 1,5 (see the table in § 1).

Whether possibly for *small* volumes the value of a will yet prove to be dependent on v, is another question. But in virtue of the above, I yet consider this not very probable. [2])

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[1]) These Proc. of March 1903.

[2]) And if this should prove to be the case, a proportionality with e.g. $v^{-2/3}$

III. The values of a_k and b_k at the critical point.

In consequence of the variability — not only of a, but also of b — it is exceedingly difficult, if not impossible, to *separate* the two constants of v. d. Waals's equation of state. For large volumes only the quantity $RTb_g—a$ can be calculated, so that nothing would be left but to calculate the corresponding value of b for an *arbitrarily* assumed value of a.

Fortunately there is a circumstance — at least for H, — which enables us to separate the values of a and b, i.e. this circumstance that very probably the *ratio* $\frac{b_g}{a}$ *is constant at all temperatures*: hence that a and b *vary* equally with T, or that a varies together with b.

Kamerlingh Onnes[1]) recently found:

$$T_k = 33°,18 \ abs. \ ; \ p_k = \frac{974}{76} = 12,816 \ atm. \ ; \ d_k = \pm \ 0,0310 \, .$$

From this we calculate:

$$b_k = \frac{RT_k}{p_k} = \frac{0,0036618 \times 33,18}{8 \times 12,816} = \frac{0,121499}{102,528} = 0,0011850 \, .$$

This is still to be multiplied by $(1+a)(1-b) = 1-(b-a)$ at 0° C., i.e. by $1—5,9 \, . \, 10^{-4} = 0,99941$. Because of this b_k becomes:

$$\underline{b_k = 1184,3 \times 10^{-6}} , \quad . \ . \ . \ . \ . \ . \ . \ . \quad (1)$$

expressed in the normal volume at 0° C. Further:

$$a_k = RT_k \times b_k \times \frac{27}{8\lambda} = 0,00014398 \times \frac{27}{8\lambda} = 0,00048592 : \lambda \, .$$

In this the correction quantity $\lambda = \frac{27}{8\gamma—1} \left(\frac{\gamma}{\gamma+1} \right)^2$ with $\gamma = (1 + 004 \sqrt{T_k}) : 2 = 0,6152$ ($\sqrt{T_k}$ being $= 5,760$) has the value $6,885 \times (0,3809)^2 = 0,9988$. Hence a_k becomes $= 0,0004865$. But this must now be multiplied by $(0,9994)^2 = 0,9988$, as we have taken the uncorrected values for RT_k and b_k. In consequence of this we get:

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or still better with $b^{-2/3}$ would be more plausible than with $v^{-1/2}$. For then (for $b^{-2/3}$) the attraction would be in inverse ratio with the molecule surface. I had, indeed, considered this last possibility before. Then the quantity b would vary directly with T, and a would simply follow the variations of b — on account of the proportionality with $b^{-2/3}$.

There are other advantages connected with the supposition $a \because b^{-2/3}$. For $v = \infty$ a would at the same time with b approach a limiting value a_g, and for $v = v_0$ the lowest limiting value a_0 would be in the same relation to a_k as $(b_k : b_0)^{2/3}$, which numerically leads to very good results.

[1]) These Proc. Vol. XX p. 178—184.

$$\underline{a_k = 485{,}9 \times 10^{-6}.} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \qquad (2)$$

Hence almost 500 . 10^{-6}, and not 300 . 10^{-6}, as we still assumed in 1903 also for T_k. The value of b_k, however, is not equal to that of b_g. We shall soon see that for a volume of about 2900 . 10^{-6} (which corresponds with the critical volume) the value of b is already about 4,4 °/₀ lower than b_g. Hence in order to calculate b_g at the critical isotherm, we shall have to increase the just found value 1184,2 by 4,4 °/₀, so that we get:

$$\underline{(b_g)_{krit} = 1236{,}4 \times 10^{-6}} \quad . \; . \; . \; . \; . \; . \; . \qquad (1^a)$$

If we suppose $a_k = (a_g)_k$, hence a not dependent on v, we get for T_k:

$$\left(\frac{b_g}{a}\right)_{krit} = 2{,}545 \;, \quad . \; . \; . \; . \; . \; . \; . \; . \qquad (3)$$

which relation we shall presently find back at the BOYLE-point.

We assumed above the value 2900 . 10^{-6} for v_k. This we namely calculate from

$$v_k = \frac{2{,}0154}{0{,}0310 \times 22412} = \frac{2{,}0154}{694{,}8} = 0{,}002901 \;,$$

so that $v_k : b_k$ would be $= 2{,}45$, which would yield the value 0,69 for γ with $v_k : b_k = (\gamma + 1) : \gamma$. This value seems rather high, because a value $\gamma = 0{,}615$ (see above) would correspond with a critical temperature of 33° abs. When we take $d_k = 0{,}030$ instead of 0,031, v_k would have become $= 2998 \, . \, 10^{-6}$, hence $v_k : b_k = 2{,}53$, $\gamma = 0{,}65$.

Now KAMERLINGH ONNES (loc. cit. p. 184) found the value 0,000398 for the coefficient of direction of the straight diameter. For the reduced value of γ follows from this $\gamma = 0{,}000398 \times 33{,}18 : 0{,}031 = 0{,}01321 : 0{,}031 = 0{,}426$, which seems too small to us, seeing that the value of γ for an *ideal* substance is 0,5, and the values for all other substances must lie above this. With $d_k = 0{,}030$ the only slightly higher value 0,44 would have been obtained.

The solution of this contradiction will probably lie in this that KAMERLINGH ONNES calculated the value of γ from the observations between about 20° and 15° abs., the critical temperature lying at 33° abs. The extrapolation of 20° (the boiling point) to 33° seems therefore rather hazardous, particularly as often only *above* the boiling point the *true* direction of the so-called straight diameter manifests itself¹). Hence the value of γ, calculated from KAMERLINGH ONNES' observations at too low temperatures, is not the same as that

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¹) We remind only of mercury, water, helium, etc.

of *our* quantity γ, which represents the coefficient of direction of the *straight line* between $^1/_2\ D_0$ and D_k in a D—T diagram.

IV. The BOYLE-point.

This is another point of great importance for the study of the a and b-values. Then $RTb_g - a$ is namely $= 0$, so that the accurate knowledge of the temperature of the BOYLE point at the same time enables us to know the ratio $b_g : a$ there. According to K. ONNES [1]) the following value follows from the values of the second virial coefficient B calculated by him — by interpolation between the temperature — 164°,09 lying very close to it, and the somewhat higher temperature — 182°,75 — for this point:

$$T_B = -165°,72\ C. = 107,37 \text{ abs.},$$

For RT_B we therefore find $0,39317 \times 0,99941 = 0,39294$, so that, therefore, $1 : 0,39294$ being $= 2,545$, is found:

$$\underline{\left(\frac{b_g}{a}\right)_B = 2,545}\ , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

i. e. **entirely the same value as we found above for** T_k.

Between 33° abs. and 107 abs., i. e. between T_k and $3^1/_4\ T_k$ the *ratio between* b_g *and* a *has remained constant*, so that we have some reason to suppose that this constancy will also be maintained for higher and lower temperatures [2]).

We may further point out that from:

$$RT_k = \frac{8}{27}\lambda\frac{a_k}{b_k} = \frac{8}{27}\lambda\varphi\left(\frac{a}{b_g}\right)_k \quad ; \quad RT_B = \left(\frac{a}{b_g}\right)_B$$

follows:

$$\frac{T_B}{T_k} = \frac{27}{8\lambda\varphi}\left[\left(\frac{a}{b_g}\right)_B : \left(\frac{a}{b_g}\right)_k\right].$$

If therefore $(a : b_g)_B = (a : b_g)_k$, the ratio $T_B : T_k$ is given by:

$$\underline{\frac{T_B}{T_k} = \frac{3,375}{\lambda\varphi}}\ , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5),$$

in which λ is the correction factor introduced by us (in the formulae for RT_k and p_k), and φ represents the ratio $b_g : b_k$ for the critical isotherm. For H_2 λ is $= 0,999$ (see § III), but for most substances with normally high critical temperature this factor rises to about

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[1]) Comm. 100*a* (These Proc. of Dec. 1907).

[2]) We remark also in this connection that the interval between T_k and $3^1/_4\ T_k$ would mean an enormous temperature range for other substances, e.g. *Fluorbenzene*. For the substance mentioned from 560° abs. to 1820° abs., i.e. from 287° C. to 1546° C.

0,97 (cf. also New Relations I, loc. cit.). As far as φ is concerned, we saw that this quantity is $= 1{,}044$ for H_2. This renders the product $\lambda\varphi = 1{,}043$ for H_2, and the ratio $T_B : T_k = \underline{3{,}236}$. But for other substances, with higher critical temperatures, for which φ gets in the neighbourhood of $1^1/_8$, $T_B : T_k$ will get into the neighbourhood of 3.

V. The value of $B = RTb_g - a$ for 20° C.

Schalkwijk[1]) found lately that his experiments at 20° C. can be represented by the relation

$$pv = 1{,}07258 + 0{,}000667^5\, d + 0{,}0_6 98\, d^2 \ (8-60 \text{ atm.}).$$

Kamerlingh Onnes and Hyndman (Comm. 78^c), however, modified the virial coefficients B and C of the above formula in $667^1.10^{-6}$ and 993.10^{-9}, to facilitate a comparison of their observations at 0° and 20° with those of Schalkwijk.

Later on Walstra (These Proc. Vol. XVII p. 217) calculated from Kohnstamm and Walstra's later experiments (These Proc. XVII p. 203) at 220° C. (125—2200 atm.) the relation

$$pv = 1.07258 + 674{,}0 . 10^{-6}\, d + 856{,}9 . 10^{-9}\, d^2 + 665{,}9 . 10^{-15}\, d^4 (200\text{–}800 \text{atm.}),$$

valid only from 200 to 800 atm., in good harmony with the corrected formula of Schalkwijk, which according to Walstra would be valid up to ± 120 atm.

By addition of a term with d^6 Walstra could derive a formula, which comprises *all* the observations — also those of Schalkwijk at lower pressures, and which is valid up to 1000 atm.

It runs:

$$pv = 1{,}07258 + 676{,}3 . 10^{-6}\, d + 882{,}15 . 10^{-9}\, d^2 + 669{,}54 . 10^{-15}\, d^4 - \\ - 0{,}151 . 10^{-21}\, d^6 \text{ (up to 1000 atm.)}.$$

Finally K. Onnes, Crommelin, and Smid (Comm. 146^b) gave the equation:

$$\underline{pv = 1{,}07261 + 657{,}12 . 10^{-6}\, d + 1292{,}6 . 10^{-9} \text{ (up to 100 atm.)}}, \quad (6)$$

in connection with their definitive determination of the isotherm of 20° C. from 60—100 atm.; instead of Schalkwijk's formula (which only holds from 4 to 60 atm.).

When from this we calculate the value of pv at 100 atm., it deviates only 0,1 % from that which can be calculated from the values found by Amagat at 0° and 100° C. The term $A = RT$ has evidently been calculated from $(1 + 0{,}0036618 \times 20) \times 0{,}999416 = = 1{,}07261$, so that for 0° C. the virial coefficient B was put at

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[1]) Thesis for the Doctorate.

$584 \, . \, 10^{-6}$. (We shall immediately see that this is *somewhat* too low, as it must lie between 588 and 594.10^{-6}).

When we now assume the value 2,545 also for 20° C. for the ratio $b_g : a$, we easily calculate from $RTb_g - a = 657{,}12 \, . \, 10^{-6}$:

$$\underline{a_{20^\circ} = 379{,}9 \, . \, 10^{-6}}; \qquad \underline{(b_g)_{20^\circ} = 966{,}8 \, . \, 10^{-6}} \quad . \quad . \quad . \quad (7)$$

As the values for 0° C. will certainly not be far from this, it is clear that the value $a = 300 \, . \, 10^{-6}$ assumed before by us and others is much too low, and must be raised to about $380 \, . \, 10^{-6}$, and b from $900 \, . \, 10^{-6}$ to about $970 \, . \, 10^{-6}$.

Accordingly the value of a (hence also that of b_g) has increased between 0° C. (273° abs.) and $T_k = 33°$ abs. in ratio of 380 to 486, i.e. from 1 to almost 1,3. And we shall presently see that this increase below T_k (to about $^1/_2$ T_k) still extends from 486 to about 740, i. e. that the attraction at 16° abs. is almost 1,5 times larger than that at the critical temperature. It is, therefore, as we remarked above, quite unnecessary to make the quantity a increase with the *volume* below Tk. The increase found is already perfectly covered by the *direct influence of the temperature.*

VI. General Considerations on the Dependence on the Temperature.

For the fact that b decreases at higher temperature a theoretical ground can be adduced in the circumstance that the value of b is *exclusively* determined at the *moment of the collisions*, and *by* these collisions of the molecules. When in *the first place* the atoms inside the molecule show a certain position of equilibrium with respect to each other, this will necessarily be modified at the collision. The atoms will approach each other more closely till the normally directed relative velocity of the colliding molecules is exhausted; not until then will the colliding molecule be rebuffed by the other in consequence of the repulsive force excited by the shifting out of the state of equilibrium. When we now calculate the value of the virial of collision on the supposition of the varying size of b during the impact, we obtain an expression (see § VII) of the form

$$b_g = 4m \times f(T) \, , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which can account for the decrease of b_g with the temperature. At low temperature the mean velocity at the impact will, indeed, be very small, so that the molecule will be comparatively little compressed, whereas at higher temperature the velocity, and consequently also the compression will be considerably greater.

Already in my paper of 1903 (loc. cit. p. 580) I remarked with regard to the decrease of b_0 stated then, without entering into further details: "*It seems that at higher temperature the atoms in the molecule may approach one another closer than at lower temperature*".

A further calculation shows us, however, that the above mentioned $f(T)$, which in approximation (see the following paragraph) is proportional with $(1-a\sqrt{T})^2$, do not sufficiently represent the values found of the virial coefficients B above 20° C. (the one fixed point, on which the calculation of $4m$ and the coefficient a is based), and especially below T_k (the second fixed point).[1]) *Too low* values are found. At 200° C. B would only be $\doteq 1170 \,.\, 10^{-6}$, while 1280 would follow from AMAGAT's experiments. And for $-252°$, $-255°$, $-257°$ would follow the only little increasing values 403, 419, and 429 for $-B$, whereas the rapidly increasing values 480, 500, and 630 (about) would in reality follow from K. ONNES's experiments.

This course would sooner point to an *exponential* function of T. In the collisions we shall, indeed, have to take account in the *second place* of the influence of the *field of force* on the distribution (density) of the molecules round the considered molecule, e.g. according to the theory of BOLTZMANN. We then arrive at an expression for b (when we namely assume the *same* function of the temperature for b_g as was found for a, which would be in contradiction with BOLTZMANN's formula) of the form

$$b_g = (b_g)_\infty \times \frac{RT}{a'}\left(e^{\frac{a'}{RT}} - 1\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

in which $(b_g)_\infty$ represents the limiting value at very high temperature, just as before $(b_g)_0 = 4m$ denoted the limiting value at very low temperature.

Now we find better agreement above 20° C. (e.g. $B = 1280 . 10^{-6}$ in perfect agreement with AMAGAT), and also below T_k, where we now find 470, 525, and $570 \,.\, 10^{-6}$ at the above mentioned temperatures. But now the values *between* 20° C. and T_k are almost all somewhat too small. This may perhaps be remedied by also making the first influence (that of the molecule-compression) play a part — though it be an insignificant one — which renders it possible to reduce the exponent a' to a somewhat smaller value, so that the curvature between 20° C. and T_k becomes somewhat slighter.

In any case the influence (at least for *hydrogen*) of the (*real*) molecule diminution by compression seems, however, in consequence of the rigid atom system, to be very slight compared with the much

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[1]) I.e. when we assume the same function of the temperature to be valid for a, and therefore make a vary together with b. (See also the Note to § 2).

greater influence of the (*apparent*) molecule diminution in consequence of the decreasing influence of the field of force at higher temperatures.

But we shall see (in the sequel) that also BOLTZMANN's function of the distribution cannot be maintained, so that we shall have to draw up an *entirely new theory* for the calculation of a and b.

VII. The virial of collision on compression of the molecules.

Though the influence in consequence of the compression of the molecule during collisions in the case of *hydrogen* considered by us may consequently be assumed to be very slight, it may yet be desirable to examine this case briefly, with a view to other substances where this influence might make itself felt to an appreciable extent.

Put the distance of equilibrium of the charges of the two atoms in the molecule $= r_0$, the quasi-elastic force for a small displacement $\delta = r - r_0$ can be represented by:

$$F = \varepsilon (r - r_0) \quad , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

in which $\varepsilon = \frac{ve^2}{r_0^3}$, when v represents the valency of the atoms (or atom groups) and e the elementary charge. If $r > r_0$, there arises an attractive force between the two atoms; if $r < r_0$, then there arises a repulsive force. Now the distance r_a, to which the atoms will most closely approach each other, can evidently be calculated from

$$^1/_2\, \mu (V_r)^2{}_n = \int_{r_0}^{r_a} 2\varepsilon (r - r_0)\, dr = \varepsilon (r_a - r_0)^2 = \varepsilon (r_0 - r_a)^2 ,$$

when μ is the mass of a molecule and $(V_r)_n$ the relative normally directed velocity, with which one molecule collides against another. In this we of course assume that at very high temperatures r_a can never become $= 0$, because this would be prevented by the very great repulsion then appearing, which is no longer represented by the above equation (10), this holding only for comparatively small values of $\delta = r - r_0$. (The factor 2 has been introduced, because at the impact the force action is exerted by the two molecules together).

Now the mean value of $^1/_2\, \mu V^2$ is equal to $\frac{3}{2} \frac{RT}{N}$, when N denotes the total number of molecules present, so that the mean value of $^1/_2 \mu (V_r)_n{}^2$ will be the $^2/_3$ part of this[1]), hence $= RT : N$. For the determination of r_a we have therefore the equation

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[1]) This is namely the mean *relative, normal* velocity. Now $V_r = V \sqrt{2}$, hence

$$\varepsilon\,(r_0 - r_a)^2 = \frac{RT}{N} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

For the further calculation of the repulsive virial we start from

$$pv = RT - \tfrac{1}{3}\,\Sigma\,\varrho\, f(\varrho)\,,$$

in which $f(\varrho)$ must be considered positive in case of attraction. Performing the summation, we get for pv:

$$pv = RT - \tfrac{1}{6}\,N \int_{\varrho_a}^{\varrho_0} dN\,\varrho\, f(\varrho)\;,$$

in which dN — the number of molecules in a spherical shell of a thickness $d\varrho$ round the molecule that is thought spherical — is represented by $dN = 4\pi\varrho^2\, d\varrho \times n \times \tau$, in which n is the number of molecules in the volume unit and τ the factor of density, which is $= 1$ for infinitely large volume, but is represented by $\tau = \dfrac{b}{b_g}\dfrac{v}{v-b}$ for arbitrary volume. As our considerations for the present only refer to b_g, we put therefore $\tau = 1$. We have divided by 2, because else on summation every pair of molecules would have been counted double. Further ϱ_0 is the molecule radius, corresponding with r_0 in (11), whereas ϱ_a corresponds with r_a. Thus we get:

$$pv = RT - {}^2/_3\,\pi N n \int_{\varrho_a}^{\varrho_0} \varrho^3 f(\varrho)\, d\varrho\;.$$

As ${}^2/_3\pi\varrho_0^3 \times N = 4m = (b_g)_{T=0}$, we also have:

$$pv = RT - \frac{\varkappa (b_g)_0}{\varrho_0^{\,3}} \int_{\varrho_a}^{\varrho_0} \varrho^3 f(\varrho)\, d\varrho\;.$$

In this $f(\varrho)$ — multiplied by 2, see above — is according to (10) $= 2\varepsilon\,(r - r_0) = 2\varepsilon\,(\varrho - \varrho_0)$. Further:

$$\int_{\varrho_a}^{\varrho_0} \varrho^3\,(\varrho - \varrho_0)\, d\varrho = \left(\frac{\varrho^5}{5} - \frac{\varrho_0\varrho^4}{4}\right)_{\varrho_a}^{\varrho_0} = \tfrac{1}{5}\,(\varrho_0^{\,5} - \varrho_a^{\,5}) - \tfrac{1}{4}\,\varrho_0\,(\varrho_0^{\,4} - \varrho_a^{\,4})$$

$$= -\tfrac{1}{20}\,\varrho_0^{\,5}\,(1 - 5x^4 + 4x^5)\,,$$

when $\varrho_a : \varrho_0$ is put $= x$. Hence ϱ_0 is the normal lineary dimension of the molecule before the impact, ϱ_a representing the smallest

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$^1/_2\,\mu\, V_r^2 = 2 \times {}^1/_2\, RT : N = 3\, RT : N$. But the mean value of $V^2{}_n$ is $= {}^1/_3$ of that of V^2, so that $^1/_2\,\mu\,(V_r)^2{}_n$ becomes $= RT : N$.

dimension at the greatest deformation reached in the collision. Hence we find finally:

$$pv = RT + n\,(b_g)_0 \times \tfrac{1}{10}\,\varepsilon \varrho_0^2\,(1 - 5x^4 + 4x^5)\,.$$

Now according to (11):

$$\varepsilon\,(r_0 - r_a)^2 = \varepsilon\,(\varrho_0 - \varrho_a)^2 = \varepsilon \varrho_0^2\,(1 - x)^2 = \frac{RT}{N}\,.$$

so that we get:

$$pv = RT\left[1 + \frac{n}{N}(b_g)_0\,\frac{1-5x^4+4x^5}{10(1-x)^2}\right] =$$

$$= RT\left[1 + \frac{(b_g)_0}{v} \times \frac{1}{10}\,(1 + 2x + 3x^2 + 4x^3)\right],$$

as $N : n$ is evidently $= v$. When we now put

$$(b_g)_T = (b_g)_0 \times \tfrac{1}{10}\,(1 + 2x + 3x^2 + 4x^3),$$

then as before we get (for infinitely large volume, and not taking the attraction into account):

$$pv = RT\left(1 + \frac{(b_g)_T}{v}\right).$$

Now we may write $1 - a\sqrt{T}$ for $x = \varrho_a : \varrho_0$, as $(1-x)^2 = \dfrac{RT}{N\varepsilon\varrho_0^2} =$ $= a^2 T$ according to (11), in which a is a coefficient, depending on the size of the quasi elastic atom force constant ε in (10), viz. $\varepsilon = ve^2 : r_0^3$, so that $a^2 = \dfrac{R : N}{v\,\dfrac{\varrho_0^2}{r_0^3}\,e^2}$. In this way we find finally for $(b_g)_T$:

$$\underline{(b_g)_T = (b_g)_0 \times \left[1 - 2a\sqrt{T} + {}^3/_2\,(a\sqrt{T})^2 - {}^2/_5\,(a\sqrt{T})^3\right]}\,, \quad (12)$$

where the form between [ ] can also be written $= (1 - a\sqrt{T})^2 +$ $+ {}^1/_2\,(a\sqrt{T})^2\,(1 - {}^4/_5\,a\sqrt{T})$.

Hence $(b_g)_T$ becomes $= (b_g)_0 = 4\,m$ for $T = 0$, but at all other temperatures $b_g < 4\,m$, in consequence of the diminution of the molecule which *temporarily* takes place during the collision. The extreme limit of this diminution is determined by the ratio $\varrho_a^3 : \varrho_0^3 = x^3$, hence by the expression $(1 - a\sqrt{T})^3$.

At low temperatures the expression between [ ] may be represented by $(1 - a\sqrt{T})^2$ in approximation.

VIII. Calculation of the values of *a*, *b*, and *B*.

When from the values of b_g at the two fixed points 20° C. and T_k, i.e. $b_g = 967\,.\,10^{-6}$ and $b_g = 1236\,.\,10^{-6}$ (see above), we now calculate the values of the two constants $(b_g)_0 = 4\,m$ and α, we find:

$$(b_g)_0 = 1393{,}4 \quad ; \quad \alpha = 0{,}01022 \quad . \quad (a_0 = 547{,}5)$$

The corresponding limiting value of a_0 is obtained from $b_g : a = = \text{const.} = 2{,}545$ (see above). Hence we have for arbitrary values of b_g and a:

$$b_g = (b_g)_0 \times f(T) \quad ; \quad a = a_0 \times f(T)\,, \quad . \quad . \quad . \quad . \qquad (13)$$

in which $f(T) = 1 - 2\alpha\sqrt{T} + {}^3/_2\,(\alpha\sqrt{T})^2 - {}^2/_5\,(\alpha\sqrt{T})^3$ in the case considered by us.

We find then further for the 2nd virial coefficient B:

$$B = RT\,b_g - a = a\left(RT\,\frac{b_g}{a} - 1\right) = a_0 f(T)\left(\frac{T}{T_B} - 1\right), \qquad (14)$$

because $b_g : a = 1 : RT_B = 2{,}545$. (See § IV). T_B is the temperature of the Boyle-point. In this way the following tables were calculated. (See the tables on this page and p. 766).

In table I all the values have been collected, which are the *foundation* of the calculation of a, b_g, and B — not only for the case that $f(T) = 1 - 2\alpha\sqrt{T} +$ etc. but also for the case that $f'(T) = = \frac{RT}{\alpha'}\left(e^{\frac{\alpha'}{RT}} - 1\right)$ is assumed. (see § VI). The values $(b_g)_\infty$ and α'

TABLE I.

t	$T_{(abs)}$	RT	$\frac{1}{RT}$	$\sqrt{T}$	$\frac{T}{T_B} - 1$	$\alpha\sqrt{T}$	$f(T)$	$\frac{\alpha'}{RT}$	$f'(T)$
200°,25 C.	473,34	1,73226	0,5773	21,76	3,409	0,2223	0,6251	0,03710	1,0188
100	373,09	1,36537	0,7323	19,32	2,475	0,1975	0,6605	0,04706	1,0239
20	293,09	1,07261	0,9323	17,12	1,730	0,1750	0,6938	0,05991	1,0306
0	273,09	0,99941	1,0006	16,53	1,544	0,1689	0,7031	0,06430	1,0328
— 103,54	169,55	0,62049	1,6116	13,02	0,5791	0,1331	0,7594	0,1036	1,0540
— 139,84	133,25	0,48765	2,0507	11,54	0,2410	0,1179	0,7844	0,1318	1,0686
— 165,72 (T_B)	107,37	0,39294	2,5450	10,36	0	0,1059	0,8045	0,1635	1,0864
— 182,75	90,34	0,33062	3,0247	9,505	— 0,1586	0,09714	0 8195	0,1944	1,1038
— 204,62	68,47	0,25057	3,9909	8,275	— 0,3623	0,08457	0,8414	0,2565	1,1399
— 217,32	55,77	0,20410	4,8996	7,468	— 0,4804	0,07632	0,8554	0,3148	1,1753
— 239,91 (T_k)	33,18	0,12143	8,2306	5,760	— 0,6910	0,05887	0,8874	0,5289	1,3180
— 252,47	20,62	$0{,}07546^2$	13,252	4,541	— 0,8080	0,04641	0,9100	0,8516	1.5775
— 255,32	17,77	$0{,}06503^2$	15,378	4,215	— 0,8345	0,04308	0,9166	0,9882	1,7065
— 257,10	15,99	$0{,}05851^8$	17,088	3,999	— 0,8511	0,04087	0.9208	1,0981	1,8199

have again been calculated from the fixed values at 20° C. and T_k. We found:

$$(b_g)_\infty = 938{,}2 \quad ; \quad a' = 1:15{,}562 = 0{,}06426\,.\,(a_\infty = 368{,}6)$$

When $a' : RT = y$ is put, we can easily calculate $f'(T) = (e^y - 1) : y$ at higher temperatures from $f'(T) = 1 + {}^1/_2\, y + {}^1/_6\, y^2 + \ldots$

In table II the values of a, b_g, and B are put together, which can be calculated from the thus found values of $f(T)$, $f'(T)$ and ${}^T/_{T_B} - 1$ from (13) and (14). The last column but one gives the values of B which would follow from K. ONNES' and AMAGAT'S experiments.

With regard to the value of $10^6 B$ at 0° C. we may state that it is clear that it will in any case lie in the neighbourhood of 590 (594 or 588 according as one theoretical interpolation formula is

TABLE II.

t	10^6 a	$10^6 b$	$10^6 B$	10^6 a	$10^6 b$	$10^6 B$	$10^6 B$ *found*	10^6 a *found* = $10^6 B$ *found* : 1) : $({}^T/_{T_B} -$
				(368,6)	(938,2)			
200,25 C	342,2	870,9	1166,4	375,6	955,9	1280,3	1280 (*A*)	375,5
100	361,6	920,3	894,9	377,5	960,6	934,1	997 (*A*) 862 (*O*)	(403 à 348)
20	**379,9**	**966,8**	**657,1**	379,9	**966,8**	**657,1**	**657,1** (*O, Sch.*)	**380**
0°	384,9	979,7	594,2	380,8	969,0	587,7	619 (*A*) 566 (*O*)	(401 à 367)
— 103,54	415,8	1058,2	240,8	388,6	988,9	225,0	240 (*O*)	414
— 139,84	429,5	1093,1	103,5	393,9	1002,6	94,9	108 „	448
— 165,72 (T_B)	440,5	1121,1	0	400,5	1019,2	0	0 „	—
— 182,75	448,7	1141.9	— 71,2	406,9	1035,6	— 64,5	(— 82) „	(517)
— 204,62	460,7	1172,5	— 166,9	420,2	1069,4	— 152,2	— 171 „	472
217,32	468,3	1191,8	— 225,0	433,3	1102,6	— 208,2	(— 243) „	(506)
— 239,91 (T_K)	**485,9**	**1236,4**	**— 335,8**	**485,9**	**1236,4**	**— 335,8**	— **336** „	**486**
— 252,47	498.4	1268,4	— 402,7	581,5	1480,0	— 469,8	— 480 ± „	594
— 255,32	501,8	1277,1	— 418,8	629,1	1601,0	— 525,0	— 500 ± „	599
— 257,10	504,1	1282,9	— 429,0	670,9	1707,4	— 571,0	— 630 ± „	740
	↓	↓	↓					
	547,5	1393,4	— 547,5					

with $f(T) =$
$= 1 - 2\,\alpha \sqrt{T} +$ *etc.*

with $f'(T) =$
$= (e^{\alpha'/RT} - 1) : \alpha'/RT$

used or another). The value $580 \, . \, 10^{-6}$ given by K. ONNES seems too low. From his experiments even the still lower value 566 is calculated, whereas from those of AMAGAT 619 would follow. The mean of the two values, viz. 593, will not be far from the truth.

Also for 100° C. the probably correct value, viz. 934, will probably lie near the mean value of the results of the two series of experiments (this mean of 862 and 997 is 930).

The advantage of the exponential expression for $f(T)$ over the other with $1-2\alpha\sqrt{T}+$ etc. is apparent especially in the last three values of 10^6B, calculated from the experiments for the unsaturated H_2-vapour below T_k, which values range from —480 to about —630. The latter expression would give for this the values —403 to —429, which increase but little and are much too low, while the exponential formula yields the better values from —470 to —570. But here possibly also the effect of quanta comes into play.

Exceedingly instructive is also the last column, in which the values of 10^6B (found), divided by T/T_B-1 are given. If namely the relation $b_g:a$ is really constant, as we have reason to suppose in virtue of what was stated in § III and IV, the quotient $B:(T/T_B-1)$ indicates the value of a at any temperature according to (14). It is then seen still more clearly that at 0° and 100° AMAGAT's observations yield too high values, those of K. ONNES too low values (in connection with the *fixed* value at 20° C.). And further that the B-values at —183° and —217° calculated from K. O.'s observations do not at all fit in with the others, as they yield too high values for 10^6a, namely 517 and 506, which values would be higher than those at T_k, which is impossible.

In the next paper I shall give something about the theoretical derivation of the exponential formula, also in connection with the earlier views of REINGANUM and myself, and of the later ones of KEESOM in this respect, followed by the drawing up of an entirely new theory on the virial of attraction and collision. Further the influence of the volume will be discussed, and also the liquid volumes in the region of saturation; then the calculation of the values of the virial coefficient B from AMAGAT's and K. ONNES' material, the results of which have already been mentioned in the above table II.

Fontanivent, 1916—1917. *(To be continued).*

Physiology. — *"A Contribution regarding the Shifting of Radio-active Equilibria under the Influence of Fluorescein"*. By Prof. H. Zwaardemaker.

(Communicated in the meeting of Sept. 29, 1917).

In an earlier paper[1] I demonstrated that the diffusible potassium in the circulating fluid of surviving organs of cold-blooded animals could be replaced by the ions of other radio-active elements. This should then be done in approximately aequiradio-active quanta. It deserves notice that with summer-frogs the dosage may be much smaller than with winter-frogs, which accounts for the difficulties workers have encountered in summertime with this laboratory animal, especially in quantitative studies. In the past months we, my co-workers and myself, succeeded in determining the ratios for the Kroneckered ventricle. We first lowered the calcium content of the artificial circulating fluids, and when this turned out unsuccessful, it was raised. An amount of 250 mgrms $CaCl_2$ (without water of crystallization) per Liter proved efficient.

It will be advisable to use distilled water in vitro. The osmotic pressure was maintained by 6½—7 grammes of sodium chloride and the reaction was made slightly alkaline by adding 200 mgrs. $NaHCO_3$. This solution is prepared the day before and can be kept (not too long though) in bottles of ordinary glass. Examined on potassium it must not contain more than 1 mgrm per Liter.

In order to ascertain whether the heart is normal, first a normal Ringer's mixture is perfused. In winter this mixture should preferably contain ± 100 mgrms of KCl per Liter. It now appears that in summer-time we have to go down to 20—50 mgrms, else the hearts will refuse to beat. If by chance the usual Ringer's mixture of the laboratory should have been used for the preliminary experiment, the hearts will perform a few beats, then stop, in order to resume pulsation for a short time, when measures have been taken to wash off the superfluous potassium. By adding a large amount of calcium to the fluid, the toxicity of potassium may be lowered.

With 250 mgrms $CaCl_2$ per Liter the summer doses range:

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[1] Proceedings Royal Society Vol. XIX p. 633.

for potassium chloride from 20 to 50 mgr. p. Liter
,, rubidium chloride from 30 to 70 ,, ,, ,,
,, a uranium salt from 0,6 to 5 ,, ,, ,,
,, thorium nitrate from 2 to 10 ,, ,, ,,

Fixing the typical winter-doses for the four salts at 100, 150, 25 and 50 mgrms per Liter, an appropriate corresponding summer dosis will be 50, 70, 1—5, and 1—10. In summer, however, the individual differences are larger and the hearts are more sensitive to the toxic effect of too massive a dosis and to the absence of the radio-active element when the dosis is small, so that the proper concentration for each salt is more difficult to find.

Approximate Metaldosis.
(mgrms)

	In Winter.	In Summer.
K.	53	20
Rb.	105	34
U.	12	24
Th.	24	5

Radio-equivalent.

	In Winter.	In Summer.
K.	0.000045	0.000018
Rb.	0.000020	0.000007
U.	0.000040	0.000008
Th.	0.000030	0.000006

It seems to me that for the Rana esculenta a somewhat smaller quantum of radio-active element will generally suffice than for the Rana temporaria, but on the other hand its toxic dosis is also smaller. What factors determine the quantity for every type of animal we have not yet been able to decide. My impression is that the temperature, provided it be maintained, the exposure to the sun on the preceding days and the nutritive condition are of some influence here. Dr. S. de Boer will discuss the summerdosage in detail in a special article in the "Archives Néerlandaises".

In an earlier volume of these Proceedings (Vol. XIX p. 1043) we pointed out that a notable antagonism exists between potassium and uranium in such a sense that they mutually neutralize each other when present in amounts of a certain ratio. I have demonstrated that the coinciding calcium exerts an influence here, which naturally led us to suppose that this it was that caused the summer-equilibria and the winter-equilibria to be so differently located. Our supposition proved to be right. To destroy the effect of both the uranium and the potassium a relatively much smaller amount of uranium is required in the circulating fluid in summer than in winter, although the same amount of potassium is taken in either season, so that the fluid is getting somewhat like the potassium-free circulating fluid just described, which is used also in the intermediate experiments. The equilibria looked for are always reached with a potassium-free fluid preceding.

The following figure shows the winter- and the summer-equilibria between potassium and uranium. The first have been determined by Mr. T. P. Feenstra, the second by Dr. S. de Boer. The character of the curves is very much alike and both decline with the larger doses more and more gently, running at last nearly parallel to the abscisse. The addition of a very large quantity of potassium salt has a disproportionately slighter effect than that of a much smaller dosis of uranium-salt. As yet only uranyl compounds were at my disposal, but no difference revealed itself here between the various salts (nitrate, acetate, sulphate).

Also the summer-equilibria between potassium and thorium and those between rubidium and uranium have been established by us. The curve of the counterbalanced concentrations runs, on the whole, like the one in the figure below.

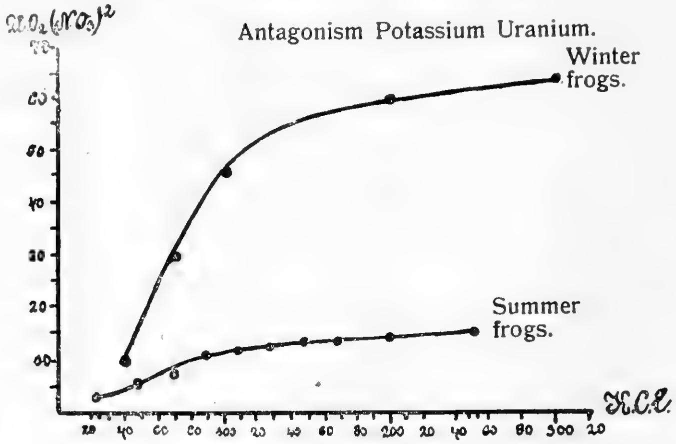

Fig. 7.

All our equilibria have been maintained for some time, 5—10 minutes at the least, and we have watched for some latent automatic pulsation that might exhibit itself, when a single contraction was elicited by a single gentle push.

Our interest was now excited to see whether by the addition of fluorescein the equilibria would be shifted, and whether this would occur in the same sense as was the case with the summer-equilibria, when compared with the winter-equilibria. The results confirmed our supposition.

I found e.g. for the equilibria:

50 mgr potassium-chloride + 5 mgr uranylnitrate = 0 (per liter);
100 mgr potassium-chloride + 15 mgr uranylnitrate = 0 (per liter), [1])
that an extra addition of 100 mgrms of fluorescein to the heart, arrested in equilibrium, made it resume its normal beats.

For the equilibrium:

50 mgrms potassium-chloride + 6 mgr uranylnitrate = 0 (per liter),
I demonstrated the same and also that radiation or an extra addition of 250 mgr of potassium chloride again caused a standstill without injuring the heart in the least, as was made out by a prolonged experiment.

The same experiment was performed for a potassium-thorium-equilibrium:

25 mgr potassium-chloride + 5 mgr thorium-nitrate = 0 (per liter).
By the addition of 100 mgrms of fluorescein the pulsations recurred and this time by an increment of 50 mgr of potassium-chloride a fresh standstill was brought about.

It will be understood that finally in all these experiments a perfusion of potassium-free fluid was administered, after which the normal Ringer's mixture made the heart recover its normal condition.

It is evident, then, that fluorescein promotes the sensibility to the radio-active elements, in the same way as the summer does, viz. the action of the heavy metals more than that of the light ones. It follows that an increment of light metal is wanted to restore the equilibrium. Mesothorium-radiation can also be taken instead of potassium. We might also put it in this way: that both the summer and the fluorescein depress the curve of the potassium-uranium equilibria (also that of the rubidium-uranium equilibria, besides that of the potassium-thorium equilibria). Calcium, on the other hand, yields a higher curve. What must be done to restore the equilibrium may be gathered from the curve itself.

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[1]) Different types of animals.

It is certain, meanwhile, that neither the summer shifts nor the fluorescein shifts depend on the radio-active elements, as these are the same in summer and in winter. We are inclined to think the different reaction of the muscle cells to be the cause. But before ascribing the different behaviour to the more or less mysterious nature of the automatic rhythm it will be good to study the factors to which the adsorption of the heavy and the light metals is due. Much [1]) in the phenomena observed may perhaps be accounted for by the mutual adsorption-extrusions of the ions. The antagonism, which manifests itself after the adsorptions have brought the ions in the vicinity of the irritable substance, does not offer any ground for explanation.

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[1]) For instance: standstill inside one minute, when a suitable uranium fluid is rapidly replaced by a potassium fluid, of itself also suitable; the approximately horizontal character of the equilibrium curve with the higher doses; the calcium effect.

Physiology. — "*On Caesium-ions and Cardiac Action*". By Prof. H. ZWAARDEMAKER.

(Communicated in the meeting of October 27, 1917).

Years ago SIDNEY RINGER[1]), in his study of the composition of artificial circulating fluids, established that the isolated frog's heart can sustain its beats for some time, if to an isosmotic, faintly alkaline sodium-chloride solution, containing the necessary quantum of calcium, caesium chloride is added instead of potassium chloride. Caesium salt was substituted for potassium salt in an equivalent molecular amount. When we carried out the same experiment last winter we did not attain a definite result, but when we discovered this year that the heart of the summer frog responds to smaller doses of potassium chloride than the heart of winter frogs, we have undertaken again SIDNEY RINGER's caesium experiment[2]). After augmenting the usual calcium amount (200 mgr. $CaCl_2$ per Liter) to 250 mgr. dry calcium chloride (without water of crystallization) the summer hearts could be made to continue beating for an indefinite space of time by the aid of an appropriate quantity of caesium chloride. Broadly speaking, we can say that, for one heart, the dosis of caesium corresponds with that of potassium and rubidium. The minimum dosis, needed by a Kroneckered frog's heart in its RINGER's mixture, is smallest in weight for potassium chloride, then follows most probably rubidium, while the dosis of caesium chloride must be slightly larger. Molecularly, therefore, the dosages may be called fairly equal. The toxic dosis is a multiple; but we also found that, whilst (in summer) double the dosis of potassium is in most cases toxic, the caesium-dosis may be many times the minimum dosis that keeps the heart beating. A little beyond the minimum dosis lies the optimum quantity. In the months of September and October it amounted to about 40 mgr. C_8Cl per Liter of potassium-free RINGER's mixture.

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[1]) SYDNEY RINGER, Journal of Physiology. Vol. 4, p. 370.

[2]) I was particularly prompted to do so, as the question was put to me by Dr. C. E. BÜCHNER, of Amsterdam whether it would be possible to demonstrate biologically the radio-activity of caesium, which had often been surmised on account of the relationship between this metal and potassium and rubidium.

When this had been established[1]) for a good many cases, I went into the question whether potassium-, rubidium-, caesium-chloride could be used promiscuously, which I found to be the case. The different substances may be alternated without any interval, provided the dosis be well chosen. It is quite unnecessary, therefore, to interpolate a perfusion of a solution of merely 7 grms of NaCl, 200 mgrs of $NaHCO_3$, 250 mgrs of $CaCl_2$ per Liter of pure water. If, however, we administer, instead of a caesium-containing fluid, a fluid containing uranium or thorium, we cannot maintain the cardiac action without an interpolation. If there is still some caesium left in the heart, it will come to a standstill when a uranium-, or thorium-fluid is sent through, which in other cases would answer the purpose, as illustrated by the subjoined figures.

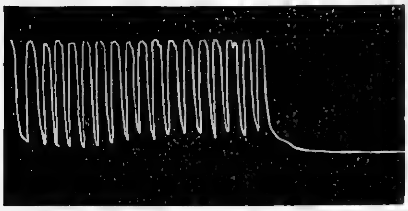

Fig. 1.

A frog's heart, fed with a caesium-fluid (40 mgr. of CaCl per liter of potassium-free Ringer's mixture) and beating vigorously, is rapidly perfused with a fresh thorium-solution (20 mgrms of $Th(NO^3)^4$ per liter of potassium-free Ringer's mixture) the moment the curve in the figure commences. The heart will stop in complete relaxation after three minutes. After 25 minutes it will as suddenly recover its tonus and commence a series of rhythmical beats. This behaviour is easily accounted for. After the caesium-beats follows the caesium-thorium-equipoise, which causes a standstill, whilst in the end the thorium evokes new contractions.

It will be remembered[2]) no doubt that exactly the same phenomenon presented itself in a previous experiment in which potassium or rubidium was replaced by uranium or thorium. There also potassium- and rubidium-salts could be used promiscuously, nay could even be mixed. The same holds good for uranium- and thorium-salts. However, a potassium-, or rubidium-salt appeared to be antagonistic to uranium or thorium-salt. The favourable effect exerted by every one of these salts *per se* is neutralized by the coincident presence of a salt of the other group. The two actions counterbalance each other, so that it is as if they do not exist and as if the heart is in the same condition as it would be when left to itself without

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[1]) Here I have much pleasure in thanking the assistant DE LIND VAN WIJNGAARDEN for his painstaking help.

[2]) H. ZWAARDEMAKER, Proceedings of 24 Febr. 1917, Vol. 25, p. 1096. (Proc. Vol. 19, p. 1048).

the usual diffusible potassium in the circulating fluid. It is then reduced to a standstill, from which it does not recover of itself, and after which it can initially resume its normal pulsations by adding in excess an appropriate amount of one of the radio-active salts, either of the one or of the other group. Now it appears that in the like equilibria caesium must be classed under the light metals. Potassium, rubidium, and caesium, therefore, constitute biologically one group; uranium and thorium the other.

In this connection it was expedient to ascertain whether the caesium-heart responded to electric stimuli, as is the case with the potassium- and the rubidium-heart. This proved to be the case. An appropriate induction stimulus produces an extrasystole. When, however, the ventricle was taken separately, the compensatory pause was absent.

Evidently the preparation acts merely upon ventricular automaticity, originating on the distal side of the atrioventricular boundary-line, and it consequently behaves like the sinus venosus as regards the rhythm after extrasystole.

It appears, then, that caesium resembles potassium also in its relation to the extrasystoles, and must be contrasted with uranium, which precludes extrasystoles [1]).

In a previous paper [2]) I have pointed out that the potassium- (resp. rubidium-)uranium-(resp. thorium-)equilibria can be largely shifted by the addition of fluorescein. This substance renders the heart more susceptible to the influence of uranium or thorium than to that of potassium or rubidium. This is why a heart, brought to a standstill by a precise counterbalancing of light and heavy metal, will resume its pulsations as soon as 100 mgrms of fluorescein per liter is added to the solution. The influence upon potassium (resp. rubidium), therefore, is inconsiderable, that of uranium is great. In this respect caesium is also to be classed with potassium. Fluorescein has next to no influence upon a heart, beating in groups under subminimal caesium-doses.

From the above we conclude that, biologically, caesium is doubtlessly to be bracketed with potassium and rubidium and that it is antagonistic to uranium and thorium, which was unknown heretofore. Suppose, therefore, that nature were to bring forth caesium-animals as it brings forth potassium-animals everywhere, they might be assumed to stand to radio-activity and electricity in the

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[1]) H. **Zwaardemaker**, Proceedings 1, May 26, 1917. Vol. 26.

[2]) H. **Zwaardemaker**, Proceedings Sept. 1917 Vol. 26, p. 255.

same way as the normal animal that we know. Such in reality is the case with regard to the caesium-heart. Mesothorium-radiation does not produce an appreciable influence. That a partial polonium-radiation should cause any alteration cannot be expected, the organ being enclosed in a pericardium, which largely obviates the penetration of the α-particles.

This withheld me from trying it. Meanwhile emanation-circulation may perhaps throw light upon this matter.

When applying it, the contrast between caesium-ions and niton-atoms, supplied by radium-emanation shows itself at once. When both ions and atoms are, in appropriate quantity, in successive perfusions without interval, brought to the surface of the cardiac muscle-cells, and perhaps penetrate into them, then the presence of ions or atoms alone will ensure pulsation, but when the same quantities occur concomitantly, a standstill will be brought about after a short wait.

If, for instance, first a circulation fluid, in which from 40 to 80 mgrs of caesium-chloride per liter, is allowed to pass and directly after it one with 150 Mache-units per liter, the vigorous caesium-beats will first gradually slow down to quiescence and subsequently be succeeded by emanation-pulsations.

The latter, however, are less broad, most likely because the calcium-dosis of the fluid is antagonised by the oligodynamic niton-dosis (absence of tonus-equilibrium).

So far as we are aware, the typical feature of the nitonatoms is the charge they emit. This, at any rate, must be the immediate agent, as the transformation-products, which the adsorbed atoms of the dissolved emanation will produce during the first four days and later on in an ever lessening amount, can hardly play a part in the short experiments described by us. This warrants the conclusion, that caesium will likewise produce a charge, but of the opposite sign, and that the detected antagonism between caesium and emanation rests on this difference of sign. ¹) It appears, then, that here also caesium must be classed among its homologues rubidium and potassium, which like caesium send out charged ions of a sign

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¹) Also a mutual adsorption-extrusion by which one metal repels the other from the surface of the cardiac muscle-cells, may perhaps account for the antagonism in the immediate proximity of the threshold of the two actions, premising that both elements then are subliminal. But we took intentionally more than the threshold of emanation (and later on of radium). Then, however, the adsorption-interpretation falls through, as, indeed it also fails before the antagonism radiation-uranium, with which our phenomenon is quite analogous.

opposite to that of the niton-atoms. Rutherford [1]) has also observed that probably potassium emits few but highly penetrable β-rays, in contradistinction to the more numerous but less penetrating ones of rubidium. One is thus led to suppose caesium to throw out still more numerous, but extremely absorbable β-rays. This is why in the circulating fluids the required caesium-dosis and the rubidium-dosis are about equal, and on the other hand why this radiation has not yet been demonstrated physically. Only the atoms from a caesium-preparation, lying very near the surface, will be capable of exerting any effect. (Chemical? or ionising?).

The emanation-experiment naturally led us to investigate with a radium-solution. Owing to their being completely absorbed the α-rays of radium exert an enormous effect in the immediate proximity. That is why only few are required to obtain the restoring effect upon the perfused frog's heart: 3 microgr. of radium per liter of circulating fluid will do. So, if this oligodynamic radium-dosis (or double this dosis) is rapidly sent through the lacunae of a frog's heart that pulsates with 80 mgrms of caesium-chloride, a standstill will commence after a short wait, just as with emanation. Afterwards rhythmical beats will reappear, this time — after the caesium has been extruded — in consequence of the radium-action. By allowing caesium to flow through again at this moment we again succeeded in evoking a fresh standstill and — after extrusion of the radium — finally again vigorous caesium-beats.

The subjoined figures illustrate the phenomenon. Fig. 2 shows the arrest of caesium-beats by radium, and the subsequent recurrence of normal radium-pulsation after the standstill, when the caesium in the heart is completely substituted by radium. 1 cm. of the abscissa corresponds with the time of 1 minute. The curve is taken from a continuous graph, in which by interchanging the feeding flasks,

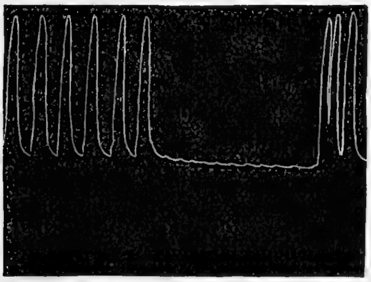

Fig. 2.

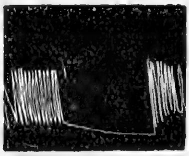

Fig. 3.

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[1]) Rutherford in Marx's Hdb. der Radiologie, Vol. 2, S. 530.

caesium- and radium-pulsations have been registered, every time with an interval of equilibria without pulsations.

The second curve originates from another experiment. The registering leaf moved along at the rate of $\frac{1}{6}$ cm. per minute. The piece cut from the graph relates to a caesium-standstill after radium, succeeded by caesium beats. The transitions in either case, illustrated here, took place per crisin.

Conclusions

1. The light radio-active metals and the allied caesium, as well as the heavy radio-active metals uranium, thorium, niton and radium, sustain the contractility of the isolated frog's heart (ventricle pulsating on its own automaticity).

Effectual doses for summer frogs are:

of potassiumchloride	from 30 to 50 mgr. per L.	(min. 20—25 mgr.)
" rubidiumchloride	from 40 to 80 ,, ,, ,,	(,, ± 30 ,,)
" caesiumchloride	from 40 to 80 ,, ,, ,,	(,, ± 30 ,,)
" uranylnitrate or acetate	from 1 to 6 ,, ,, ,,	(,, ± 0,7 ,,)
" thoriumnitrate	from 2 to 10 ,, ,, ,,	
" emanation less than 100 Mache-units		
" radiumsalt less than 3 millionths of a mgr. per Liter.		

2. A biological antagonism exists between:

$$\left\{\begin{array}{l}\text{Potassium}\\ \text{Rubidium}\\ \text{Caesium}\end{array}\right\} \begin{array}{c}\rightarrow\\ \leftarrow\end{array} \left\{\begin{array}{l}\text{Uranium}\\ \text{Thorium}\\ \text{Emanation}\\ \text{Radium}\end{array}\right\}$$

3. In all probability caesium emits β-rays of very low penetrating power, upon which depends its similar effect to that of potassium and rubidium.

Anatomy. — "*The auditory and visual cortex in the brains of two Madurese.* By Mr. C. VAN NOORT. (Communicated by Prof. J. BOEKE).

(Communicated in the meeting of May 26, 1917).

In the collection of brains, brought home from the Dutch East Indies by Prof. J. BOEKE, there were two sets, belonging to inhabitants of the isle of Madura near Java. These brains were kindly put at my disposal for the following investigations. The material was used for studying the cortical cell-lamination of some regions. Particular attention was paid to deviations from what we know about the cortical cell-lamination of the human brain. These latter have, for convenience' sake, been collectively named European brains, since it was impossible to give a nearer indication about their racial origin.

BRODMANN's fundamental work on cortical cell-lamination has been my guide. In indicating cortical layers and in naming cortical areas I have followed his nomenclature.

Material and method.

The brains were distinguished as *A* and *B*. Cerebrum *A* is that of a Madurese, deceased at 36; *B* of a Madurese, deceased at 38. Both were preserved in formalin.

For studying the auditory cortex I made serial sections of this region after embedding in paraffin. At regular distances of 0,5 mm a section of 10 μ thickness was stained with cresyl-violet and examined. In this way besides the structure also the extent of the various cortical areas could be very accurately determined.

The extent of the visual cortex was always sufficiently clearly demarcated macroscopically by GENNARI's line.

For comparison it was necessary also to study the cortical cell-lamination of these cortical areas in the European brain.

For this purpose I made a series of the HESCHL-convolution of a European brain. Moreover Dr. DROOGLEEVER FORTUYN gave me a series of the whole auditory cortex for comparison. I also had the literature at my disposal.

The two cortical regions were separately studied one after the other. I began with the auditory cortex and examined:

HESCHL'S *convolution(s) and the first temporal convolution.*

In the literature I found publications on this subject by HAMMARBERG (1895), CAMPBELL (1905), RAMON Y CAJAL (1906), ROSENBERG (1908), BRODMANN (1909), and DROOGLEEVER FORTUYN (1911).

All these authors see in HESCHL'S convolution and the first temporal convolution two different areas. BRODMANN restricts himself to the demarcation of the two areas and gives no description of them. The areal limits, according to him, do not as a rule coincide precisely with the anatomical borders of the cortical parts. HESCHL'S convolutions are divided by him into two architectonically different areas 41 and 42. In this he stands alone. No more than other investigators have I been able to confirm this view. As a general characteristic of the temporal lobe he mentions a relatively strong development of the IVth, Vth and VIth layers.

CAMPBELL illustrates his accurate descriptions by clear drawings. DROOGLEEVER FORTUYN entirely agrees with CAMPBELL. The area (41 + 42) — following BRODMANN'S nomenclature I shall retain these figures — occupying the HESCHL convolutions, is, according to them, characterised by the occurrence of large cells in the IIId layer, the lamina pyramidalis. These cells which differ from the pyramidal cells in shape as well as by their size, are at once recognised as a special type. Therefore CAMPBELL and DROOGLEEVER FORTUYN thought it best to denote them by the name of giant cells, differing herein from MARINESCO and GOLDSTEIN (1910), who assign to these cells the confusing and little characteristic name "large pyramidal cells".

Also in the first temporal convolution — pretty well the same as BRODMANN'S area 22 — these giant cells occur, but in so much smaller number that according to CAMPBELL it is easy to distinguish these two areas by this alone. Besides by the number of giant cells however area 22 can be distinguished from area (41 + 42) by the much greater richness in large pyramidal cells in the lam. pyramidalis.

CAJAL and ROSENBERG in their study of these areas give descriptions of the different cellular shapes in each separate layer. As for the present purpose this seemed superfluous, I have refrained from this.

The distinction between areas (41 + 42) and 22 ROSENBERG sought in quite a different manner than CAMPBELL and DROOGLEEVER FORTUYN. However, he also makes mention of the occurrence of giant cells, especially again in the HESCHL-convolutions. By measurements of the cortical layers ROSENBERG arrived at the conclusion that the ratio of the layers I, II, and III to IV, V and VI, differs in these areas. In area (41 + 42) the three upper layers only occupy $^1/_3$ of the cortical breadth, while in area 22 they are of the same breadth as the lower three layers.

Besides the corresponding layers in the two areas still show individual differences in breadth.

I became convinced that great care is necessary in estimating the relative breadth of the layers. The influence of the convexity and concavity of the cortex on the breadth of the different layers is so considerable that the mentioned difference in breadth-ratio may be the result of this cause alone as well in the one area as in the other.

From Dr. DROOGLEEVER FORTUYN's preparations and from my own series in conjunction with the literature, I arrived at the following conclusions:

1. It is superfluous to divide HESCHL's convolution(s) into two areas 41 and 42. This area is indicated by 41 + 42.

2. For distinguishing between area (41 and 42) and area 22 the number of giant cells is of particular importance.

Besides these points the greater richness in large pyramidal cells of the III^d layer of area 22 must be noted.

Differences in breadth-ratio, as are mentioned by ROSENBERG, I also found in my preparations, although not so conspicuously. For the reasons, mentioned above, however, I do not attach so much importance to them.

The accompanying figures 3 and 4 are typical for areas (41 + 42) and 22. They were taken from Dr. DROOGLEEVER FORTUYN's paper.

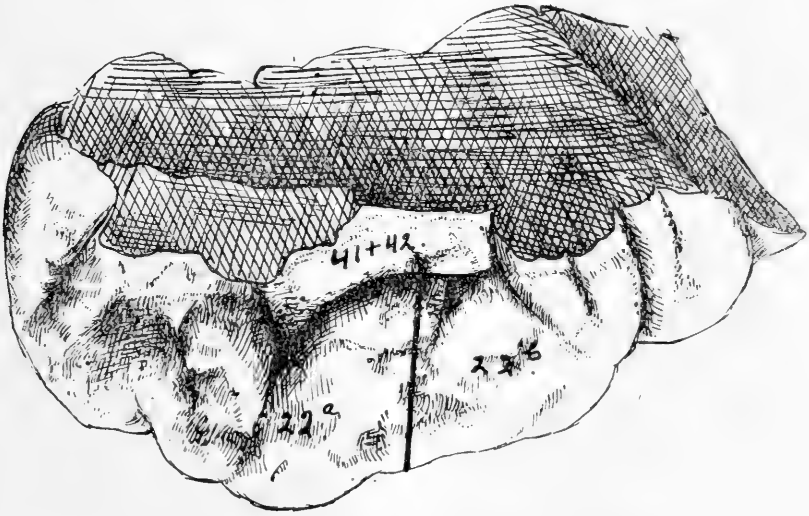

Fig. 1. Left temporal lobe. Madurese brain *A*.

In figure 3 three giant cells are seen.

In figure 4 also three giant cells lie in the III^d^ layer. We further see here one such cell that has accidentally got into the IV^th^ layer. In these figures we see clearly expressed the relatively smaller breadth of the three first layers in area (41 + 42) (fig. 3) as compared with those in area 22 (fig. 4).

In the Madurese brain *A* the small degree of development of HESCHL's convolution is noteworthy. It is only single.

Figure 1 shows the left temporal lobe. It is a drawing of the lower wall of the Sylvian fissure. Here also the areal limits are indicated as they should be according to my views. BRODMANN's area (41 + 42) remains accurately restricted to HESCHL's convolution.

Area 22 on the other hand I have been obliged to divide into two, in deviation from what has always been done with European brains.

Figures 5, 6, and 7 represent these three areas. Figure 5 shows the type of area (41 + 42) in the brain of this Madurese. It discloses a fact of very great importance. The giant cells namely are entirely lacking here. I have been unable to prove their existence in this whole area. This is the more noteworthy since all publications and, also my own series of European brains characterise this area as rich in giant cells.

Simultaneously with my own investigations VERGOUWEN (1917) has attempted to gain an insight into the variability of the number of these giant cells in hearing and in deaf-mute Europeans, by accurate countings of their number in area (41 + 42) in these two classes of people. As far as this investigation concerns the brains of hearing people, it is also of importance for my own work.

Now he found this variability to range between wide limits. In one case he even found no giant cells at all in this area in a normally hearing European. I shall presently return to these results.

Then the scarcity of cells in area (41 + 42) in these Madurese brain should be noted, more particularly of the laminapyramidalis. At irregular distances the pyramidal cells seemed to have been locally removed. Owing to the small distance through which I could follow up the cortex in the accompanying figures, I have not been able to give expression to this circumstance in the drawing.

Figures 6 and 7 represent the two araes into which I subdivided BRODMANN's area 22.

The two areas are called 22^a and 22^b. In fig. 1 we see that the demarcation-line of these two areas lies straight across the I^st^ temporal convolution and starts from a point in the fissure, separating the HESCHL-convolution from the 1^st^ temporal convolution.

The only characteristic in which 22^a differs from 22^b, is the occurrence of a few giant cells in 22^b, which are entirely lacking in 22^a.

In fig. 7, typical for area 22^b, we notice two giant cells. Compared with fig. 5 — HESCHL-convolution — fig. 6 as well as fig. 7 — 1st temporal convolution — show, in agreement with what has been found in European brains, a larger number of pyramidal cells in the III^d layer.

Figure 2 shows the left temporal lobe of the Madurese brain *B*.

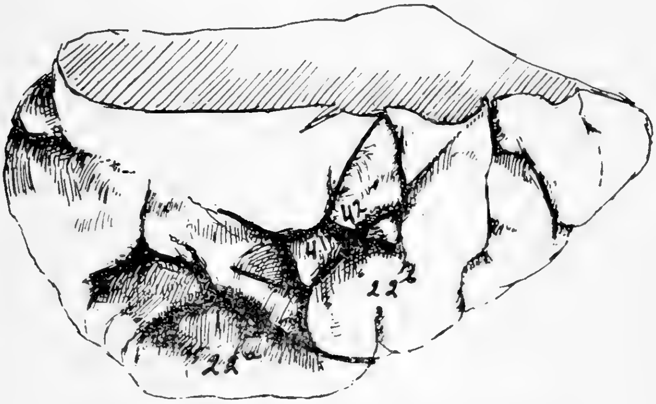

Fig 2. Left temporal lobe. Madurese brain *B*.

Here also the HESCHL-convolution appears to possess a low degree of development. Also microscopical examination revealed a striking agreement as well in the extent as in the cyto-architectonical characters of the different areas. Area (41 + 42) is not frontally restricted to HESCHL's convolution, however. In my attempt to define the anterior border of this area I was hampered by a badly preserved part of the cortex. For this reason I have been unable to draw this border.

For the rest figs. 5, 6, and 7 can be applied without alteration to illustrate the cell-lamination of the areas (41 + 42), 22^a and 22^b in this case.

The fact that such deviations agree so much in two different individuals of the same race would render it unjustifiable to ascribe them to pathological causes or to individual variability.

Also the single case of VERGOUWEN — in all other publications area (41 + 42) is described as rich in giant cells — cannot influence the results for these two Madurese brains.

How far we may speak here about racial differences must be settled by examining a larger number of brains. My results with these two specimens certainly justify the expectation that in this direction results may be obtained.

So I have commenced to study other important cortical regions as to differences in cell-lamination and areal limitation, as compared with European brains.

First of all I examined

The visual cortex.

On the cell-lamination of this area I may be brief. The lam. granularis interna — Brodmann's IV^th^ layer — is in this area clearly separated into three layers, of which the middle one is rich in fibres and as stria Gennari can easily be followed up with the unaided eye. The first three layers relatively show a poorer development.

This situation is very constantly found in many primates. So I was not surprised to find no deviations in the cell-lamination of the Madurese brains.

More promising seemed the results of an investigation on the extent of this area — Brodmann's area 17.

Ellioth Smith has studied this area in a large number of Egyptian brains. In the course of his investigations it proved necessary to compare the macroscopical cortical structure of a very large number of human and primate brains. The fissures and sulci, to which great importance must always be attached for the orientation and demarcation of areas in the occipital lobe, were determined by him. Now it appeared that the relations of these sulci are not nearly so simple as had always been assumed until now.

Also Smith points out the necessity of always first considering the configuration of the occipital pole in the light of these comparative studies, before proceeding to the limitation of a cortical area, especially of area 17. Inversely area 17 helped him to recognise atypical sulci.

The material at Smith's disposal was, apart from its diversity, very large. His publication convinced me that for the limitations of a cortical area in this region nothing could be expected except by using a similar mode of investigation. Now, my limited material precluded any attempt in this direction.

For this reason I have been obliged to restrict myself to studying the cyto-architectonical deviations from European brains and to mention limitations of cortical areas only when they did not involve accurate knowledge and study of the variations in the configuration of cerebral sulci. This procedure commended itself all the more since on this subject opinions are far from unanimous.

Also Brodmann (1908) has found in the brains of Hereros and Hottentots relations in the configuration of the sulci such as are

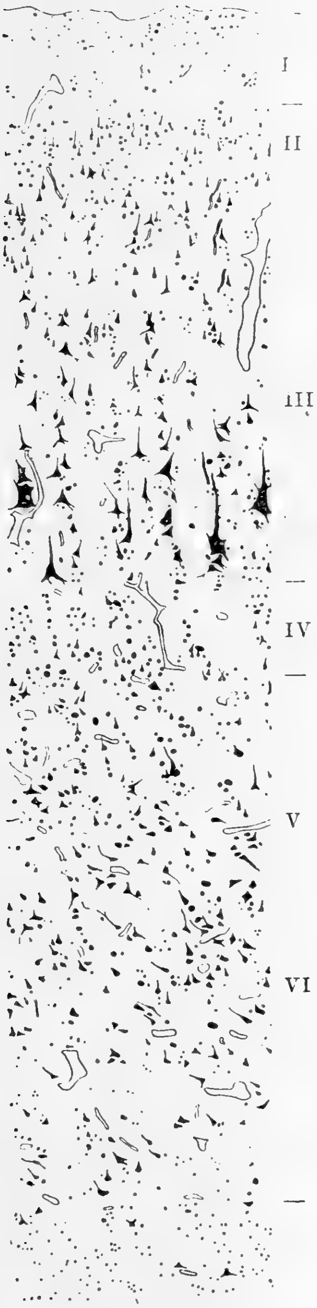

Fig. 3.

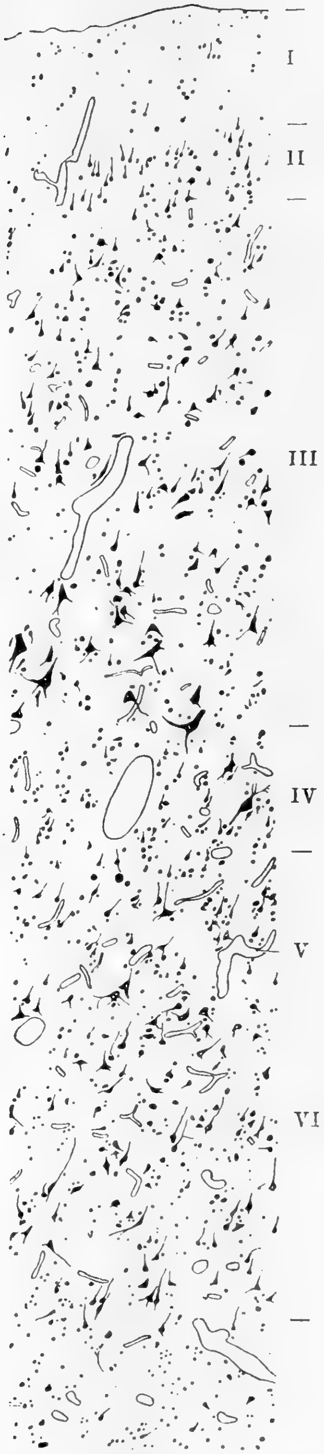

Fig. 4.

mostly met with in the anthropoid apes. He agrees in the main with Elliot Smith in his views about the extent and limitation of the areas.

In my two specimens I have found the area striata clearly

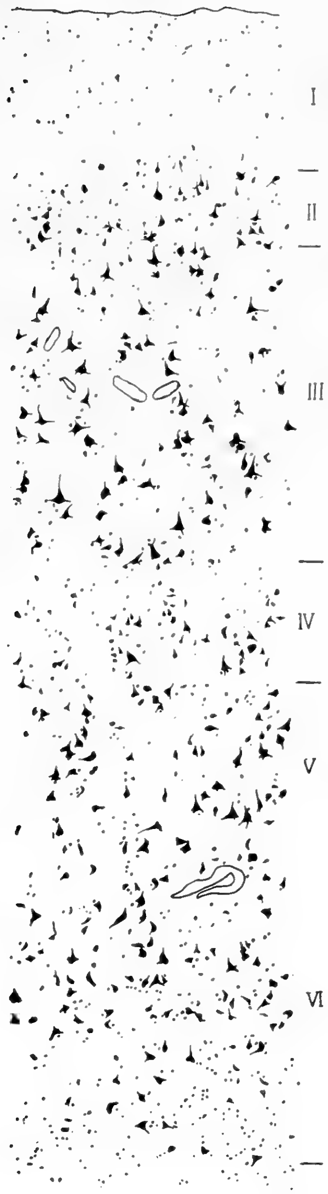

Fig. 5.

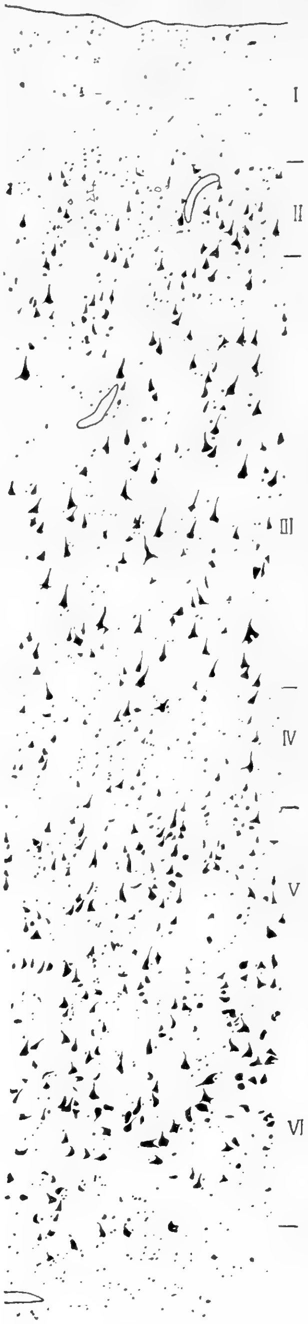

Fig. 6.

Fig. 7.

bordered laterally by the sulcus lunatus (E. SMITH). In one specimen this sulcus lay just before the occipital pole on the lateral surface of the brain; in the other it was found much more frontally, so that area 17 occupied a larger part of the lateral surface of the brain.

Leiden, *Anatomical Cabinet. Histological Department.*

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Anatomy. — *"On the variability of the number of giant pyramidal cells in the Heschl convolution in man"*. By J. P. VERGOUWEN Jr. (Communicated by Prof. J. BOEKE).

(Communicated in the meeting of May 26, 1917).

Some years ago Dr. A. B. DROOGLEEVER FORTUYN observed that in a case of non-congenital deaf-mutism the cortex of the HESCHL convolution (Area [41 + 42] after BRODMANN) in some places contained very few giant pyramidal cells and in other places none at all. However, he found the same circumstance in a case where there was no deaf-mutism. (Archives of Neurology, Vol. 5, 1911).

In order to be able to form a judgment on the variability of the number of giant pyramidal cells in that part of the human cerebral cortex, the following investigation was undertaken, extending over 10 normal cases and 5 deaf and dumb.

The portions of the normal cerebral cortex of the HESCHL convolution, embedded in paraffin, were cut in sections of 10 μ, and of every fifty sections one was taken out and stained for examination with cresyl-violet. The cerebral portions of the deaf and dumb were stained with methylene-blue, and again one out of every fifty sections was examined.

Among the normal cortical portions 8 were left and 2 right, of the deaf and dumb 4 were left and 1 right.

In all the obtained sections the number of giant pyramidal cells was determined. The following figures show how this number varied in the counted sections.

(The number of figures in each series indicates the number of sections examined in that case).

n. A. r. : 7.4.1.3.1.3.5.3.2.3.4.4.6.2.2.3.8.4.5.2.3.2. 1)
n. B. r. : 2.3.1.1.2.1.1.2.2.3.1.2.2.2.4.5.
n. C. l. : 3.2.2.0.1.1.0.3.3.2.2.1.3.2.2.4.3.3.0.
n. D. l. : 5.6.2.5.3.6.5.4.3.4.4.4.3.2.4.2.2.3.2.2.1.
n. E. l. : 3.2.2.2.1.0.0.2.1.1.3.4.0.2.1.1.2.0.0.
n. F. l. : 0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.0.
n. G. l. : 1.4.1.3.2.1.2.2.2.2.1.3.1.1.4.1.2.2.3.3.
n. H. l. : 1.3.1.0.1.2.0.2.0.0.1.0.2.0.0.1.2.2.0.
n. J. l. : 0.2.0.1.2.3.1.4.2.1.3.1.1.4.1.1.1.2.0.0.
n. K. l. : 1.2.2.0.1.1.0.0.1.1.1.0.1.0.1.0.2.2.2.0.

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1) n = normal, d = deaf and dumb, r = right, l = left.

d. Ms. l. : 0.0.1.0.
d. Ms. r. : 0.1.0.0.0.1.1.0.
d. Homo. l. : 0.1.0.0.0.0.1.2.1.0.0.1.0.1.0.0.0.1.0.0.1.1.0.1.1.0.0.0.0.
0.1.2.1.2.3.1.0.0.
d.West. l. : 4.3.3.4.5.4.3.3.5.2.3.4.6.2.3.3.4.3.2.2.
d Gb. l. : 0.0.3.1.1.1.2.1.2.4.1.0.1.2.0.1.2.0.3.0.1.2.3.2.1.1.4.5.4.
1.3.4.4.2.2.2.3.3.2.3.1.3.3.1.1.2.1.

We must not, of course, stop at these absolute figures, as we wish to know how many giant cells were in each case present in the whole cortical content. As this cortical content is difficult to determine and on account of the fact that all the cases furnished different numbers of sections, it was tried to find a relative value, representing the mutual relation of all the cases.

For this purpose the relative cortical content was determined by transferring all sections on thin cardboard by means of a drawing prism, the same magnification being always applied. Care was taken to omit the medullar portions. Next the drawings of all these parts of the cortex on paper were cut out, after which they were weighed for each case separately, so that a cortical content was obtained, proportional to the weight of the paper sections.

Since these weights were unequal and therefore could not yet be compared, a paper weight of one gram was taken as unit. From *a.* the sum of the numbers of giant cells observed, and *b.* the weight of paper of all cortical sections, it was now possible to determine for each particular case how many giant cells were found per gram of relative cortical content and these final values could be compared among each other.

The following results were obtained: (See table p. 790).

The problem is now to determine from these results what the variability is of the number of these giant cells. In this determination the normal portions were separated from those furnished by the deaf-mute, and for both categories the method given by JOHANNSEN [1]) was applied.

Let us begin with the normal cases.

First of all the middle-value was here calculated from our data, i.e. the centre from which all variations must be measured.

Starting from $A = 25$, the average difference between A and the middle-value M_n was calculated at $-$ 0.8883, the middle-value thus becoming $M_n = 25 - 0.8883 = 24.111$.

For the standard deviation $\sigma = 14.414$ was found.

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[1]) W. JOHANNSEN, Elemente der exakten Erblichkeitslehre. Deutsche Ausgabe, 1909, Dritte Vorlesung.

Case	Total number of giant cells	Relative cort. content	Number p. gram
n. A. r.	77	gram 1.430	53.846
n. B. r.	34	„ 1.485	22.895
n. C. l.	37	„ 1.350	27.407
n. D. l.	72	„ 1.850	38.702
n. E. l.	26	„ 1.145	22.707
n. F. l.	0	„ 1.200	0
n. G. l.	41	„ 1.445	28.097
n. H. l.	17	„ 1.240	13.709
n. J. l.	30	„ 1.170	25.641
n. K. l.	18	„ 2.205	8.113
d. Ms. l.	1	„ 0.250	4
d. Ms. r.	3	„ 0.960	3.125
d. Homo. l.	22	„ 1.670	13.173
d. West l.	68	„ 1.850	36.757
d Gb. l.	89	„ 3.910	22.762

However, it is not quite admissible to adopt this middle-value without further discussion, since the number of cases (10) is relatively small, while the middle-value is known to be most exact if the number of examined cases is as large as possible. So for a determination of the most accurate middle-value we have to take into account the number of cases examined as well as the standard deviation, since both are related with the mean error of the middle-value, as is expressed by the formula $m = \sigma : \sqrt{n}$ (m = mean error of the middle-value, σ = the standard deviation and n the number of cases examined; [JOHANNSEN]). So if n becomes very large $\sigma : \sqrt{n}$ will continually decrease, which means that then the absolute middle-value M_n will continually approach the first calculated middle-value M, as the mean error becomes smaller. We find that here this mean error is $m_n = \pm 4.56$, the absolute middle-value becoming $M_n = 24.111 \pm 4.56$, thus ranging between 28,671 and 19,551.

In the same way we proceeded with the deaf-mute cases. Here $A = 15$ was taken as starting point, the average difference between A and M_d found to be $+ 0.963$, M_d working out at 15.963. The mean error of the middle-value for $\sigma = \pm 12.604$ and $n = 5$ was determined at $\pm$ 5.637; the absolute middle-value for the deaf-mute cases thus became $M_d = 15.963 \pm 5.637$, and so appeared to lie between 21.600 and 10.326.

No important difference between right and left was stated.

If we now compare the two categories, the question arises: what do these middle-values M_n and M_d prove?

The limits of M_n and M_d overlap, so they do not prove a difference in variability between the normal and the deaf-mute cases. This would be otherwise if the highest value of M_d had been smaller than the lowest of M_n. In this latter case we might state that in the deaf and dumb the number of giant cells in the HESCHL convolution is considerably reduced and this diminution might be ascribed to the deaf-mutism. The calculations, however, show that we are not justified in drawing this conclusion.

Thus the variability of the number of giant cells in the HESCHL convolution is very great. Compare e.g. the case n. F. l., in which not a single giant cell is found, with the case n. A. r., where the number is relatively very great. We get the impression that the number of giant cells in the cortex of this region is in the deaf and dumb occasionally considerably less than in normal people; on the other hand the cases n. F. l. and d. West l. weaken this impression again.

The extent of the variability we find expressed in a table given by JOHANNSEN [1]. In this table the play, left to the variations, is given, and it is stated that 99.7 % of the cases will fall within the space $S = M \pm 3\,\sigma$.

So we get for the normal cases:

$S_n = M_n \pm 3\sigma = 24.111 \pm 3 \times 14.414 = 24.111 \pm 43.242$, so that S_n is the space between 67.353 and 0.

In the deaf-mute cases we find

$$S_d = M_d \pm 3\sigma = 15.963 \pm 3 \times 12.604 = 15.963 \pm 37.812,$$

the space ranging from 0 to 53.505; the upper limit lies somewhat lower than with the normal cases.

From the preceding investigation we may conclude:

1. that the variability of the number of giant cells in the HESCHL convolution is very great,

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[1] JOHANNSEN, l. c. Fünfte Vorlesung.

2. that there is no difference in variability in the HESCHL convolution of normal and deaf and dumb people, although an examination of the microscopic image of the two categories might suggest such a difference.

3. that DROOGLEEVER FORTUYN's [1]) surmise that a diminution in the giant cells might be ascribed to deaf-mutism, which surmise was suggested to him by the examination of two cases of deaf-mutism, is not borne out by the facts.

Leiden, Anatomical Cabinet, Histological Department.

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[1]) Archives of Neurology, Vol. 5, 1911.

Geology. — *"On some fossils from Celebes, believed to belong to the Oligocene."* By Prof. K. MARTIN.

(Communicated in the meeting of June 30, 1917).

Mr. G. F. DOLLFUS [1]) has described a number of fossils given him for examination by Mr. E. C. ABENDANON. Among the conclusions derived by this author from these fossils the principal one is: "Nous nous croyons donc fondé à reconnaître *avec certitude pour la première fois*, la présence de l'Oligocène dans l'Insulinde". The fossils on which this judgment is based originate for the greater part from the alluvium in the neighbourhood of Enrekang on the lower course of the river Saädang in Celebes, while three of them were found in limestone, forming part of a ridge east of the Saädang and south east of Enrekang. These latter came from two different finds, from the Boentoe Képé and from the B. Leworong [2]). That all these fossils would originate from the same stage cannot be proved, of course, the less so as they were not collected by professional geologists. Meanwhile we may provisionally assume with DOLLFUS that this is indeed the case.

The most important fossil for a determination of age has been called by DOLLFUS *Tympanotomus* (*Vicarya*) *Verneuili d'Arch. var.* [3]), for the author remarks: "La découverte du genre Vicarya à l'île Célebès est tout à fait intéressante et confirme l'extension de cet horizon entre Java et les Philippines". Now this fossil is no Vicarya at all, as will appear from what follows.

Of the genus *Vicarya d'Archiac* three species have originally been published, *V. Verneuili d'Arch.* [4]), *V. fusiformis Hislop* [5]) and *V. callosa Jenkins* [6]). Later it has appeared, that *V. fusiformis* had wrongly been called a *Vicarya*, and COSSMANN finally described it

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[1]) Paléontologie du voyage à l'île Célèbes de M. E. C. ABENDANON, Leiden 1915.

[2]) For the finds see: E. C. ABENDANON, Geologische en geographische doorkruisingen van Midden-Celebes, (Geol. and geogr. expeditions across Central Celebes) Part I, Leiden 1915, pp. 423—425; further Atlas pl. VII. 1916.

[3]) p. 30, plate I, fig. 807.

[4]) D'ARCHIAC ET JULES HAIME, Descript. d. anim. foss. de l'Inde, p. 298, Paris 1853.

[5]) Quarterly Journ. Geol. Soc. Vol. XVI, p. 177, pl. 8, fig. 36*a*—36*c* (1859).

[6]) l. c. Vol. XX, p. 57, pl. 7, fig. 5 (1863).

as *Morgania fusiformis*[1]). The two other forms, *V. Vernueili* and *V. callosa,* are difficult to compare, as the former is only incompletely known, but the rich material of *V. callosa.* present at Leiden from Java, Borneo and the Philippines, renders it extremely probable that all the forms of *Vicarya,* until now described, belong to one single species, which is very variable in shape and sculpture. This is also DOLLFUS' view, for he gives *V. callosa* as synonymous with *V. Vernueili.*

The chief characteristic of *Vicarya* as contrasted to other forms resembling it, is the slit of the outer lip, reminding of *Pleurotoma.* Of this nothing is seen in the fossil from Celebes, it only possesses a broad and deep bend, reaching upwards to the row of nodes, and of which the author justly says: "nettement analogue à celui de *Tympanotomus*". In fact, with this genus also the sculpture agrees: "qui est également celle de *Tympanotomus*"; but this sculpture again deviates entirely from that of the genus *Vicarya.* For in specimens of this latter form, of about equal size as the fossil pictured by DOLLFUS, the nodes on the younger part of the shell are much more prominent already and are at much larger distances from each other than in the fossil from Celebes. This also holds for the objects figured by BECKER[2]), which according to the author: "sont plus voisins des nôtres qu' aucun des autres figurés". That in the fossil from Celebes the slit-band, corresponding with the slit of the outer lip, is lacking, is evident after what precedes.

No more than it belongs to *Tympanotomus* and the mentioned fossil from Celebes, figured by DOLLFUS, *Vicarya* is an Oligocene genus. We know that the fossils, described by D'ARCHIAC, originate from different strata and have wrongly been united to a single fauna; it is difficult therefore, to state with any certainty the age of *V. Verneuili.* It has been tried to remedy the confusion and so this fossil has for a time been regarded as upper cretaceous; according to FEDDEN[3]), however it belongs to the Gaj.-group, which is considered by VREDENBERG as the youngest Oligocene[4]), by

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[1]) Essais de Paléoconchologie comparée VIII, p. 164 (1909).

[2]) Un. States Geol. Survey 1901, Annual Report XXI, Part. 3, pp 624 and 625. — The author has failed to notice that these figures agree completely with those published by me; for BECKER here translated a paper by myself (Ueber tert. Foss. von den Philippinen; Sammlgn. Geol. R. Mus. Leiden I, Vol. 5, p. 52. See the figures on pp. 67 and 68 of this paper).

[3]) Mem. Geolog. Surv. of India, Vol. XVII, Pt. 1, 1879, p. 206.

[4]) A. summary of the Geology of India, Calcutta 1907, p. 60

H. Douvillé on the other hand as Burdigalian ¹). This latter view is more justified, since the Gaj.-group contains *Lepidocyclina marginata Mich.*, which belongs to the uppermost Aquitanian ²). In complete agreement with this the Javanese *V. callosa* is solely found in the Njalindoeng and Tjilanang beds, i. e. in old and young Miocene sediments, this species being also known as Miocene from the Philippines and Japan. The Tjilanang beds, however, are also by Dollfus quoted ³) as Miocene (Helvétien), so that there is a general confusion in regard to the fossil which was the starting point for determining so-called Oligocene deposits.

Of the other fossils, described by Dollfus as Oligocene the following have been brought into relation with already known species:

I. From the alluvium of Enrekang.

1. *Conus cf. substriatellus* H. Woodw. ⁴). The fossils from Sumatra decribed by Woodward, are not reliable as to the localities where they were found ⁵). Moreover the mentioned species is only based on an unserviceable cast. Hence the age of the fossil from Celebes, which is compared with it, cannot be settled.

2. *Cypraea cf. subelongata* H. Woodw. ⁶). For this the same remarks hold good as for the preceding species.

3. *Strombus cf. maximus K. Mart.* Is found in Java in the upper Miocene Tjilanang beds.

4. *Turritella cf. angulata Sowerby* ⁷). The two badly preserved objects figured under this name by Dollfus, have whorls, the profile of which differs entirely from that of *T. angulata Sow.*, and it is not clearly to be perceived on what the determination is based. Nor is their any ground for considering *T. acuticingulata Jenkins* as synonymous with *T. angulata*; for the former species is identical with

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¹) Sammlgn. d. Geol. R. Mus. in Leiden I, Vol. 8, p. 258.

²) P. Lemoine and R. Douvillé, Mém. Soc. Géol. de France, Paléontologie, T. XII, Fasc. II, Paris 1904, p. 31.

³) p. 54.

⁴) Notes on a collection of fossil shells, etc. from Sumatra. (Geol. Mag. 1879, Dec. II, Vol. VI, pl. 2, fig. 2.

⁵) See: Sammlgn. I. Vol. 8, p. 207.

⁶) l. c. fig. 3.

⁷) Trans. Geol. Soc. of London, Ser. II, Vol. 5, Part. 2, pl. 26, fig. 7. (1840).

T. acuticarinata Dkr.[1]) and not to be identified with *T. angulata*. On this account the author himself says: "Les noms ci-dessus donnés ne sont qu' une approximation pour une étude ultérieure à suivre sur des matériaux plus complets". For the determination of age it is important that *T. acuticarinata* is among the commonest fossils of the Tjilanang beds, in which probably also *T. angulata* is found[2]). In British India the latter species, including varieties, is stated to occur from the Ranikot to the Gaj-group[3]). (Yprésien—Burdigalien).

5. *Turritella cf. assimilis Sowerby*[4]). Also this determination is called an "approximation" only, as it is based on a single incomplete object. Agreement with *T. assimilis* there is none, judging from the figures. The zone at which this species occurs in British India, is not known.

6. *Venus non-scripta Sowerby*[5]). As far one can judge from the figures this determination is correct, but the species is identical with *Clementia papyracea Gray*[6]), it occurs in the Gaj-group of British India and in the Indian Archipelago from the lower Miocene to the present day.

7. *Cardita cf. veretrapezoides Gregorio.*

On this determination I have been unable to form a judgment. The species mentioned belongs to the Auversien or Priabionien of Europe.

8. *Fungia decipiens K. Martin spec.*

By this name *Cycloseris decipiens Mart.*[7]) is meant, occurring in the finds denoted by JUNGHUHN by *C, O* and *P*, and also mentioned by DOLLFUS. The author has failed to notice, however, that *O* corresponds with the typically upper Miocene Tjilanang beds and *P* with the equivalent limestone of Liotjitjangkang. In both these localities

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[1]) K. MARTIN, Die Tertiärsch. auf Java, p. 69.

[2]) Sammlgn. I, Vol. 9, p. 47.

[3]) FEDDEN l. c.

[4]) Trans. Geol. Soc. l. c., fig. 8.

[5]) l. c. pl. 25, fig. 8.

[6]) Die Tertiärsch. auf Java p. 99.

[7]) l. c. p. 143.

I have collected this species myself.[1] The sediments of locality *C* are certainly not older.[2]

II. *From the ridge S.E. of Enrekang.*

1. *Cerithium filocinctum Boettg.*[3]).

This species belongs to the badly preserved fossils, described by BOETTGER, which have already caused much difficulty, and is based on a single very incomplete object. Now with this specimen fossils from the *B. Leworong* are identified which have neither been delineated nor sufficiently described and which likewise are in a very unsatisfactory state of preservation: "Test blanc pulvérulent". I consider it quite impossible to make such a determination, even approximately.

Cerithium filocinctum, according to BOETTGER, occurs in the Eocene of Borneo, in stage γ; this, however, has turned out to be Oligocene (Stampien?)[4]).

2. *Trochus* (*Zizyphinia*) *cf. Lucasi Brongniart.*

From what DOLLFUS says about the fragments reckoned to this species and originating from the *B. Képé*, they must be very badly preserved. At any rate it is unsafe to class them with *T. Lucasi* from the Priabonien. Further, as identical with these *T. padangensis Boettg.* is considered[5]), originating from Batoe Mandjoeloer in West Sumatra, from strata which formerly were reckoned to the Eocene, but which have since turned out to belong to the lower Miocene[6]).

3. *Turbo obliquus Jenkins.*

This species has been described from upper Miocene sediments in Java, later it has also been proved to occur in the Pliocene of Sonde, but has never been found in the Palaeogene. It is identical with the still living *T. petholatus L.*[7]), although DOLLFUS emphatically points out that the two species are very different. The explanation is to be found in the fact that the determination is faulty, as appears from the description; for the spiral ornamentation, men-

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[1] Sammlungen I, Vol. 9, pp. 39 and 51.

[2] l.c. Vol. 6, p. 184, note.

[3] Die Eocänform. v. Borneo, Palaeontographica Suppl. III, Lfg. 1, p. 10, pl. 1, fig. 1.

[4] Sammlungen, Neue Folge, Bd. II, p. 211.

[5] Die Tertiärform. v. Sumatra I, p. 73.

[6] Sammlgn. I, Vol. 9, p. 340.

[7] Die Fossilien von Java, p. 274.

tioned with the fossil from the *B. Képé,* is not found at all with *T. obliquus.*

Summarising, we obtain the following results as to the vertical distribution of the species, if the determinations of DOLLFUS are provisionally accepted without reserve:

Determination according to DOLLFUS.	Age.
Conus cf. substriatellus.	unknown.
Cypraea cf. subelongata.	unknown.
Strombus cf. maximus.	younger Miocene.
Cerithium filocinctum.	Oligocene (Stampien?)
Vicarya Verneuili (= V. callosa).	older and younger Miocene.
Turritella cf. angulata.	Eocene to younger Miocene?
Turritella cf. assimilis.	unknown.
Turbo obliquus.	younger Miocene to the present day.
Trochus cf. Lucasi (= T padangensis).	upper Eocene and older Miocene T. (padangensis).
Venus non-scripta.	older Miocene to the present day.
Cardita cf. veratrapezoïdes.	upper Eocene (Europe).
Fungia decipiens.	younger Miocene.

So of 3 of the above 12 species the age is unknown; of the remaining 9, 5 are only met with in Miocene or younger deposits, and of these 2 are still found in the present day fauna. A single species is only known in the Eocene of Europe, 2 on the other hand, about whose range great uncertainty prevails, would occur as well in Eocene as in Miocene beds; a single one is Oligocene and probably belongs to the Stampien.

Now, if all these fossils are grouped together, as DOLLFUS does, they would plead for a Neogene age of the fauna; for only *Cerithium filocinctum* and *Cardita cf. veretrapezoides* can be adduced against this view. However, the very great uncertainty in the determination of the *Cerithium* and the fact that the *Cardita* is unknown in India and is only identified with reserve by DOLLFUS, deprive these two species of every weight.

Apparently the greater part of the denominations are meant by DOLLFUS as approximations only, as appears from the added cf. As of certain occurrence he only gives, besides the above-mentioned *Cerithium*: *Vicarya Verneuili, Turbo obliquus, Venus non-scripta* and *Fungia decipiens.* None of these four species is older than Miocene and two of them are still alive. So on account of his own determinations Dollfus should have placed the fauna in the Neogene. However, *Vicarya* and *Turbo* have been wrongly determined; so only *Venus non-scripta* (= *Clementia papyracea*) and *Fungia decipiens* remain.

Besides the 12 fossils here dealt with, Dollfus has still published 9 others which are described as new, and 3 of which only the genus could be stated. Among the forms considered as new are two corals, among the genera mentioned in all probability *Eupatagus;* all the other fossils are mollusca. If these objects had not been so badly preserved, something might perhaps be concluded from their relationship with other known forms; this is not the case now.

Conclusion: In the alluvium of Enrekang Neogene fossils are found, while older, more particularly Oligocene fossils, have until now not been proved in that locality. No more is there any proof for the occurrence of Oligocene in the ridge S.E. of Enrekang.

Geology. *"On the Miocene Fauna of the West-Progo Mountains in Java"*. By Prof. K. MARTIN.

(Communicated in the meeting of June 30, 1917).

Some years ago I made a few communications to this society on the mountain range extending on the right bank of the Kali Progo, west of Jogjakarta, and on the limestone found in these "West-Progo Mountains" [1]. For the geographical and geological details I refer to these provisional communications and to the more elaborate description in my travelling journal, in which this limestone is determined as lower Miocene, although the fauna found in it had not been completely studied then [2]. All the material has now been worked up, and in what follows I propose to give a short survey of the chief results of the examination of this fauna [3].

Two finds of fossils are of special importance, the Goenoeng Spolóng, near Djoenggrangan, and the Kembang Sokkóh, close to Bomaas, of which the former yielded 69 different species and the latter 84, excepting corals and some echinoidea, the investigation of which proved impossible on account of the present circumstances [4].

The determined fossils are in the first place mollusca, especially gastropoda, a few scaphopoda, some thirty lamellibranchiata and a small number of foraminifera which have been examined by Dr. L. RUTTEN.

The fossils of the G. Spolóng have changed into a fine-grained mass of clear transparent calcite, but nevertheless possess only very little rigidity and easily disintegrate, when the marl filling their cavities absorbs water. They are therefore difficult to prepare.

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[1] "Enkele beschouwingen over de geologie van Java" (Verslag Mei 27, 1911) and "Verdere beschouwingen over de geologie van Java" (Verslag Maart 30, 1912).

[2] Sammlungen des Geolog. Reichs-Museums in Leiden, Ser. I, Vol. 9, pp. 56—76 and 108—111.

[3] The complete publication will appear in the organ of the Geological Mineralogical Museum at Leiden, partly in order to retain the connection with other related papers and partly on account of the circumstance that the special descriptions are to be accompanied by a number of plates which could not very well be incorporated in the Proceedings of the Academy.

[4] The material necessary for comparison and the literature required were inaccessible through the stagnation of international collaboration.

Still, a very suitable material is finally obtained. The fossils from the Kembang Sokkóh, originating from clay, have been excellently preserved like most objects from the Miocene strata of Java; often their lustre is still present and even remnants of colours are occasionnally seen.

The fauna examined shows that the formation in which it is found, must have been deposited in shallow water, for all genera and also the species, as far as they still exist, are found at the present day along the coasts at a small depth. This agrees with the circumstance that many corals occur locally and lithothamnia are generally spread. Moreover the limestone, which is the main constituent of the just-mentioned formation, represents a thick closed cover, such as is only possible with genuine shore-formations.

There are facies differences, however; for the clay on the Kembang Sokkóh has been deposited in the neighbourhood of a river mouth, as may be inferred from the very frequent occurrence of *Potamides.* Insignificant layers of lignite between the clay must probably be attributed to land plants that were buried near the mouth in the river mud. This reminds of what I have on a former occasion communicated on the eocene sediments of the Kali Poeroe, where in the vicinity of the lignite many *Melanidae* were found[1]. On the G. Spolóng, however, shells of *Potamides* are not numerous, lignite is absent and the distribution of the genera of mollusca and foraminifera in these two finds is very different. Notwithstanding this almost half the fossils of the G. Spolóng are also found on the Kembang Sokkóh.

The mollusca clearly present an Indopacific character, for not only the species surviving to the present day that are found in the deposits of the West-Progo Mountains, belong to the area of the Indian fauna, but also there exists in the latter a whole series of relatives of the examined fossils. Moreover the habitat of all related recent species taken together lies almost entirely within the present Indopacific region, especially in the area of the islands between Asia and Australia. It extends on one side from the Chinese coast to the Admiralty Islands and Australia, on the other side as far as Madagascar and The Cape.

From the Neogene fauna of Europe the mollusca of the West-Progo Mountains are entirely different. The foraminifera on the other hand present a different picture: of the eight species of this animal group that were determined with certainty in the deposits

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[1] Die Fauna des Obereocäns von Nanggulan auf Java (Sammlgn. Neue Folge, II, p. 213).

here described, no fewer than four are also found in European strata, namely in the Eocene *Clavulina angularis d' Orb.* and *Cl. parisiensis d' Orb.*, in the Miocene *Spiroloculina crenata Karrer*, in the Miocene and Pliocene *Gypsina globulus Reuss.* This widespread horizontal distribution in the Tertiary period corresponds to a still wider distribution at the present day, for the mentioned *Spiroloculina* and *Gypsina* inhabit the Atlantic and the Indian and Pacific Oceans; both species of *Clavulina* occur in the Atlantic, the former also in the Indian Ocean and the latter in the Pacific. Of the remaining surviving species found in the formation here dealt with, *Orbitolites marginalis Lamk.* occurs again in the three Oceans, while *Polystomella craticulata F. e. M.* is spread from the Mediterranean through the Indian Ocean to the Pacific and the uncertainly determined *Orbiculina adunca F. e. M.* is found from the Atlantic as far east as the Philippines.

Also the vertical distribution of the foraminifera differs from that of the mollusca, as is best seen from the numbers of the surviving species in both groups. Among 103 determined mollusca 7 recent forms are found, among 9 determined foraminifera 6—7. Hence the Rhizopoda would make a much younger impression when compared with the mollusca if it were not that also the extinct subgenus *Flosculinella* was found in it, besides *Lepidocyclina* and *Miogypsina*.

From what precedes it follows:

1. That the foraminifera mentioned do not alter in the least the Indopacific character of the Neogene Indian fauna, though they may occur outside the Indopacific area and partly in the European tertiary.
2. That the foraminifera, generally speaking, cannot serve in the same way as the mollusca for determining the age of the strata and that this animal group is also less serviceable for investigations on marine geography.

As to this latter point we may remember here the extraordinarily wide horizontal and vertical distribution of *Orbitolites complanata Lamk.*, whereas the highly developed nummulinidae have proved eminently suitable for an exact determination of the age of deposits.

How shall we now explain the difference in distribution observed between mollusca and foraminifera and between the groups of the foraminifera among themselves? To me it would seem that it is closely related to our conception of a species. The species namely with which one has to deal in palaeontology are no physiological but morphological species. The individuals of such species of mollusca and foraminifera as we are here only concerned with, agree in a single anatomical element, the shell. Such an agreement may exist, however, while other elements are absolutely different. An excellent illustration of this is afforded by *Marginella glabella L.* and *Pseudomarginella v. Maltzan* from the isle of Gorée, for here gastropod shells which are quite indistinguishable from each other are inhabited by entirely different animals. Lowly developed forms of foraminifera, however, are much more difficult to distinguish than shells of mollusca, so that not only the "species", but also even the genera are often connected with each other by transitions. It is easily understood that for this reason such morphological species may have a large horizontal and vertical distribution, while with the nummulinidae, the structure of which is so very complicated, this is by no means the case. The premised longevity of so many foraminifera is probably only the result of our incapacity to distinguish the species on account of their simple shells.

However this may be, the whole character of the Tertiary fauna of the West-Progo Mountains is in complete agreement with the theory formerly developed by me, according to which the Javanese Sea was separated from the Tethys since the upper Eocene.

For a determination of the age of the deposits we have in the first place the fact that out of 103 determined species of mollusca no more than 7, i.e. 6,8 %, survive to the present day. Moreover of the species described as new, relatively few related forms could be noted in the recent fauna. In the oldest Neogene sediments until now known in Java, the lower Miocene strata of Rembang, slightly more than 13% recent species are found; hence the deposits of the West-Progo Mountains cannot be younger than these. The question even arises whether they should not perhaps be reckoned to the Oligocene.

If we investigate the relationship between the fauna of the West-Progo Mountains and the younger and older Tertiary faunas of the Indian Archipelago, it appears that 24—25 species from the strata here dealt with are found in the Neogene deposits of the Dutch East Indies, none, on the contrary, in the upper Eocene of Nanggoelan. Further there exist relations with the Gaj-group of Sind (Burdigalian). From what precedes it appears that the West-Progo beds must be placed at the base of the Neogene and cannot belong to the Oligocene. So they are of lower Miocene age.

For the above reasons the foraminifera cannot serve for a percentage calculation of forms still existing; for the Indian Archipelago material for comparison in this respect is entirely lacking. But among this animal group the lower Miocene *Miogypsina thecideaeformis Rutten* occurs in the fauna of the West-Progo Mountains, which is found near Balik Papan in East Borneo together with *Alveolinella bontangensis Rutten* and *Lepidocyclina flexuosa Rutten* [1]), and with these two latter species *Alveolinella globulosa Rutten* and *Lepidocyclina spec.* from the deposits here dealt with are closely related. So what we know about the foraminifera agrees with the conclusion derived above from the mollusca.

The examination of the foraminifera has also confirmed that a foraminiferal (globigerina) ooze occurring on the brook Tegalsari, in the vicinity of Nanggoelan, is equivalent with the lower Miocene sediments of the West-Progo Mountains. This equivalence had formerly been derived from general geological considerations.

When attempting more accurately to fix the age of the West-Progo deposits (within the older Miocene period) in respect to the just-mentioned Rembang beds, we meet with great difficulties. For the foraminifera of Rembang, which have been studied by DOUVILLÉ [2]), in all probability belong to the Middle Aquitanian stage, while those of the West-Progo Mountains according to our present knowledge point to a younger formation, Upper Aquitanian or even Burdigalian. On the other hand the Rembang beds, as stated above, present a higher percentage of still living mollusca and on this account must be considered as younger than the West-Progo beds.

The chief difficulty is that in these latter deposits also Miogypsina occurs. In my opinion it is possible, however, that this genus is found in still older sediments than has been assumed until now. For when grouping rocks on account of the foraminifera, hardly any attention has yet been paid to the different facies, and especially to the depth at which they originated. The circumstance that according to the more recent investigations of DOUVILLÉ [3]) *Orthophragmina* and *Lepidocyclina* occur simultaneously in the American Oligocene, which until recently was considered impossible, shows that one has to be very careful when classifying deposits only on account of the foraminifera.

Meanwhile the mentioned incongruity cannot be solved for the present and one must be content to assign to the deposits of the West-Progo Mountains here dealt with, the general term lower Miocene, without being able to clear up their relation to the Rembang strata, which also belong to the lower Miocene.

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[1]) L. RUTTEN, Studien über Foraminiferen aus Ost-Asien. (Sammlgn. I. Bd. 9, p. 287).

[2]) Les foram. d. couches de Rembang (Sammlgn. I, Bd. 10, p. 19).

[3]) Les Orbitoïdes de l'île de la Trinité (Compt. rend. d. séanc. de l'Acad. d. Sciences. T. 161, p. 87. 1915).

Physiology — *"On the Nature of the Constituent of Intestine-Extracts, which exerts a Stimulating Influence upon the Gastro-Intestinal Movements."* By Dr. J. W. LE HEUX. (Communicated by Prof. C. A. PEKELHARING.)

(Communicated in the meeting of September 29, 1917).

In 1912 an article was issued from the Pharmacological Institute of the Utrecht University entitled: "Zur Kenntniss der Entstehung der Darmbewegung" of Dr. WALTHER WEILAND [1]).

In this article WEILAND reports his striking experience that when different parts of the gastro-intestinal tract (stomach, small intestine, large intestine) of various animals (rabbit, cat, and dog), after being duly cleaned, are put in water of 38°, the fluid possesses after some time the quality to largely modify the movement of the surviving small intestine of these animals.

The effect of these aqueous extracts on the surviving small intestine, which is not specific for any type, evinces itself in a broadening of the contractions or in an increment of tonus, or in both, an effect, therefore, resembling that of pilocarpin also in that it can be counteracted by a small dose of atropin. On further inquiry WEILAND found that the active constituent of these extracts is not of a fermenting nature, but that after boiling and filtering the solution and evaporating it down to small bulk, the remaining dark-coloured residue was as active as the primary extract. Another purification was effected by treating with absolute alcohol the extract that had been evaporated to dryness on the waterbath and filtering it, by again evaporating the filtrate and subsequently extracting the residue with ether; the ether was then removed from the limpid filtered ether solution and the rest was dissolved in water. According to WEILAND a solution is obtained in this way that contains nearly the original quantity of the active constituents. To litmus the reaction of this solution is distinctly alkaline; it contains, however, only traces of nitrogen. The reaction after MILLON is negative, biuret-reaction faintly positive. Phosphotungstic-acid and phospomolybdic acid yield a large white precipitate, platinic-chloride a slight one. No precipitate resulted from potassium-mercury-iodide, potassium-

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[1]) Arch. f. d. ges. Physiologie Bd. 147 S. 171 1912.

periodide, sublimate, and picric acid. In acetone the substance appeared to be insoluble. It now turned out that the extracts thus purified, did not only act upon the surviving small intestine, but also upon the intact animal. **Weiland's Röntgen**-tests showed that a potent positive influence was exerted on the movements of the stomach and of the small intestine in intact cats and rabbits. The essential influence this substance (or these substances) seemed to have in originating the movement of the small intestine rendered further investigation necessary. Prof. **Magnus** now suggested to me to endeavour to abstract the active constituent of these extracts in a pure condition and to determine the chemical structure. The present paper is a provisional report of this inquiry.

Initially I prepared the extracts from the small intestines of a cat and proceeded as follows:

The cat was profoundly anaesthetized under the bell and killed by a blow on the neck, the small intestine was removed from the animal and put in a dish with warmed Tyrode solution, washed out twice with this liquid, each time transferred to a fresh solution, and at last it was irrigated by a powerful jet of water. Subsequently the gut was tied up on both ends, and put in about 100 c.c. of distilled water of 38°; only the ends of the gut did not touch the water. After an hour the gut was taken out, the slightly opalescent but slightly coloured fluid was boiled up on a copper gauze, evaporated down to a volume of 30 c.c., filtered, and the filtrate was then evaporated to dryness in a porcelain dish on the waterbath. The brown residue was subsequently extracted with absolute alcohol, the alcoholic solution was filtered and evaporated to dryness, the remainder was extracted with ether. After filtration the ether was evaporated and the residue was taken up in water.

The extracts prepared and purified in this way proved (though not always) to include the looked for active constituent. During our inquiry, however, it appeared that in using more material the alcohol abstracts the active substance only slowly and partially from the tough residue of the aqueous extract. We, therefore, evaporated the residue to dryness on purified quartz-sand and extracted it with alcohol for some time in extracting apparatus.

From the then obtained dark coloured alcoholic solution the solvent was removed by distilling, the remainder was dried in vacuo over sulphuric acid and after this extracted with a large amount of ether. The difficulty I encountered here was that only a portion of the active substance was transmitted to the ether, especially after a preliminary purification with acetone. Also without this a considerable portion remained undissolved even when the extraction was performed with a large amount of ether. In a small amount hardly anything was dissolved.

It stands to reason that in our further experimentation we did not attempt to prepare an ether-extract, but that the residue of the alcoholic solution was only washed with a little ether. What was then left, was taken up in water and treated as follows:

Sulphuric acid was added to the solution to 5 per cent, and a concentrated solution of phosphotungstic acid was added, which produced a large, white precipitate. This was filtered by suction, washed and decomposed in the usual way with baryta water. The filtrate was freed from phosphotungstic acid by baryta and the solutions thus formed, were examined for their effect upon the surviving small intestine of a rabbit.[1] It appeared from this that, when we work with concentrated solutions, about 80°/₀—90°/₀ of the active constituent had been precipitated with phosphotungstic acid. Admixture of silvernitrate and silvernitrate with baryta yielded in the solution, through decomposition of the phosphotungstic precipitate, only inconsiderable residues, which generally contained no active substance. But in the filtrate of these precipitates we have not been able to find an undiminished quantity of the active substance. If this filtrate was evaporated down, after removal of traces of silver-salt and baryta, and subsequently extracted repeatedly with slight quantities of absolute alcohol, an addition of alcoholic sublimate solution gave a white precipitate, which contained the active substance, though not in toto. By extracting this precipitate with boiling water and concentrating this solution, we could not manage to abstract a pure mercuric chloride. Just as with sublimate we also obtained with platinic-chloride in alcoholic solution a precipitate that principally contained the active substance. This precipitate with platinic-chloride was soluble in a very small quantity of water. By concentrating the solution or by addition of alcohol no pure compound was set free. After decomposing the platinum-compound with hydrogen sulphide and addition of gold-chloride solution only a few crystals segregated, which melted at 215°—222°. A repetition of this experiment with a large amount of material did not enable us to isolate the active substance or a compound of it in a pure condition. The quantity of the gold-salt was very small and contained only a portion of the active constituent of the primary intestine extract.

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[1] The extracts were always examined for their effect after MAGNUS' method with the excised small intestine of the rabbit in 50 cc. of Tyrode-fluid at 38°.

An attempt to obtain a larger amount of material from the intestines of pigs and oxen did not yield the looked for result, though here also the activity of the extracts was satisfactory. Meanwhile, since by the method, thus far employed, the activity disappeared almost entirely, I tried to obtain a purified condition by a simpler method, viz. through extraction with various solvents that could readily be removed.

With a view to this we prepared an extract from five small intestines of cats, the alcoholic solution was evaporated to approximate dryness and the residue was extracted first with ether and then with chloroform. What remained out of solution was treated with a little glacial acetic acid. After evaporating this solution the remainder was dissolved in 5 cc. Tyrode-fluid and the activity of these solutions was determined.

It now appeared that the portion dissolved in acetic acid exhibited a much stronger activity than could be expected in an extract not subjected to a preliminary treatment. By heating with acetic acid, or stronger still with acetic acid anhydride, the activity seemed to be largely increased, in some cases five-hundred fold. After acetylating the extract of one small intestine $^1/_{5000}$ part could still be seen to exert distinct influence, whereas of the primary extract $^1/_{10}$—$^1/_{12}$ was needed to obtain this result. I always availed myself of this quality of the extracts in my further experiments to ascertain to what extent the various precipitates and filtrates still contained part of the active constituents. The large increase of activity after acetylating, in connection with the property of the looked for substance of yielding precipitates with phosphotungstic acid and in alcoholic solution with sublimate and with platinic-chloride, naturally led us to suppose that cholin, or an analogous compound, might be the active constituent of the extracts as it was well known that the physiological effects of cholin are as a rule largely increased by acetylating.[1]) We, therefore, ascertained whether the effect of cholin upon the gut is also increased by acetylating. This proved to be the case as, indeed, GUGGENHEIM and LÖFFLER[2]) have also reported.

We did, however, not succeed in isolating cholin in a pure condition from extracts of cats' small intestines, nor in making an approximately quantitative determination of the amount of cholin

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[1]) HUNT and TAVEAU, Bull. No. 73 of the Hygienic Laboratory of the Public Health and Marine Hospital Service. Washington (1911).

DALE, Proc. Physiol. Soc. Journ. of Physiol. Vol. 48. 111 (1914).

[2]) GUGGENHEIM und LÖFFLER, Bioch. Zeitschr. 72 319 (1916).

Id. id. Bioch. Zeitschr. 74. 208 (1916).

present. We, therefore, carried out another series of tests with extracts from rabbits' small intestines, which were always much more active than extracts of cats' intestines. Our procedure was the following:

The rabbit was killed by a blow on the neck and dehematized from the carotids; the small intestine was cautiously detached from the mesenterium and subsequently put in an abundant quantity of warm Tyrode-solution. Hereafter the gut was perfused with this fluid three or four times and every time transferred to another dish with fresh Tyrode-fluid. After being tied up on both ends and rinsed again it was suspended in 75 c.c. of water at 38°. The gut was taken out after an hour. A slightly alkaline reaction of the colourless and perfectly limpid aqueous extract was now noticeable. By infusion of carbonic acid or an admixture of some drops of 1/10 nHCl the reaction to litmus is made neutral, and rapidly boiled up. A flocculent precipitate is thrown down and on filtration there ensues a clear solution which (foaming being precluded) is evaporated down to a very small volume under a diminished pressure at 50°. A subsequent admixture of 25 cc. of methylalcohol produces a precipitate which contains little or nothing of the active substance and can be readily removed by filtration. The alcoholic solution thus obtained, possesses in undiminished degree the activity of the primary aqueous extract.

After removing the greater part of the methylalcohol by distillation, acetone is added to 4—5 times the quantity of the extract, which gives a large precipitate, containing only a small portion of the active constituent. After filtration the solution is evaporated down to a small volume under a lower pressure and subsequently placed in a vacuum-exsiccator. The pale yellow residue is then repeatedly extracted with small amounts of absolute alcohol until KI_3, added to a few drops of the filtrate, arrests the precipitation. As STANÊK has pointed out this is a delicate reaction on cholin. By saturating this solution with sublimate a white precipitate is produced that, after some hours, is filtered by suction and washed out first with an alcoholic sublimate solution and subsequently with absolute alcohol. The precipitate is then extracted with a little warm water, acidulated with a drop of hydrochloric acid. After cooling down, or if necessary concentrating, lightly tinged crystals segregate in the shape of little columns. After repeated crystallization the melting point was found to be 244°; when mixed with the mercurous double salt of pure cholin (melting point 246°) we found 244°—245°. To identify our experience still further platinum-salt was prepared, which melted at

215° and did not exhibit a lowering of the melting-point in the mixture-experiment. For the melting point of the gold double salt we found 238°—239°, corresponding with that of the gold-salt prepared from pure cholin.

Through some microchemical reactions we could also demonstrate that we had to do here with cholin. With sodium gold chloride the precipitate consisted of yellow obliquely truncated columns, completely resembling those prepared from pure cholin; with potassium mercury iodide (MAYER's reagent) a double salt was given, crystallizing into fine needles; likewise the precipitate with picrolonic-acid as well as with potassium periodide looked outwardly like that obtained from cholin.

Likewise the action upon the isolated rabbit's small intestine in Tyrode fluid appeared to resemble that of cholin.

A solution containing 0,3 mgrms of the compound isolated from the small intestine, possessed a distinct influence upon the excised rabbit's small intestine of about similar extent to the effect of 0,3 mgrms of cholin-hydrochloride; after heating with acetic acid anhydride an amount corresponding with 0,003 mgrms of the detected compound appeared to possess a considerable stimulating activity, approximately similar to an equal amount of acetyl-cholin.

This, then, proves that cholin is an active constituent of the small-intestine extract. The quantity of cholin, derived in this way as a mercurous double compound from the extract of a small-intestine, amounts to about 1 mgrm. This is certainly not all the cholin present in the primary aqueous extract, much less the total amount in the intestinal wall, as we know that the precipitate, obtained with acetone, contains also a small part of the active substance. The alcoholic filtrate of the sublimate precipitate is always more or less active and in this the presence of cholin can be readily demonstrated, by means of the periodide test. By acetylating a greater activity could be obtained, as with cholin, in the acetone precipitate as well as in the alcoholic filtrate. Assuming that the activity of these fractions, occurring with the isolation of cholin, may also be ascribed to cholin, it appears that at least 75 % of the looked for active constituent of the rabbit's small intestine may be attributed to cholin. However, I wish to accentuate that not in all experiments such a high percentage of the cholin, present in the extracts, could be isolated, as part of it gets lost more or less in the various experiments. This was evident from the control-tests in which cholin was added to the primary extracts; after the usual experimentation only part of the cholin could be found again.

MAGNUS [1]) has previously pointed out that the intestinal movements are effected under the influence of AUERBACH's plexus and points to metabolic processes in these nerve centres as the most probable cause of the stimuli.

From WEILAND's [2]) experience that a substance can by a simple method be derived from the intestine, which largely increases the movements of the surviving small intestine, he concludes that, just as with the automatic respiratory movements, the cause of the automatic intestinal movements must also be a chemical stimulus.

From the above we conclude that the intestinal extracts contain an amount of cholin that may in a high measure be made responsible for their stimulating influence upon the gut. Further inquiry showed that cholin is a substance, which occurs abundantly in various parts of the animal body. The rôle which this substance has to play in the body, has as yet not been discovered. The above inquiry has rendered it highly probable that cholin plays an important part in bringing about the automatic intestinal movements.

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1) MAGNUS Ergebnisse der Physiologie. 7e Jahrgang 8. 47.

2) WEILAND Loc. cit.

Pharmacological Institute of the
Utrecht University.

Chemistry. — "*On the Passivity of Chromium*". (First communication). By Dr. A. H. W. ATEN. (Communicated by Prof. A. F. HOLLEMAN).

(Communicated in the meeting of Oct. 27, 1917).

1. *Introduction.*

The passivity of chromium has been extensively investigated by HITTORF [1]). The results of these investigations are briefly the following:

On anodic polarisation, depending on circumstances, chromium can dissolve bivalently (as chromousion), trivalently (as chromicion) and six valently (as chromate).

When dissolving as chromousion the metal is active. It has a potential which is negative with respect to the hydrogen electrode, and which lies between that of Zn and Cd. In this state the metal is also chemically active, it generates hydrogen from acids etc. If on the other hand the metal dissolves as chromate, it is passive. It then presents a strongly positive potential, and does not act on acids.

Solution as chromicion has only been found in a few cases. The circumstances under which this takes place, are not accurately known.

Chromium is activated by bringing it in contact with strong acids, especially with hydrochloric acid. The activity is further promoted by rise of temperature, presence of chlorine ions, cathodic polarisation and especially by bringing the chromium in contact with molten chlorides ($KCl + NaCl$, $ZnCl_2$).

The passivity is promoted by anodic polarisation and by contact with oxidizing agents (bromine water, nitric acid).

HITTORF exclusively examined chromium that had been prepared by aluminothermical way according to GOLDSCHMIDT.

HITTORF's results and those of other investigators will be discussed more in particulars in connection with the results obtained by me.

2. *Summary of the different theories of passivity.* [2])

The earliest theory of passivity, which we owe to FARADAY [3]),

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[1]) Zeitschr. f. physik. Chemie **25**, 729 (1898); **30**, 481, (1899); **34**, 91 (1902).

[2]) For a full account cf. among others FREDENHAGEN. Zeitschr. f. physik. Chemie **63**, 1 (1908), ALLEN, LE BLANC, SCHOCH, REICHENSTEIN, SENTER, Chem. News, **109**, 25, **63**, **75**, 87, 115, (1915); **108**, 249, (1913). BENNETTS and BURNHAM, Zeitschr. f. Elektrochemie **22**, 377, (1916).

[3]) Phil. Mag. **9**, 61 (1836).

attributes the cause of passivity to the presence of a skin of oxide which separates the metal from the liquid. With this view HABER and GOLDSCHMIDT [1]), HABER and MAITLAND [2]) and KRASSA [3]) concur. These last investigators assume that the skin of oxide does not entirely cover the metal, but is porous or presents cracks, through which the metal is partly in contact with the liquid. This accounts for the fact that there are different degrees of passivity, which are caused by a more or less perfect separation between metal and liquid.

Also DUNSTAN and HILL [4]) consider the formation of a protecting layer as the cause of the passive state, which iron and other metals assume in a solution of potassium bi-chromate. This oxide theory is rejected by MÜLLER and KÖNIGSBERGER [5]) and MICHELI [6]), who could not optically demonstrate the presence of oxide on a passive metal.

According to the oxide theory passivity is a mechanical phenomenon, whereas the other theories ascribe the cause of passivity to a chemical change in the metal or in the liquid.

Of these theories the oxygen theory comes nearest to the oxide theory. According to this theory, which is defended by JORDIS [7]), FREDENHAGEN [8]), MUTHMANN and FRAUNBERGER [9]), and FLADE [10]), the metals in passive state possess a charge of oxygen, which retards the going into solution of the metal catalytically. The oxygen present can partly combine with the metal to oxide, which dissolves in the solid metal, or is absorbed by the metal [11]).

In opposition to this oxygen theory the hydrogen theory may be placed, according to which the metals in active state have a hydrogen charge, which accelerates the solution of the metal catalytically. The hydrogen can partially be present as hydrure, which dissolves in the metal. This hydrogen theory is chiefly advocated by GRAVE [12]) and RATHERT [13]). One view, however, does not exclude the other. It is possible that in active state a hydrogen charge furthers the

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[1]) Zeitschr. f. Elektrochemie **12**, 49 (1905).

[2]) Ibid. **13**, 309, (1907).

[3]) Ibid. **15**, 490, (1909).

[4]) Journ. Chem. Soc. **99**, 1853, (1911).

[5]) Zeitschr. f. Elektrochemie **15**, 742, (1909).

[6]) Arch. Sci phys. Nat. Genève, **115**, 122, (1900).

[7]) Zeitschr. f. Elektrochemie **11**, 787, (1905).

[8]) Zeitschr. f. phys. Chemie **43**, 1, (1913); **63**, 1, (1908).

[9]) Sitzungsber Bayr. Akad. **34**, 201, (1904).

[10]) Zeitschr. f. physik. Chemie **76**, 513, (1911).

[11]) BENNETTS and BURNHAM, Zeitschr. f. Elektrochemie **22**, 377, (1916).

[12]) Zeitschr. f. physik. Chemie **77**, 513, (1911).

[13]) Ibid. **86**, 567, (1914).

solution, whereas in passive state an oxygen charge counteracts the solution.[1])

About the way in which the oxygen retards the establishment of the electromotive equilibrium, or the hydrogen accelerates it, few special suppositions are found in the literature. It may, however, be assumed that in general it is supposed that the establishment of the *heterogeneous equilibrium metal-electrolyte* is influenced by the gas charge.

The above-mentioned theories have all this in common, that in passive state there exists no heterogeneous equilibrium between the metal and the solution. On the other hand there are also theories according to which there does exist heterogeneous equilibrium between metal and electrolyte, but the internal equilibrium in the electrolyte or in the metal is disturbed.

LE BLANC[2]) assumes, that the equilibrium between anhydrous and hydrated ions in the solution sets in slowly. When in consequence of anodic polarisation the concentration of the anhydrous ions at the anode increases, these combine in some cases only slowly with the water present.

This increases the concentration of the anhydrous ions greatly, though the total ion concentration increases only little. According to LE BLANC the cause of passivity would, therefore, be a concentration polarisation in the solution. Also KUESSNER[3]) and SCHILDBACH[4]) see passivity in this way.

The second possibility, that passivity is the result of an internal change of the metal, has repeatedly been pronounced. HITTORF[5]) speaks of a "Zwangzustand", without expressing an opinion as to what it consists in.

Also BYERS[6]) thinks that passivity is owing to a particular condition of the metal, which is brought about by a great number of different influences.

FINKELSTEIN[7]) and W. J. MÜLLER[8]) assume that in passifiable

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[1]) FOERSTER, Elektrochemie wässriger Lösungen, Leipzig 1915, 367.

[2]) Zeitschr. f. Elektrochemie **9**, 636, (1903); **11**, 705, (1905). Zeitschr. f. physik. Chemie **46**, 213 (1903) Boltzmann Festschrift 1904, 183, Chem. News. **109**, 63, (1914).

[3]) Zeitschr. f. Elektrochemie **16**, 754, (1910).

[4]) Ibid **16**, 967, (1910).

[5]) Zeitschr. f. physik. Chemie **25**, 729, (1898); **30**, 481. (1899); **34**, 91, (1902).

[6]) Journ. Amer. Chem. Soc. **30**, 1718, (1908); **32**, 750, (1910); **33**, 17 57, (1911); **34**, 1368, (1912); **35**, 759, (1913).

[7]) Zeitschr. f. physik. Chemie **39**, 91, (1902).

[8]) Ibid. **48**, 577. (1904); Zeitschr. f. Elektrochemie **15**, 696, (1909).

metals ions occur of different valency. In active state the metal contains chiefly ions of lower valency, in passive state ions of higher valency.

According to Smits 1) passivity is caused by a deficit of metal ions in the metal in consequence of the splitting up $m \rightarrow m^{+} + \theta$ proceeding more slowly than the removal of metal ions from the metal in the anodic solution. Le Blanc's and Smits's views have the advantage that they cannot only account for the anodic passivity, but also for the abnormal polarisation voltages occurring in the cathodic deposition of some metals, the overvoltage in case of gas generation, and more. Besides, gas charges of the metal or solved substances in the electrolyte can also exert a positive or negative catalytic influence on the velocity of the establishment of the homogeneous equilibrium.

3. *The potential of equilibrium of chromium.*

The most characteristic property of passifiable metals is, that during anodic and cathodic polarisation in solutions of their salts, they present abnormal polarisation voltages, i.e. voltages which are greater than can be accounted for by concentration polarisation in the liquid.

For the right understanding of the electromotive behaviour of chromium, in the first place the knowledge of the equilibrium potential is therefore required.

The records in the literature are very divergent.

Hittorf gives 1.184 V for the E. M. F. of the combination Cr | HCl | $NaNO_3$ | $AgNO_3$ | Ag. The normal potential of silver is + 0,80 V. From this would follow for chromium in hydrochloric acid $E_h = -0{,}38$ 2). According to Neumann 3) E_h in chromicsulphate is — 0,50, in chromicchloride — 0,48. Chromium amalgam gives the highest negative value. Rathert 4) found for chromium according to Goldschmidt in $0.1\,n$ H_2SO_4 $E_h = -0{,}39$, for chromium, which was deposited electrolytically according to Carveth and Curry 5)

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1) Zeitschr. f. physik. Chemie, **88**, 743, (1914); **90**, 723, (1915); **92**, 1. (1916).

2) When calculated with respect to the hydrogen electrode = 0, the potentials are denoted by E_h. The measurements have all been made with the normal calomel electrode The potentials indicated in the following chapters by E give the potential difference of the combination Cr | electrolyte | satur. sol. KCl | n. Calomel electrode.

3) Zeitschr. f. Elektrochemie **7**, 656, (1901).

4) Zeitschr. f physik. Chemie **86**, 567, (1914).

5) Journ. f. phys. Chemistry **9**, 353, (1905).

0,49. FLADE[1]) gives for the active potential in 0.1 *n* H_2SO_4 —0,32, FREDENHAGEN[2]) in *n* H_2SO_4 — 0,48. MUTHMANN and FRAUNBERGER[3]) found for the potential of chromium according to **GOLDSCHMIDT**, which had been activated by cathodic polarisation in potassium hydroxide — 0,64 in *n* KCl.

The given values vary accordingly from — 0,32 to — 0,64. FOERSTER[4]) takes as potential — 0,48. The "Messungen elektromotorischer Kräfte"[5]) give — 0,6.

In view of the great differences in these values it seemed desirable once more to determine the potential of equilibrium of chromium. Metallic chromium can probably only be in real equilibrium with a solution of a chromous salt, which contains a very small quantity of chromic salt.

This follows from the value of the chromous-chromic-potential, which according to MARZUCCHELLI[6]) amounts to — 0,36, according to FORBES and RICHTER[7]) to — 0,40, when it is assumed that the equilibrium potential of chromium is about — 0,5. It is, therefore, required for the determination of the potential of equilibrium to examine chromium electrodes which are in contact with a solution of a chromous salt. For this was chosen chromous sulphate, as it is easy to prepare.

4. *Preparation of chromoussulphate.*

For the preparation of chromoussulphate a solution of 200 gr. of chromicsulphate in 300 gr. of water and 100 gr. of sulphuric acid was reduced at an amalgamated lead cathode at 15° with a current density of 15 amp./dm^2.[8]). A blue solution of chromoussulphate is then formed, from which part of the salt is deposited in solid state. To be able to collect this without its coming into contact with the air, the reduction was carried out in a porous cup, in the bottom of which there was a hole closed by a rubber stopper. When the reduction was completed, the porous cup was placed in tube *I* of the apparatus drawn in figure 1. This tube was filled with pure carbonic acid through *A* or *B*. Then the stopper was pushed out of the bottom of the porous cup, so that the liquid and the crystals

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[1]) Zeitschr. f. physik. Chemie **88**, 569, (1914).

[2]) Ibid. **63**, 1, (1908).

[3]) Sitzungsber. Bayr. Akad. **34**, 201, (1904).

[4]) Elektrochemie wässriger Lösungen, Leipzig 1915, 191.

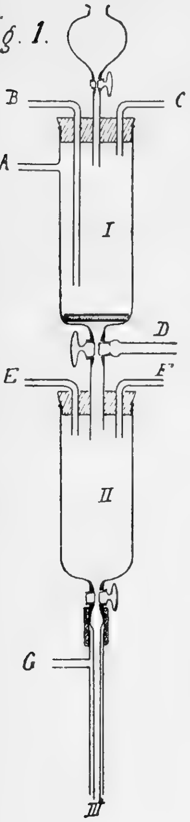
[5]) Abh. d D. Bunsengesellschaft N^0. 5, 8, (Halle 1911, 1915).

[6]) Zeitschr. f. Elektrochemie **11**, 882, (1905).

[7]) Journ. Amer. Chem. Soc. **39**, 1140, (1917).

[8]) FOERSTER, Electrochemie wässr. Lösungen, Leipzig 1915, 532.

Fig. 1.



of chromoussulphate ran into tube *I*. The porous cup was removed and tube *I* was closed at the top. The liquid was sucked off through the three-way-cock *D*, the crystals remained on the asbestos filter in *I*. These crystals were washed with oxygen-free water, and the washing water sucked off through *D*. Then the chromoussulphate was dissolved in water, and this solution conveyed to tube *II*, which was likewise filled with carbonic acid. From *II* this solution could be conveyed into other vessels through *III*, in which carbonic acid was admitted through *G*, so that also in this operation the chromous-sulphate was only in contact with carbonic acid.

In this way a pure blue solution of chromous-sulphate was obtained. It was generally not free from acid, because it is impossible to wash till all the sulphuric acid has been expelled without a great loss of chromous-sulphate. To remove the free sulphuric acid, a small quantity of baryt water was finally added.

5. *The chromium electrodes used.*

For the measurements of the potential three kinds of electrodes were used. First small pieces of chromium according to Goldschmidt, which were cemented into glass tubes, in which a drop of mercury and a copper wire was brought.

Secondly chromium, which had been deposited electrolytically from a solution of $12^{0}/_{0}$ chromic-sulphate and $13^{0}/_{0}$ chromic acid [1]) on a copper wire of 3 mm. thickness. The current density amounted to 80 Amp./dm^2 at a velocity of revolution of the cathode of 800 Rev./min.

The amount of metal deposited by the current was only $15^{0}/_{0}$ of the theoretical. The metal was of a fine grey colour, and adhered well to the electrode when the layer was not too thick; otherwise there was a tendency to scale off.

The third kind of chromium electrodes consisted of copper wire which had been electrolytically chromated in a solution of chromic chloride, which contained 100 gr. Cr. per L. [2]). Under the same

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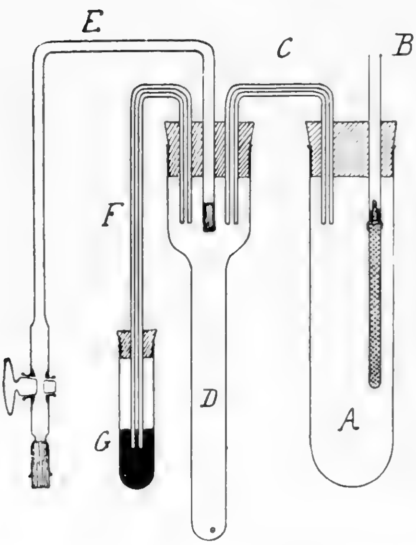
[1]) Foerster, Elektrochemie wässriger Lösungen, Leipzig 1915, 491.

[2]) Carveth and Mott. Journ. f. phys. Chemistry **9**, 231, (1905).

circumstances as above the current efficiency was here 30°/$_0$. The metal had a greater tendency to scale off than that from chromic acid and chromic sulphate.

These electrodes were cemented into glass tubes, and six of them were brought in a rubber stopper on tube *A* in figure 2. Tube *A* was connected with tube *D* by means of a siphon *C*, and tube *D* by a siphon *E* with a saturate solution of potassium chloride, and further with a normal electrode. The siphon *E* was closed on both ends by a stopper of filtering paper, and could further be entirely closed by a cock. Through the stopper of tube *D* passed a bent capillary *F*, the lower end of which was under mercury. This gave an opportunity to the liquid in *A* and *D* to expand. The tubes *A* and *D* were quite filled with a solution of chromous sulphate, and then closed by the stopper. In this way the solution in *A* was perfectly shut off from the air, and yet in conductive connection with the calomel electrode. Altogether four tubes *A* were connected with *D*, which each contained six electrodes.

Fig. 2.



6. *Measurements of the Potential.*

Each of the tubes *A* contained two electrodes of chromium according to Goldschmidt (1 and 2), two electrodes of electrolytic chromium from chromic sulphate and chromic acid (3 and 4), and two of electrolytic chromium from chromic chloride (5 and 6). All the potentials were measured with respect to a normal calomel electrode. The solutions contained about 0.3 mol. Cr.SO. per L.

On the whole the potentials presented the following course. When the electrodes had been brought in contact with the solution, the values of the potential at first diverged greatly. When the solution was feebly acid, the potential was about —0.7 to —0,8 Volt. In neutral solution the potentials were mostly smaller negative, especially for the chromium according to Goldschmidt. Here the potential was generally from —0,2 to —0,0. After a few days all the electrodes of electrolytic chromium (3, 4, 5, and 6) reached, however, an almost constant potential of —0.55. Also the chromium of Goldschmidt sometimes got this value, sometimes it remained more positive. As an example a series of observations has been extensively given in table I. The solution used was at first feebly acid, generation of hydrogen took place, and the solution assumed a green colour by formation of chromic sulphate.

TABLE I. Potential.

Number of the electrode	May 5, $2^h.30$	May 5, 4^h	May 8	May 11	May 15	May 18
1	— 0.612	— 0.627	— 0.538	— 0.518	— 0.528	— 0.510
2	— 0.600	— 0.600	— 0.3	— 0.1	— 0.1	— 0.1
3	— 0.722	— 0.800	— 0.540	— 0.543	— 0.561	— 0.540
4	— 0.809	— 0.793	— 0.543	— 0.552	— 0.541	— 0.527
5	— 0.460	— 0.515	— 0.508	— 0.521	— 0.527	— 0.518
6	— 0.517	— 0.610	— 0.522	— 0.527	— 0.530	— 0.512
Mean of 1, 3, 4, 5, and 6			**— 0.530**	**— 0.532**	**— 0.537**	**— 0.522**

The electrode 2 had a very fluctuating potential after 3 days, the other potentials approach to a pretty constant value, which amounts to about —0.53 V for this series of observations.

The same phenomenon was met with for all the observations. In table II the means of the potentials of all electrodes of electrolytical chromium have every time been given. Always two or more days had elapsed between the measurements.

The mean of all these 136 observations is — 0,55 V. *Accordingly*

TABLE II.

Nr.	Potential.					
I	— 0.534	— 0.537	— 0.539	— 0.540	— 0.540	- 0.542
II	— 0.535	— 0.541	— 0.545	— 0.555	—	—
III	— 0.567	— 0.565	— 0.567	— 0.561	—	—
IV	— 0.556	— 0.551	— 0.554	— 0.554	—	—
V	— 0.554	— 0.544	— 0.545	— 0.548	— 0.547	—
VI	— 0.542	— 0.542	— 0.542	— 0.541	—	—
VII	— 0.562	— 0.563	— 0.563	—	—	—
VIII	— 0.530	— 0.532	— 0.537	— 0.522	—	—

in a solution of chromous sulphate, electrolytical chromium presents an almost constant potential of —0,55 V., when the metal has been in contact with the solution for some days. For shortness we shall call this potential the *end potential.*

By anodic or cathodic polarisation the potential of the chromium can be temporarily modified. When then the metal in the solution is left to itself, the potential returns to — 0,55, as appears from table III.

TABLE III.

Nr.	Before polarisation.	After polarisation.	After 20 min. rest.	After 1 day's rest.
3	— 0.569	— 0.753	— 0.578	— 0.570
4	— 0.532	— 0.775	— 0.591	— 0.522
5	— 0.540	— 0.778	— 0.568	— 0.547
6	— 0.541	— 0.732	— 0.608	— 0.553

3 and 5 had been anodically polarized, 4 and 6 cathodically, with 7 m.A. for 40 sec. It appears that electrolytic chromium is activated not only by cathodic, but also by anodic polarisation. In this, however, the metal is more positive immediately after the breaking of the current than at first, and then falls rapidly to a negative value.

When the electrodes are subjected to different previous treatments, they have at first different potentials, which, however, approach to — 0,55 again after some days.

Four chromium electrodes which had been in contact with a solution of $CrSO_4$ for a week, and which had then been exposed to the air for a quarter of an hour, immediately gave potentials of — 0,54 to — 0,55, when brought into a new solution of $CrSO_4$, which potentials remained constant in further observations.

Four other electrodes, which had been chromated electrolytically, the same day and had been exposed to the air for some hours, gave at first potentials of — 0,27 to —0,53. After ten days they had become constant and amounted to from — 0,55 to — 0,57.

Four newly chromated electrodes were cathodically polarized in sodium hydroxide. In $CrSO_4$ they at first presented potentials of — 0,70 to — 0,75, after ten days of — 0,55 to — 0,57.

Four electrodes of electrolytic chromium, which had been in diluted bromine water for some hours, gave — 0,65 to — 0,76 in chromous sulphate. After ten days from — 0,55 to — 0,56.

One would be inclined to consider this final potential, which electrolytic chromium assumes under all circumstances in $CrSO_4$, as the real potential of equilibrium of chromium.

It is, however, possible that the potential of — 0,55 V. ($E_h =$ = — 0,27) does not correspond with the equilibrium $Cr^{\cdot\cdot}_{metal} \rightleftarrows Cr^{\cdot\cdot}_{sol}$. but with another equilibrium.

In the first place the chromium in these solutions of $Cr^{\cdot\cdot}$ might assume the potential corresponding with the equilibrium $Cr^{\cdot\cdot} \rightleftarrows Cr^{\cdot\cdot\cdot} + \Theta$. To examine this, in a number of measurements by the side of the chromium electrodes a tin electrode was brought into the liquid which is best suited for the determination of the chromous-chromic potential according to Mazzucchelli. These tin electrodes showed on an average a potential of — 0.68, hence $E_h = -0.40$, the chromous-chromic potential lying at $E_h = -0.36$ according to Mazzucchelli. When we take into consideration that the solutions used contain much more chromous than chromic, the agreement may be called satisfactory. If we wanted to consider the end potential of — 0.55, which the chromium electrode gets, as the chromous-chromic potential, we should have to assume that through insufficient establishment of the equilibrium with respect to chromium the chromous-chromic potential is 0,13 V more positive than with respect to tin. This is not impossible, because also with respect to a platinum electrode the chromous-chromic potential is about 0.16 V more positive than with respect to mercury [1]).

The second possibility is this, that we have to do here with a hydrogen potential, that therefore not the chromium itself, but the hydrogen dissolved in it, is electromotively active. When the chromium at its surface has a hydrogen charge of one atmosphere, a hydrogen ion concentration in the solution of about 10^{-5} would be required for a potential of — 0.55 V. Such a hydrogen ion concentration could, probably, occur in a solution of chromoussulphate which is saturated with chromous hydroxide.

There is, however, still another potential to be observed for chromium electrodes, which must have a special meaning, and this renders it difficult to assume that the end-potential at — 0.55 is the potential of equilibrium.

When newly chromated electrodes are brought into a very feebly acid solution of chromous-sulphate, they present a potential lying in the neighbourhood of — 0.7 to — 0.8 V. These values vary pretty considerably. For chromium that had been deposited from chromic sulphate and chromic acid, 20 % of the observations lay below — 0,8, 65 % between — 0.8 and — 0.7, and 15 % above — 0.7.

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[1]) FORBES and RICHTER, Journ. Amer. Chem. Soc. **39**, 1144, (1917).

For chromium out of chromium chloride these values were 5 %, 75 % and 20 %. The mean of all the observations lies at — 0.75 V for the first kind of chromium, at — 0.71 V for the second.

This potential is not only presented by the electrodes when they are brought in fresh condition into $CrSO_4$, but also when after the end potential has been reached, they are polarized cathodically or anodically in chromoussulphate, or cathodically in sodium hydroxide or are treated with bromine water, or are brought once more in an acid solution of $CrSO_4$.

The above mentioned operations have as result a renewal of the chromium surface. In case of anodic polarisation and treatment with bromine water hydrogen could also be withdrawn from the metal. In case of cathodic polarisation in $CrSO_4$, and action of diluted acids hydrogen can also be added to the metal. Only the cathodic polarisation in sodium hydroxide does not give a renewal of the surface, but only hydrogen charge.

In this last case the negative potential, which the metal takes, might be ascribed to a strong hydrogen charge, in which then the hydrogen, not the chromium, was electro-motively active. In the same way the negative potential which electrolytic chromium gets on renewal of the surface, might be the hydrogen potential of the hydrogen present in electrolytic chromium. In contact with the liquid this chromium might lose its hydrogen, till the hydrogen pressure corresponds with one atmosphere, in which the potential might rise to — 0,53. Against this view, that the potential of about — 0,75 V is the potential of hydrogen which has been dissolved in the metal under a high pressure, different objections may be advanced, however. In the first place it would then be strange that the electrodes after anodic polarisation, in which hydrogen is withdrawn from the metal, present the same potential as after cathodic polarisation, in which hydrogen is added.

A greater difficulty is, however, this, that a piece of chromium of GOLDSCHMIDT in hydrochloric acid spontaneously generates hydrogen at a potential of about — 0,75 V. In this there is no question of a hydrogen charge which the metal gets through external influences, but the metal itself is able to generate hydrogen at this potential. The potential of chromium itself must, therefore, be — 0.75 V or higher negative here. In hydrochloric acid the potential can still be considerably more negative. In 25 % hydrochloric acid — 0,84 was found, in 6 % — 0,76, in 1,5 % — 0,74.

In general a metal, that generates hydrogen out of an acid, will present a potential lying between that of the metal itself and of a hydrogen electrode. The greater the overvoltage for hydrogen generation is at the metal, the nearer the potential will be to that of the metal. This appears e.g. clearly for amalgamated zinc, pure zinc, and zinc in contact with platinum in hydrochloric acid. The first is most strongly negative, the last least so. A metal can, therefore, for this reason never possess a too negative potential.

There is, however, another cause which can render the potential too negative, viz. complex formation by the hydrochloric acid, in consequence of which the concentration of the elementary metal ions is kept at a small value. This is the case for zinc: in strong hydrochloric acid this has a much too negative potential. Thus also the potential of — 0,84, which chromium presents in 25 °/₀ hydrochloric acid, can be too negative through complex formation. For 6 °/₀ and 1,5 °/₀ hydrochloric acid this will be observed to a much smaller degree, so that the value — 0,75 is certainly not too negative. The same value of — 0,75 V is found when chromium amalgam, prepared according to FÉRÉE [1]) is brought into chromous sulphate. After some days also these amalgam electrodes assume the end-potential — 0,55 V. Pyrophorous chromium, according to FÉRÉE, was slightly less negative, viz. — 0,70 V.

Hence it follows from the above that electrolytic chromium and chromium amalgam in chromous sulphate, just as chromium of GOLDSCHMIDT in hydrochloric acid, present a potential of about — 0,75 V or — 0,47 V with respect to the hydrogen electrode. This value is in agreement with that found by NEUMANN, RATHERT and FREDENHAGEN, and also with that assumed by FOERSTER.

This potential, which might be called the *active* potential, will probably lie not far from the potential of equilibrium.

This can only be reached when the metal contains a sufficient quantity of hydrogen, which is present in the metal for electrolytic chromium, and which is generated in acids by the metal for chromium of GOLDSCHMIDT. The hydrogen must, therefore, catalytically promote the establishment of the equilibrium here. That the potential runs back to — 0,55 V or — 0,27 V after some days with respect to the hydrogen electrode, may be explained so that the metal loses so much hydrogen that the hydrogen pressure corresponds with one atmosphere. This quantity of hydrogen will then be too small to serve as catalyst for the establishment of the equilibrium, and this end-potential will be a hydrogen potential, not a chromium potential, as was already suggested as a possibility on p. 822.

Amsterdam, October 1917. *Chemical Lab. of the Univ.*

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[1]) Compt. Rend. **121**, 822, (1895).

Chemistry. — *"A Third Kind of Sulphur Molecule".* (Fifth Communication). By Dr. A. H. W. ATEN. (Communicated by Prof. A. F. HOLLEMAN.)

(Communicated in the meeting of Oct. 27, 1917).

1. *Introduction.*

In the first publication on this subject [1]) the solubility of rhombic sulphur was treated in mixtures of sulphur chloride and sulphur, which were heated to temperatures of 75°—175°, and were then quickly cooled down. It then appeared, that this heating greatly enhanced the solubility of rhombic sulphur. This phenomenon is explained in this way, that the rhombic sulphur, when heated with sulphur chloride, is converted to another form of sulphur, S_π .

The question how sulphur behaves, when it is heated with other solvents, has only been briefly treated in the preceding communications.

What had appeared was, that the conversion of S_λ into S_π takes place at 100° not only in sulphur chloride, but also in carbon sulphide. To enable us to make a better comparison between sulphur chloride and other liquids as solvent, the solubility of rhombic sulphur was determined in mixtures of sulphur and toluene, which had been heated to different temperatures.

2. *Increase of solubility of sulphur by heating in toluene-solution.*

The solubility of rhombic sulphur in toluene amounts to 0,92°/₀ at 0°. When a solution of sulphur in toluene is heated at 150° for some hours, it is found that after cooling the solubility has become greater at 0°, and the more so as the original solution contained more sulphur. This is accompanied by a change of the colour of the solution. A solution of S_λ in toluene is almost colourless at 0°. After being heated the same solution exhibits the yellow colour of S_π at 0°. From this follows that also in toluene S_λ is partly converted into

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[1]) Zeitschr. f. physik. Chem. **81**, 257 (1912). Following communications ibid **83**. 442 (1913); **86**, 1 (1913); **88**, 312 (1914).

S_π. From the increase in solubility of rhombic sulphur in toluene after heating, the quantity of S_π formed can be calculated.

In the first place it now appeared that the equilibrium $S_\lambda \rightleftarrows S_\pi$ sets in much more slowly in toluene than in sulphur chloride. In toluene it is about six hours at 140° before the equilibrium has been established, whereas in sulphur chloride at the same temperature only a few minutes are required for it. (First communication, p. 265). In view of this 140° was chosen as lowest temperature at which determinations were made. Determinations were also carried out at 150° and 160° to study the influence of the temperature on the equilibrium.

The determination of the solubility of sulphur in toluene after heating took place as follows: A mixture of toluene and sulphur of known concentration was heated for six hours at 140°, for four hours at 150° or for two hours at 160°, and then rapidly cooled, in which part of the sulphur separated. After addition of a small quantity of rhombic sulphur the mixture was stirred for an hour at 0°. Then a sample was taken from the solution, and the sulphur content was determined by evaporation of the toluene.

The results are given in Table I. In figure 1 the solubility of the sulphur after heating, is graphically represented as function of the concentration of the mixture.

TABLE I.

140°		150°		160°	
Concentration of the solution	Solubility at 0°	Concentration of the solution	Solubility at 0°	Concentration of the solution	Solubility at 0°
3.4 %	1.18 %	3.6 %	1.24 %	3.2 %	1.24 %
6.2	1.42	6.6	1.50	6.2	1.54
9.2	1.64	10.6	1.79	15.0	2.40
22.0	2.69	22.6	2.96	21.6	3.00
27.6	3.23	30.1	3.54	30.0	3.64

The lines for 140°, 150°, and 160° stop at about 36, 41, 47% sulphur, mixtures of a higher sulphur content not being homogeneous at these temperatures. [1])

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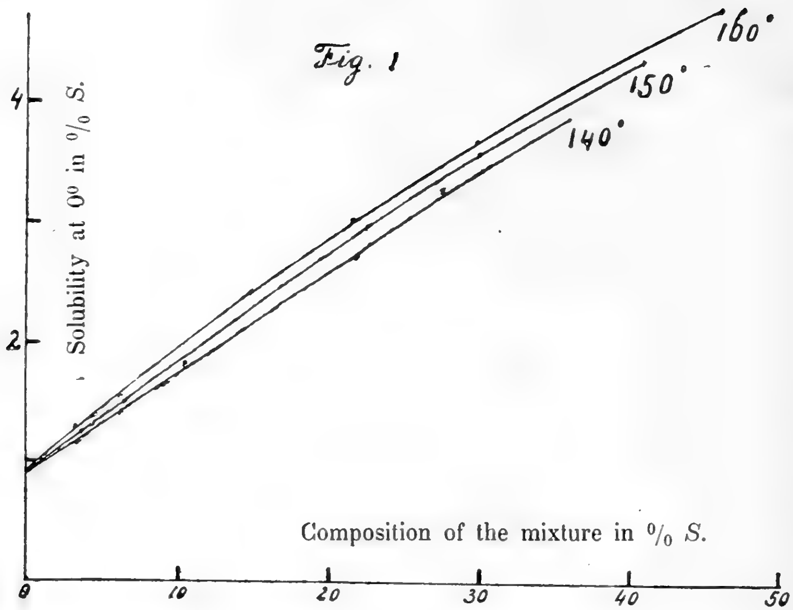
[1]) Kruyt. Zeitschr. f. physik. Chemie 65, 502, (1909).



3. *The equilibrium* $S_\lambda \rightleftarrows S_\pi$ *in solutions in toluene.*

From the solubility of rhombic sulphur in toluene, after the solution has been heated, the quantity of S_π that is present in the solution can be calculated. Thus we know the equilibrium $S_\pi \rightleftarrows S_\lambda$ in a solution of given concentration at a given temperature.

When we call the solubility of rhombic sulphur at 0° S, the S_π-content of the solution is given by:

$$S_\pi = 0.68\ (S - 0.92)$$

(4th communication). In this way we find, in case of equilibrium, the quantities S_π and S_λ present, which are given in table II.

TABLE II.

140°		150°		160°	
S_λ	S_π	S_λ	S_π	S_λ	S_π
8.6	0.51	9.2	0.63	8.1	0.63
15.2	0.97	16.0	1.11	15.0	1.20
21.4	1.39	24.2	1.67	31.9	2.82
43.0	3.37	43.7	3.90	42.1	3.96
50.3	4.39	53.2	4.96	53.0	5.13

To facilitate a comparison with the values of sulphur chloride, these values are expressed in atom percentages.

These lines of equilibrium for 140° and for 160° are drawn in figure 2 on rectangular coordinate paper. It appears from these lines that the S_π-content at 160° is only little higher than at 140°. In this range of temperature an important shifting of the equilibrium does, therefore, not take place.

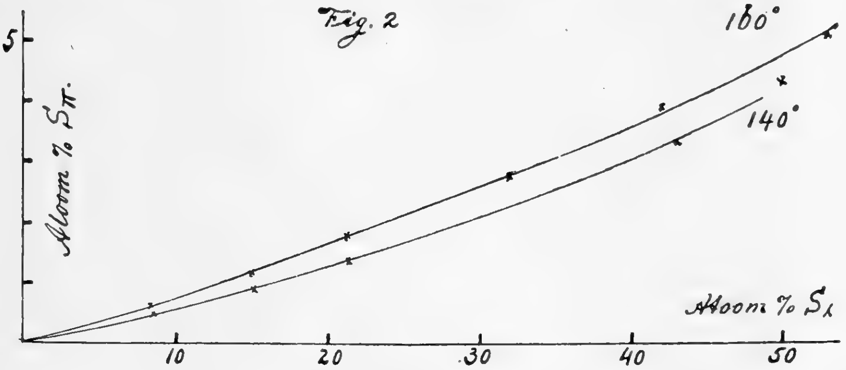


Fig. 2

Seen from below the lines are convex, i.e. with increasing total sulphur content the relative quantity of S_π increases more than the relative quantity of S_λ. From this follows that the toluene present shifts the equilibrium towards the side of S_λ. In very dilute solution, where the quantity of toluene is practically constant, the equilibrium must move towards the side of S_π with increasing dilution, as the molecule S_π is smaller than the molecule S_λ (fourth communication p. 369). The accuracy of the experiments in case of great dilution is, however, not sufficient to allow us to demonstrate this.

4. *Comparison of the equilibrium* $S_\lambda \rightleftarrows S_\pi$ *in toluene and in sulphur chloride.*

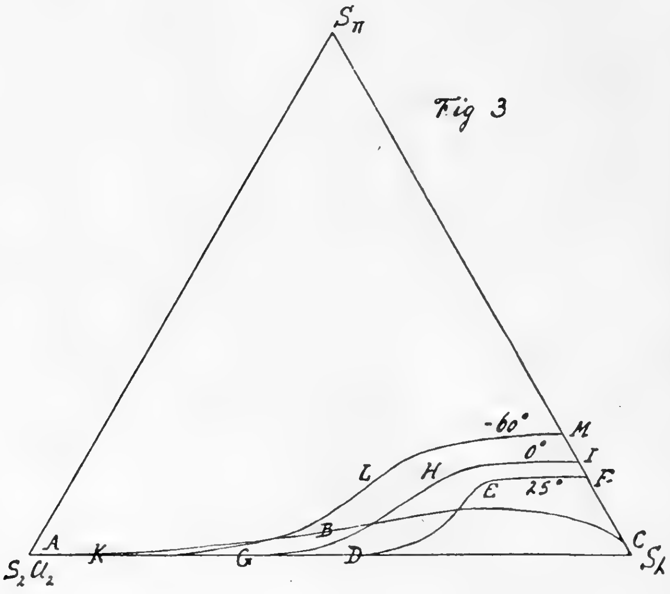
In order to be able to compare the quantity of S_π in toluene with that in sulphur chloride, we must also calculate the quantities of S_π and S_λ, present in case of equilibrium, for these latter solutions from the solubility of rhombic sulphur. This is, however, not possible without further suppositions about the course of the solubility lines of S_λ in mixtures of S_π with sulphur chloride.

The course of these solubility lines being unknown, only a probable situation of the equilibrium $S_\lambda \rightleftarrows S_\pi$ in sulphur chloride can be given, and this in the following way:

We assume a certain situation for the line of equilibrium $S_\pi \rightleftarrows S_\lambda$, and construct the solubility lines for 25°, 0°, and — 60° with this line and the values of the solubility in Table I of the first communication.

From the more or less probable form of the solubility lines found thus, we can then decide whether the assumed line of equilibrium is more or less probable.

Thus it has been assumed in figure 3 that the line of the equilibrium $S_\lambda \rightleftarrows S_\pi$ in sulphur chloride at 100° has the same form as in toluene at 140°.



This line is $A\ B\ C$ in figure 3, the part $B\ C$ is extrapolated, except the point C. From this line we find for the solubility curves at 25° $D\ F\ F$, at 0° $G\ H\ I$, at —60° $K\ L\ M$.

The course of these solubility lines is very improbable. In the first place, according to these lines the solubility of S_λ in sulphur chloride would be greatly enhanced by S_π, especially at — 60°. For sulphide of carbon, in which S_λ dissolves almost equally well as in sulphur chloride, the solubility of S_λ is, on the contrary, but little increased by S_π (3rd communication 2, 3, 4). It can, therefore, not be accepted that by addition of S_π the solubility of S_λ is so much increased, as is drawn in figure 3.

In the second place the points M, I and F, which indicate the solubility of S_λ in pure S_π at — 60°, 0°, and 25°, lie very close together, and at a very great S_λ-content.

This would mean that rhombic sulphur, even at — 60°, would possess a solubility in S_π of 77 $^0/_0$. This, too, is improbable, because in the best solvents, sulphur chloride and sulphide of carbon, the solubility only amounts to 10 $^0/_0$ at the same temperature.

The line of equilibrium $S_\lambda \rightleftarrows S_\pi$ in sulphur chloride must, therefore, be drawn so that the solubility lines run more steeply at D, G, and K, and that the points F, I, and M get to lie farther apart. This is satisfied in figure 4.

The situation of the solubility lines drawn here, is however not probable either, for here the solubility of S_λ in sulphur chloride would be lessened by addition of not too large quantities of S_π.

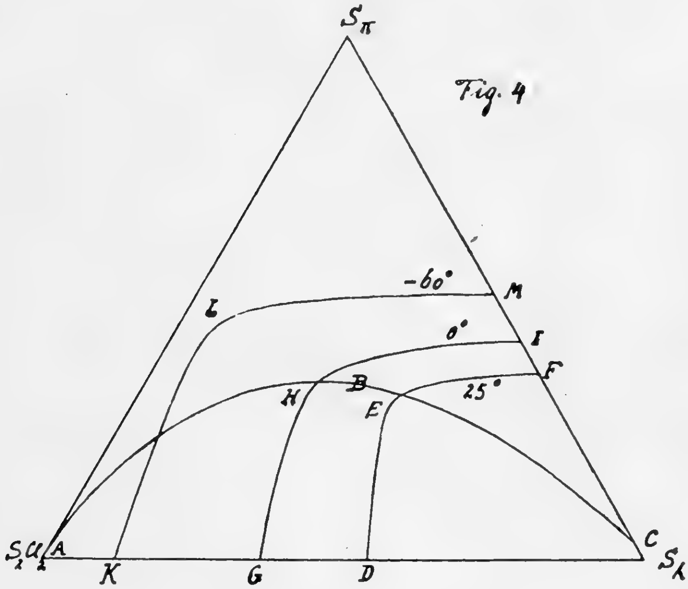
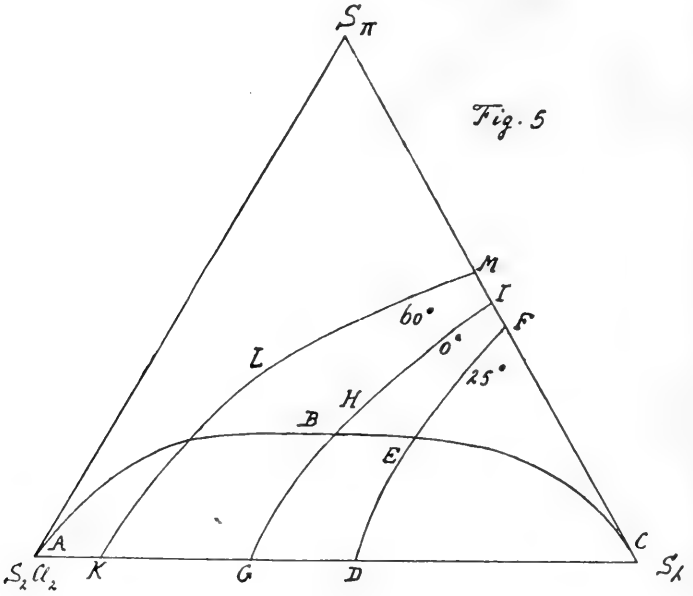


Fig. 4

If, however, the homogeneous equilibrium line is given a form as ABC in figure 5, we arrive at a plausible shape of the solubility lines. It is therefore probable that in figure 3 the line of the equilibrium $S_\lambda \rightleftarrows S_\pi$ lies too low, in figure 4 too high, and that figure 5 gives about the true situation. We may, therefore, assume the equilibrium $S_\pi \rightleftarrows S_\lambda$ in sulphur chloride at 100° in a mixture of 50 at. $^0/_0$ of total sulphur to lie at about 25 $^0/_0$ S_π. In toluene the equilibrium mixture at 140° contains not quite 5 at. $^0/_0$ S_π. Accordingly sulphur chloride has a particularly specifically favourable influence on the formation of S_π. That this is not exclusively caused by the greater solubility of S_λ in sulphur chloride, appears from an observation recorded in the third communication. It was found there that a solution in sulphide of carbon with 56 at $^0/_0$ of total sulphur contains only about 3 at. °/₀ of S_π at 100°, when the equilibrium $S_\pi \rightleftarrows S_\lambda$ has been established.



The quantity of S_π, which is formed in sulphide of carbon, is therefore much smaller than in sulphur chloride, and of the same order of magnitude as in toluene.

Chemical Laboratory of the University.

Amsterdam, June 1917.

Chemistry. — "*Revision of the Tables for the Strength of Spirit*". By Prof. N. SCHOORL and Miss A. REGENBOGEN. (Communicated by Prof. P. VAN ROMBURGH).

(Communicated in the meeting of Oct. 27, 1917).

For the derivation of the strength of spirit [1]) from its specific gravity and temperature only those tables are officially in use in Holland that were drawn up in 1860 by E. H. VON BAUMHAUER, then university professor at Amsterdam with his assistant VAN MOORSEL, the foundations of which are laid down in a Treatise published by the Royal Academy of Sciences (1860), the tables in extenso calculated for the use of areometers for the determination of the specific gravity having appeared at Amsterdam in 1861. Besides the Department of Finance, induced by the law of 1862 concerning the excise on spirits and the Royal Warrant and the Resolution of 1863 for the enactment of this law, has published the tables for the determination of the percentages of pure alcohol in distilled liquids according to the indications of the centigrade areometer and thermometer, which tables likewise rest on VON BAUMHAUER's data. These tables are still in use by the excise officers.

The Dutch Pharmacopoeia has also adopted VON BAUMHAUER's Tables for spirit at 15° since its Third Edition (1889). One of us was charged with the revision of these spirit tables by the Pharmacopoeia Committee, with a view to the preparation of a new Edition of the Dutch Pharmacopoeia.

In other countries VON BAUMHAUER's tables, though he has also published them in German, are not in use. In some countries the official tables are still those of GAY-LUSSAC (1824); in most countries the tables are used which were calculated from MENDELEJEFF's observations (1869), which greatly surpass those of VON BAUMHAUER in accuracy. Besides from 1911 to 1913 OSBORNE and MAC KELVY carried out new measurements, which lay claim to great accuracy, at the Bureau of Standards of Washington.

In order to fulfil our charge we have in the first place made absolute alcohol with the best of the means known for this, viz. distillation over CaO, shaking with freshly precipitated Ag_2O, and

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[1]) The term "spirit" is used here for mixtures of (ethyl) alcohol and water.

finally distillation over metallic Ca. We have further ascertained that on fractionated distillation with HAHN's still-head (1910) this alcohol did not yield fractions of different specific gravity, the best criterion that actually all the water had been removed.

We had further an opportunity to compare alcohol derived from two different sources viz. from the "Supra-grain spirit" and the "Prime Grain ($^1/_3$) and Molasses ($^2/_3$) mixed", both originating from the Delft Yeast and Spirit Works. MENDELEJEFF only accurately determined the specific gravity of the absolute alcohol from grain spirit. Our investigation has revealed that in this respect there is no difference with molasses spirit.

Besides we were enabled to use a more accurate method for the determination of the specific gravity than earlier investigators by the application of the picnometer given by JOHNSTON and ADAMS for the determination of the specific gravity of solid substances. Through the way of closure of this picnometer with a plane ground closing cover the error of the meniscus formation is entirely obviated, no correction needing to be applied for the weight of the vapour of the examined substance, which fills the space between liquid mirror and stopper otherwise. We used such a picnometer of somewhat more than 100 cm., filled it immediately at the mouth of the still-head with exclusion of the moisture from the atmosphere, and applied the corrections in the weighings for the reduction to vacuum. Thus the measurements could be made accurate to the fifth figure of decimals of the specific gravity, in which as chief source of error remained the accurate adjustment of the temperature. This must take place with an accuracy of 0,01° in the thermostat, for in consequence of the great coefficient of expansion of alcohol a difference of 0.01° corresponds with a difference of about 0,00001 in the specific gravity. But besides, the thermometer which is used for the determination of these temperatures, must agree down to 0,01° with the international hydrogen scale. We were not quite sure about this last point, because we had only an opportunity to compare our thermometer with the best, but pretty old standard thermometer of the Metereological Institute at De Bilt.

Our results for specific gravity $\frac{15}{4}$ (l.l.) of alcohol are the following: (See table on the following page).

Comparison of the specific gravity of absolute alcohol immediately after the last distillation over calcium and saturation with dry air, and also after the gases dissolved at the airpump had been boiled out

before the boiling out	0,79360
after the boiling out	0,79362

	"Supra Grain"	"Prime Grain ($^1/_3$) and Molasses ($^2/_3$) mixed"
Single repeated distillation over quick lime		0.79390
Last distillation over Ca. .	0.79363	0.79360
After second distillation over metall. Ca	0.79363	

Fractionated distillation of the absolute alcohol from "Prime Grain ($^1/_3$) and Molasses ($^2/_3$) mixed", in six fractions.

	Observed	Mean
First fraction	0.79361	
	0.79361	0.79361
	0.79361	
Third fraction	0.79359	
	0.79362	
	0.79361	0.79360
	0.79358	
	0.79360	
	0.79360	
Last fraction	0.79359	
	0.79359	0.79359

Comparison of the alcohol "Supra Grain" and "Prime Grain and Molasses"-spirit at most accurate adjustment of the temperature at 15° C. of the international hydrogen scale, in which the specific gravity of water is 0,99913 (according to THIESEN c.s. 1900)

"Supra Grain"	"Prima Grain ($^1/_3$) and Molasses ($^2/_3$)"
0.79361	0.79361
0.79359	0.79360
0.79361	0.79359

When our results are compared with the values referring to the spec. gr. of absolute alcohol in the literature after 1860, the observations before 1889 (introduction of the international hydrogen scale for the temperature) must be recalculated for the temperature and the observation of Mendelejeff (1869) also for the erroneous specific gravity of water used by him, viz. 0,99918 at 15° (according to Kopp). We then find:

	Observer	Observation	sp.gr. $\frac{15}{4}$ (l.l.) on international hydrogen scale
1860	von Baumhauer	sp.gr. $\frac{15}{4}$ (l.l.) 0.79415	0.794085
1869	Mendelejeff	sp.gr. $\frac{15}{4}$ (l.l.) 0.79367	0.793572
1884	Bureau des P. et M.	sp.gr. $\frac{15}{15}$ (l.l.) 0.79433	0.79359
1887	Mendelejeff	sp.gr. $\frac{15}{4}$ (l.l.) 0.79363	0.793565
1901	Cook	sp.gr. $\frac{15}{4}$ (l.l.) 0.79357	0.79357
1904	Morley	calcul. from Mendelejeff	0.79357
1904	Crismer	sp.gr. $\frac{15}{4}$ (l.l.) 0.79366	0.79366
1905	Winkler	sp.gr. $\frac{15}{4}$ (l.l.) 0.79363	0.79363
1906	Klason and Norlin	sp.gr. $\frac{10}{4}$ (l.l.) 0.79792	0.79365
1906	" " "	sp.gr. $\frac{20}{4}$ (l.l.) 0.78938	
1908	Andrews	sp.gr. $\frac{25}{4}$ (l.l.) 0.78510	0.79355
1910	Acree	sp.gr. $\frac{25}{4}$ (l.l.) 0.78507	0.79352
1911	Kailan	sp.gr. $\frac{25}{4}$ (l.l.) 0.78513	0.79358
1911	Osborne and Mc Kelvy	sp.gr. $\frac{15}{4}$ (l.l.) 0.79360	0.79360

When we leave the first observation out of account, the mean of the others becomes 0,79359, which is in good harmony with our observation. Absolute alcohol is (besides water and the aqueous solutions of some salts and of cane sugar) perhaps the only liquid the specific gravity of which is known with an uncertainty of one or two units in the fifth decimal figure.

The agreement of the result of our measurement with that of OSBORNE and MAC KELVY proves a posteriori that our thermometer agrees down to one or a few 0,01° with that of Washington.

It is clear that VON BAUMHAUER's measurement is among the less accurate ones. The cause of this must be ascribed to his rather inaccurate way of determination of the specific gravity, which he has carried out by hydrostatic weighing with an immersion body of 53 ccm., in which method especially the adjustment of the temperature meets with great difficulties, and the attraction of moisture from the atmosphere is also difficult to avoid.

Besides many objections are to be made to the determinations of the specific gravity of mixtures of alcohol and water which have been carried out by VON BAUMHAUER, and which he executed with a small pearshaped "body" of only 13 ccm. capacity, which allows only a measurement of the sp. gr. down to 0,0001 even with the present better constructed chemical balances. In v. B.'s values, however, differences of as much as 0,0007 could be demonstrated between two measurements of mixtures of the same concentration (86,9 °/₀ vol.). And yet VON BAUMHAUER claims for his results an accuracy of 0,00001. It is certainly not exaggerated when in general we assume a possible error in VON BAUMHAUER's determinations of 0,0005, and consider only the third decimal of his sp. gr. as valid.

We have accurately determined the sp. gr. $\frac{15}{4}$ (l.l.) of a number of mixtures of absolute alcohol and water, and found the best agreement with OSBORNE and MAC KELVY's results (Washington). It appears from the subjoined table that this agreement is so much better than that with the results of VON BAUMHAUER, and even better than with those of MENDELEJEFF, that we do not hesitate to recommend the table of the Bureau of Standards (Washington) as the very best that is known at present.

In order to be able to compose tables for different temperatures from 10° to 30° we have also accurately determined the sp. gr. $\frac{t}{4}$ (l.l.) both at 25° and at 15° for the mixtures of the mentioned table and for some others, and calculated from this the modulus of expansion between these temperatures for different concentrations by graphical interpolation. In order to be able to compare our results with those of other investigators we have had to assume the modulus of expansion between 10° and 30° as independent of the temperature, which is the case for the mixtures with much water only in rough approximation.

% weight of alcohol	Our own determinations	According to Bureau of Standards	According to Mendelejeff	According to von Baumhauer[1]
8.176	0.98555	0.98557	0.98561	0.98547
10.695	0.98212 (2 ×)	0.91210	0.98215	0.98212
	0.98209			
10.931	0.98180	0.98180	0.98185	0.98182
14.130	0.97771 (3 ×)	0.97774	0.97782	0.97776
	0.97768			
19.282	0.97154 (3 ×)	0.97156	0.97160	0.97164
42.528	0.933715 (3 ×)	0.933705	0.933785	0.93399
51.877	0.913675 (2 ×)	0.91360	0.91370	0.91400
	0.913655 (2 ×)			
67.342	0.87813 (2 ×)	0.87815	0.87818	0.87844
85.971	0.83277 (4 ×)	0.83278	0.83278	0.83311

We then find as modulus of expansion (change of the spec. gr. per 1° temperature variation):

% (weight) alcoh. in the mixture	von Baumhauer from 15° to 25°	Mendelejeff from 10° to 30°	Bureau of Stand. Washington. from 15° to 25°	From our own observations 15° to 25°
0	0.00020	0.00018	0.00020.5	0.00020.4
10	25	25	26.1	25
20	42	42	42.9	42
30	60	60	61.1	61
40	71	73	73.4	73
50	78	74.5	79.1	79
60	81	82	82.4	82
70	83	83.7	84.7	84
80	83	86.0	86.1	84.4
90	84	87.5	86.5	84
100	84	84.5	85.4	83.4

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[1] The values of the three last colums have been calculated by interpolation

From these series of values appears in any case the peculiarity that the curve of the moduli of expansion of mixtures of alcohol and water exhibits a maximum at a concentration of about 80 °/₀, and a point of inflection at about 20 °/₀.

In conclusion we subjoin an extract of the table of the sp. gr. $\frac{15}{4}$ (l.l.) for °/₀ (vol.), as they have been given by VON BAUMHAUER in comparison with the better values, confirmed by us of the B. of S. at Washington, rounded off to the fourth decimal.

°/₀ vol.	According to VON BAUMHAUER	Accord. to the Bureau of Standards	°/₀ vol.	According to VON BAUMHAUER	Accord. to the Bureau of Standards
0	0.9991	0.9991	55	0.9239	0.9237
5	0.9920	0.9919	60	0.9131	0.9130
10	0.9857	0.9857	65	0.9017	0.9016
15	0.9802	0.9802	70	0.8897	0.8896
20	0.9751	0.9752	75	0.8770	0.8769
25	0.9700	0.9701	80	0.8636	0.8635
30	0.9646	0.9647	85	0.8498	0.8491
35	0.9585	0.9585	90	0.8338	0.8335
40	0.9512	0.9512	95	0.8159	0.8157
45	0.9427	0 9430	100	0.7941	0.7936
50	0.9338	0.9338			

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to the concentration of our mixtures; those of MENDELEJEFF have besides been recalculated with respect to the better spec. gr. of water and to the intern. hydrogen scale.

Geology. — *"On the Rate of Denudation in Java"*. By Dr. L. RUTTEN. (Communicated by Prof. Dr. C. E. A. WICHMANN.)

(Communicated in the meeting of November 24, 1917).

The present investigation is based on the tables of annual content of silt and of the annual estimates of the discharge of a number of rivers in Java, besides on occasional estimates of the solid material dissolved in their water.

It was the interesting thesis of Dr. L. G. DEN BERGER [1]) that first called my attention to the regular observations of some rivers in Java and Madura in point of their discharge, the amount of dissolved materials and their silt-content. On being asked for further information Mr. DEN BERGER kindly sent me the silt- and discharge-estimates for some typical rivers in Java, which valuable collection was afterwards supplemented by Mr. WEBER, chemist at the agro-geological Laboratorium of the Department for Agriculture. I also received valuable information from Dr. E. C. J. MOHR, Director of this Laboratory. Some modern literature was afforded me by Dr. B. G. ESCHER of Weltevreden. To these gentlemen I wish to express my warm thanks for their assistance.

With a view to difficulties of irrigation in the Seraju-system and in connection with newly projected irrigation-works Mr. MOHR had, in the year 1907, samples of water collected at regular intervals in several places in this system, which were examined at Buitenzorg. He had moreover estimates made of the discharges of these several rivers. The results of this investigation [2]), important from an agricultural point of view, prompted geologists to make similar observations also for other projected irrigation works, which were worked out later on especially by Mr. DEN BERGER and Mr. WEBER, and the practical use of which appears unequivocally from the thesis just alluded to. The values found were not only important for the agriculturist whom they taught what he brought on the fields to be irrigated, quantitatively and qualitatively, but also for the geologist,

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[1]) L. G. DEN BERGER. Landbouwscheikundige onderzoekingen omtrent de irrigatie op Java. Delft. 1915.

[2]) E. C. J. MOHR. Over het slibbezwaar van eenige rivieren in het Serajoedal enz. Mededeelingen, uitgegeven door het Dept. van Landbouw, n⁰. 5. Batavia 1908.

as they bear witness to the powerful denuding agents at work in demolishing the mountains in this tropical island and illustrate how far the rate of denudation depends on the geological condition. Since, to my knowledge, the Indian geological literature does not contain anything about these problems, I entered upon a study of the available data in December last; the results have been brought together in the accompanying table and will be discussed in detail below.

The available data were:

1. Silt-contents, denoting the amount of matter held in suspension in the river water, estimated twice a day during a whole year. A few times a month simultaneous estimates were made of the silt content of the surface water and the bottomwater.

2. Discharge-estimates, corresponding with the preceding data, indicating the amount of water, in cubic meters per second, carried past the point of observation.

3. Content of dissolved solid material in the riverwater, estimated mostly only once every two months or less often.

A combination of the data 1 and 2 shows the amount of silt per second, day, month and year carried past the place of observation; that of 2 and 3 shows the amount of dissolved solid material, transported from the riversystem per month and per annum, so that apart from the material transferred to the sea along the bottom, we get in this way a survey of all the materials carried away from the riversystem in one year. Most of the observations being made in the lower course, where no doubt the bulk of the transported material in suspension and in solution are discharged, we may assume that the values found in this way, cannot be far below the real amount of the transported matter.

The total amount of the materials carried past the place of observation — by weight — was reduced to cubic meters by dividing by 2.5 — which was taken for the average specific gravity of the transported stones — and then the average annual denudation was computed by apportioning the result over the system above the place of observation.

For some rivers the observations were not made for a whole year. In those cases we made interpolations.

In view of the available material it is not pretended that these results are strictly accurate. Observations only twice a day, interpolations for some months, observations of the dissolved materials only some times a year, the absence of data of the material transported along the riverbottom, they are all factors that bring down the accuracy of the figures. On the other hand the material need not be so accurate, since the essential factor is not the extent of denudation — perhaps not even when only approximated to some tens of per cents — but only the order of this number.

In a way a rough control of the data was rendered possible. The total quantity of water carried yearly past the place of observation must of necessity be considerably smaller than the total amount of rain descended upon the river system, which represents the product of the area of the system and the mean annual rainfall. This is easy to understand if we consider that one part of the supplies of rain is removed by evaporation, another sinks into the ground, while again another portion is absorbed by the vegetation in the system. In the following pages we shall examine how far the data could stand the test of our control.

At first I made minute calculations with all the available data, but before long I deemed a rough operation of endless multiplications and additions quite sufficient. It is out of place here to reproduce the very comprehensive tables, thus obtained; I hope to make known the results of further inquiries in this direction in a future communication. The most important data of the several systems have been worked out in the said table; I subjoin some special remarks about them.

From the Tjiliwong, which drains the northern slopes of the Gedeh-Pangrango massif, nearly all the water is carried off through a large irrigation channel, the East-Slokkan, near Katu Lampa, some kilometers above Buitenzorg. So, though not the whole volume of water — consequently not all the silt — that is carried past Katu Lampa, is taken into account, the difference will be only very small, as the total annual volume of water passing through the Slokkan (2,6 10^8 m^3) is not in an abnormal ratio to the total annual rainfall in the system above the place of observation (5.2 10^8 m^3).

The Tjiliwong derives nearly all its water from a volcanic country, mostly from young volcanic breccia, tuffs and agglomerates of the Gedeh Pangrango, for a smaller portion from the so-called old-andesite massif of the G. Kentjana, G. Paseban and others, for a very small portion from the so-called miocene breccia, east of Buitenzorg[1]). The rocks in the region of the "Old Andesites" are petrographically not distinguishable from those of the Gedeh-Pangrango massif; the part of the miocene breccia, drained by the Tjiliwong

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[1]) R. Verbeek and R. Fennema. Geologie van Java en Madoera. 1896.

	Average yearly fall of rain in mm. e e	Total annual fall of rain in river-basin above place of observation in m³. f f
± 0	± 4000	± 5.2 × 1
± 1	± 3000	± 6.8 × 1
±	000—3500	9 × 10⁹—10 ×
± 1	± 4000	± 8.4 × 1
± 0	± 5000	± 13.5 ×
±	± 2600	± 1.1 × 1
±	± 2600	± 2.6 × 1
±	± 2000	17.2 × 10
±	± 2500	25 × 10
	± 2000	4.5 × 10

	Dissolved matter in milligrams pro Liter												Highest quantity of mud in mgr. per Liter	Lowest quantity of mud in mgr. per Liter	Highest produce in m^3 per second	Lowest produce in m^3 per second	Highest transport in kg. per second	Lowest transport in kg. per second	Quotient q r	Highest monthly transport in tons	Lowest monthly transport in tons	Quotient t u	Highest daily transport in percents of yearly	Lowest daily transport in percents of annual transport	Transport of the 10 greatest spates in percents of annual transport	Annual transport of mud in tons	Annual transport of dissolved matter in tons	Total annual transport in tons and m^3	Extent of the river-basin above place of observation in □ km	Annual denudation in mm	Average yearly fall of rain in mm.	Total annual fall of rain in river-basin above place of observation in m^3.	Annual quantity of water in m^3, streaming along place of observation	Stones in riverbasis Observations.	Year
	January	February	March	April	May	June	July	August	September	October	November	December																							
	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	ij	z	aa	bb	cc	dd	ee	ff	gg	hh	
TJILIWONG	71.2			65.2	78.0	79.2	74.3	72.6					1638 21. II. a.m.	3 4. X. p.m.	22 22. III. a.m.	1.75 16. IX.	18.018 21. II. a.m.	0.0135 4. X. p.m.	± 0.13×10^4	3529 November	224 August	15	± 2.2	0.01	± 11.2	± 19000	± 18 000	± 37 000 T. ± 14 800 M^3.	± 130	± 0.1	± 4000	± 5.2×10^8.	± 2.6×10^8.	Andesites etc. Place of Observation: Katulampa.	1. XI. 1912 – 31. X. 1913
TJILAMAJA		121			134	134	161		176	186		141	8120 12. III. a.m.	47 28 VIII. p.m.	247.1 3 IV. a.m.	1.46 30. VIII.	996.4 22. XII. a.m.	0.0837 28 VIII p.m.	± 1.2×10^4	200 000 April	800 August	250	± 4.1	0.001	± 30	± 750 000	± 80 000	± 830 000 T. ± 332 000 M^3	± 225	± 1.4 (too high)	± 3000	± 6.8×10^8.	± 5.6×10^8	Half marls etc., half volcanic stones. Pl. of Obs.: Sungapan.	13. VI. 1915 – 12. VI. 1916.
TJIMANUK		131.6	118.8	125.0		149.0	165.0	187.0		117.0			9397 24. I. a.m.	15 30. VIII	696.17 5. III. p.m.	3.2 30. VIII.	1880 22. III.	0.048 30. VIII.	± 4×10^4	? 1 100 000 March	1040 August	1058	± 5.1	0.00016	± 36	> 2400 000 < 4000 000	> 300 000 < 450 000	> 2700 000 T. < 4450 000 T. > 1080 000 M^3. < 1780 000 M^3.	± 3000	0.36–0.59 (too low)	3000–3500	9×10^9–10×10^9	2×10^9 3×10^9	Nearly all Andesites etc. Pl. of Obs.: Rentang.	?
TADJUM			85.2			119.6	95.2	91.8			98.2	111.4	4639 26. XII. p.m.	4 23. IX p.m.	98.09 19. V. a.m.	2.159 X	182,3 19. V. a.m.	0.0098 23. IX. p.m.	± 1.8×10^4	26 000 March	110 September	236	± 9.3	0.0011	± 46.6	± 120 000	± 44 000	± 160 000 T. ± 64 000 M^3	± 210	± 0.31	± 4000	± 8.4×10^8	± 4.4×10^8.	Most Andesites etc., Some marls Pl. of Obs.: Tipar	21. II. 1914 20. II. 1915
SERAJU	115.6	108.0				128.4	140.8	140.2			135.4	118.6	7432 1. III	18 13. X. a.m.	2390.1 10. I. a.m.	73.1 14. X. a.m.	6244.0 28. II. a.m.	1.35 13. X. a.m.	± 0.46×10^4	2 530 000 March	9000 September	281	± 5.4	0.0015	± 29	± 9600 000	± 1130 000	± 10700 000 T. ± 4300 000 M^3.	± 2700	± 1.6	± 5000	± 13.5×10^9.	± 9.4×10^9	Nearly all formations of Java. Pl. of Obs.: Mandirantjan.	21. II. 1914 – 20. II. 1915.
PENGARON		247.6		205.4			245.4	253.6		241.6			42095 21. XI.	18 6. V.	92.6 29. IV.	0.4 VIII.	1656.87 29. IV.	0.0198 6. V.	± 8.3×10^4	148 500 December	320 July	451	± 18	0.00008	± 63	> 360 000 < 500 000	± 16 000	> 380 000 T. < 520 000 T. > 152 000 M^3. < 208 000 M^3	± 41	3.7–5	± 2600	± 1.1×10^8.	0.6 0.8×10^8	Most marls, some volcanic stones Pl. of Obs. Kali Kajen.	23. II. 1913 – 31. I. 1914
DJRAGUNG		303		244			317	342		303			18807 12. XII p.m.	10 22. VIII p.m.	111.3 26. I p.m.	0.04 26. IX.	1459.1 26. I. p.m.	0.00056 26. IX.	± 260×10^4	> 133 000 Jan.	40 August	3325	± 31	0.00001	± 75	< 350 000 > 45 000	± 27 000	± 400 000 T. ± 160 000 M^3	± 101	1.6 (too low)	± 2600	± 2.6×10^8	0.8–1×10^8.	Marls and volcanic stones. Pl. of Obs.: Sapen.	12. II. 1913 31. I. 1914
LUSI			198				411		620			264	15735 7. VIII. p.m.	8 6. VII a.m.	> 320 8. I.	0.1 Aug. Sept. sev. days	1391.2 28. XII. p.m.	0.003 6. VII. a.m.	± 48×10^4	? 750 000 Jan.	400 September	1875	± 5.5	0.00005	± 30	> 1700 000 < 2500 000	± 170 000	± 2250 000 T. ± 900 000 M^3.	± 860	1.05	± 2000	17.2×10^8.	6 10×10^8	Marls, limestones, quaternary Pl. of Obs. Kalang Losari.	?
BRANTAS		184	195			212	243	243					9830 10. IV. p.m.	41 22. IX a.m.	863.2 21. II.	31.7 3. XI.	4915.0 10. IV. p.m.	1.51 23. X.	± 0.33×10^4	1 390 000 April	7500 October	185	± 4	0.002	± 17	± 5800 000	± 130 000	7100 000 T. 2840 000 M^3	± 10 000	0.28 (too low)	± 2500	25×10^9	± 6.5×10^9.	Nearly all volcanic stones Pl. of Obs.: Modjokerto (Lengkong).	1914
BANJUPUTIH		469		652		578			539	572			24000 8. II p.m.	39 15. X. a.m.	?	5.0 sev. days	?	0.206	?	? 14 000 Jan.	1250 October	11	?	?	?	> 70 000 < 100 000	± 115 000	> 185 000 T. < 225 000 T. > 74 000 M^3. < 90 000 M^3.	± 225	0.3 0.4	± 2000	4.5×10^8.	> 1.9×10^8.	Volcanic stones Pl. of Obs. Lewung.	8. III. 1915 – 7. III. 1916

is the most recent portion of the folded, sedimentary Tertiary east of Buitenzorg and is composed entirely of andesitic sandstones and tuff-breccia.

The Tjilamaja, rising on the northern slopes of the Tangkuban Prahu massif, drains, according to Verbeek and Fennema's map 1) a region of recent volcanic rocks, of old-miocene breccia, miocene marls and quaternary. I have no personal knowledge of the system, but here I feel urged to point to an error on a map in the archives of the "Dutch Colonial Petroleum Company", on which a considerable portion of the so-called miocene breccia are indicated as marls, shales and sandstones, an error quite similar to those regarding the country east of Buitenzorg 2). This requires qualification, as it appears that in the Tjilamaja system marls and volcanic rocks in reality counterbalance each other, whereas according to Verbeek and Fennema's map the volcanic rocks (*ml* and *v*) by far exceed the marly rocks.

For this river the obtained denudation value is somewhat exaggerated as the volume of water (5,6 10^8 m^3) carried past the place of observation is too near the value of the total annual rainfall in the system above the place of observation (6,8 10^8 m^3). Anyhow in the system of the Tjimanuk young volcanic rocks prevail. In its northern career the river also drains a small tract of miocene marls, it not being excluded that in the large tract of "miocene breccia", southwest of the G. Tjerimai, there still occur marls in a rather considerable quantity.

With regard to the Tjimanuk the obtained annual denudation is most likely an understatement, as the volume of water (2—3 10^9 m^3) passing the place of observation is too small a portion of the total rainfall in the system (9—10 10^9 m^3). True, in this extensive system with so many sawahs the water evaporates on a large scale, but the obtained ratio seems to be too low an estimate of the remaining flow of rainwater.

In the Kali Tandjum system young volcanic rocks, miocene breccia and miocene marls occur above Tipar, the place of observation. On Verbeek and Fennema's map, however, the volcanic rocks (*ml* and *v*) far preponderate over the clayey marly rocks.

The Kali Seraju system is one of the most "mingled" systems of all Java, as it includes beside large tracts of marls, miocene breccia and recent volcanic rocks, also eocene and cretaceous rocks.

The Djragung and the Pengaron derive part of their water from

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1) See note 1 foregoing page.

2) L. Rutten. "Old Andesites" and "Brecciated Miocene" east of Buitenzorg. Proceed. of the Royal Acad. Vol. XX. Amsterdam 1918, p. 597—608.

the greatly plicated zone of miocene marls of Central Java, another part from the deposits of the Ungaran, overlying the marls. In the Kali Pengaron system marls largely predominate, which is not so much the case in the Djragung system.

Most likely the obtained denudation value is somewhat too low for the Djragung. The quantity of water, streaming past the place of observation (0,8—1, 10^8 m^3) is in a somewhat abnormal ratio to the rainfall in the system (2,6 10^8 m^3).

We do not encounter exclusively miocene limestones and quaternary in the Kali Lusi system, as indicated on VERBEEK and FENNEMA's map. According to maps in the Archives of the Dutch Colonial Petroleum Company, its southern affluents drain almost exclusively a region of soft marls and other allied rocks; its northern affluents, however, one where chiefly limestones abound. Quaternary occurs especially in the large Blora plain. I am not acquainted with this system. The extensive Brantas system is chiefly built up of volcanic rocks. Occasionally these are also miocene limestones, marls and breccias, but these are quantitatively of no importance.

The denudation estimate of the Brantas, obtained from the available data, is too low for two reasons. First of all in these rivers the silt-content of the bottom water is considerably higher (about 70 °/₀) than that of the surface water, whereas in all the other rivers examined the one or the other is richer in silt, without any assignable constant ratio. Now considering that the silt-estimates mostly concern the superficial water — specimens of bottomwater were taken only about six times per month — the denudation value obtained for the Brantas-system must be some tens of per cents below the truth. Secondly the quantity of water carried past the place of observation (6.5 10^9 m^3) is slight as compared with the total rainfall in the system (25 10^9 m^3), even though we make allowance for the enormous evaporation in riversystem abounding in sawahs and in complicated rivercourses.

The Banjuputih gathers its supplies solely from the young volcanic massif of the Idjen. The denudation value of this river will be too low on the one hand, because the quantity of rainwater (1,9 10^8 m^3) carried past the place of observation, is a little too small in relation to the rainfall in the riversystem (4,5 10^8 m^3); on the other hand it must be a little too high, as among the transported "dissolved materials" there are many sulphates and sulphuric acid, which belong to the products constantly evolved afresh in the water of the Kawah Idjen, so that they do not constitute a factor in the denudation proper.

When summarizing what has thus far been said, we can divide the ten rivers, for which data were available, into five groups.

1. Rivers containing exclusively, or nearly so, volcanic rocks: Tjiliwong, Brantas, Banjuputih.
2. Rivers with predominating volcanic rocks, side by side with marls etc. Tjiwaduk, Tadjum.
3. Rivers with about an equal quantity of volcanic and clayey-marly rocks; Tjilamoja.
4. Rivers with predominating marly rocks: Djragung-Pengaron.
5. Rivers with very "mingled" systems; Seragu, Lusi.

I regret that values for rivers with systems containing marly rocks only were wanting, considering that for two such rivers, the Kali Tjolo and the Lotjo, descending from the marly region south east of Wirosari (East Semarang) — we had the disposal of full silt-estimates. We feel indebted to Mr. Weber for his kindness to endeavour repeatedly to obtain the corresponding discharge-estimates, though he did not succeed.

It could be anticipated that the mean denudation values in Java would turn out to be relatively considerable. A high annual temperature, added to an abundant rainfall promotes a rapid weathering of the rocks; the rainfall yields large transporting watervolumes, which, owing to the marked land sculpture display great rapidity and force. In the east of Java a long drought with strong insolation accelerates the dislodging and crumbling of the rocks; the prevailing rocks (marls, concretionary shales and volcanic tuffs, breccias and agglomerates) are readily weathered and the layer of humus is — though not at all inconsiderable — much less compact than in other less cultivated tropical islands.

In tabulating the extent of denudation, we shall group the rivers into divisions according to the geological condition of their basins and shall give for every river the obtained value of annual denudation, adding in another column the figures between which, with much probability, the real values of annual denudation must lie; in doing so we have availed ourselves of the above speculations on the accuracy of the figures obtained.

I have not been in a position to consult original literature on dates of denudation in other countries, so that for comparisons I was restricted to data in older and newer handbooks.

In any case denudation in Java seems to proceed very rapidly. In calculating the duration of the American palaeozoicum C. D.

Walcott [1]) has assumed an average yearly denudation of 1 foot in 10000 years or about 0,03 mm. per annum, stating at the same time that the maximal annual denudation was then held to be 1.5 mm. (1 foot per 200 years). This maximum is even surpassed by the Seraju, Djragung and Pengaron; the average rate of denudation, admitted by geologists, is surpassed some times by the Tjiliwong, in whose system the clastation of rocks proceeds much more slowly than is the case with some other Java-rivers that have been examined.

Systems built up entirely or nearly so of volcanic rocks	Obtained Annual Denudation (mm.)	Actual Annual Denudation (mm.)
Tjiliwong	0.1	0.1—0.15
Brantas	0.28	0.35—0.6
Banjuputih	0.35	0.3—0.4
Systems with predominating volcanic rocks		
Tjimanuk	0.42	0.4—0.8
Tadjum	0.31	0.3—0.4
Systems with counterbalancing volcanic and clayey marly rocks		
Tjilamaja	1.4	1.0—1.4
Systems with predominating clayey marly rocks		
Djragung	1.6	1.6—2.5
Pengaron	4.3	3.7—5.0
"Mingled" systems		
Lusi	1.05	1.0—1.4
Seraju	1.6	1.4—1.8

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[1]) C. D. Walcott, Journ. of Geology. I. Chicago. 1893. Quoted from E. Dacqué. Grundlagen der Palaeogeographie. Jena 1915.

Grabau[1]) fixes the rate of denudation for the Mississippi system at 1 foot in 4640 year or 0,07 mm. per annum, for the Ganges at 1 foot in 1751—2628 years or about 0.15 mm. per annum.

In 1877 1177000 m.^{3} of matter was carried past Tetschen on the Elb. This gave a yearly denudation of about 0.03 mm.[2]).

These instances show that the rate of denudation in Java far surpasses that of the rivers studied thus far. This conclusion also holds for a comparison with other basins; the data for Java, however, deserve further consideration.

The above summary clearly shows that the rate of denudation depends largely on the geological condition, that it is smallest in volcanic regions and gradually increases, according as in a system the volcanic rocks are superseded by the miocene clays and marls. In the Pengaran system — where by no means only miocene marls occur — denudation progresses thirty times more rapidly than in the Tjiliwong system, composed entirely of volcanic rocks. Among the given factors, which may contribute theoretically to the high annual denudation the nature of the rocks appears to be of paramount importance.

The influence of the rainfall is evidently vere slight; the Tjiliwong system with an amount of rainfall of 4000 m.m. has a much smaller yearly denudation than the Lusi-system with only 2000 m.m.

Nor is the influence of the relief of the country as great as might a priori be supposed: the Tjiliwong and the Banjuputhi systems with their marked relief have a considerably lower annual denudation than the Lusi with a relief smaller than any other river examined.

The exceedingly great difference of the carrying power of the various rivers in the wet and in the dry monsoon is very striking. In this respect the Kali Djragung is most remarkable. In the month of January — in the middle of the wet season — of the year of observation it transported more than 13300 tons of suspended matter, in the month of August — in the middle of the dry season — only 40 tons.

J. **Walther**[3]) has stated that in desert regions the rare but catastrophic rains can act rather powerfully in the clastation of mountains. We are a priori inclined to assume for the humid tropics a more even distribution of denuding forces for the whole year, but

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[1]) W. Grabau. Principles of Stratigraphy. New-York. 1913. p. 247—248.

[2]) H. Credner. Lehrbuch der Geologie. IX. Auflage. 1902.

[3]) J. Walther. Das Gesetz der Wüstenbildung. 2 Auflage. 1912.

a study of the combined table tells us that also here the effect of catastrophic rains which occur rarely, may be very great.

If we wish to know how the denuding agents are working in certain regions we shall have to confine ourselves to small river-systems, as in the larger ones now in this, now in that tributary heavy rains will fall, whose local influence is of necessity distributed over the whole system and is consequently enfeebled. Of the rivers examined the Djragrung and the Pengaron have the smallest systems resp. 101 and 41 km². It now appears that the maximal silt-content, in these rivers, transported on one day, is resp. 31 °/₀ and 18 °/₀ of the annual transport, while in either rivulet the 10 largest spates carry off resp. 75°/₀ and 63°/₀ of the annual transport, a conclusive evidence of the catastrophic action of the heaviest rain-storms. More striking figures may perhaps be obtained from available observations of still smaller systems, judging from the well-known local nature of many violent tropical cloud-bursts.

Still it would not do to assume such a relatively important action of the heaviest rainstorms for all rivers in Java. This action will no doubt be most significant in Central and Eastern Java, where a great contrast prevails between the wet- and the dry-monsoon, it will be much less in Western Java with a more even rainfall so that the Tjiliwong and the Tjilamaja — rather small rivulets — carried along in their 10 heaviest bandjirs only 11.2 °/₀ and 30 °/₀ of the annual transport.

From what has now been said it will be seen that the annual denudation in the predominantly volcanic regions of Java is a number of the order of 0,5 mm., whereas in the regions of the sedimentary deposits the average annual denudation will amount to about 2 mm. These values give rise to the following speculations:

The folding of the neogene in Java occurred on the borderline between pliocene and quaternary, perhaps still in the latter. The Trinil bone-beds held by some to be most recent pliocene, by others old quaternary — have still partaken of the folding. This is hardly discernible in the environs of Trinil itself, but quite unmistakable in the environs of Modjokerto and Surabaya, where volcanic sands, agglomerates and tuffs, (petrographically quite similar to the Trinildeposits and including locally also bones of Stegodon) exhibit a distinct and even a rather great plication [1]).

R. D. M. VERBEEK [2]) has indeed endeavoured to demonstrate that

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[1]) L. RUTTEN. Verhandel. Mijnb. Geol. Genootschap. III, p. 149—151. 1916.

[2]) R. D. M. VERBEEK. Molukken-Verslag. Jaarb. van het Mijnwezen. 37. 1908 Wetensch. Gedeelte. p. 783 seq.

the bone-beds south of the G. Pandar overlie unconformably the recent tertiary marls — though he also admits a slight upheaval of these beds — but first of all the unconformability indicated by him in a profile is not at all in keeping with his own researches (inclination of the marls far too sharp); secondly, the supposed unconformability is based on the junction of two bonebeds that are wide apart from each other (Dungbrubus and G. Bulak), though only a minute study of details can tell us whether there is, or there is not, any fault between the finding places; anyhow their junction is not borne out by local observation.

The subsidence on the boundary between pliocene and quaternary gave rise to upheavals of the order of 1000—2000 m. in Eastern and Central Java. The mountain range thus engendered has been almost completely peneplaned in the quaternary, after which, on the thus formed peneplain, the most recent formations of some volcanoes i.a. of the Ungaron and the Merbabu have been deposited, as may be beautifully seen on the railway-line between Kedung Djati and Willem I [1]. After a, probably vertical, very young upheaval the present valleys penetrated far into the marl-zone through the overlying volcanic deposits.

The subsidence was, then, succeeded by a denudation of 1000—2000 Meters, which with an annual denudation of 2 mm. must have required a period of 500000--10000000 years, the length of the quaternary period in Java. This very rough estimate does not badly correspond to a recent estimate of the length of the quaternary period in Europe, fixed by PENCK in 1908 at 500000 years [2].

The average height of the volcanic massifs (without reckoning the peaks) in Java may be fixed at a number of the order of 1000 m. With a yearly denudation of 0.5 mm. this would point to total disappearance of the present volcanoes of Java in 2000000 years. If, in this connection, we bear in mind that it has generally been admitted that the Tertiary has lasted many times longer than the Quaternary, it is easy to conceive that "old-miocene" volcanoes in Java can hardly be expected to possess discernible crater-rims. Nevertheless VERBEEK and FENNEMA [3], have indicated discernible crater-rims for numerous volcano-massifs — which they pretend to be "old-

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[1] I must advance here that though I am personally acquainted with this part of Java, I should hardly have observed these features of the landscape on a short excursion, if the results of an inquiry by Dr. W. HOTZ, who made a geological map of this region in 1911, had not come to my knowledge.

[2] In E. DACQUÉ, l.c. p. p. 273.

[3] R. D. M. VERBEEK and R. FENNEMA l.c.

miocene". Now it is evident from the foregoing that in that case either the volcano-massifs cannot be old-miocene, or the crater-rims do not exist. This statement is of some moment also for regions in other islands. VERBEEK[1]) has asserted in his study of the antiquity of the eruptive rocks in Amboina that they cannot be tertiary, since they are entirely without volcanic shape — in contradistinction to old-miocene and perhaps even older volcanic massifs in Java. From what has been said in this paper it is evident that *this* argument for the antiquity of the Amboinites carries no weight.

Sindanglaja, July 27, 1917.

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[1]) R. D. M. VERBEEK, Over de geologie van Ambon. I and II. Verh. Kon. Akad. Wetensch. VI. 7. 1899, VII. 5. 1900.

Zoology. — "*The wing-markings of Arctiidae*". By Prof. J. F. VAN BEMMELEN.

(Communicated in the meeting of November 24, 1917).

As exposed in former publications, based on my investigations of Rhopalocera and Hepialids, the analysis of wing-markings leads to the assumption of an original pattern, common to all members of the group, and modified in various but not independent ways in the several families, genera, and species. At the same time these studies have induced me to propose a set of general rules about wing-design: among others the original identity in the markings of fore- and hindwing, upper- and underside, etc., these conclusions in many cases agreeing with those of my predecessors, notably with EIMER's.

Therefore I felt a little astonished when I found that DE MEYERE, in his recent publication: Zur Zeichnung des Insekten- im besonderen des Dipteren- und Lepidopterenflügels, though continually referring to my investigations, and paying them the honour of his critical remarks, only superficially mentions the above named hypothesis about a general primitive pattern, and also pays but slight regard to the necessity I insisted upon, of comparing in every case the two wing-pairs as to both their surfaces and of always asking where the original conditions have remained most plainly. In some passages DE MEYERE cursorily pays attention to differences between upper- and underside, fore- and hindwing, but in many others he does not even mention which part of the wing-design he has in view, nor does he seem inclined to deduce general rules from the rich treasure of his observations, except his assertion that: "also here (viz. amongst Lepidoptera) "though on a more restricted scale" (than amongst Diptera) "the various directions of the development of wing-markings play a part, the stress being laid by him on the word "various". Yet, according to my view, the detailed study of the underside of *all* Lepidopteran-wings, and its comparison with the upper surface, is all the more necessary, because especially in Heterocera the inferior wing-side has practically been disregarded, as is proved by the almost complete absence of figures, even in the most recent works. I therefore prepared for several years an attempt to supply this deficiency, and had come to the conclusion, that the above mentioned general supposition, as well as the rules about colour-design connected with it, may be applied to all groups of Lepidoptera; the undersides of Heterocera-wings showing a far greater fundamental similarity in design than could possibly be concluded from the far more altered condition of the upper surface, where the contrast between fore- and hindwing is usually very sharp.

Evidently the detailed comparison of all existing Lepidoptera-patterns must be claimed as a condition for such a general inference, but at the same time it may be asserted, that its probability increases with every new group, for which its applicability can be proved. On this account I consider the special inspection of small and sharply-limited families highly important, and think this should precede a general survey of the whole order, as given by DE MEYERE, in which necessarily each family can be examined only cursorily and superficially.

That I have chosen Arctiïdae this time, is to a certain point a matter of accidental predilection, Sphingidae or Noctuïdae being probably as suitable as a starting point, Geometridae even more so.

Partly my choice may be justified by the vivid colours and the seemingly fantastic markings which characterise many members of this family and by which probably also DE MEYERE has been moved to mention the Arctiïdae in the very beginning of his paragraph on Lepidoptera as an uncommonly clear and complete instance of the dispersal of spots, in connection with the system of longitudinal wingveins, and frequently to choose his examples from this family in the course of his treatise.

Arctia caja may serve as a fit starting point, especially so because on the superior wingsurface of this form the contrast between fore- and hindwing is particularly strong, in regard as well to the pattern as to the hues, displayed in its composition. The forewing shows an apparently bizarre marmoration in creamy white and darkbrown, the hind one a group of five black blotches, with a lustrous blue centre and a thin yellow outer circle, arranged on a background of crimson.

This same contrast in pattern and hues between fore- and hindwings may be retraced in several congeneric species, but with many modifications, which in my opinion are very instructive. Though I do not propose in this paper to consider the colours as such, I wish to remark, that red is often replaced by yellow, brown by black. The creamy white may rise to deep yellow, the blue lustre on the black markings of the hindwings may be absent, as also their yellow lining. Lastly the contrast between light and dark hues may almost or completely disappear, fore- and hindwing or one of them becoming selfcoloured.

The comparison of the upper side of the forewings in different specimens of *Arctia caja* already leads to the conviction, that the capricious winding of the white interspaces between the dark-brown areas may be deduced from a regular alternation of light and dark transversal bars. For along the anterior margin this regularity is unmistakably present in all specimens, as well as in the majority of the remaining Arctiïdae and in cognate families, seven dark blotches alternating with six light spaces, if we count from the wingtip to the root, the latter being frequently covered by a narrow white tegulum. Indicating the dark spaces with the cyphers 1 to 7, the light bars with the letters A to F, we observe that A, B, E and F communicate with a longitudinal white streak, which winds along the median part of the wingflat from the root till near the outer margin, and sends out four transverse branches to the back-margin. The question arises, whether these branches may be considered as the prolongations of a corresponding number out of the 5 anterior transverse bars, and if so, of which of them; I prefer, however, to leave this question unsolved for the present. The white bars C and D on the contrary are isolated spots; D reaching to the radial nerve, C advancing somewhat farther towards the middle, and so entering the discoidal cell.

The light bars are not all of the same width, but they generally are somewhat narrower than the intervening dark spaces; yet in different specimens the dimensions are highly variable.

This variability has been studied and statistically arranged for a very extensive material by K. SMOLIAN[1]). As however his investigations do not in the first place touch on the phylogeny of the colour-pattern, they need not be considered here further. I only wish to remark, that SMOLIAN also assumed seven dark and six light transverse bars on the forewings, and found them back on the hindwings. Of this set of six lightcoloured bars on the forewings a certain number, varying from nought to six, might reach the hindmargin. A look however at the fourteen schematic figures, by which SMOLIAN represents these seven cases with their various sub-forms, shows that in none of them except the very last one (all six light bands extending across the whole wing) the fourth light spot (counted from the wing-root), proceeds farther than the hind-limit of the

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[1]) Kurt SMOLIAN, Ueber die Variabilität des braunen Bärenspinners und die Beziehungen desselben zu den ihm nächstverwandten Arten, Jenaische Zeitschr. f. Nat. Wiss. Vol. L, 1913.

discoïdal space, this same fourteenth variation moreover not presenting a pure instance of real continuity of the bars from fore- to

Fig. 1. Arctia caja.
Schematic wingpattern after Smolian.

hindmargin, but only a case of partial self-colour, which has caused the almost complete disappearance of dark bars in the proximal wing-area with the exception of a single isolated little spot, this part of the wing therefore having changed almost entirely to an uninterrupted light field.

One might feel inclined to conclude from this observation that spot 4 (Smolian's *d*) differs in character from the rest of the six spots along the front-margin. I doubt however the soundness of this conclusion, on account of a comparison with allied species, in which this spot, though restricted in the majority of them to the area of the discoïdal cell, yet sometimes shows distinct connections with a light bar extending to the hind margin, e. g. *Arctia hebe* and *fasciata*. In a still stronger degree this is the case in *Pericallia picta* (Seitz, Grosschmetterlinge Vol. X, Taf. 24) and *Carminopyga lichenigera* and *proserpina* (ibid. Vol. II, T. 17, *g* and *h*), which show six dark and six light bars running from fore- to hindmargin in unvarying width. At the same time it may be observed that the six dark bars of *proserpina* correspond to the seven brown areas of *caja*, and that the condition in *lichenigera*, where seven light and seven dark bars occur (those near the outer margin being incomplete) leads to the supposition that in *proserpina* and *caja* the most proximal light bar, i. e. the one near the wing-root is absent.

Let us apply to the study of the pattern on the upper side of the forewings in *Arctia caja* the usual methods of investigation, scil.:

1. Comparison of the condition of the colour-markings in different specimens, especially in varieties and aberrations.
2. Comparison with the pattern of the underside.
3. ,, ,, ,, ,, on upper- and underside of the hindwing.
4. ,, ,, ,, ,, of other Arctiids.
5. ,, ,, ,, ,, of allied families.

These all of them lead to the same conclusion Smolian came to, scil: that the basis of this pattern are seven transverse dark bars, but at the same time clear indications are seen, that these bars owe their origin to the coalescence of spots, instead of having secondarily dissolved into series of spots, as was Smolian's view.

Equally convincing are the proofs that originally the patterns on fore- and hindwings, as well as on upper- and underside, were identical; the strong contrast in this regard existing between the two wingpairs, especially at the superior surface, therefore being a consequence of secondary modification. In the same way the study of the underside clearly proves, that although the original similarity, both in pattern and in hues, between fore- and hindwing, has better maintained itself than on the upper surface, yet its colour-markings have suffered a reduction, which in many cases has only left the discoïdal spot, sometimes accompanied by a few markings at the front- and outer margins. With respect to this existence of special wing-areas, in which the darker pigment accumulates by preference, and maintains itself to the very last, the Arctiïds behave in correspondence to general rules, which can be laid down for all Heterocera, may be even for all Lepidoptera and other winged Insect-classes.

According to my view the arguments, leading to the above named conclusion, can only be discussed with the aid of numerous coloured illustrations and detailed descriptions, but this seems to me rather unnecessary, it being sufficient to point to such forms as *Rhyparia purpurata*, in which the upper surface of the forewings has preserved the original rows of spots in almost complete and regular state, except the second from the inner side. (Fig. 2 and 3).

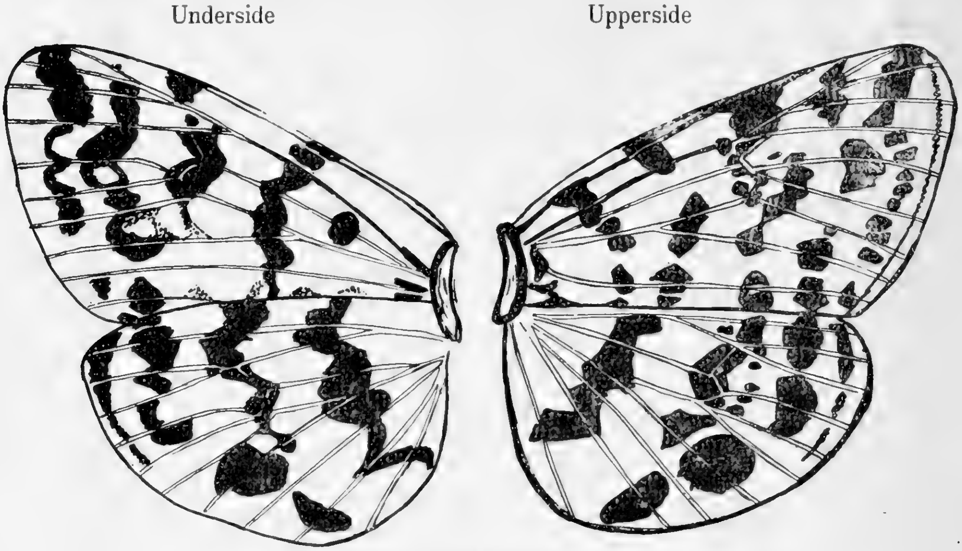
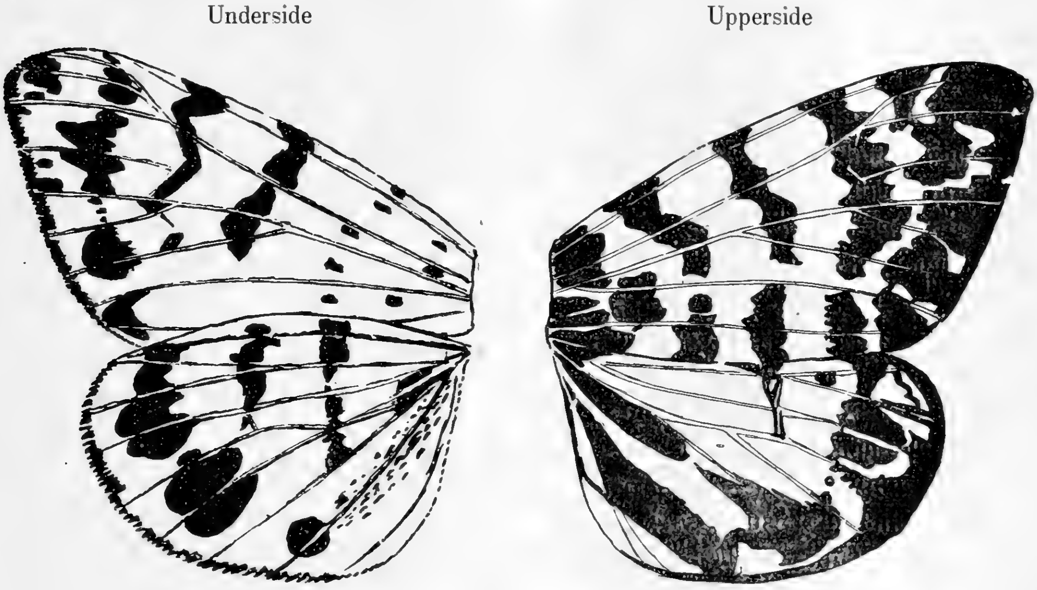


Fig. 2. Rhyparia purpurata.

Fig. 3. Rhyparia purpurata (upperside in black, underside hatched).

Arctia maculosa and *Ocnogyna corsicum* var. *sardoa* also present good instances of regular rows of spots. In a still higher degree this is the case in *Deiopeia* (*Utetheisa*) *pulchella*, and here indications are found, that the original number of the transverse bars may have been even greater, these indications neither wanting in *Rhyparia* itself. In *Deiopeia* for the rest the contrast between fore- and hindwing, and especially between upper- and underside, is still stronger than in *Rhyparia*, and the pattern of the under-surface shows fargoing modifications and discontinuities. Should my views, as explained in the foregoing, be right, then the upper side of e.g. *Coscinia cribrum* bears the traces of the seven series of spots in a more or less complete state, (especially in the variety *rippertii*), *Coscinia striata* on the contrary being more modified in this part of her colour-pattern, as in this form the rows of spots have coalesced more or less in a longitudinal direction and thus changed into coloured streaks filling the internervural spaces. Yet traces of two transverse rows of spots can still be clearly distinguished, represented by a submarginal row of more or less independent, dark, internervural streaks, and by the discoïdal spot. The latter remains in existence, even when the remaining design completely vanishes, as is also the case on the hindwings and the underside of both wingpairs.

In this way we are led without difficulty to the supposition, that the colour-pattern of Arctiidae should be deduced from an ancestral fundamental form, in which a light ground is divided into seven fields by a corresponding number of transverse rows of dark spots. These rows run uninterruptedly from fore- to hindmargin, on both sides of the fore- as well as of the hindwings.



Underside

Upperside

Fig. 4. Ocnogyna corsicum, var. sardoa.

The question however arises: is this ancestral form really the representative of the original condition of wing-marking, or has it in its turn arisen by modification from a still more primitive pattern,

Fig. 5. Utetheisa (Deiopeia) pulchella.

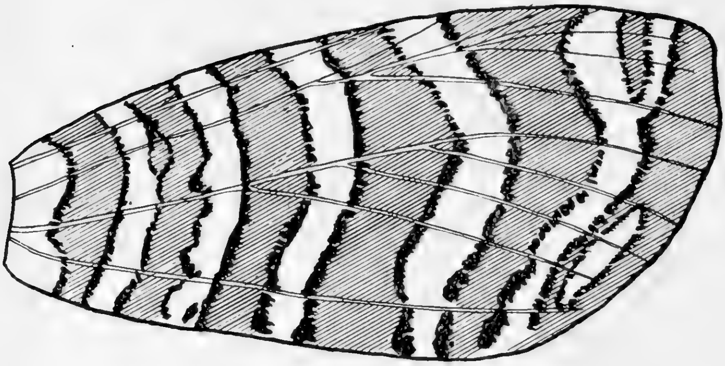


Fig. 6. Carcinopyga lichenigera.

in which the transverse rows of spots were still more numerous. The abovementioned traces of an originally higher number of spots in *Uthetheisa pulchella* already points in this direction, but the supposition is especially supported by a comparison with the *Hepialids,* in which the number of rows, composing the dumbbell- or hourglass-pattern, which I consider as the primitive design, is nearly twice as large.

Should my supposition prove right, then the colour design of Arctiidae (as well as of numerous other Heterocera and probably also Rhopalocera) should not be considered as a representative of the primitive Lepidopterous pattern, but on the contrary as the homologue of the secondary Hepialid design. The latter in its turn has issued from the primitive one by higher differentiation of alternative rows of spots, e.g. of the rows 2, 4, 6, 8, 10, 12, 14, if, for convenience sake, we infer for a moment, that the original number really amounted to fourteen.

Now coming to the question, if still more indications of this original design can be found in the family of Arctiidae, the answer, in my opinion may be this, that here and there traces of it can be discovered, though very incomplete and indistinct. *Spilardia* (*Diacrisia*) *multiguttata* (Seitz. Vol. X, Taf. 22), *Pericallia ricini* (ib. X 25*a*), *Alphaea fulvohirta* (ibid. X, T. 25) show a division into ten alternately light and dark areas along the front side of the median vein: in the first species the dark parts wear the character of spots, in the latter the light ones. The increasing of the number is the consequence of subdivision of three out of the seven spots along the front margin.

Moreover the question may be proposed, if the variegated pattern in black, white and red, as occurring on the front wings of *Utetheisa* (*Deiopeia*) *pulchella* and other species of this genus, might possibly be founded on an original repartition of the wing-field into a number of bars twice as large as that indicated by the rows of black markings.

According to the ingenious supposition of DE MEYERE, the red markings in this pattern represent the groundcolour, while the white spaces should be considered as light courts around the black spots, these courts having coalesced so as to form a white mazework surrounding the red patches. It cannot be denied that this inference finds a powerful support in the design on the forewings of another Arctiid, scil. *Argina cribraria,* where the black spots on the yellow ground are surrounded by light courts, which but rarely enter into connection with each other, and are totally absent on the underside, as they also sometimes are on the superior surface, and always on both sides of the hindwing, whose design is for the rest quite similar to that of the forewing.

But in *Utetheisa* we are struck by the fact that on the borderlines between red and white spots brown demarcation-lines occur, which contribute to their character of independent spots with a specific form. Moreover in a few cases red spots are found to be divided into halves by a similar brown median bar. This makes me doubt, whether DE MEYERE's conception of this and similar colour patterns gives a clear and complete insight into their real nature. For I think it risky to start from the supposition that the colour pattern of Lepidoptera-wings should be composed of a groundcolour, serving as a ground against which markings of another hue stand out. According to my view no fundamental contrast exists between ground colour and markings; they have a common origin and become modified in the same way, by similar influences.

This view can be backed by numerous arguments. First it is seen in several groups of moths and butterflies that the light and dark shades are substitute for each other, some species showing dark spots on a light ground, others near akin to them light spots on a dark one. Obviously the connection between these two cases is not such that the dark spots in the species of the first group have grown light in those of the second, while the light ground colour at the same time darkened, but quite the contrary, that the dark spots grew larger and entered into connection with each other, thereby forming a network in which the remnants of the light ground-colour remained as isolated spots. Compare e. g. *Rhyparia purpurata* with *Callimorpha dominula* and *Arctia villica.*

In the same way as rows of spots can coalesce and form bars, the dissolving of the original design may proceed further and lead to complete selfcolour. An intermediate stage in this process is formed by the coalescence of part of the markings so as to form a groundcolour, while the rest of the spots stand out against it.

Neither does there exist a fundamental difference between light and dark colouring matter: black spots in one species being represented in an allied one by such of an identical shape and place, but of a different hue.

Generally this change in the shade of spots does not occur simultaneously over the whole of their surface, but starts from their centre and spreads to their circumference, this giving rise to annular spots with a light centre and a dark ring. In the genera *Ecpantheria* and *Halesidota* all stages of this transformation may be found side by side in one and the same individual, and on comparing various specimens of the same species, it appears that the identical spot is entirely dark in one, annulated with white core in the other. Nor can arguments be detected for ascribing a different character to dark spots with a light ring, as in *Argina*, in comparison to light spots with a dark one. Consequently in my opinion, when considering the genesis of wing design, no plausible reason can be found to distinguish between spots of a more active behaviour in the transformation of the pattern, and a ground colour that plays a more passive rôle. In the Hepialids for instance the pattern is formed by the regular alternation of biconvex and biconcave spots, forming what I have called the OXO motive of wing design, these spots being equipotential in so far as they undergo similar modifications in colour, size and arrangement, under the influence of identical causes.

So I cannot agree with DE MEYERE's view, who considers the O-spots as the markings, the X-spots however as the areas of ground colour, spared out between them. To assign a special character to part of the design is a matter of view or impression. Now examining the wings of those Hepialids that show the highest regularity in their markings, (e.g. species of the genus *Charagia*), with an impartial eye, we find that not the lighter O-spots, but quite on the contrary the darker X-spots make the impression of being independent positive elements of the design, between which the first mentioned appear as areas of a ground colour. Especially strikingly this is shown by the male of *Ch. mirabilis*, where in contrast to the female, the X-spots have grown into larger and more complicated markings, which I have compared to perforated cotton-plugs. Yet it may be stated, that likewise in the female those X-spots which belong to the secondary pattern, possess a well-pronounced independent character, as their colour in comparison to those of the primary ones has increased in deepness, hue and lustre. Though in a lesser degree, the same may be said of the O-spots on either side of those modified secondary X-spots.

Furthermore we see in the variety *chrysomallon* of *Ch. ramsayi* the spots of the primary pattern all melted together into a smooth ground, on which those of the secondary one stand out with great distinctness, these latter thereby forming a new pattern that possesses a great deal of similarity to the so called primary pattern of Arctiids. This comparison therefore gives a certain amount of probability to the supposition that the background on which the Arctiid pattern stands out, owes its origin to the coalescence of a number of separate spots, this ground-colour-formation being nothing more than a special case of self-coloration.

My views seem to me to find a support in the conditions of the colourpattern in the family of *Hypsidae*, so nearly alien to the Arctiids that many Lepidopterologists consider it as a subfamily of this latter group. Species of the genus *Agape* belonging to this subfamily show a number of dark spots along the proximal part of the frontmargin of the forewings, at regular distances of each other. Supposing these spots to occur along the whole length of the margin, their number would exceed a dozen, so they might perhaps be considered as remnants of the primary pattern.

No less remarkable and instructive is the complete design, in the female of *Agape orbicularis*, where the wing-field of both fore- and hindwing is divided by the colour design into two absolutely different parts. In this instance the upper- and underside are almost though not wholy alike, the first is somewhat more differentiated, the latter here and there is made diffuse by partial melanism. Comparison with the male and with allied species gives the impression, that the proximal pattern, consisting of dark spots on a light ground, has been driven back in the direction of the wing-root by the distal one, which shows the wellknown type of the filling up of the internervural cells by dark pigment with a light median streak. (Also DE MEYERE uses the term supplanting to this case). For a comparison of the stages of this phenomenon I point to the series *remigera, subfascia, cariae, producta, septentrionalis* among others (Seitz. Vol. X, T. 27).

Ultimately the proximal pattern must totally give way to the distal one (*butleri, proxima, eugenia, fuscipennis, bhawaua, papuana*). In one single case however (*octrealis*) the proximal pattern extends over the greater part of the forewing, the distal one only leaving traces of its presence along the outer margin.

The hindwing shows a greater tendency to lose all traces or nearly so of the distal pattern, its colour design thereby being restricted to the usual dark spots, arranged in concentric transverse arches in different numbers, on a light back ground.

Groningen, November 1917.

Physiology. — *"On the effect of H_2O_2 on the peristalsis of the isolated intestine."* By Prof. HAMBURGER and E. BROUWER.

(Communicated in the meeting of October 27, 1917).

After one of us had demonstrated that oxygen as H_2O_2 — unlike free oxygen — stimulates the white blood corpuscles into a more active phagocytosis [1]), it seemed of importance to examine the way in which other vital processes are affected by these two forms of oxygen. For this purpose we have experimented on the isolated intestine.

These experiments were carried out with rabbits' intestines. After the animals had been killed by a blow in the neck or by opening the carotid, the belly was quickly opened, a jejunum convolution was taken out and pieces of it were suspended according to the method of MAGNUS, in a heated TYRODE-solution, through which oxygen was led. This results, as we know, in regular rhythmical intestinal movements if the gas beads pass in a slow continuous current through the fluid [2]).

First we investigated how a modification of the *oxygen current* affected the contractions. We found that even a considerable increase *had not the slightest effect upon the curve.*

Of course we do not wish to infer from this that oxygen is not of the utmost importance for the function of the intestine. The reverse was already discovered by MAGNUS in 1904: when the oxygen-current is stopped, the contractions cease after a state of excitement — "einer dyspnoïschen Erregung" — which was also noticed by HAMBURGER with regard to the phagocytes [3]).

After it had become evident that an increase of oxygen beyond a certain minimum took no effect we have added H_2O_2, and this always brought about *an increased action* of a shorter or longer duration. Mostly it consisted in the tonus being considerably heightened, whilst the contractions grew smaller (fig. 1); in other cases the tonus-increase was less great, but the oscillations of the separate contractions had become much greater. The frequency remained

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[1]) H. J. HAMBURGER, Internat. Zeitschr. f. physik. chem. Biologie 2, 255 (1915).

[2]) MAGNES. Pflüger's Archiv **102**, 125. (1904).

[3]) H. J. HAMBURGER, Internat. Zeitschr. f. physik. chem. Biologie **2**, 249 (1915).

about the same; now it was somewhat greater, now again somewhat less.

The minimum quantity of H_2O_2 affecting the intestine is very small. We observed for instance a distinct reaction of the organ at a concentration of 1 : 1300000. Many times, however, the concentration had to be higher, evidently because the sensitiveness is

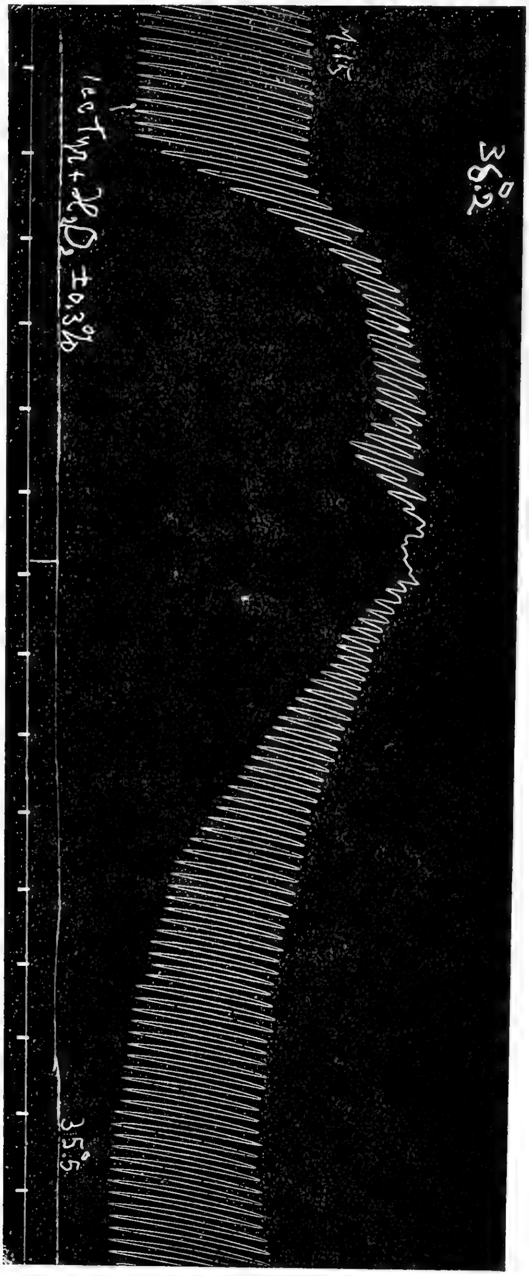

Fig. 1. Effect of H_2O_2 on the intestinal movements of the rabbit. Time in minutes.

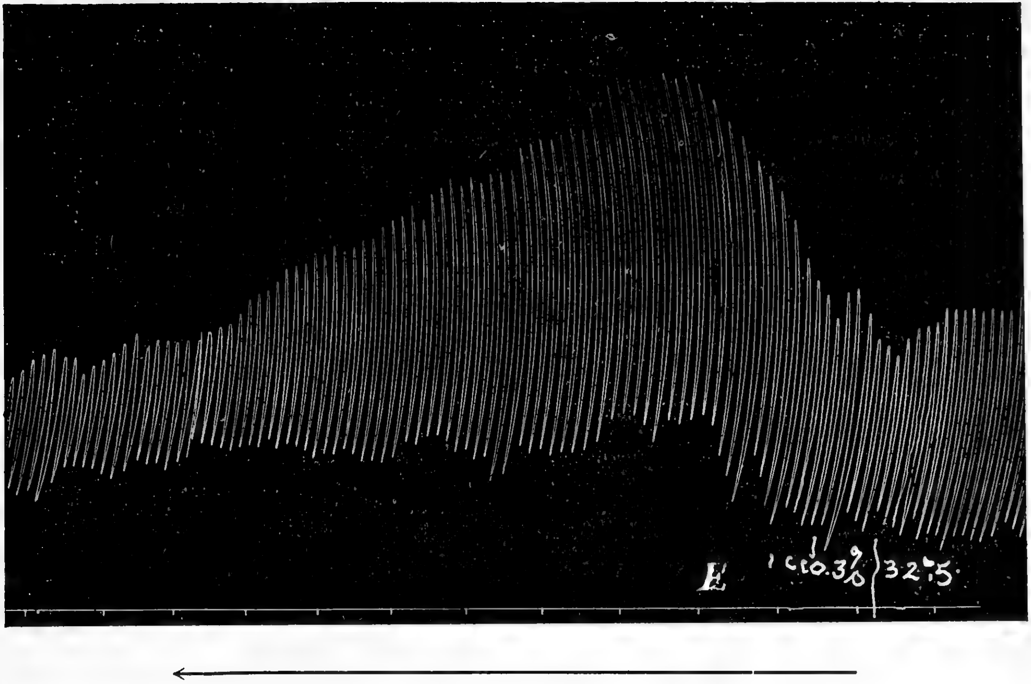

Fig. 2. Effect of H_2O_2 on the intestinal movements of the rabbit. Time in minutes.

different in different rabbits. The effect of H_2O_2 does not last. When it has passed off, an equal dose will no longer produce the same result, so that the dose which at first proved very effective, gives, when administered for the second or third time no visible changes in the curve. *Probably substances promoting the intestinal motion are decomposed by H_2O_2.* This becomes more apparent on larger doses being applied. The contractions then become smaller until the movements stop altogether. This lesion takes place when the concentration of H_2O_2 is about 1 : 15000. If the intestine is placed in a new TYRODE-solution, the contractions slowly come back. Hence we have to deal here with a *reversible process*, just as in the case of the phagocytes. If, however, the intestine has been damaged too much, the contractions do not come back.

VERWORN'S theory looks, as we know, upon narcosis as a kind of acute dyspnoea. It seemed of some interest to investigate if the oxygen of *peroxides* is prevented from acting when the intestine is narcotized. It then appeared that the contractions of a piece of intestine which was treated with $CHCl_3$, stopped at a concentration of about 1 : 1800. Now if H_2O_2 was added in a concentration of 1 : 15000 or less, the tonus was indeed raised for a time, whilst the contractions returned during this increased tonus. This tonus-increase was likewise of a short duration. Afterwards an addition of H_2O_2 had no longer any effect.

This suggested the question whether perhaps H_2O_2 is decomposed by $CHCl_3$. Therefore a parallel experiment was carried out. Two glasses with a solution of H_2O_2 1 : 17000 in TYRODE's fluid were heated to 37° and kept at this temperature during the experiment. To one of the glasses was added a $CHCl_3$-solution so that its concentration became 1 : 1500.

In the following table the quantity in cc of $KMnO_4 \frac{N\,11}{1000}$ has been given, necessary to neutralize 25 cc. of the solution.

Time	Tyrode-fluid with $CH\,Cl_3$	Tyrode-fluid without $CH\,Cl_3$
3.00	8 09 c.c. of $KMnO_4$ sol	8.23 c.c. of $KMnO_4$ sol
3.20′		7.54 c.c. „
3.30′	6.96 c.c. „	
4.15′	5.25 c.c. „	5.30 c.c. „

It becomes evident that the amount of H_2O_2 decreases equally in both glasses, so that most likely a decomposition of chloroform by H_2O_2 will be out of the question.

Some experiments were carried out on the effect of another oxydating substance viz. K_3FeCy_6. Just as in the case of the phagocytes no favourable result could be obtained. Not even in the presence of CO_2, which might lead us to expect a quicker penetration into the cells.

SUMMARY.

In very weak concentrations (sometimes 1 : 1300000) H_2O_2 acts upon the intestine. This action chiefly consists in an increased tonus during a shorter or longer time. This tonus-increase likewise sets in when H_2O_2 acts upon the narcotized intestine.

We intend to discuss on some future occasion the significance and the meaning of this H_2O_2-action.

October 1917.

Physiological Laboratory
in the University of Groningen.

Anatomy. — "*Complicated Mendelian segregation in the heredity of headform in man.*" By G. P. Frets. (Communicated by Prof. C. Winkler.)

(Communicated in the meeting of November 24, 1917).

In a first communication on this subject, it appeared that some factors working in the same direction, as it has been deduced by Nilsson-Ehle from his experiments for the heredity of characteristics of cerealia, and has been proved by further cultures, can likewise explain in different regard the motion of heredity in our material. The exceptions, however, that Nilsson-Ehle himself found, and that compel to accept still special hypotheses, the complications that are clearly shown by animal material also, as is clearly proved by the experiments of Davenport, and induce us e. g. to the introduction of the notion of potency of factors of heredity, and reversed predominance, compel us to examine if this is likewise the case for our material. This must, however, be preceded by the consideration, if perhaps the difficulties that our results contain for the purely Mendelian explanation, result from the suppositions that formed the basis of our investigation.

Hitherto, namely, we have investigated if the shape of the head, consequently the index, follows the rules of segregation of Mendel; it is however also possible that the shape of the head is a complicated property, and length and width mendel separately. We intend to examine this now: the difficulties for the explanation, e. g. the contradictions of table IV and table V might be explained by it.

If the length and the width mendel separately, the combination of the length of the father and the width of the mother, or the reverse, might cause a strongly deviating shape of the head. A perusal of the values of the head in table I of the cases that have been collected already in table II teaches that this simple possibility is in general not realized in the inheritance.

There is however more. The brachycephalic shape of the head can come into existence in two ways, namely by shortening and by widening. In the material treated here by far the greater part of the heads with a high index are short heads. The few wide

TABLE I. Head values belonging to series of indices of which a single one strongly deviates.

Family number	Head values	Father	Mother	Children								Table number
				Sons			Daughters					
				1	2	3	1	2	3	4	5	
I	L	19	19	19	18.7	18.5	18	17.3				I
	B	14.5	15.3	15.3	15	15	14.5	14.5				
	Ind	76.3	80.3	80.3	80.2	81.1	80.6	84.3				
III	L	18.8	18.2	19.4	18.5	17	17.2	17.8	17.7	17.2		I
	B	14.4	14.8	15	14.6	14.3	14.4	14.4	13.9	14		
	Ind	76.5	81.5	77.3	79.2	84.1	82.7	80.9	78.6	18.3		
XVII	L	19.8	18.1	19.3	19.7	19.6	18.6	17.3	17.5			I
	B	15.4	14.8	15.2	15.6	15.6	14.5	15.3	14.9			
	Ind	77.8	81.7	78.7	79	79.5	77.7	88.4	84.8			
CLXIV	L	18.8	18.5	19.7			18.7	18.6	17.8	18.2	16.6	I
	B	14.9	14.4	15.1			14.9	14.3	13.5	14.2	13.8	
	Ind	79.2	77.8	76.3			79.7	76.8	75.8	77.8	83.5	
CXCVI	L	19.8	18.3	19.4	16.7		17.5					I
	B	14.8	14.5	14.4	13.7		13.7					
	Ind	74.7	79.5	74.1	82		78.3					
XVI	L		18.5	19.5	20.8		17.9	19	18.8	18.3		II
	B		14.9	16.3	15.3		15	15.8	15.5	15		
	Ind		80.5	83.8	73.5		84	83.5	82.3	82		
CLIV	L						18.7	19.7	18.9	18.3	18.7	II
	B						14.6	14.8	15.1	15.1	15	
	Ind						77.8	75.1	79.6	82.5	79.2	
XXXVII	L	19.4	18.6	18.4	15.3		16.7					III
	B	15	14.3	14.4	12.6		13.3					
	Ind	77.3	76.8	77.9	82.3		79.6					
CCXV	L	19	18.2	18.4			17.4					III
	B	14.8	14.7	14.3			14.9					
	Ind	78.1	81	77.5			85.6					
CXV	L		18.1	18.4	16.6		17.5	18.3	17.3	17		IV
b	B		13.8	14.1	12.9		14.5	13.8	13.8	13.4		
	Ind		76.3	76.8	77.7		83	75.4	79.6	79		

heads are moreover as a rule large heads. We shall now try to discover if the twofold origin of brachycephaly can explain the differences in the tables IV and V. For this purpose besides the indices the two dimensions of the head are indicated in the the tables I to IV. If there are some factors of heredity for the length and some for the width of the head, and if the superior length, resp. the superior width is more or less dominant over the inferior length, resp. the inferior width, then in one case, namely when the brachycephalic head is at the same time large, the brachycephalic shape of the head can be dominant over the dolichocephalic shape; in the other case, when the brachycephalic head is little, it can be recessive. This depends consequently upon the composition of the factors of the length and the width, at all events in so far as this can exercise its influence.

If now we try to obtain a preliminary impression of the question whether length and width mendel separately, by examining the material, then it appears, that this is by no means always the case. A great length of the head of one of the parents will only very seldom be found with a very small width of the other, a little length seldom with a great width. This phenomenon, the correlation of properties, has very often been met with in the domain of the experimental doctrine of heredity and been more explicitly analysed for plants. Bateson and Punnett have given a mendelian explanation of it with the assistance of the hypothesis of coupling and repulsion of factors. With absolute coupling of factors the two properties can be represented by one factor of heredity, as they do not show a separate segregation. With relative coupling there is a more or less pronounced preference for certain combinations of factors. The coupling has hitherto been studied for two properties i. e. for the dihybridic crossing. As in this case four kinds of gametes are found, and it is admitted in usual circumstances that these in equal number, — i. e. as $1:1:1:1$ — are formed, we can represent with coupling the proportions of the number of gametes by the formula $n:1:1:n$ From the numeric proportions of the dihybridic scheme $9:3:3:1$ we can then calculate, how many individuals of each combination must be expected in the experiment. There is however still another coupling possible, i. e. such a one as can be represented by the formula $1:n:n:1$. It has been found in some cases, that, if a diheterosygotic individual, consequently $Aa\ Bb$, has taken existence from a crossing of $AABB$ and $aabb$, a coupling according to the formula $n:1:1:n$ can occur, and if the individuals $AAbb$ and $aaBB$ have taken part in the crossing, the formula $1:n:n:1$ may hold for the coupling of the factors. In both cases one obtains then from pairing of exteriorly the same individuals (*Aa Bb*'s among each other) different groups of descendants. Baur found with races of Antirrhinum in the first case coupling, in the second case common segregation.

It is quite possible, that in the motion of heredity of the shape of the head, coupling of factors for the length and the width occurs. This means consequently, that in general the shape of the head can be conceived as one single series of mendelian properties (because the factors for the length and the width have coupled) but that in some cases deviations will be found, which are the consequence of the meeting of gametes of the very rare not coupled factors of the length and the width. We shall take account of this consideration for the further examination of the tables.

Table I and II contain examples of families a single member of whom possesses a strongly deviating headform. This deviating headform is nearly always brachycephalic (the single time (fam. XVI), that the deviation is dolichocephalic, only one of the parents is known; the great lengths of the heads of the other members of the family render it very likely that the unknown father has also had a large head) and this brachycephaly always relates to a short head; in one case (CLXIV) this is not so; yet there the width of the head neither surpasses that of the other members of the family, and the length (18.3) is considerably less than that of the others (18.7—19.7). More exact than to say that in the families of table II one single brachycephalic headform appears, which points to recessivity of brachycephaly, it is to distinguish hereby, that in these cases (table I), the brachycephaly is restricted to a very short head, and that the shorter head is recessive with regard to the large one.

At the occurrence of separate deviations one must also think of accidental causes. One may have to do with a not-inheritable variation, consequently a modification. Exterior circumstances, illness or trauma, can likewise be the cause of a deviation. With an entirely unexpected result one will have to think of the great experimental mistake by the analogon of what is called in plants vicinism. For the persons of table I the first cause cannot directly be traced, as they are still young or not further known. With regard to the second cause of family 164 d_8 with the deviating index 83.5 and short, small head, we remark, that it is a backward girl, who suffered much from illness in the first years of her life, and has been nursed during a long time in the children's hospital on account of weak condition of the osseous system. Only very late the child learned to walk. Of table I d_2 fam. 90 is a girl of 4 or 5 years, having a somewhat deformed head on account of difficult partus.

Let us now examine moreover, if the distinction with the brachycephalic shape of the head can solve the contradiction of the data of the tables IV and V. For this purpose we must consequently consider the tables II to IV. It appears then, that in the families of table II, where thus one of the parents and most of the children have a high index, the parent with the high index has always a large head. Only case 87*e* (in an inferior degree case 179*b*) deviates from this rule. In fam. 87*e*, the father whose mother's sister has a short and small head (*L* 17.8, *W* 15.2, *Ind.* 85.3) renders perhaps possible that among the children there are three short heads, (compare below the explanation for family 3e).

The tables III and IV contain the families of table V. In the cases of table III the parent who has a high index, has always a short, small head, none or only a single one of the individuals has here a high index. Fam. IIIe is likewise interesting. Both parents are brachycephalic and have a short head. According to what we stated before, there must consequently be many small and short heads among the children. Of the twelve children the lengths of the heads of the two eldest sons surpass those of the parents; five childeren have very high indices and short heads, four others are small brachycephalic, only three are mesocephalic, two of them have a small narrow head (here we must take into account, that most of the children are still young). These data confirm thus tolerably well, that the short, small head is recessive, if it is admitted in this respect, that there are some factors for the length and some for the width, that the factors for greater length, resp. greater width are more or less dominant over those for inferior length and inferior width, whilst there is coupling between the factors for length and width. Of the Fam. 113 both parents have short heads and high indices, both children are brachycephalic (84 and 89 and shortheaded).

Of the families of table IV mentioned likewise on table V the brachycephaly of one of the parents relates to a rather large and broad head. In the first case, family XI, the dolichocephaly of the mother is however connected with a small, narrow head. In the cases of table II the dolichocephalic parent has always a large head. The little dimensions of the head, especially the little width can in my opinion explain the comparatively low values of the indices of the children. This holds perhaps likewise for fam. 147 and fam. 130. Here the father is likewise brachycephalic and has a large and wide head, whilst the mother is mesocephalic and has a small head. A beautiful case is fam. 147 with 9 sons and 2 daughters. This case can likewise be conceived as an example of the type of heredity of Nilsson-Ehle with greater or less dominance of the great lengths and breadths with coupling. The index of one son surpasses that of the father, one agrees with it, one is a little lower; one daughter has an intermediary index whilst of the other children the indices differ little from that of the mother, and two surpass it somewhat.

The families 179*c*, 185 and 32*f* show all a slight degree of predominance; one might class them with table II, 179*c* and 195 likewise with table VII.

Especially the families 68 and 76 offer great difficulties for the explanation. The fathers have in these cases rather low indices and large heads, the mothers high indices and rather large and broad heads. In the first case all four children have the same or lower indices than the father, in the second case two of the three children have somewhat lower indices than the father, and one has an intermediary index. These two families withdraw themselves from the explanation tried here (comp. p. 874).

The family 147 of table IV mentioned before shows a phenomenon, that is likewise expressed more or less in other families. We have here to do with parents with diverging indices (85 and 79); of the eleven children three sons have indices nearly equal to that of the father, whilst of the eight other children the indices correspond with that of the mother (78—81.8). Apparently there is here analogy with the mutation-crossings of de Vries [1]) segregation in F_1 and simple repetition of the properties of the parents. As this crossing can also be explained by combination of factors, as we indicated above, and the formula $DR \times RR = DR + RR$ can be applied, in which consequently in this case the father is conceived as DR and the mother as RR, and a similar result, that is a Mendelian one (Pearl) has been obtained in more complicated cases, we shall as yet conceive this crossing as not clashing with the rules of segregation of Mendel.

Still something more is to be said about the heredity of the short, small head. In family 17 (table I) suddenly two short-headed ones appear among the children; the mother's sister is likewise short-headed (L 17.4 B 14.8 Ind. 84.7). Of several families of table II, where one of the parents has a large and broad head, several

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[1]) Hugo de Vries. Die Mutationstheorie II and Gruppenweise Artbildung 1913. S. 109.

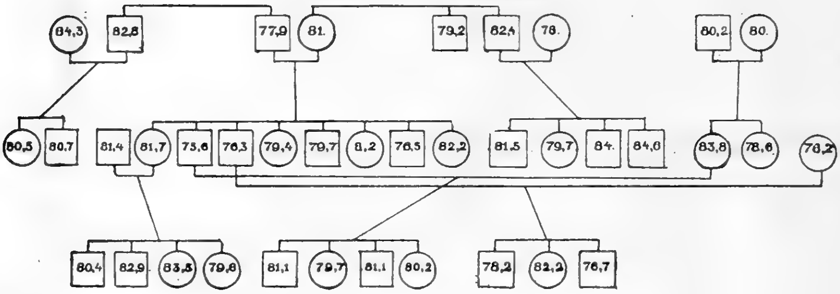
children have likewise a short and small head. So of fam. 3*b* three children are short-headed, of fam. 73, of fam. 234 and of fam. 87*c* likewise; of fam. 16*c* four children are short-headed. When perusing the tables, which will be published later, we are struck by the same fact. This occurrence in groups of the short, small head points to its independent signification for heredity. There must be a special reason for its appearance: we have to do with a new segregation. As in the different breeds of mice of Cuénot, Durham and others the crossing of the gray mouse with the albino-mouse gives besides gray and albino mice likewise black ones, so here also at the crossing of two shapes of heads a third type appears among the children. This phenomenon of the concealed existence of a factor, revealing itself only at a special crossing is called by Bateson epistasy. We can admit that for the formation of the short, small head the meeting of two factors is necessary; if the large and wide head possesses one of these factors, then by crossing this factor can combine itself with the other factor for the small head and produce this headform. The crossing 124 is likewise interesting for this conception, the mother has a short and small head and a high index (86.9), the father is brachycephalic (82.2) has a not large head, all six children of this family have short and small heads with high indices (81, 85, 86.5, 87.5, 88, 86.5). This case is anologous with the crossing albino × black = all black.

I am not of opinion that my material can already prove the conception given here, but it is already of importance that we can bring back a conspicuous phenomenon to a thoroughly investigated form of segregation.

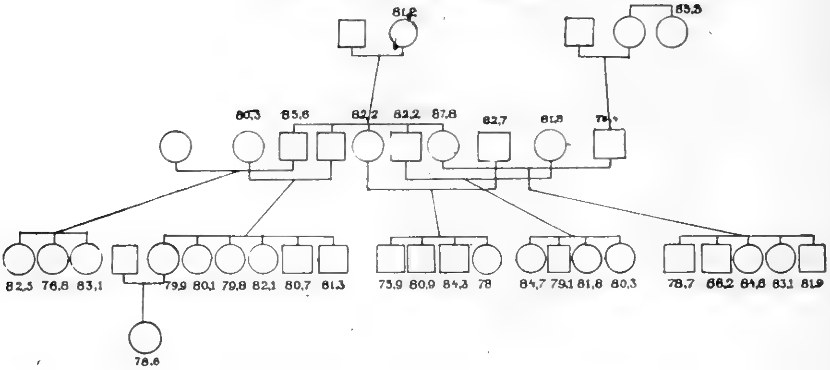
The sex exercises certainly likewise its influence on the shape of the head. This appears already from the curves (Fig. 2, 3 and 6. The complete material indicates also distinctly that the heads of women are smaller than those of men. When perusing the tables we are struck by the fact how seldom the large head of the mother returns among the children. Consequently special conditions must be satisfied too, if the large headform is found in woman; there is a preference for the small, short head in woman. I remind here of the investigations of Pearl, Morgan and others into heredity restricted to sex (correlation of sex), but I defer the examination of the material with regard to this phenomenon to a future time, when I can dispose of a more extensive material.

When testing our material to the Mendelian experience we must take into consideration some more complications. So we know an intensifying and a weakening factor, likewise a transformation-factor.

Fam. 22 (38 members).



Fam. 137 (33 members).



Fam. 173 (26 members).

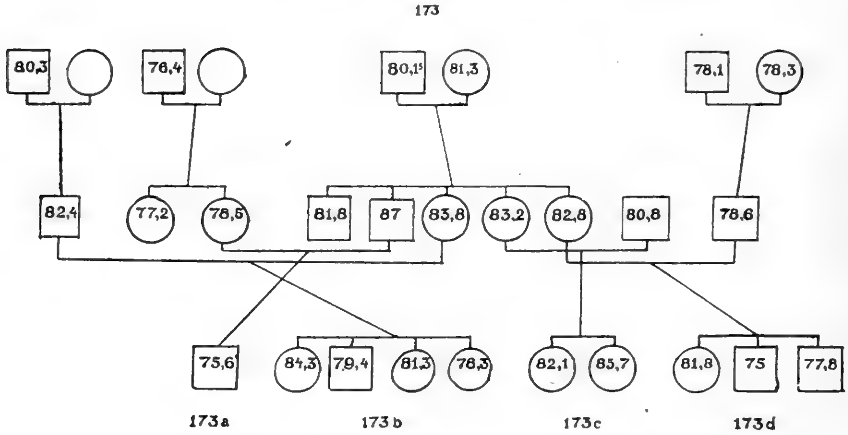


TABLE II. Headvalues of families, of whom one of the parents and most of the children have a high index.

Family number	Head values	Father	Mother	Children									Table number
				Sons			Daughters						
				1	2	3	1	2	3	4	5	6	
III *b*	L	18.7	18.9				18.2	17.3	18.7	17.7$^{7)}$	18.3$^{16)}$	17.1$^{4)}$	
	B	15.4	14.8				15.1	15.1	15.7	15	15.2	14.7	
	Ind	82.4	78.3				83	87.2	83.9	84.7	83	86.1	
LXXIII	L	19.2	18				17.7	17.6	17.7				
	B	14.9	15.4				15	14.8	15.4				
	Ind	77.3	85.3				84.7$^{7)}$	84.1	87.3				
LXXXV	L	19.7	18.3	18.3$^{14)}$	18.8$^{16)}$	17$^{18)}$	18.2	17.8$^{5)}$	18.1$^{2)}$	16.1$^{17)}$			I
	B	14.8	15.5	15.3	14.9	13.5	14.8	14.2	14.3	12.9			
	Ind	75	84.7	83.6	78.9	79.4	81.3	79.2	79	80			
CLXVII	L	20.8	18.3	20.1	18.4		18.6	17.9					I
	B	15.9	16.3	17.2	15.8		15.5	14.2					
	Ind	76.4	89.2	85.8	86.1		83	79.3					
CXXXIV	L	19.2	18.3	18.4			17.5	17.3$^{14)}$	17.2$^{15)}$				I
	B	15.8	16	15.9			14.8	15	15.3				
	Ind	82.2	87.3	86.3			84.5	86.7	87.9				
XVI *c*	L	19.8	19	17.8	16.7		19.3	17.8	19.1	18.7	17		I
	B	15.4	15.8	15.3	14.1		15.8	15	15.7	15	14.8		
	Ind	77.8	83.5	85.9	84.3		81.6	84.2	82.2	80.2	87		
LXXXVII *c*	L	20	17.3	19.5	17.4$^{7)}$	18.5$^{13)}$	16.8$^{7)}$	17.2$^{16)}$					I
	B	15.1	15.2	15.3	15	15.2	14.2	14.3					
	Ind	75.4	87.8	78.7	86.2	81.9	84.6	83.1					
CLXXIX *f*	L	19.7	17.8	18.0$^{10)}$			17.3$^{16)}$	16.7	16.4$^{9)}$	15.3			I
		15.2	15.2	14.5			14.9	13.9	13.3	12.7			
	Ind	77.2	85.2	80.6			85.8	83.2	80.8	83			
LXXXVIII	L	19.8	18.5	18.6			19.1	16.9					III
	B	15.5	15.4	15.5			14.6	14.2					
	Ind	78.3	83.2	83.3			76.4	84					
XXII *e*	L	20.1	18	18.5	18		18.2	16.4					V
	B	15.2	15.2	15	14.6		14.5	13.2					
	Ind	75.6	83.8	81.1	81.1		79.7	80.2					
LXVI	L	18.2	18.3	15.7	14.6		18	17.5	17.9	16.4	17.1		III
	B	16.1	14.8	14.4	13.2		16.2	15.2	15.4	15.1	14.7		
	Ind	88.5	81.1	91.4	89.5		90	86.9	86	92	85.8		

ABLE III. Head values of families of whom one of the parents has a high index, whilst only a single one of the children has a high index.

Family number	Head values	Father	Mother	Children													Table number
				Sons						Daughters							
				1	2	3	4	5	6	1	2	3	4	5	6	7	
XIX	L	19	17 8	19.2	18.7					18.1	17.3	17.8	18				I
	B	14 9	15	14.9	14.8					14.4	14.1	13.8	14.2				
	Ind	78.5	84.2	77.6	79.4					79.8	80.5	77.5	78.5				
XVII	L	19.3	17.4	19.8	19.5	19.7				18.8	17.6						I
a	B	15	14.8	15	14.8	15				14.2	13.9						
	Ind	76.7	84.7	76.6	76	76.2				75.5	78.8						
VII	L		17.4	20	18.9					18.8							II
	B		14.6	14.9	15.1					14.4							
	Ind		84.2	74.5	79.9					76.7							
CLXXXII	L	19.3	17.5	19.3	19					18.4	18.5			zu	17.6		I
d	B	15	14.9	15.7	15					14.7	15.1			van	14.9		
	Ind	77.5	85.2	81.3	78.9					79.8	81.3			m	84.5		
XCVI	L		17.5	18.1						18.6							II
	B		14.8	14.3						14.5							
	Ind		84.6	79						77.7							
III	L	17.8	17.7	18.8	18.3	17	17.6	16.2	16.8	16 *)	17.3	17.3	17	16	16.6		III
e	B	14.7	15.2	14.9	14.8	14.7	13.8	13.8	13.1	12.8	14.1	14	14.3	14.2	14		
	Ind	83	86	79.1	81	86.1	78.5	85.2	78.2	80	81.8	80.8	84.5	86.5	84.2		
XVI	L	19.7	17.9	19.2	18.7	18.3	18.2	18.2		18.3	18.2	18.1	18.2	17.3	17.4	17	III
b	B	15.5	15	15	15.1	14.5	15.2	14.7		14.7	14.5	14.4	14.5	14.2	14.2	14.2	
	Ind	78.7	84	77.9	80.7	79.7	83.4	80.7		80	79.7	79.5	79.7	81.8	81.6	83.2	
XXXIV	L	20.7	18	17.4	13.6					17.9	16.8						III
c	B	15.6	14.9	13.1	11.6					13.7	12.8						
	Ind	75.3	83	75.5	85.2					76.8	76.3						
XII	L	19.6	17.9	18.9						17.7	18	17.8	17.9				
	B	15.6	14.7	15.3						14.4	14.3	13.9	14.1				
	Ind	79.6	82.1	80.8						81.4	79.2	78.1	78.4				

*) Son, consequently 7 sons and 5 daughters.

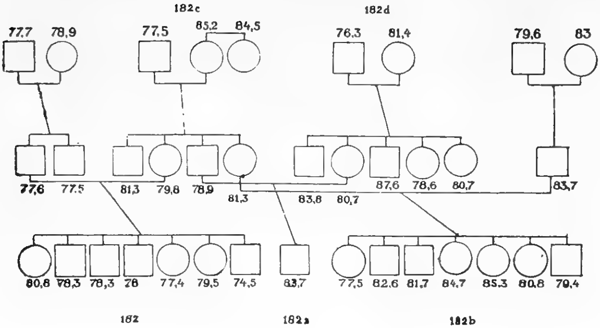
ABLE IV. Head values of families of whom one of the parents has a high index, whilst only a single one of the children has a high index.

Family number	Head values	Father	Mother	Children											Table number
				Sons						Daughters					
				1	2	3	4	5	6	1	2	3	4	5	
XI	L	18.8	18.4	19.3						17.3	18.5	18	18.3		I
	B	15.7	13.7	14.6						14.2	14.9	14.5	14.5		
	Ind	83.4	75	76						81.8	80	80.6	79.4		
CXXX	L	19.9	18.3	20											I
	B	16.9	14.4	15.1											
	Ind	84.9	78.4	75.5											
CXLVII	L	18.8	18.1	19.4	18.8	18.7	17.7	19.3	18.5	18.8 1)	18.2 1)	18.1 1)	18.9	17.9	I
	B	16	14.3	15.4	15.1	15.8	15.1	15.6	14.5	14.7	15.8	14.7	15.2	14.6	
	Ind	85	79	79.3	80.2	84.4	85.2	80.8	78.5	78	86.8	81.2	80.2	81.8	
CLXXIX	L	20.2	18	18.8	19.7					18.2	17.3	18.2			I
c	B	16.1	14.8	15.2	15.9					14.7	14.2	14.1			
	Ind	79.7	82.2	80.8	80.7					80.7	81.8	77.5			
CXCV	L	19.2	18.2	19.5						18.1	17.8	17.9			I
	B	15.2	15.2	15.3						14.8	14.7	14.6			
	Ind	79.2	83.3	78.8						81.5	82.6	81.8			
XXXII	L	19.3	18.3	19.5						18.6	18.3	17.6			
f	B	15.3	15.4	15.3						15.2	14.8	14.6			
	Ind	79.2	84.1	78.7						81.7	80.8	85			
LXVIII	L	19.8	18.3	19.7	19.8	19.3				15.7					I
	B	15.8	15.3	15.5	15.4	15.4				12.1					
	Ind	79.8	83.6	78.7	77.8	79.8				77					
XXXVI	L	19.7	18.2	19.7						20.2 2)	18.8 2)				I
	B	15.5	15.6	15.2						16.5	14.5				
	Ind	78.7	85.9	77.4						81.3	77.1				

1) Sons consequently 9 sons and 2 daughters.

2) Two daughters of the mother's first marriage.

Fam. 182 (34 members).



In his experiments Nilsson Ehle has admitted when the proportion of the numbers in different generations made it impossible to admit different factors working in the same sense, that at the meeting of factors that were being examined, with others to which the examination did not extend, the former were differently influenced by the latter. For our material this means e. g. that the shape of the head has not (need not have) the same signification for the heredity in different races. Dolichocephaly e. g. occurring together with blue eyes and great length of the body could bear another proportion than dolichocephaly and e. g. brown eyes. It is very possible that this influence exists. Bean has drawn the attention to this fact in his examination of hair-types [1]). The conception of potency of factors of heredity (Davenport, Goldschmidt) is not in keeping with the spirit of Mendelism, is much more in keeping with the earlier conceptions of heredity, when people spoke of power of heredity. If we peruse the families of the tables we meet, it is true, a single one with whom we see a strong predominance of brachycephaly, resp. dolichocephaly. Examples are the fam. 3 and 34 (vide the genealogical trees pag. 448). We have explained their motion of heredity on Mendelian assumptions (p. 447) by admitting favourable combinations of factors. This is especially possible for fam. 34. For fam. 3 it is certainly striking how there, especially in the fam. 3c brachycephaly dominates among the grand-children (values of 85—95). Instead of

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[1]) R. B. Bean, Heredity of hairform among the Filipinos. American Naturalist, Vol. 45, p. 528.

greater potency of the factors of heredity we might admit here also a separate factor of intensity.

As summary and conclusion we premise now that in our material the great line of Mendelism is to be recognized. There are distinct indications of segregation and of independency of factors of heredity. With regard to the nature and the number of the factors of heredity the phenomena of segregation are too much complicated for us to be able to give a sufficient explanation by admitting one pair of factors of heredity. (brachycephaly—dolichocephaly). One pair of factors of heredity for the length, and one pair for the width, that segregate independently of each other are insufficient for the explanation. We have therefore tested the data by the scheme of heredity of NILSSON-EHLE, in which some factors working in an equal sense are admitted. Here there are also two possibilities viz. some factors for the shape of the head, or some factors for the length and for the width of the head. We have admitted the latter possibility with the addition, that the factors for length and width are coupled; there is thus a preference for special combinations of factors. There is likewise some dominance of the large dimensions over the small ones. The short, small head shows distinctly recessivity, is moreover hypostatic with regard to the large, wide head.

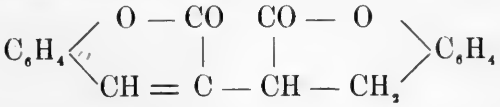
At the occurrence of different proportions in different cases of the same indices, also when they relate to the same dimensions, we must always consider, that a special value is the expression of modification (not inheritable variation), of exterior influences (illness and trauma), of heterozygoty, of combination of factors and of complications of segregation. The different behaviour of heredity of the same indices is thus in favour of segregation.

The results of this investigation are less a final conclusion than a guide for further investigations by which this material will be augmented and completed.

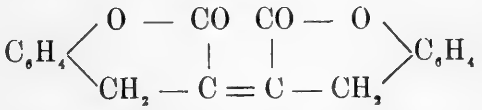
Chemistry. — "*Action of Solar Light on Cumarin*". By A. W. K. de Jong. (Communicated by Prof. P. van Romburgh).

(Communicated in the meeting of Nov. 24, 1917).

Giacomo Ciamician and Silber[1]) have communicated that cumarin in solid state and also in aqueous and alcoholic solution changes by solar light into hydrodicumarin, which substance was synthetically obtained by Fittig and Dyson[2]), starting from salicylaldehyde and succinic acid. According to the last investigators the formula of the hydrodicumarin was the following:



Ciamician and Silber write, that when we do not wish to assume a tetramethylene ring in the polymerised cumarin, the following formula is to be preferred in their opinion to that given by Fittig and Dyson:



This formula, however, does not take the properties of hydrodicumarin into account. According to Fittig and Dyson this substance is namely converted into hydrodicumaric acid, a mono-basic acid, by long boiling with "ziemlich concentrirten freien Alkalien".

The cumarin ring is difficult to open by boiling with alkalis, so that this property of the hydrodicumarin is in perfect agreement with the formula given to it by the two investigators. A substance that should possess the formula given to hydrodicumarin by Ciamician and Silber, should easily give a di-basic acid on being heated with alkalis, as there are here no cumarin rings, but hydro-cumarin rings present, which are easily opened when the substance is heated with alkalis.

In connection with the possible stucture of the truxillic acids, two of which arise by the action of light from the normal cinnamic

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[1]) Ber. 35, 4130 (1902); Ber. 47, 642 (1914).

[2]) Ann. 255, 280 (1889).

acids, it seemed not impossible to me that there occurred a tetramethylene ring in the product of illumination of cumarin. If this were the case, however, the substance would have to yield not a monobasic, but a di-basic acid on treatment with alkalis, and the product of illumination of cumarin would not be identical with the hydrodicumarin of FITTIG and DYSON.

The possibility of this was not excluded, as CIAMICIAN and SILBER give 262° as melting point of their product, FITTIG and DYSON having found 256° for the hydrodicumarin.

I have prepared the product of illumination of cumarin by illumination of an aqueous solution that was *not* covered by glass, while the hydro-dicumarin was made according to the directions of FITTIG and DYSON. The melting-points of the substances agreed with those given by the investigators. A mixture of the two substances melted, however, already at 225°. This proves that the substances differ.

This may be confirmed by what follows. The product of illumination of cumarin crystallizes from chloroform in microscopic plates, which often look like rectangles; hydrodicumarin separates out of the solvent mentioned in microscopic needles joined to sheaves.

The product of illumination of cumarin dissolves in sodium hydroxide of about 30 °/₀ in a few minutes at the ordinary temperature. Through hydrochloric acid it is precipitated from this solution unchanged. When the alcalic solution is boiled for an hour with an ascending still-head, then on addition of hydrochloric acid to the cooled solution a di-basic acid separates, which first crystallizes in fine microscopic needles, which, however, are soon transformed to thicker needles that join to stars and crosses. The acid melts at 245°; the melt smells strongly of cumarin, and easily dissolves in alcohol at the ordinary temperature. Also in a sodium-carbonate solution the melt is readily soluble.

As has already been communicated by FITTIG and DYSON, the hydrodicumarin gave after being heated for a long time with alkalis, a mono-basic acid, the hydrodicumaric acid, which when heated to the melting point, was again converted to hydrodicumarin, which is insoluble in alcohol, and also in a sodium-carbonate solution.

It appears, therefore, clearly from this that the product of illumination of cumarin is no hydrodicumarin.

I hope shortly to give a fuller discussion of the structure of the product of illumination of cumarin.

Buitenzorg, July 1917.

Mathematical Analyse. — *"Sur une propriéte des fonctions de variable complexe"*. Note de M. ARNAUD DENJOY. (Présenté par Mrs. W. KAPTEYN et L. E. J. BROUWER).

(Présénte dans la séance de Décembre 29, 1917).

Je me propose de démontrer le théorème suivant:

Si une fonction analytique $F(x)$ et sa dérivée $F'(x)$ sont *régulières et non nulles* en tout point d'un contour *simple* C, à l'intérieur duquel $F(x)$ peut se mettre sous la forme:

$$F(x) = (x-a_1)^{\alpha_1} \;.\;.\;.\;.\; (x-a_n)^{\alpha_n}\, G(x), \tag{1}$$

les a_j étant intérieurs à C, G étant *régulière et non nulle* dans C, et les a_j, α_j étant indépendants de x, *si l'argument de $F(x)$ varie dans un sens constant quand x décrit le contour C*, 1°. *$F'(x)$ possède à l'intérieur de C, $(n-1)$ zéros distincts ou confondus différents des a_j* 2°. *toute courbe d'équation Arg $F(x) =$ cte pénétrant dans C, aboutit en l'un des points a_j ou passe en l'un des zéros propres à F'.*

En effet, soit $\Gamma(x)$ une détermination de *log* $G(x)$. $\Gamma(x)$ est par hypothèse holomorphe dans C et sur C.

On a:

$$\frac{F'(x)}{F(x)} = \frac{\alpha_1}{x-a_1} + \;.\;.\;.\;.\; + \frac{\alpha_n}{x-a_n} + \Gamma'(x) = \frac{V(x)}{(x-a_1)\ldots(x-a_n)}.$$

D'après son expression, V est holomorphe dans et sur C, et V ne s'annule en aucun point a_j. En outre, par hypothèse, V ne s'annule pas non plus sur C. A l'intérieur de C, les zéros de $F'(x)$ distincts des a_j coïncident donc avec les zéros de V. Nous voulons montrer que V possède à l'intérieur de C, $(n-1)$ zéros distincts ou confondus. Or,

$$\frac{F'(x)}{F(x)} = \frac{d}{dx} \,.\, log\, F(x)\,.$$

F et F' étant réguliers et différents de zéro sur C, le second membre est, en tout point de C, une fonction continue de x, de méme que les logarithmes et *a fortiori* les arguments de F et de F'.

Supposons que C ait, en chacun de ses points, une tangente bilatérale unique, variant continûment. Soit α l'angle avec Ox de cette tangente dirigée dans le sens positif de parcours de C. Soit s l'arc de C compris entre une origine choisie sur C, et le point variable x. On a:

$$e^{i\alpha}\frac{F'(x)}{F(x)}=\frac{d\,log\,|F|}{ds}+i\frac{d\,Arg\,F}{ds}.$$

Donc, la partie imaginaire de $e^{i\alpha}\frac{F'}{F}$ garde un signe constant dans le mouvement de x sur C. Donc la variation totale de Arg. $e^{i\alpha}\frac{F}{F}$ est nulle. Or, C étant un contour simple, parcouru dans le sens direct, α augmente de 2π. Donc, Arg. $\frac{V}{(x-a_1)\dots(x-a_n)}$ diminue de 2π. Les a_j étant intérieurs à C, Arg. V augmente donc de $2(n-1)\pi$. Donc, V a $(n-1)$ zéros distincts ou confondus à l'intérieur de C.

Supposons maintenant que C ne possède pas en chaque point une tangente déterminée. Soit $2\delta_1$ la plus courte distance à C des points ζ qui sont, soit des zéros, soit des singularités de F ou de F'. δ_1 est positif puisque aucun de ces points n'est sur C. Soit $2\delta_2$ la distance minimum de deux points de C entre lesquels la variation de Arg. $F(x)$ est égale en valeur absolue à un multiple entier de 2π. Soit δ le plus petit des deux nombres δ_1 et δ_2. En un point quelconque x_1 de C, menons l'arc de courbe $g(x_1)$ d'équation:

$$Arg.\,F(x)=Arg.\,F(x_1),$$

limité de part et d'autre de x_1 à la longueur δ. Puisque F est régulier et non nul sur C, pour chaque point de C, l'arc $g(x_1)$ est déterminé. Comme F' ne s'annule pas sur ce même arc, celui-ci ne possède que des points simples.

Deux arcs $g(x_1)$, $g(x_2)$ sont entièrement distincts. En effet, les deux arcs $g(x_1)$, $g(x_2)$ correspondent à des arguments de F différents, puisque Arg. F varie dans un sens constant sur C. Cela étant, ou bien $\frac{1}{2\pi}$ Arg. $\frac{F(x_2)}{F(x_1)}=\theta_2-\theta_1$ n'est pas un nombre entier, et alors, les deux arcs ne pourraient se rencontrer qu'en un point où F est irrégulier ou nul, circonstance impossible, puisque, leur demi-longueur étant $\delta\leq\delta_1$, aucun ne contient de points de cette sorte. Ou bien $\theta_2-\theta_1$ est un entier non nul, et les deux arcs $g(x_1)$ et $g(x_2)$ n'ont pas de points communs, d'après $\delta\leq\delta_2$. Si e_1, e_2 sont les extrémités de $g(x_1)$ respectivement intérieure et extérieure à C, e_1 et e_2 décrivent chacun une courbe sans point multiple, l'une Γ_1 intérieure à C, l'autre Γ_2 contenant C à son intérieur. Entre Γ_1 et Γ_2, on peut déformer indifféremment C sans rencontrer de point ζ.

Or la condition que Arg. $F(x)$ varie dans un sens constant équivaut à celle-ci, que C coupe une fois et une seule chacun des arcs $g(x_1)$. Donc, si le contour C présente des irrégularités, il est possible de substituer à chaque point x_1 de C un point x'_1 de l'arc correspondant $g(x_1)$ de façon que la courbe C' des points x'_1, qui coupe une fois et une seule chaque arc $g(x_1)$, soit continue et admette en tout point une tangente unique variant continûment. C' contient à son intérieur les mêmes points ζ que C. Or, la première partie du théorème est vraie pour C'. Donc elle l'est aussi pour C. Elle est donc générale.

Conservant toutes les autres hypothèses de l'énoncé, supposons simplement que Arg $F(x)$ *ne possède pas sur C les deux sens de variation.* Sur certains arcs ω de C, Arg $F(x)$ pourra être constant. Sur un tel arc ω, *log* $|F|$ est continu et varie dans un sens constant, puisque ni F ni F' ne s'annulent sur ω. Quand x décrit ω, le point figuratif de $u = e^{ia}\frac{F'}{F}$ parcourt sur l'axe réel un segment dont les deux extrémités sont d'un même côté de l'origine. Si la tangente à C varie continûment, comme le point u n'a pas de positions de part et d'autre de l'axe réel, la variation de Arg u sur le contour décrit par x est encore nulle. La première partie du théorème subsiste.

Si, en dehors ou aux extrémités des arcs ω, C était, en certains points, dépourvu de tangente, on introduirait les arcs $g(x_1)$ relatifs à tous les points de C, sauf aux points intérieurs aux arcs ω. L'ensemble des $g(x_1)$ formerait des régions auxquelles toutes les parties de C étrangères aux arcs ω seraient intérieures, et où l'on pourrait déformer C sans lui faire traverser de zéros ni de singularités de F ni de F', de façon à lui donner une tangente continue en conservant les conditions du théorème. Celui-ci, vrai pour le nouveau contour, l'est aussi pour C.

La première partie du théorème et cette dernière extension étant établies en toute généralité, démontrons la seconde partie.

Si une courbe g d'équation Arg $F(x) = \text{cte}$ avait dans C un certain point γ, et si, prolongée dans les deux sens à partie de γ, elle ne rencontrait pas de point ζ, elle aboutirait des deux parts à C, en des points respectifs α et β. Les deux arcs de C séparés par α et β, augmentés de l'arc de g intérieur à C, formeraient deux contours simples ne contenant pas de point ζ et sur aucun desquels Arg F ne posséderait les deux sens de variation. Alors F possédant q points a_j à l'intérieur de l'un de ces contours, et $(n-q)$ à l'intérieur de l'autre, F' aurait à l'intérieur de ces mêmes contours respectivement $(q-1)$ et $(n-q-1)$ zéros (comptés chacun avec son ordre de multiplicité), donc en tout $(n-2)$ zéros et non pas $(n-1)$. Le théorème est donc entièrement démontré.

La première partie appelle diverses observations.

D'abord, la démonstration reposant simplement sur ceci que le point $e^{i\alpha} \frac{F'}{F}$ ne tourne pas autour de l'origine, *il suffirait* pour l'exactitude de la *première partie* du théorème, *que le sens de variation de Arg F sur C fût le même, simplement chaque fois que le module de F passe par un maximum ou par un minimum.*

Considérons le cas où C contient un zéro régulier et simple de F'. On suppose toujours $F(\xi) \neq 0$, de façon que l'argument de F ne soit indéterminé en aucun point de C. Pour préciser le sens de la première partie du théorème, il convient de savoir si ξ doit ou non être considéré comme intérieur à C. Nous examinerons pour cela comment il convient de modifier un arc de C contenant ξ et indifférement petit, de façon que le sens de variation de Arg F sur le nouvel arc ne soit ni double, ni différent de celui de C. La ligne $\operatorname{Arg} F = \operatorname{Arg} F(\xi)$ possède en ξ un point double à tangentes rectangulaires. Les deux branches γ_1 et γ_2 de cette courbe délimitent quatre angles curvilignes de sommet ξ. Une ligne $\operatorname{Arg} F = \operatorname{Arg} F(\xi) + \varepsilon$ se compose, si ε est assez petit, de deux arcs distincts, intérieurs respectivement à deux angles, opposés par le sommet, formés par γ_1 et γ_2 et disposés relativement à γ_1 et à γ_2 comme le sont par rapport à leurs asymptotes les deux branches d'une hyperbole équilatère. Suivant le signe de ε, les arcs sont dans l'un ou dans l'autre des deux couples d'angles ainsi placés. Numérotons par exemple 1 et 3 ceux qui correspondent à $\varepsilon > 0$, 2 et 4 les deux autres. Supposons que γ_1 désigne la branche séparant les angles 1 et 2 d'une part, 3 et 4 d'autre part. γ_2 sépare d'une part 1 et 4, d'autre part 2 et 3. Arg F croît, quand on traverse soit γ_1 en passant de 2 à 1 ou de 4 à 3, soit γ_2 en passant de 4 à 1 ou de 2 à 3.

C ne peut pas pénétrer dans deux angles opposés. C traverse une des deux branches γ_1 et γ_2, et une seule. Si donc C ne contient pas un arc de l'autre branche, il est entièrement situé (sauf en ξ) d'un même côté de cette dernière. On peut, aux abords de ξ, déplacer C de ce même côté sans changer le sens de variation de Arg F. ξ doit donc être considéré comme situé du même côté de C que la branche γ_1 ou γ_2 non traversée.

Si C n'est pas tangent à la branche non traversée, il ne peut pas présenter une tangente unique en ξ. Donc, si *C possède un point anguleux en ξ, ξ doit être regardé comme situé du côté de C opposé à l'intérieur de l'angle* (ouvert de moins de π) *formé par les deux arcs de C séparés par ξ.*

Si C, traversant γ_1, contient un arc γ'_2 compris dans γ_2, C quitte

γ_2, suffisamment prolongé, du même côté aux deux extrémités de γ'_2. Je dis que ξ doit être considéré comme situé du côté de C, où se trouvent les parties de γ_2 étrangères à γ'_2.

En effet, si par exemple Arg $F(x)$ est non décroissant sur C, C doit atteindre γ_2 par l'angle 2 et le quitter par l'angle 1, ou atteindre γ_2 par l'angle 4 et le quitter par l'angle 3. Plaçons-nous dans la première hypothèse.

Traçons un trait α_1' β_1' aussi court et voisin de ξ qu'on le voudra, et normal à γ_1, α_1' étant dans l'angle 2 et β_1' dans l'angle 1. Sur le trait α_1' β_1', Arg F croît. L'arc de courbe: Arg $F =$ Arg $F(\alpha_1')$ situé dans l'angle 2, et suivi à partir de α_1' en s'éloignant de γ_1 vers la partie quasi-asymptote à γ_2, coupe C en un point α_1 très voisin de l'extrémité initiale de γ_2'. De même l'arc de la courbe Arg. $F =$ Arg. $F(\beta_1')$, situé dans l'angle 1 et suivi à partir de β_1' dans le sens quasi-asymptote à γ_2, coupe C en β_1, très voisin de l'extrémité terminale de γ_2'. Soit C'' le contour obtenu en modifiant C entre α_1 et β_1 par la substitution du premier parcours au second. Sur le parcours $\alpha_1\,\alpha_1'\,\beta_1'\,\beta_1$, Arg F ne décroît jamais, tout comme sur C entre α_1 et β_1, en passant par l'arc γ_2'. Si α_1' et β_1' sont assez voisins de ξ, les deux contours C et C' ne comprennent entre eux aucun point ζ. A leur intérieur, le nombre des a_i est le même, et il doit donc en être ainsi du nombre des zéros de F', si le premier de ces deux nombres détermine le second. Il faut donc considérer ξ comme situé relativement à C ainsi qu'il l'est par rapport à C'', c'est-à-dire du côté de l'arc γ_2' opposé à celui où C quitte γ_2'.

Si l'arc γ_2' où Arg. F est constant, contient plusieurs zéros simples de F', dont aucun n'est un point anguleux pour γ_2', tous ces zéros doivent être regardés comme placés, relativement à cet arc, du côté opposé à celui où, initialement et finalement, C se sépare de γ_2. Des considérations analogues aux précédentes justifient cette conclusion et permettent d' en éclairer le sens dans tous les cas.

Si le point ξ de C est un zéro de F' multiple d'ordre p, la courbe Arg. $F =$ Arg. $F(\xi)$ possède $(p+1)$ branches simples se croisant en ξ et séparant $2\,(p+1)$ angles curvilignes θ de même ouverture. Dans ces divers angles, au voisinage de ξ, Arg. F est alternativement supérieur et inférieur à Arg. $F(\xi)$. Pour que Arg. F varie sur C dans un sens constant, il faut que les deux arcs de C séparés par ξ soient dans deux angles θ adjacents ou séparés par un nombre pair d'angles θ. Si ces deux angles sont adjacents, C possède (ou pourra être déformé de façon à posséder) un point anguleux en ξ. On considère ξ comme situé, par rapport à C, du côté opposé à l'intérieur de l'angle formé par les deux branches de C.

Si ces deux angles ne sont pas adjacents, q couples d'angles θ sont du côté intérieur de C, $p-q$ du côté opposé. *F' doit être considéré dans* l'application du théorème *comme possédant q zéros intérieurs à C et confondus avec ξ*. On le montre en adjoignant à C les arcs Arg. $F =$ Arg. F (ξ) issus de ξ et intérieurs à C.

Application. Si une fonction F (x) a son module constant sur un contour simple C où elle est régulière et où sa dérivée F' ne s'annule pas, si de plus F est, à l'intérieur de C, de la forme (1), *F' s'annule $(n-1)$ fois à l'intérieur de C en des points distincts des zéros ou infinis de F'.*

Car le sens de variation de Arg. F sur C est constant, sinon, en un point où il se modifierait, F' s'annulerait, puisque $\frac{d \log |F|}{d x}$ est toujours nul quand x décrit C.

Si C passe en un zéro simple ξ de F', les considérations antérieures permettent de définir les conventions sous lesquelles le théoréme subsiste. La ligne $|F(x)| = |F(\xi)|$ possède deux branches β_1 et β_2 se croisant en ξ. Arg. F possède en ξ un maximum sur l'une des branches, un minimum sur l'autre. Donc, pour que, sur le contour C, Arg. $F(x)$ varie en sens constant, il faut qu' à la demi-branche β_1 ou β_2 par où C arrive en ξ, succèdent l'une ou l'autre des deux demi-branches β_2 ou β_1 orthogonales à la première. ξ devra être regardé comme extérieur à l'angle droit dont il est le sommet et dont les deux demi-branches considérées forment les côtés.

On peut aussi arrondir C au voisinage de ξ par une modification infiniment petite, de façon que la tangente tourne dans un sens constant d'environ $\pm \frac{\pi}{2}$. ξ reste en-dehors de la convexité de cet arc. La variation de Arg. F sur le contour C ainsi tracé, possède un sens constant. C est un contour simple, auquel le théorème fondamental s'applique.

Si ξ est un zéro multiple d'ordre p de F', les deux arcs de C aboutissant en ξ font un angle géométrique $K \frac{\pi}{p+1}$, K étant entier et au plus égal à $p+1$. Pour que Arg. F varie en sens constant sur C, il faut que K soit impair. Si $K = 2K' + 1$, $\left(K' \leq \frac{p}{2}\right)$, le théorème est vrai, à la condition de considérer F' comme possédant en ξ, K' zéros intérieurs à l'angle curviligne formé par les deux arcs de C se réunissant en ξ. On s'en assurera encore en ajoutant à C toutes les branches de la courbe $|F| = |F(\xi)|$ issues de ξ et intérieures à ce même angle.

Physiology. — "*A Contribution to the pathological histology of striated muscle-tissue*". By P. NIEUWENHUIJSE. (Communicated by Prof. C. A. PEKELHARING).

(Communicated in the meeting of December 29, 1917).

During my stay on the isle of *Urk* as medical officer I found in the dorsal muscles of a plaice a rather big hard white knob, which consisted of striated muscle tissue and looked like a tumour.

At the microscopical examination I was struck with the peculiar changes of the muscle fibres which were quite unknown to me, and which in the beginning I considered as belonging perhaps specifically to muscles of a fish.

When however later on I found that similar changes also occur in human muscles, it became evident that we had to do in this case with a process of more universal significance, which can appear in striated muscles in general.

Macroscopical examination: A plaice (Pleuronectus platessa), caught in the North-sea, long 31 c.m. showed at the right upper side of its body an irregular hard knob nearly as big as a child's fist which was prominent under the undamaged skin.

In sections it appeared that this knob consisted of muscle tissue, rather hard of consistence, pale coloured with yellow spots here and there and sharply marked against the surrounding tissue.

The knob could in some places easily be detached from the surrounding muscle tissue, so that it reminded us in many respects of a fibromyoma in the musculature of the uterus and consequently looked quite like a tumour.

The knob consisted principally of two parts, separated by connective tissue, which diverged in the proximal direction and so in some sections they made the impression of two single knobs (Fig. 1).

The plaice was for the rest normal and well nourished; neither in the other muscles nor in the internal organs any abnormities could be stated.

Microscopical: The pieces were fixed in formalin (10 $^0/_0$) imbedded in paraffin or celloidin and stained in different ways. Some pieces were before imbedding hardened in Müller's solution; after that a number of frozen sections were made and finally by means of teased preparations I tried to isolate some fibres.

In the preparations we could see that the knob consisted only of striated muscle tissue, the fibres of which, showing many signs of degeneration, were of different size.

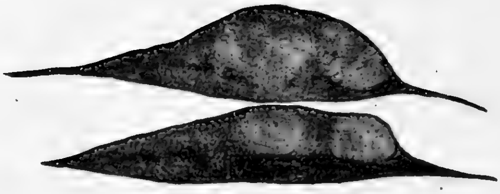

Fig. 1. Section of the fish, in which the muscle-knob is to be seen. The knob is divided into two parts, which diverge in the proximal direction and make by doing so the impression in the lower section as if there were two single knobs.

The fibrous tissue between the muscle fibres had increased; the arrangement of the great septa of connective tissue pretty well agrees with normal proportions, but the thinner bundles surrounding the separate muscle fibres are often irregularly and considerably thickened.

In some places in the connective tissue an accumulation of small cells with round irregularly coloured nuclei and a small quantity of protoplasm is to be found; other sections show much fatty tissue with rests of degenerated muscle fibres.

Neither haemorrhage nor bloodpigment could be stated.

Studying the structure of the muscle fibres, the question arose how the normal dorsal muscles of Pleuronectus were built. It is well known that fishes normally show peculiar grouping of the bundles of fibrils and this in such a way that the fibres of a given muscle group in a given fish species ordinarily have in this respect a characteristic structure.

Lansimaki [1], who studied this question carefully, found in the dorsal muscles of Pleuronectus that each fibre showed in its exterior part a thin layer of ribbon-shaped bundles of fibrils, whilst the inner part was filled up with cylindric bundles.

In self-made preparations of a normal plaice I could confirm these statements (Fig. 2).

In the preparations of the muscle-knob the fibrils are generally coarser than normal; the transverse striation, however, shows the usual dimensions, and is in all non-degenerated fibres distinctly visible. The typical normal form and grouping of the bundles of fibrils is in the various smaller fibres clearly to be seen, the big fibres however do not often show the peripheral ribbon-shaped fibrils. Some very small fibres possess only ribbon-shaped bundles which form a starlike figure in transverse sections.

In longitudinal sections one can see that the fibres here and there give off greater and smaller branches. The myoplasm has in some places a little increased in volume.

The muscle-nuclei are in the smaller as well as in the bigger fibres not more numerous than normal generally they are situated at the periphery, but sometimes also between the fibrils; the nuclei are somewhat swollen; nuclear divisions could not be detected.

Among the big non-degenerated fibres many show a curious and striking structure. In transverse sections they seem to consist of two concentric parts: the inner

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[1] T. A. Lansimaki (Helsinfors) Ueber die Anordnung der Fibrillenbundel in den quergestreiften Muskeln einiger Fische. Anat. Hefte. Heft. 126, S. 254.

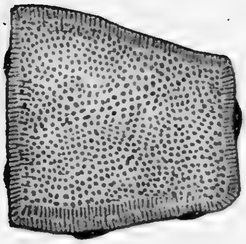

Fig. 2. Cross-section of a normal muscle-fibre of a plaice; one sees the transversely cut cylindric bundles of fibrils in the centre and a small layer of ribbon shaped bundles on the edge (Zenker's liquid, iron-haematoxylin, enlargement 600).

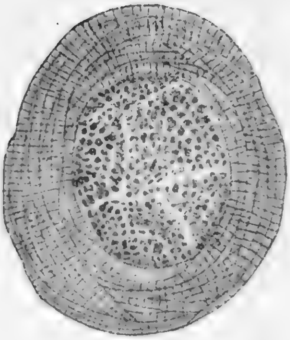

Fig. 3. Cross-section of a fibre of the muscle-knob. One sees two concentric parts: an interior part with fibrils which are transversely cut and an exterior part with circular fibrils. (formalin, ironhaematoxylin, enlargement 500).

part shows the cross-cut longitudinal fibrils, while in the exterior part only circular fibrils are visible. Between the two parts a narrow fissure is generally to be seen (Fig. 3 and 4).

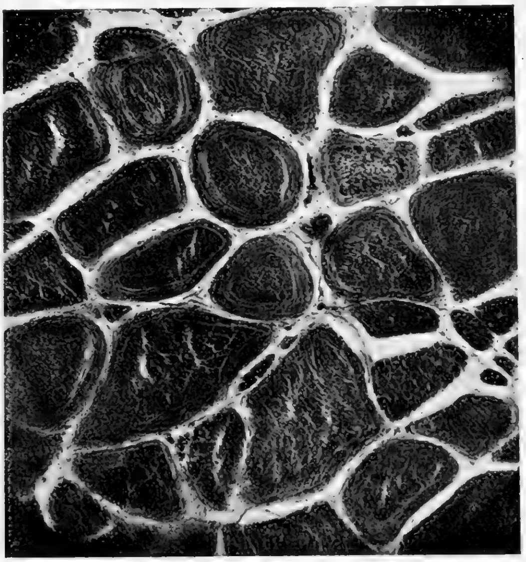

Fig. 4. Fotograph in which a great number of fibres as mentioned in fig. 3, are visible.

In longitudinal sections we see, that the circular fibrils do not appear along the whole length of the fibre but are localized to one or more places, while the muscle fibre has become here more voluminous (Fig. 5). The fissure between the central and peripheral fibrils is not always present: here and there longitudinal fibrils bend aside and join the circular-ones (Fig. 6).

In teased preparations it was further to be seen that many peripheral fibrils do not surround the fibres in rings, but go around them in a spiral fashion.

The fibres in question are generally bigger than normal; they branch sometimes just like the other fibres; in their neighbourhoud one often sees very thin muscle-elements, which are a single time in connection with the peripheral fibrils. The fibres often undergo degeneration, about which more later on.

The nuclei are greatly swollen; they have become oval vesicules with a darkly stained point in the centre and remind us sometimes of protozoa. Their number has not or only a little augmented; they are situated either in the circumference of the fibre or in the centre and frequently between the peripheral and central fibrils. Nuclear divisions were not to be found.

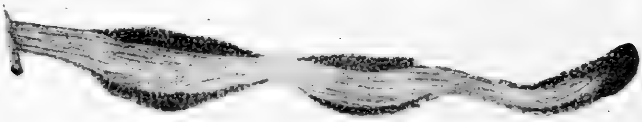

Fig. 5. Longitudinal section of a fibre as mentioned in fig. 3. One sees that the circular fibrils only appear in some places of the fibre. (enlargement 60).

As mentioned above, most muscle fibres show considerable signs of degeneration: first an extensive fatty degeneration, further a change, which strikingly resembles the well known "Zenker"-degeneration.

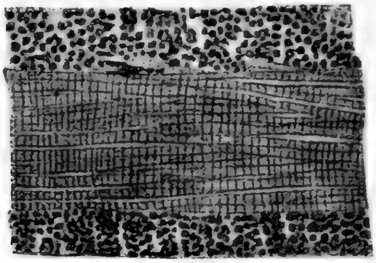

Fig. 6. Longitudinal section of a fibre as mentioned in fig. 3 more enlarged than the preceding one. One sees that some longitudinal striped fibrils join the circular ones (formalin, ironheamatoxylin, enlargement 500).

This change appears locally, and does not occupy the whole fibre; both the longitudinal and the transverse striation get invisible so that a homogeneous mass results, which afterwards is reduced to pieces and coarse corns.

Between the homogeneous pieces in more advanced stages many round nuclei appear. The contents of some fibres become liquid and get resorbed and consequently the walls of the fibre fall together.

The above mentioned muscle fibres with the mantle of circular fibrils seem very seldom to undergo the "Zenker" degeneration. They very often show however another kind of degeneration: their fibrils become thinner, lose their transverse striation and are reduced to very fine threads and grains; these grains get resorbed, so that in the end a rather homogeneous mass remains, which can hardly be stained and which is easily distinguished from the homogeneous masses in the "Zenker" degeneration.

This degeneration principally occupies the circular fibrils, which are often already quite changed into very small threads and grains while the central fibrils are still normal.

In many fibres the figure becomes still more intricate, because bundles of longitudinal fibrils are visible between the circular ones. This phenomenon sometimes appears to such an extent, that we can call it a real twisted-work. This twisted-work is generally only to be seen in the peripheral fibrils, but may also occur in the central part of the fibre.

The twisted fibrils always show signs of degeneration and it is a general rule that: the more the figure is intricate, the more the degeneration is advanced.

These pathological changes are nearly always accompanied by thickening of the muscle fibre, which may become so extensive that a diameter of $250 \times 250\,\mu$ in transverse sections is not uncommon.

With this kind of degeneration there is not much activity of the nuclei; in some spots they have become more numerous, but accumulations of nuclei as in the fibres with "Zenker" degeneration were not to be seen.

Although the muscle-knob seemed to be distinctly contrasted with the surrounding tissue, microscopical investigation showed us the presence of divers degenerated fibres and also fibres with a mantle of circular fibrils in the immediate vicinity.

With the exception of the above described groups of cells in the connective tissue no signs of inflammation were apparent; these cells have probably something to do with the evacuation of rests of degenerated muscle fibres; we may therefore not deduce from this that it is a process of inflammation.

We looked carefully for the presence of bacteria, protozoa and of other parasites, but without result. 1)

The microscopical examination of the brain, the spinal cord and some peripheral nerves showed nothing interesting.

Some viscera were examined, but these neither showed any abnormities.

Among the different pathological changes which were present, we must pay special attention to the numerous peculiar muscle fibres, which in transverse sections seem to exist of two concentric parts, the exterior part containing circular fibrils, the interior part containing longitudinal-ones.

How can this figure be explained?

As in the beginning I could not find in the literature any description of these changes, nor any connecting statement which made it possible to explain them, I was inclined to think that these changes could possibly be connected with the peculiar structure of the muscle fibres of fishes, which normally already show a difference between the centre and the periphery and to consider this stucture as the cause of these curious changes.

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1) I wish to express my gratitude to Mr. J. H. Schuurmans Stekhoven Jr., interem-zoologist of the Institute for Tropical Hygiene at Amsterdam, for his kind help with this part of my investigations.

This interpretation, however, was not quite satisfactory because the difference between the centre and the periphery of a muscle fibre of Pleuronectus is really not a difference in structure, but a difference in grouping of the fibrils, while all the fibrils are, as far as it is known, identic in form and function. Fortunately I perceived at last that similar changes were already described in human muscles viz. in a case of acromegaly that was communicated by C. MARTINOTTI in 1902.[1])

As I could not get possession of his original publication, I am obliged to cite the following sentences of an epitome of the "Ergebnisse der allgemeinen Pathologie und pathologische Anatomie", 12 Jahrg. p. 317. Muscle fibres of the musculus biceps show the following pathological changes: "Es bestand Hypertrophie der Muskelfasern mit beträchtlicher Vermehrung der Muskelkerne. An einigen Muskelfasern sah man Bündel von querverlaufenden Fibrillen, die die längsverlaufenden umfassten oder durchkreuzten. Die querverlaufenden Fibrillen waren nicht überall, sondern nur an einzelnen Stellen der Muskelfasern zu sehen und konnten nicht auf weite Strecken verfolgt werden. Verf. glaubt dass die Fibrillen mit abnormen Verlaufe neugebildet seien und dass sie einer Form von pathol. Muskelhypertrophie entsprechen, die vielleicht mit den akromegal. Prozesse in Zusammenhang steht".

Although the muscle nuclei in this case are more active than in our preparations, the resemblance of the two pathological changes is so striking, that we may surely identify them.

In the human pathology a few cases more are mentioned in which the alterations resembled our process in some ways.

ERB[2]) described in 1891 in a case of dystrophia muscularis progressiva "ineinandergeschachelte Muskelfasern", of which he says p. 184 "Stellenweise sieht man auch sehr sonderbare Spaltungs- und Theilungsfiguren, wie wenn sich im Centrum der Faser eine neue Faser entwickelt oder abgespalten hätte".

DURANTE[3]) described in 1900 in a case of hypertrophy of the muscles of the arm similar figures which he interprets as being

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[1]) C. MARTINOTTI: Su alcune particalarità di struttura della fibra musculare striata in rapporto con la diagnosi di acromegalia. Annali di freniatria e scienze affini del R. Manicomio di Torino Vol. XII, p 76.

[2]) W. ERB: Dystrophia muscularis progressiva. Deutsche Zeitschr. f. Nervenheilk. Bd. I S. 173.

[3]) G. DURANTE: Hypertrophie musculaire volumétrique vraie du membre superieur par augmentation de volume des fibres musculaires. Comptes Rendus de la Soc. Anat. Paris Mars 1900.

the result of concentric divisions; he says p. 275: “C’est ainsi que cette division peut être concentrique et au lieu de donner naissance à deux fibres acollées, peut séparer deux éléments, l’un entourant l’autre comme les parois d’un tube entourent le mandrin qu’on y introduit”.

It is a pity that neither in the text nor in the figures these authors give any indication about the course of the fibrils, so that it is impossible to compare their changes with those in our case. Their interpretations appear to me to be not very plausible; it is difficult to understand how it can be possible, that a new fibre develops within the centre of an old one, and it is still more difficult to imagine how a muscle fibre by means of division could give rise to two concentric parts; both ideas are so little analogical with other better known processes.

Erb described in the dystrophia muscularis progressiva a second peculiarity, which may be important in this respect; he saw in transverse sections that many muscle fibres were encircled by very small and larger muscle elements, which he could follow a good way in the surrounding connective tissue; they seemed to bifurcate there and resembled in their behaviour the capillaries. **Erb** considers them fine bifurcations of muscle fibres and was reminded by them of the so called “Muskelknospen” of **Neumann**.

Although these figures, superficially considered, have nothing to do with ours, this statement of **Erb** seems to me of great interest.

However important we must consider the above mentioned data of the literature, still they do not explain to us the nature of the changes in question.

If we now try with the aid of the preparations to analyze the peculiar figures, we must begin to look for the origin of the circular fibrils.

In transverse sections we see that they are nearly always separated from the central fibrils by a small fissure, but in longitudinal sections we could demonstrate a distinct connection between them: in some places we saw many central fibrils bend aside and join the peripheral ones. Teased preparations taught us that the peripheral fibrils do not follow exactly the circular direction, but often go around the fibre in a spiral fashion.

I should like to say thus, that the muscle-fibres give off here and there thin lateral branches which do not enter into the surrounding tissue, but remain within the sarcolemma and go around the fibre in circles and spirals.

The fibre is enveloped in this manner over a certain length by a thick mantle of these branches, so that in cross sections we see two concentric parts, which however is not the result of a concentric division, but of abnormal branching.

If moreover we consider that we have to do with fibres which are strongly proliferating, take as evidences the hypertrophy, the longitudinal divisions and the presence of a great number of young fibres, it may be clear that this process is nearly related to the well known lateral buddings of muscle-fibres the so called "Muskelknospen" of NEUMANN, especially because the before mentioned second observation of ERB already informed us of the encircling of muscle fibres by elements which have grown out of these "Muskelknospen."

Therefore I should wish to conceive the peripheral mantle of the fibres as a complex of defectively developed and abnormally grown lateral branches.

With this conception it is easily understood that the circular fibrils often appear only on one or more spots of the fibre, and leave sometimes a great deal of the fibre free; it is just as well comprehensible that on transverse sections we generally see a narrow fissure between the central and peripheral fibrils, moreover it will be clear that sometimes out of these fibres originate very thin muscle-elements, which can be followed into the surrounding fibrous tissue, whilst finally it becomes explainable that the peripheral fibrils, abnormal of origin as they are, sooner undergo degeneration than the central fibrils which form the real fibre.

This conception leaves of course many facts unexplained: first of all is it not clear why these fibres should give off so many abnormal branches, and why their nuclei have swollen so peculiarly and taken the form of vesicules, while the nuclei in the surrounding fibres are much smaller and more solid; finally the curious behaviour of the fibres with regard to degenerations is still unexplained: while the surrounding fibres undergo "ZENKER" degeneration, we see here rather constantly the above described peculiar reducing of the fibrils to very fine threads and grains.

All this cannot be quite accidental, because these alterations appear too regularly; I could not find however a satisfactory explanation for these facts.

We now come to the question how this abnormally proliferated tissue must be considered: is it allowed to classify it among the tumours or not?

Macroscopically it looks quite like a tumour, but we shall see that after microscopical examination this diagnosis cannot be sustained; firstly we find in rhabdomyoma always very small muscle-fibres (in cross sections generally smaller than 20 μ, only RIBBERT found some bigger fibres up to 80 μ); here however the fibres are generally enlarged, some of them even reach a diameter of 300 μ.

Secondly one sees in rhabdomyoma that the small muscle-fibres have a very defective structure; frequently they can hardly be recognised; a rhabdomyoma of a fish published by FIEBIGER [1]) showed just as well very small elements which could scarcely be identified with muscle-fibres.

In our case however the small fibres have generally a normal structure, whilst the abnormities appear in the big fibres. Further we see that the arrangement of the septa of fibrous-tissue resembles for the greater part the normal conditions; we do not see here a capricious grouping of the muscle-fibres, as can be expected in tumours, and finally we see that hypertrophic fibres also occur in the normal muscle-tissue that surrounds the knob. From these observations I conclude that in this case we have not to do with a new grown tissue which to a certain extent carries on an independent existence, but with a tissue that has grown by hypertrophy and abnormal proliferation of muscle-fibres which were already locally present.

Such a local proliferation of striped muscle-fibres looking like a tumour is very uncommon.

In the literature I could only find one similar case: LORENZ [2]) described a "geschwulstartige Hypertrophie" in the musculus glutaeus maximus of a man 52 years old, which has grown after "Ueberanstrengung".

Here he found very big muscle-fibres even 100 μ and 200 μ in transverse section. The fibrils were coarser than normal, the transverse striation was less distinct, in longitudinal sections he saw many divisions of muscle-fibres; divers fibres showed degeneration. ("ZENKER" degeneration and "fibrillaire Zerklüftung").

The nuclei of the sarcolemma had not augmented, signs of inflammation were not present.

The case of LORENZ rather resembles ours; as for the etiology it does not teach us anything.

Neither in our case could I find the cause of this abnormal proliferation: bacteria, protozoa and other parasites were not to be found, evident signs of inflammation could not be discerned, the peri-

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[1]) J. FIEBIGER: Ein Rhabdomyom bei einem Kabljau. Zeitschr. f. Krebsforschung Bd. 7. S. 382.

[2]) Handb. der spec. Pathol. u. Therapie (NOTHNAGEL) Bd. XI. S. 415. 1904.

pheral nerves, the spinal cord and the brain did not show us any changes and with regard to the communication of MARTINOTTI I took moreover special notice of the hypophysis, which however was quite normal.

Still it could be possible that we had to do with an excessive regeneration of the muscle-tissue, after a trauma, but for this we have no positive data.

Though in this case we have to do with a local tumourlike proliferation of muscle-tissue, of which no cause can be discerned, and which is of no use for the body, still I should not like to speak of a tumour for just in strange proliferations of the tissues of lower animals, it is in my opinion far better not to be too liberal with the word tumour.

Mathematics. — "*Some Considerations on Complete Transmutation.*" (Fifth Communication.) By Dr. H. B. A. Bockwinkel. (Communicated by Prof. L. E. J. Brouwer.)

(Communicated in the meeting of January 27, 1917).

23. If we take, as we did in N°. 19 (preceding communication) the development of $T(vu)$ as a representation of the *compound* transmutation $T_1 = (Tv)$ applied to u, of which transmutation the first component is the operation of multiplication, and the second an arbitrary one, that development is a special case of a more general one that represents the resultant T of two transmutations T_1 and T_2, which are *both* arbitrary. This development is also mentioned by Bourlet, but merely in a *formal* way, without any indication as to eventual domains of validity. As Bourlet frequently uses the formula, it is of importance to be on clear ground with respect to it.

Previously we observe something about the functions $a_m(x)$ of a series P, representing a transmutation which is complete in certain circular domains. If (α) is such a domain of completeness, we saw before (N°. 4, 1st communication) that this implies that the quantity

$$a_x = \overline{lim}|a_m(x)|^{\frac{1}{m}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

is limited in (α) and consequently has a finite upper limit, which we indicated by $a(\alpha)$ or simply by a. We shall now consider the majorant functions $\overline{a_m}(x)$ of $a_m(x)$ (cf. a note in N°. 7, 2nd communication). Have they also the property that a limit exists as indicated in (6)?

According to a well-known formula from the theory of functions which, moreover, we have already frequently used, we have in a domain $(\alpha') < (\alpha)$ [1])

$$\overline{a_m}(x_0 + \alpha') < \frac{\alpha|a_m(x_m)|}{\alpha - \alpha'},$$

in which x_m is the point on the circumference of (α), where $a_m(x)$

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[1]) The magnitude $\overline{a_m}(x_0 + \alpha')$ is according to what we understand by a majorant function, real and positive.

has its maximum modulus. From this it follows

$$[\overline{a}_m(x_0+\alpha')]^{\frac{1}{m}} < \left(\frac{\alpha}{\alpha-\alpha'}\right)^{\frac{1}{m}} |a_m(x_m)|^{\frac{1}{m}}$$

and consequently

$$\overline{\lim_{m=\infty}}\, [\overline{a}_m(x_0+\alpha')]^{\frac{1}{m}} \leq \overline{\lim_{m=\infty}}\, |a_m(x_m)|^{\frac{1}{m}}. \quad . \quad . \quad . \qquad (52)$$

The left-hand member is simply equal to $\overline{a}(\alpha')$, the upper limit for the domain (α') of

$$\overline{a}_x = \overline{\lim}\ |\overline{a}_m(x)|^{\frac{1}{m}},$$

and this because *all* functions $\overline{a}_m$ have their maximum modulus exactly in the point $x_0+\alpha'$ of the domain (α'). For the right-hand member it would not hold without more that it is equal to $a(\alpha)$, if we did not adhere to the so-called uniformity-supposition of N°. 4, that corresponding to any given arbitrarily small number ε, there is for *all* points x of the domain (α) *one* integer m_ε, such that

$$|a_m(x)|^{\frac{1}{m}} < a_x + \varepsilon \ , \text{ for } m \geq m_\varepsilon \, .$$

On this supposition it is not possible that the right-hand member of (52) differs from $a(\alpha)$. For, let us suppose that this were the case, and that we had for example

$$\overline{\lim_{m=\infty}}\, |a_m(x_m)|^{\frac{1}{m}} = a(\alpha) + \delta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (53)$$

where δ is a certain positive number. Take $\varepsilon = \frac{1}{2}\delta$. We have then for $m > m_\varepsilon$

$$|a_m(x_m)|^{\frac{1}{m}} < a_{x_m} + \tfrac{1}{2}\delta \leq a(\alpha) + \tfrac{1}{2}\delta,$$

contrary to (53). That the limit in the right-hand member of (52) cannot be *less* than $a(\alpha)$, appears at once, if we observe that for an *arbitrary* point x of the domain (α)

$$|a_m(x_m)|^{\frac{1}{m}} \geq |a_m(x)|^{\frac{1}{m}},$$

consequently

$$\overline{\lim_{m=\infty}}\, |a_m(x_m)|^{\frac{1}{m}} \geq a_x$$

and therefore also, as this inequality holds for *any* point x of (α),

$$\overline{\lim_{m=\infty}}\, |a_m(x_m)|^{\frac{1}{m}} \geq a(\alpha)$$

The inequality (52) passes consequently into

$$\overline{a}(\alpha') < a(\alpha)$$

Further we have evidently

$$\overline{a}(\alpha) \geq a(\alpha).$$

We will now put the case that $\overline{a}(\alpha)$ is a *continuous* function of (α) in the interior of the interval $(0, A)$, where as before we indicate by A the upper limit of the α-values, for which $\overline{a}(\alpha)$ is finite; we shall perhaps recur to the question as to whether this is the only possible case. From the combination of the two last equations it follows then that

$$\overline{a}(\alpha) = a(\alpha) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

For the series $\overline{P}$, arising from P by substituting for the coefficients a_m their majorant-functions, the number $\overline{\beta}$ corresponding to α is now determined, according to formula (7) (1st communication), by

$$\overline{\beta} = \alpha + \overline{a}(\alpha)$$

Thus according to (54) we have also

$$\overline{\beta} = \alpha + a(\alpha) = \beta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (54a)$$

We arrive consequently at the lemma: The series $\overline{P}$ has the property of completeness in any domain (α) in which P possesses the same property, such that the domain corresponding to (α) is the same for both series, at least on the simplifying supposition we have made here, that β increases continuously with α.

24. We can now prove the following theorem: *If the components* T_1 *and* T_2 *of the compound transmutation* $T = T_2 T_1$ *satisfy the conditions:*

1. T_1 *and* T_2 *are normal transmutations;*

2. *The series* P_2 *belonging to* T_2 *is complete in a circular domain* (α) *with corresponding domain* (γ)*; the series* P_1 *belonging to* T_1 *is complete in* (γ) *with corresponding domain* (β)*;*

3. *As pairs of associated fields, except the two pairs that must have been understood in stating the condition under* 1, *may be considered: for* T_1 *any pair of associated fields of* P_1, *for* T_2 *any pair of associated fields of* P_2 [1]*; then the following statements are valid:*

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[1] We remind the reader of the fact that, if a series P, which is complete in a domain (ξ) with corresponding domain (η), is to be called a *normal* transmutation, as a F. F. associated with the N. F. (ξ) possibly only the group of functions, belonging to a circle being a little greater than (η) comes into consideration; for it appears from the example given in N°. 13 (2nd communication) that the *continuity* may be disturbed if corresponding to (ξ) as a N. F. O., (η) is assumed as a N. F. F.

1. *The resultant T is a normal transmutation with a pair of fields of which the N. F. is the circle (α), and the F. F. consists of the functions belonging to a circle (ϱ) arbitrarily little greater than (β).*

2. *The series P belonging to T is complete in (α) with a corresponding domain which is at most equal to (β).*

We suppose (α) and (γ) not to be the *maximum* domains of completeness of P_2 resp. P_1. Then, since corresponding circle radii, according to our supposition, increase and decrease *continuously* with each other, there corresponds to any number ϱ surpassing β by an arbitrarily small amount, a number ϱ', such that the radius which for the series P_2 corresponds to α, i.e. γ, is less than ϱ', and the radius that for the series P_1 corresponds to ϱ', and is consequently greater than β, is less than ϱ. The transmutation P_1, and consequently also T_1, according to premise 3, is in that case normal, if we take as N. F. O. the circle (ϱ') and as N. F. F. the circle (ϱ); in the same way P_2 and T_2 are normal, if we take as N. F. O. the circle (α) and as N. F. F. the circle (ϱ') [1]. If u is a function belonging to (ϱ), then, in connection with what was just mentioned, $v = T_1(u)$ is a function belonging to (ϱ'), and $w = T_2(v)$, or

$$w = T(u),$$

a function belonging to (α). The transmutation therefore produces for all functions belonging to (ϱ), a transmuted that is regular within and on the circumference of the circle (α). Since among these functions there are as a matter of course the integral rational ones, we have, in order that the conclusion that T is normal may be admitted, only to investigate the question of continuity. This does not cause any difficulty. For according to what has just been observed in connection with premise 3, T_2 is continuous in the F. F. of functions that belong to (ϱ'), and the N. F. (α); i.e. corresponding to any positive number τ, chosen arbitrarily small, there is an amount η, such that

$$|w = T_2(v)| \leq \tau, \text{ in the closed domain } (\alpha),$$

if

$$|v| < \eta \quad , \text{ in the closed domain } (\varrho').$$

Again premise 3 includes that T_1 is continuous in the F. F. of functions belonging to (ϱ), with respect to the N. F. (ϱ'); i. e.:

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[1] If we state in future, without any additional observation that we take a circle (r) as N. F. F., we shall mean by it that the F. F. consists of *all* the functions belonging to the circle in question. This is the case with which we are principally concerned and it is therefore easy to have for it a shorter expression that characterizes the F. F.

Corresponding to any positive number τ, chosen arbitrarily small, there is an amount δ, such that

$$|v \equiv T_1(u)| < \eta, \text{ in the closed domain } (\varrho').$$

if

$$|u| < \sigma \text{ , in the closed domain } (\varrho).$$

From the two preceding conclusions we may infer that, corresponding to any positive, arbitrarily small number τ there is an amount σ, such that

$$|w \equiv T_2 T_1(u) \equiv Tu| < \tau, \text{ in the closed domain } (\alpha),$$

if

$$|u| < \delta \text{ , in the closed domain } (\varrho).$$

The transmutation T consequently is *continuous* indeed if (α) and (ϱ) are taken as corresponding numerical fields, and the proof of the normality of T is therefore established.

There remains to be proved the second point of the conclusion.

Let us consider a function u belonging to the circle (ϱ); as we saw when proving the first point, T_1 produces for such a function a transmuted belonging to (ϱ'). $T_1(u)$ may moreover, according to the functional theorem of MAC-LAURIN proved in N°. 15 (3rd communication), be represented by $P_1(u)$ in the domain (ϱ'); this too holds in connection with premise 3. We have therefore

$$v \equiv T_1(u) = \sum_{0}^{\infty}{}_k \frac{\lambda_k u^{(k)}}{k!},$$

if we indicate the coefficients of the series P_1 by $\lambda_k(x)$ or simply λ_k. This series converges *uniformly* in the domain (ϱ') (cf. N°. 4, 1st communication). According to the proposition of N°. 18 (4th communication) the transmuted $w = T_2(v)$ of v may be found in the domain (α) by applying T_2 term by term to the series, so that we have

$$T(u) = \sum_{0}^{\infty}{}_k T_2\left(\frac{\lambda_k u^{(k)}}{k!}\right) \quad \ldots\ldots\ldots \quad (55)$$

The next step we shall take consists in developing, for each term of the series obtained, the transmutation in the functional series of TAYLOR treated in N°. 19 (4th communication), and that in such a way that we shall consider in the product $\lambda^k u^{(k)}$, the factor $u^{(k)}$ as "origin" and λ^k as "increase". According to our investigation in that number, and in connection with the normality of T_2 mentioned before, the development in question is indeed valid in the domain (a), because both $u^{(k)}$ and λ_k belong to (ϱ'); for $u^{(k)}$ belongs as well as u to (ϱ), consequently certainly to $(\varrho') < (\varrho)$, and λ_k belongs to (ϱ'), because it is a coefficient of the series P_1, which is complete in (ϱ').

The development in question is produced by formula (42′) and reads [1])

$$\left.\begin{aligned} T(u) = & \left[P_2(u)\lambda_0 + P'_2(u)\,D\lambda_0 + \frac{P''_2(u)}{2!}D^2\lambda_0 + \dots\right] \\ & \left[P_2(u')\lambda_1 + P'_2(u')\,D\lambda_1 + \frac{P''_2(u')}{2!}D^2\lambda_1 + \dots\right] + \\ & \cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\ + & \frac{1}{k!}\left[P_2(u^{(k)})\lambda_k + P'_2(u^{(k)})\,D\lambda_k + \frac{P''_2(u^{(k)})}{2!}D^2\lambda_k + \dots\right] + \\ & \cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \end{aligned}\right\} . \quad (56)$$

In this, every horizontal row answers to a term $\lambda_k u^{(k)}$, so that, if the doubly infinite scheme is summed up according to *rows*, the result is finite, and equal to (55). It is necessary now to know that the result is *independent* of the grouping, and that this holds even for the *triply* infinite scheme that results from (56) by taking the series P written at full, which we shall regularly do in future. In other words we have to prove that the triple scheme is an *absolutely* convergent one, and in this respect the proposition of the preceding paragraph will be of use.

We represent the coefficients of the series P_2 by μ_k, more amply $\mu_k(x)$. If we replace λ_k and μ_k by their natural majorant-functions $\overline{\lambda_k}$ and $\overline{\mu_k}$, and if we define by means of these latter the transmutation $\overline{P_1}$ and $\overline{P_2}$, which for the sake of brevity we shall call the *natural majorants* of P_1 and P_2, the proposition in question states that $\overline{P_1}$ is complete in the domain (ϱ'), with a corresponding domain smaller than (ϱ), because this was the case with P_1; in the same way $\overline{P_2}$ is complete in (a) with a corresponding domain smaller than (ϱ'). $\overline{P_1}$ therefore represents just as P_1, a normal transmutation, if as corresponding numerical fields, the circle (ϱ') (N. F. O.) and the circle (ϱ) (N. F. F.) be taken, and $\overline{P_2}$, as well as P_2 represents a normal transmutation, if the circles (a) and (ϱ') are considered as corresponding numerical fields. The transmutation P_1 produces there-

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[1] We have written for the sake of comparison with the next scheme (56), P_2 and its derivatives instead of T_2 and its derivatives, which, according to the functional theorem of MAC LAURIN, is permitted here.

fore for the majorant-function $\overline{u}$ of u, belonging to (ϱ) as well as u does, a transmuted

$$\overline{P}_1(\overline{u}) = \sum_0^\infty{}_k \frac{\overline{\lambda_k}\,\overline{u^{(k)}}}{k!},$$

which is regular in the closed domain (ϱ'), while the series converges uniformly in that domain. From this it follows in the first place that the transmutation $\overline{P_2}$ produces for the function $\overline{P}(\overline{u})$ a transmuted that is regular in the closed domain (α), and secondly, in connection with the normality of $\overline{P}_2$ just mentioned, that this transmuted is obtained in that domain, by applying the transmutation term by term to the series found.

We arrive at

$$\overline{P}_2\overline{P}_1(\overline{u}) = \sum_0^\infty{}_k \overline{F}_2\left(\frac{\overline{\lambda_k}\,\overline{u^{(k)}}}{k!}\right). \quad . \quad . \quad . \quad . \quad . \qquad (\overline{55})$$

Finally, on similar grounds as in the case of formula (55), each term of the last sum may be developed in the functional series of TAYLOR. If in this case in the product $\overline{\lambda_k}\,\overline{u^{(k)}}$, $\overline{u_{(k)}}$ is considered as "origin" and $\overline{\lambda_k}$ as "increase" we arrive at the following scheme

$$\left.\begin{array}{rl}\overline{P}_2\overline{P}_1(\overline{u}) = & \left[\overline{P}_2(\overline{u})\,\overline{\lambda}_0 + \overline{P}'(\overline{u})\,D\overline{\lambda}_0 + \dfrac{\overline{P}_2''(\overline{u})}{2!}\,D^2\,\overline{\lambda}_0 + \ldots\right] \\ & + \left[\overline{P}_2(\overline{u'})\,\overline{\lambda}_1 + \overline{P}'_2(\overline{u'})D\overline{\lambda}_1 + \dfrac{\overline{P}_2''(\overline{u'})}{2!}\,D^2\overline{\lambda}_1 + \ldots\right] \\ & . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ & + \dfrac{1}{k!}\left[\overline{P}_2(\overline{u^{(k)}})\,\overline{\lambda_k} + \overline{P}'_2(\overline{u^{(k)}})D\overline{\lambda_k} + \dfrac{\overline{P}_2''(\overline{u^{(k)}})}{2!}\,D^2\,\overline{\lambda_k} + \ldots\right] \\ & . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{array}\right\} \quad . \qquad (\overline{56})$$

in which we conceive again the $\overline{P}$-series to be written at full, so that it is a triple scheme corresponding term by term to the triple scheme (56).

If in this *majorant-scheme* the value $x_0 + \alpha$, is given to x, so that $x - x_0$ is real and positive, it is clear that all the terms are real and positive. But we know *one* method of grouping these terms, in which we arrive at a convergent series, viz. the one that originally led to the scheme. According to a well-known proposition we may conclude from this that *any* combination of the elements into one or more simply infinite series or into a simple series of suchlike series, regularly leads to the same sum. For other values of x

in the domain (α) the absolute values of the terms, both in the first and in the second scheme are not greater than those of the corresponding ones in the second scheme for $x = x_0 + \alpha$; from this it follows again that for all suchlike values of x the elements of both schemes form an absolutely convergent aggregate, the sum of which is independent of its grouping.

We apply this result in such a way as to combine all the terms of the same power of $Du\,(D\overline{u})$ into one single term of that same power; this way of grouping produces in either case the resulting series required. This series therefore converges absolutely and uniformly in the domain (α) for all functions u belonging to (β); for if a function belongs to (β), there is also a somewhat greater circle (ϱ) to which it belongs. In other words the series is complete in the domain (α), with a corresponding domain that at most is equal to (β). Thus point 2 of the proposition has been proved.

The theorem of this paragraph has consequently been established entirely, but the object we proposed in the beginning of N°. 23 has not yet quite been reached. The theorem really states that the resulting series is complete and also gives some indications as to the dependence between domains corresponding to each other with respect to that series, but it says nothing about the way in which the coefficients may be calculated. Now we did not intend to point this out here, since it has already been done by Bourlet, who put the result produced by the scheme (56) in a definite, elegant form. We might therefore keep silent about it if from our previous considerations it followed as a matter of course that the form in question is *correct*; in fact in that case we could only *repeat* what is found in Bourlet's paper. But in order to arrive at the understanding of the correctness referred to, a further explanation seems to be necessary and we shall therefore for a moment call the attention to this point.

In determining the series P, Bourlet uses the *operative function*, introduced by him; i. e. an expression $f(x,z)$ determined by

$$f(x,z) = a_0 + \frac{a_1 z}{1!} + \frac{a_2 z^2}{2!} + \ldots, \quad . \quad . \quad . \quad . \quad (57)$$

from which the transmuting series, applied to the function u is derived, by replacing z^m by $D^m u$; at the same time it holds that the formal n^{th} derivative of this series with respect to z, is the operative function that answers to the n^{th} derivative of the transmutation P, as it is easy to see. The operative function, originally only a *symbol*, has for a series that is complete in a domain (α), the property that, if z be taken as a complex number it represents for any value of x in the domain (α), and for arbitrary values of z, a *convergent* power-series in z, and consequently also a function of z regular at $z = 0$; the name *operative*-function is justified by this. The reason of it is at once clear if we notice that the peculiar character of the *complete* transmutation consists in the coefficients $a_m(x)$ being for any value of x in the domain of completeness (α) less in absolute value than the m^{th} power of a positive number $a(\alpha)$ independent of m. If we indicate the operative-series belonging to P_1 and P_2 respectively by $f_1(x,z)$ and $f_2(x,z)$ or simply by f_1 and f_2, f_1 and f_2 are regular functions of z in a neighbourhood of $z = 0$, if x is a point of the domain (α). Since the resulting series P and $\overline{P}$, resp. arising from the schemes (56) and $(\overline{56})$, are as already found, both complete in the domain (α), the corresponding operative-series $f(x,z)$ and $F(x,z)$ will further for any value of x in the domain (α), converge as well in a neighbourhood of $z = 0$, and represent there a regular function of z.

These operative-series might also have been obtained more directly from the schemes in question, by first replacing $D^k u$ $(D^k\overline{u})$ everywhere by z^k, and afterwards collecting all the terms with z^k as a factor. This term is indeed the one to be substituted for the term with the factor $D^k u$ $(D^k\overline{u})$ in the series P $(\overline{P})$ found above; for, the latter had arisen by collecting all the terms in the original scheme with $D^k u$ $(D^k\overline{u})$ as a factor, and all these terms and no others pass into terms with z^k. Since now, in consequence of the distributive property of multiplication, the resulting coefficients both of $D^k u$ $(D^k\overline{u})$ and z^k, are equal to the sum of all separate coefficients and by the substitution in question the coefficients remain unchanged, the two resulting coefficients are equal, and consequently the sum of the terms to be substituted for the terms with the same power of $D^k u$ $(D^k\overline{u})$, is equal to the term to be substituted for their sum, as was to be proved.

The substitution in question makes the first scheme pass into

$$\left.\begin{array}{l}
\left[\lambda_0 f_2 + \dfrac{\partial \lambda_0}{\partial x}\dfrac{\partial f_2}{\partial z} + \dfrac{1}{2!}\dfrac{\partial^2 \lambda_0}{\partial x^2}\dfrac{\partial^2 f_2}{\partial z^2} + \dots\right] \\
+\left[(\lambda_1 z) f_2 + \dfrac{\partial(\lambda_1 z)}{\partial x}\dfrac{\partial f_2}{\partial z} + \dfrac{1}{2\,!}\dfrac{\partial^2(\lambda_1 z)}{\partial x^2}\dfrac{\partial^2 f_2}{\partial z^2} + \dots\right] \\
\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \\
+\left[(\lambda_1 z^k) f_2 + \dfrac{\partial(\lambda_1 z^k)}{\partial x}\dfrac{\partial f_2}{dz} + \dfrac{1}{2!}\dfrac{\partial^2(\lambda_1 z^k)}{\partial x^2}\dfrac{\partial^2 f_2}{\partial z^2} + \dots\right] \\
\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot
\end{array}\right\} \quad . \quad . \quad (58)$$

with the restriction that, in using the scheme, condensed expressions, as

$$\left(\frac{\partial^p(\lambda_q z^q)}{\partial x^p}\right)\left(\frac{\partial^p f_2}{\partial z^p}\right)$$

are again replaced by their developments according to powers of z. The scheme therefore remains for the present triply infinite. For the majorant-scheme $(\overline{56})$ a similar one as the foregoing is obtained; it is not necessary to write this down. If in this last we give to x the value $x_0 + a$ and to z a certain real positive value ζ, all elements of that aggregate are real and positive. But if these elements are grouped such that the elements with the same power ζ^k of ζ are added we get, as it follows from what we mentioned above, a convergent series. *Any* grouping of this *special* aggregate gives consequently the same sum. Again, for other values of x in the domain (a), and for values of z with a modulus not greater than ζ the elements of both aggregates are in absolute value at most equal to the corresponding ones of the *special* aggregate just mentioned. Thus the elements of the schemes for the values of x and z mentioned, form an *absolutely* convergent aggregate, the sum of which is independent of the way of grouping; the same holds of course for sub-divisions of both schemes. We therefore can also say that for a given, arbitrary value of x in the domain (a), either scheme produces for *any* grouping of the elements, *one and the same* entirely definite *collection of values*, when z takes all values in a certain neighbourhood of $z = 0$.

This aggregate of values is consequently also identical with that which was obtained for the special grouping mentioned above (combination of terms with equal powers of z), and of which we said that it forms a regular function of z in the neigbourhood mentioned. As such an aggregate of values can only be represented in *one* way by a power-series in z, the schemes may be *analytically operated* on, that is all reductions which are valid in analysis, if z is a *number*, may be performed. If such reductions be applied to sub-divisions of both schemes, they again give rise to completely definite functions of z, as will be clear at once after all that has been said, and it need never be feared that for a combination of the aggregates other than the one considered first, a functional expression in z will be arrived at leading to another and consequently wrong operative-series.

If now the scheme (58) is added according to columns, we find the expression indicated by BOURLET for the resulting operative function, viz.

$$f(x, z) = f_1 f_2 + \frac{\partial f_1}{\partial x}\frac{\partial f_2}{\partial z} + \frac{1}{2!}\frac{\partial^2 f_1}{\partial x^2}\frac{\partial^2 f_2}{\partial z^2} + \quad . \quad . \quad . \quad (59)$$

In special cases this result may be further simplified and collected into a closed expression; as stated already, the well-known analytical reductions always hold. Of course, if the result is to be used, we have always to develop it again into a power-series according to z.

We finally observe that in all that precedes we may replace the three numbers α, ϱ' ϱ, respectively by α, β, γ, if P_1 remains continuous if (γ) and (β) are taken for its associated numerical fields, and P_2 if the same be done with (α) and (γ). This is the commonest case.

25. To elucidate the general theory we shall give a few examples. We first take $T_1 = T_2 = D^{-1}$, so that $T = D^{-2}$, and further assume as before the origin O as centre of our domains, consequently $x_0 = 0$. The transmutation D^{-1}, as defined in N°. 16, is normal; the corresponding series is complete in any circle (ξ), with corresponding domain $(2\,\xi)$. For the three numbers α, γ, β, we can take here α, 2α, 4α. The premise 3. is also satisfied; for D^{-1} is not only normal, if (ξ) and $(2\,\xi)$ are considered as associated numerical fields, but also, if (ξ) and (ξ) are taken as such.

We can consequently apply our theorem, and infer from it that the transmutation $T = D^{-2}$ is normal with N.F.O. (α) and N.F.F. $(4\,\alpha)$. This is correct, for of the simple transmutation D^{-2} it is known that with preservation of (α) as N.F.O., (α) may as well be taken as N.F.F. It remains still to verify point 2. of the theorem, i.e. to prove that the series P corresponding to D^{-2} is complete in (α), with a corresponding domain that is at most equal to $(4\,\alpha)$. To this purpose we shall apply BOURLET's formula. If we put $f_1(x, z) = f_2(x, z) = \varphi(x, z)$, we have, as it appears from the series belonging to D^{-1} (see among others N°. 16),

$$\varphi(x,z) = \sum_{0}^{\infty}{}_m \frac{(-1)^m x^{m+1} z^m}{(m+1)!} = \frac{1 - e^{-xz}}{z}$$

From this it follows

$$\frac{\partial^n \varphi}{\partial x^n} = (-z)^{n-1} e^{-xz}$$

so that according to (59) we have,

$$f(x,z) = \left(\frac{1 - e^{-xz}}{z}\right)^2 - \frac{e^{-xz}}{z}\sum_{1}^{\infty}{}_n \frac{(-z)^n}{n!}\frac{\partial^n \varphi}{\partial z^n}$$

Further

$$\sum_{1}^{\infty}{}_{n} \frac{(-z)^n}{n!} \frac{\partial^n \varphi(x.z)}{\partial z^n} = \varphi(x, z-z) - \varphi(x,z),$$

and thus

$$f(x,z) = \frac{1-(1+xz)e^{-xz}}{z^2}.$$

If we develop this according to powers of z, we arrive at

$$f(x,z) = \sum_{0}^{\infty}{}_{m} \frac{(-x)^{m+2} z^m}{(m+2)m!}$$

so that to $D^{-2}u$ corresponds the series

$$Pu = \sum_{0}^{\infty}{}_{m} \frac{(-x)^{m+2} u^{(m)}}{(m+2)m!}.$$

This series, which according to the theorem is known to converge uniformly in the domain (a), for functions belonging to $(4\,a)$, does so already for functions belonging to $(2\,a)$, as may at once be verified by means of the often used majorant value for $u^{(m)}$.

26. As a second example we take $T_1 = T_2 = S_{\omega(x)}$, consequently $T = S^2{}_{\omega(x)}$, in which $S_{\omega(x)}$ is the operation of substitution, with $\omega(x)$ or ω as substitution-function, which we defined in N°. 17 and recognized there as a normal one. We consider, as we did there, a neighbourhood of the origin, and have seen then that as a pair of associated fields may serve: any pair of fields of which the N.F. is a circle (ξ) smaller than the circle of convergence (A) of $\omega(x)$, and the F. F. consists of functions belonging to the circle (σ), if σ is the maximum modulus of $\omega(x)$ in the domain (ξ). We found further that the series belonging to S is complete in any domain (ξ), as meant here, with as its corresponding domain a circle (η), the radius of which is at least equal to the number σ mentioned; from which we can at once derive that premise 3 of our theorem is also satisfied. Of the three-numbers mentioned in premise 2, a must be chosen small enough to ascertain that the number γ, which corresponds to a for the series $P_2 (= P_1)$ answering to S, is less than the radius of convergence A of ω. It may of course occur that this is impossible, viz. if a number γ *greater* than A already corresponds to $a = 0$. We shall therefore consider a case in which this last circumstance does not occur; premise 2 is then satisfied and we have

$$\gamma = a + |\omega(x_m) - x_m|, \quad \dots\dots\dots \quad (60)$$

$$\beta = \gamma + |\omega(x'_m) - x'_m|, \quad \dots\dots\dots \quad (61)$$

if x_m is the point on the circumference of (a), and x_m' that on the circumference of (γ), where $\omega(x) - x$ assumes its maximum modulus.

We can therefore apply the theorem inferring from it that the transmutation $T = S_\omega^2$, and the corresponding series P are normal, with as N. F. O. (α), and as associated N. F. F. a circle which at most is to be taken equal to (β) [1]. It may easily be verified that this is true: $T = S^2$ is evidently a new substitution with the substitution function

$$\omega_2(x) \equiv \omega\{\omega(x)\};$$

in consequence of which the resulting series P is known together with the given series P_1 and P_2, for which reason we shall not make use here of BOURLET's formula. The number that for that resulting series P corresponds to α, we call β_1; β_1 is then determined by

$$\beta_1 = \alpha + |\omega_2(x_\mu) - x_\mu|,$$

in which x_μ is the point on the circumference of (α), where $\omega_2(x) - x$ gets its greatest modulus. The result that the theorem produces, will be in accordance with it if $\beta \geq \beta_1$. This is indeed the case. In the first place $\omega(x_\mu)$ is a point that does not lie outside the circle (γ). For, as already observed, the maximum modulus

$$\sigma = |\omega(x_\sigma)|$$

of ω, on the circumference of (α), is at most equal to γ. A fortiori we have therefore

$$|\omega(x_\mu)| \leq \gamma,$$

and consequently $\omega(x_\mu)$ is a point lying either within or at most on the circumference of (γ). From this it follows again, in connection with the meaning of x_m'

$$|\omega(x'_m) - x'_m| \geq |\omega\{\omega(x_\mu)\} - \omega(x_\mu)| \equiv |\omega_2(x_\mu) - \omega(x_\mu)|. \quad . \quad (62)$$

Again we infer from the signification of x_m

$$|\omega(x_m) - x_m| \geq |\omega(x_\mu) - x_\mu|. \quad . \quad . \quad . \quad . \quad . \quad (63)$$

The relations (60), (61), (62) and (63) give now rise to the following reductions:

$$\begin{aligned}
\beta &= \alpha + |\omega(x_m) - x_m| + \omega(x'_m) - x'_m| \\
&\geq \alpha + |\omega(x_\mu) - x_\mu| + |\omega_2(x_\mu) - \omega(x_\mu| \\
&\geq \alpha + |\omega(x_\mu) - x_\mu) + \omega_2(x_\mu) - \omega(x_\mu)| \\
&\geq \alpha + |\omega_2(x_\mu) - x_\mu| \\
&\geq \beta_1,
\end{aligned}$$

which is the result required. That not only for P, but also for $T = S^2$ itself, β may be taken as N. F. F. corresponding to (α) as N. F. O., follows again from this result, if we notice that $\beta_1 \geq \sigma_1$, σ_1 being the greatest modulus of $\omega_2(x)$ in the domain (α), and that

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[1] We have comprised here point 1 and point 2 of the theorem in *one* expression.

the latter quantity indicates the radius of the smallest domain that may be taken as N. F. F. together with (α) as N. F. O.

That, besides the symbol of inequality, the symbol of equality may occur here, is proved by the special case in which $\omega = c + x$, c being a constant. Thus, in this case the majorant-value, indicated by our theorem for the number corresponding to α for the resulting series P, at the same time provides the *correct* value, from which it appears that the theorem does not necessarily give a majorant-value which is too large.

For the sake of completeness we mention that we have here for the two operative-functions $f_1 = f_2 = \varphi$

$$\varphi = \sum_{0}^{\infty}{}_m \frac{(\omega - x)^m z^m}{m!} = e^{z(\omega - x)} .$$

As the resulting transmutation is also a substitution with $\omega_2(x)$ as the function of substitution, we must have

$$f = e^{z(\omega_2 - x)},$$

which is really given by BOURLET's formula.

27. Third example. We take $T_1 = S_\omega$, $T_2 = D^{-1}$, consequently $T = S_\omega D^{-1}$. As both components are transmutations already treated in the preceding examples it may suffice to elucidate a few points. For the three numbers α, γ, β we may evidently take here: $\alpha, \gamma, 2\gamma$, if γ is again determined by (60). We shall not further explain the first part of the theorem, because the transmutation SD^{-1} is not to such a degree known as a *simple* transmutation that in our verification of the point we might refer to known things: the test would consequently be more or less a repetition of the general proof. But we may verify point 2. of the theorem, that is to say, determine either by means of BOURLET's formula or in another way, the resulting series P, and ascertain whether it satisfies the statement contained in that point.

We therefore proceed to prove that the number β_1, which, for the series P_1, corresponds to α, is smaller than

$$2\gamma = 2\alpha + 2|\omega(x_m) - x_m| \quad . \quad . \quad . \quad . \quad . \quad . \quad (64)$$

We may easily determine the series P here in a *direct* way, that is to say by means of the transmuted ξ_k of the function x^k.

Apparently we have

$$\xi_k = S_\omega D^{-1}(x_k) = \frac{\omega^{k+1}}{k+1} .$$

If this result is substituted in formula (24), we arrive after some reductions at

$$a_m = \frac{(\omega - x)^{m+1} - (-x)^{m+1}}{m+1}.$$

The quantity $a_x = \overline{lim}\,|a_m|^{\frac{1}{m}}$ is therefore equal to

$$|\omega - x| \quad \text{or} \quad |x|,$$

according as the former or the latter expression has the greatest value. The maximum value $a(\alpha)$ of a_x in the domain (α) is therefore equal to

$$|\omega(x_m) - x_m| \qquad \text{or} \qquad \alpha,$$

according as the maximum modulus of $\omega - x$ on the circumference of (α) is greater or less than α. We have therefore in these two cases resp.

$$\beta_1 = \alpha + |\omega(x_m) - x_m| \qquad \text{or} \qquad \beta_1 = 2\alpha \quad . \quad . \quad . \quad (65)$$

For the series P_2 belonging to S_ω the number that corresponds to α is now equal to the first amount, and for the series P_1 belonging to D^{-1} the number that corresponds to α is equal to the second amount. We may therefore say that for the series P, which belongs to the combination $T = S_\omega D^{-1}$, the number corresponding to α is equal to the *greater* of the two numbers that correspond to α for the *separate* series.

In any case, the verification required has been accomplished, for each of the two amounts (65) is less than (64).

28. Finally we consider the combination $D^{-1}S_\omega$. Here too it may suffice to verify point 2 of the theorem. For the three numbers α, γ, β, we may take here $\alpha, 2\alpha, \beta$, where β is determined by the formula

$$\beta = 2\alpha + |\omega(x'_m) - x'_m|, \quad . \quad . \quad . \quad . \quad . \quad . \quad (66)$$

if x'_m is the point on the circumference of (2α), where $\omega - x$ assumes its maximum modulus. The resulting series P we determine again directly. BOURLET's formula, which in the preceding case would still have been efficient, though less easy to handle, produces such an intricate form here that it is difficult to be surveyed. It is on the other hand very easy to work with the quantities ξ. We have

$$\xi_k \equiv D^{-1} S_\omega(x^k) = D^{-1}[\omega^k(x)] = \int_0^x \omega^k(t)\,dt,$$

and from this by means of formula (24)

$$a_m(x) = (\xi - x)^m = \int^x [\omega(t) - x]^m\,dt$$

Now

$$\left|\int_{0}^{x} [\omega(t) - x]^m \, dt\right| \leq \int_{0}^{x} |\omega(t) - x|^m |dt| \leq |\omega(\xi) - x|^m |x|,$$

if, in the t-plane, $t = \xi$ is the point on the straight line from O to x, where $\omega(t) - x$ gets it maximum modulus. Thus we have

$$a_x \equiv \lim |a_m|^{\frac{1}{m}} \leq |\omega(\xi) - x|$$

and therefore

$$a_x \leq |\omega(\xi) - \xi + \xi - x| \leq |\omega(\xi) - \xi| + |\xi - x|,$$

or, since x is a point of the domain (α),

$$a_x \leq |\omega(x_m) - x_m| + \alpha$$

so that further

$$a(\alpha) \leq |\omega(x_m) - x_m| + \alpha\,.$$

Thus we have finally for the number β_1, which, for the series P, corresponds to α, the inequality

$$\beta_1 \leq |\omega(x_m) - x_m| + 2\alpha$$

If this be compared with (66), and if we notice the signification of x_m and x'_m, it will be seen that we have $\beta_1 < \beta$, so that point 2 of the theorem is verified.

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CONTENTS.

Mathematics. — *"Some Considerations on Complete Transmutation"*. (Sixth Communication). By Dr. H. B. A. Bockwinkel. (Communicated by Prof. L. E. J. Brouwer).

(Communicated in the meeting of March 31, 1917).

In the preceding communication we treated of the transmutation $T = T_2\, T_1$, which is obtained when two complete transmutations T_1 and T_2 are applied to some regular function u. We saw there that the resulting transmutation is likewise *complete* in some pair or other of associated fields, the mutual dependence of the new N. F. O. and the new N. F. F. being to some extent established. We further gave a strong proof of the formula determining the resulting series P, which was furnished by Bourlet without domains of validity being mentioned by this author. As we have seen the formula expresses the so-called operative function of the resulting series P in those of the components P_1 and P_2 and differential coefficients of them. Again in giving some examples to illustrate our theorem of N°. 24, we observed that the method to find the resulting series by means of the just mentioned formula of Bourlet, is often much more difficult in practice than a somewhat more direct method, according to which first the functions $\xi_m(x) = T_2\, T_1(x^m)$ are determined, and then, by the symbolic formula (24)

$$a_m = (\xi - x)^m \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

the coefficients $a_m(x)$ of the resulting series P. Bourlet, however, has been able to apply his formula with success to questions of a more theoretical character.

The examples mentioned give rise to the question whether it is possible by means of the more direct method to find a general formula which expresses the coefficients a_m of the resulting series P in the coefficients λ_m and μ_m of the composing series. We thus arrived at a rather simple symbolic formula, which allowed us to shew again the completeness of P, the statement about corresponding domains being the same as in the foregoing communication. The investigations which led us to this result, gave us an opportunity to establish other more simple formulae, which served us to go on further, and which have moreover a certain interest in themselves. Again it seemed convenient to add some further formulae to those already obtained forming with the latter a more or less complete system. The development and the discussion of all these formulae is the subject of the following pages.

30. In all symbolic formulae to be treated of, the closed expressions occurring in the right-hand members of them must be developed according to ascending powers of one or more letters, these powers having no meaning in themselves, but obtaining one when the exponents (upper indices) are replaced by (lower) indices. Now, often certain reductions are allowed which would also be valid if the letters denoted variable *quantities*, whether or not being restricted to certain domains. Such reductions we shall call *analytical reductions*. The principal condition which should be noticed in order to be able to perform an analytical reduction with symbolic expressions is that *equal* symbols occurring in different parts of them, have the *same* meaning, this being the same fundamental condition if the letters denote numbers.

Generally speaking an analytic reduction is permitted if the *proper meaning of the result is the result of the proper meanings*, when by the latter the result is meant which would be obtained if the proper meanings were introduced *before* the reduction mentioned is performed. Thus we may have an *analytic sum* of symbols or a *product*. In the first case we shall often have to apply the rule that in a polynomial consisting of symbolic powers of the same letter, before substituting indices for exponents, terms involving *equal* powers a^k may be added analytically. For the proper meanings of such terms are quantities involving *equal* factors a_k, the coefficients in the symbolic terms being respectively equal to those in the proper ones. The sum of these coefficients multiplied by a^k is the analytic sum of the symbols and the same sum multiplied by a_k is the sum of the corresponding proper expressions. Thus the latter sum is indeed the proper meaning of the former.

If we have a *product* of symbolic powers of the same letter a, we should carefully state whether *the product of their proper meanings* is meant by it, or *the proper meaning of the analytic product*, that is of the single power which is obtained by multiplying the powers of a according to the ordinary rule giving as new exponent the sum of the partial exponents. For the proper meaning of the analytic product of a certain number of powers of the same letter a is not in general equal to the product of proper meanings of all factors.[1] We shall always have to deal with such products of

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[1] See, however, the example in N⁰. 35.

powers of a letter a that have to be multiplied analytically before the proper meanings are substituted. In other words, the proper expressions will always be *linear* functions of quantities involving the same letter a and different indices k.

To begin with we observe that the functional theorem of MAC-LAURIN, treated in the 3rd communication leads to a generalization of the symbolic formula (23)

$$\xi_m \equiv T(x^m) = (x+a)^m , \quad . \quad . \quad , \quad . \quad . \quad . \quad (23)$$

which expresses the transmuted ξ_m of the rational integral functions x^m in terms of the coefficients a_m of the series P answering to the normal transmutation T. Formula (23) is valid in any circular domain to which belong all functions a_m and ξ_m; the existence of such domains is one of the characteristics which make a transmutation normal, according to the definition we gave in N°. 15.

When the series P is complete in the domain (α) then, according to the just mentioned theorem

$$Tu = Pu \equiv \sum_0^\infty {}_m \frac{a_m u^{(m)}(x)}{m!},$$

for functions u which belong to the domain (β) corresponding to (α).

The right-hand member may apparently be denoted by the symbol $u(x+a)$, provided we interpret this in the following way: substitute for the symbol the power-series in the letter a which answers to the function $u(x+a)$ if that letter means a complex number. This power-series is unique, since x is a point in the domain (α) and u a function belonging to (β) and thus certainly to (α).[1] We therefore obtain the symbolic formula

$$T\,u(x) = u(x+a), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (67)$$

valid in (α) and of which (23) forms a particular case.[2]

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[1] Considerations of uniqueness were really of use already when in the 3rd communication we put for the formula

$$\xi_m = \sum_0^m {}_k\, m_k\, x^{m-k}\, a_k$$

the symbolic formula (23); in fact, if the expansion of $(x+a)^m$ in a power-series according to a were not unique, special reference should be made to the fact that the series in the right-hand member is meant and no other. But no one thinks of uniqueness in the development of a binomial, nor did we in writing our 3rd communication. Nevertheless, in the light of the present general developments, in which the uniqueness of a power-series forms the principal part, it seemed convenient to us to mention this point.

[2] We have to take care that in the first term of the expansion the factor a^0

For the m^{th} derivative of Tu we may as well give a symbolic formula. We saw in the 4th communication that this quantity, defined by PINCHERLE by means of (45), may in the domain (α) also be found by formula (39):

$$T^{(m)}(u)=\sum_{0}^{\infty}{}_k \frac{a_{m+k}\,u^{(k)}(x)}{k!} \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

of course for functions u belonging to (β). Instead of this formula we may write symbolically

$$T^{(m)}(u)=a^m\,u\,(x+a) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (68)$$

which formula has (67) as a particular case $(m=0)$. This might perhaps give occasion to make the mistake of substituting in the factor a^m index for exponent before developing the form $u\,(x+a)$ in a power-series of the letter a; this should first be done, then multiplication by a^m should be performed, and finally exponents should be replaced by indices.

We now come to the symbolic representation of the more general functional theorem of TAYLOR, dealt with in the 4th communication. Applying (67) to the product of the functions v and u both belonging to (β) we get

$$T(v(x)u(x))=v(x+a)\,u(x+a) \quad . \quad . \quad . \quad . \quad . \quad . \quad (69)$$

provided no other meaning be as yet assigned to it than that the right-hand member be regarded *as a whole*, according to which it has to be replaced by the power-series in a which corresponds to the function $w\,(x+a)=v\,(x+a)\,u\,(x+a)$, if a denotes a number. This power-series, however, is to be obtained by multiplying the partial series corresponding to $v\,(x+a)$ and to $u\,(x+a)$ according to the well-known rule, and then ordering the resulting aggregate so that terms involving the same power of a are combined. If, now, we collect into one all terms of the aggregate containing the *same* factor

$$\frac{a^m u^{(m)}(x)}{m!}$$

which is due to the expansion of $u\,(x+a)$, the result for all values of m is the functional series of TAYLOR. For the whole of those terms corresponding to a definite value of m is represented by

$$\frac{a^m\,v\,(x+a)}{m!}\,u^{(m)}(x),$$

which, through (68), is equal to

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is not omitted, as was the case with (23) and m.m. more general with all symbolic expansions we shall treat of.

$$\frac{T^{(m)}(v)u^{(m)}(x)}{m!}$$

But this is, if we consider $v(x)$ as "original point" and $u(x)$ as "increment", exactly the general term of the series in question, the validity of which we proved in the 4th communication. This proof[1]), as a matter of course, consists in shewing that the change in the term-grouping is permitted, the convergence of the aggregate being absolute. It may therefore be grouped in an arbitrary manner so that the symbolic formula (69) admits of the following interpretation: replace both functions $v(x+a)$ and $u(x+a)$ by their power-series in the letter a, then form the aggregate arising from the multiplicative combination of the series-terms, and substitute indices for exponents. If the so obtained aggregate be ordered according to indices of a we simply get the functional series of MAC-LAURIN for $Tw=T(vu)$; if it be ordered according to powers of Du, the functional series of TAYLOR for Tw in a "neighbourhood" of $w=v$ is obtained; if, lastly, the aggregate in question should be ordered according to powers of Dv, we should find the functional series of TAYLOR for a "neigbourhood" of $w=u$. The symbolic formula (69) contains all these different cases; we only wish to observe that, if we expand the right-hand member according to powers of Du, the general coefficient in that expansion, which is, except for the factor $1/m!$, equal to

$$a^m\, v(x+a),$$

or to $T^{(m)}(v)$, has in *this very form* a meaning only in domains (a) *smaller* than (r_1), where r_1 is the a-value to which the radius of convergence r of the function v corresponds as a β-value; whereas in N°. 20 we saw that the other form of the coefficients in question, viz. that defined by (45), possibly has a meaning in domains greater than (r_1).

31. We now come to our principal object; to construct a symbolic formula which expresses the coefficients a_m of the series P answering to the composed transmutation $T=T_2T_1$ in terms of the coefficients λ_m and μ_m of the partial series P_1 and P_2. As we said already, first the functions

$$\xi_k = T_2T_1(x^k),$$

into which T transforms the integral powers of x, are determined for the purpose, in order to derive from them, by means of

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[1]) We wish to insert here the remark that the proof we refer to becomes simpler if the majorant-functions $\overline{a}_m$ of a_m are used, as we did in the 5th communication.

formula (24) (mentioned again in the beginning of the present communication), the functions a_m. The difference from the course followed in the previous communication consists therefore in the determination of $T_2 T_1$ for the *particular* function x^k instead of at once for the arbitrary function u. This can but lead to simplification.

We retain all notations and suppositions of N°. 24, and thus especially assume the existence of three numbers α, γ, β, having the properties explained there. To begin with, we observe that x^k belongs to the circle (β), hence $T_1(x^k)$ to (γ), hence $T_2 T_1(x^k)$ to (α). In other words ξ_k is a function that is regular in the closed domain (α), and we at once add the remark that the regularity of a_m follows from this by means of (24). We further develop $T_1(x^k)$, as $T_1 u$ in N°. 24, in the series of MAC-LAURIN, which, however, here simply becomes the finite series (23) (copied in the previous paragraph). Thus the transmutation T_2 may be without any addition applied *term by term* to that series, whereas the same operation in N°. 24 wanted some further explanation the series in question being there infinite. We therefore have, in terms with *proper* meanings,

$$\xi_k = \sum_0^k {}_i\, k_i\, T_2(x^{k-i} \lambda_i),$$

valid in (α). The quantity $T_2(x^{k-i}\lambda_i)$ may in this domain be determined by means of (69), since $\lambda_i(x)$ as well as x^{k-i} belong to (γ); this gives

$$T_2(x^{k-i}\lambda_i) = (x+\mu)^{k-i}\, \lambda_i\,(x+\mu).$$

Substituting this result in the foregoing formula we find

$$\xi_k = \sum_0^k {}_i\, k_i\,(x+\mu)^{k-i}\, \lambda_i\,(x+\mu), \quad . \quad . \quad . \quad . \quad . \quad (70)$$

without anything wanting to be proved, provided we replace each of the $k+1$ terms of this series separately by its own proper meaning, and add them after this being done. The proper meaning in question is: substitute for the expressions $(x+\mu)^{k-i}$ and $\lambda_i(x+\mu)$ their power-series in μ, multiply those series term by term, and finally replace the exponents of μ by indices: then, the so obtained aggregate converges absolutely and uniformly in (α). But the same holds for each new aggregate that arises from the collection of a *finite* number of suchlike aggregates. Thus the $k+1$ aggregates corresponding to the right-hand member of (70) need not be kept apart from one another.

One method of grouping the elements of the aggregate consists in taking all those elements with the same index of μ, or, if indices have not yet been substituted for exponents, with the same exponent, together, and thus we may interpret the right-hand member of (70) as follows: replace it by its expansion in a power-series of the letter μ and substitute indices for exponents. That a uniquely determined power-series corresponds to the right-hand member of (70) hardly needs any further mentioning, this having already been stated for each of the $k+1$ terms separately. The manner of grouping considered here makes it clear, however, that the expression in question may be transformed analytically before proceeding to its interpretation, owing to the fact that a function in the neighbourhood of a regular point can but be expanded in *one* power-series. This remark will be of use when λ's of *different* indices are in some relation to each other so that further reductions of (70) are possible. But a *general* reduction of (70) is not possible since in none of the $k+1$ terms of the series occur terms with the same index at λ.

But further symbolization of the formula for ξ_k is possible if we replace the index at the letter λ by an exponent; if, at the same time, we omit for a moment the form $(x+\mu)$ from λ, we way write

$$\xi_k = \sum_{0}^{k} {}_i\; k_i (x+\mu)^{k-i} \lambda^i, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (70')$$

If this be interpreted such that, before performing other reductions, the exponent of λ be replaced by an index and the form $(x+\mu)$ be added, the foregoing formula is produced again and there is nothing to be established. But a new result is obtained if we do not consider each of the $k+1$ nembers of the sum *as a whole*, but every product $(x+\mu)^{k-i+1}\,\lambda$, where λ^i stands for $\lambda_i(x+\mu)$, as the sum of $k-i+1$ magnitudes the symbolic representation of which is obtained by the development of the binomial $(x+\mu)^{k-i+1}$ and the multiplication of each of its terms by λ^i as if λ and μ were numbers [1]). The total symbolic aggregate obtained in that way from (70′) is an *ending* power-series in λ and μ, so that any other development of (70′) than the special one mentioned leads to the same power-series. Now the expression in question can be analytically reduced to $(\lambda+\mu+x)^k$, so that finally we have the symbolic formula

$$\xi_k = (\lambda+\mu+x)^k, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (71)$$

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[1]) The correctness of this interpretation of the product mentioned has been pointed out at the end of the previous paragraph, and, as is evident from the exposition there, the interpretation consists in considering the product as the symbolic representation of the expansion in the TAYLOR series of $T_2[x^{k-i}\lambda_i(x)]$, $\lambda_i(x)$ being the "origin" and x^{k-i} the 'increment". This is contrary to what in N⁰. 24 led to the formula of BOURLET, where we took $\lambda_i(x)$ as the "increment" and $u^{(i)}(x)$ as the "origin".

the interpretation of which is implied in what precedes. We only wish to call the attention to the characteristic fact that the letters λ and μ must not at all be treated in the same manner: first comes the change of exponents into indices of λ, then the same change with regard to μ.

Finally the last step: the determination of the coefficients a_m from the quantities ξ_k by means of (24). If we put in this formula the right-hand member of (71) we find

$$a_m = \sum_0^m {}_k\, m_k\,(-x)^{m-k}\,(\lambda+\mu+x)^k \quad . \quad . \quad . \quad . \quad . \qquad (72)$$

and there is nothing to be proved, if we substitute in each of the $m+1$ members of this sum separately for $(\lambda+\mu+x)^k$ its proper meaning. In order to get this latter we must expand the trinomial in its power-series in λ and μ: each of the terms then has its own real value as is explained above, and the same therefore holds for the product of such a term by the factor $m_k(-x)^{m-k}$. We thus obtain for each of the $m+1$ members of (72) an aggregate consisting of a finite number of elements each of which is characterized by a definite symbolic power of λ and μ. The total number of elements arising from the $m+1$ members is therefore also finite, so that it forms a new aggregate that may be arranged arbitrarily. If this be done in such a way that terms involving the same powers of λ and μ are collected — these may be added analytically, the meaning of a product $\lambda^p \mu^q$ depending only on the exponents p and q and not on its source — then we obtain a power-series in λ and μ. But the *same* power-series evidently corresponds to all expressions which can be derived analytically from the right-hand member of (72). Since, now, this latter is equal to $(\lambda+\mu)^m$, we may finally write

$$a_m = (\lambda+\mu)^m = (\{\lambda\}_{x+\mu}+\mu)^m \quad . \quad . \quad . \quad . \quad . \quad . \qquad (73)$$

where the last member shews more explicitly the signification which is to be assigned to the formula. This is as follows: expand the binomial $(\lambda+\mu)^m$ analytically in its power-series in λ and μ; substitute indices for the exponents of λ and in $\lambda_i(x)$ replace x by $x+\mu$; again develop the so obtained functional expression in a series according to ascending powers of μ and finally substitute in these powers indices for exponents.

This is the symbolic formula we had in view, expressing the coefficients a_m of the resulting series P in terms of the coefficients λ_m and μ_m of the components P_1 and P_2. In deriving this formula we have met with another, viz (71), which expresses the resulting quantities ξ_m also in λ_m and μ_m. But if we want this formula as a *final* result we had better write it in the following simpler form

$$\xi_m = (\lambda + x)^m = \{(\lambda + x)^m\}_{x+\mu} . \quad . \quad . \quad . \quad . \quad . \quad (74)$$

32. Formulae (73) and (74) are valid in the domain (α), as it has been shewn in the foregoing paragraph. It still remains to be proved by means of (73) that the resulting series P is complete in (α) with a corresponding domain that is at most equal to (β), a statement we gave in the previous communication. To do this we shall make use of the following proposition, the proof of which we do not give, first because it is very easy, and secondly because the proposition may perhaps be established elsewhere:

The upper limit for $m = \infty$

$$\overline{\lim_{m=\infty}} \left| P_m + Q_m \right|^{\frac{1}{m}} ,$$

of the m^{th} *root of the modulus of the sum of two complex quantities* P_m *and* Q_m, *both defined in the aggregate of positive integral m-values, is equal to the greatest of the two upper limits*

$$\overline{\lim_{m=\infty}} \left| P_m \right|^{\frac{1}{m}} \quad , \quad \overline{\lim_{m=\infty}} \left| Q_m \right|^{\frac{1}{m}}$$

of the m^{th} *roots of the moduli of those two quantities separately. If the two latter limits be equal then the former is never greater than each of them.*

An analogous proposition is, as a corollary of the one just mentioned, valid for a sum consisting of an arbitrary finite number of terms, this number not depending on m.

The proposition will serve us to investigate the m^{th} root of the modulus of the coefficient $a_m(x)$ of the resulting series P. If we work out the righthand member of (73) in the prescribed manner, we obtain

$$a_m = \sum_{0}^{m} {}_k \, m_k \, \mu^{m-k} \left[\lambda_k + \frac{\mu}{1} \lambda_k{}' + \frac{\mu^2}{2!} \lambda_k{}' + \dots \right] =$$

$$= \sum_{0}^{m} {}_k \left[m_k \sum_{0}^{\infty} {}_i \frac{\mu_{m-k+i} \, \lambda_k{}^{(i)}}{i\,!} \right], \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (75)$$

this equality containing only *proper* expressions. We assume again, as in the previous communication, that (γ) is not the *maximum* domain of completeness for the series P_1, so that there is a domain $(\gamma') > (\gamma)$, in which P_1 is likewise complete; let the domain corresponding to this latter be denoted by (β'). We may suppose β' to be *arbitrarily little* greater than β — provided γ' be chosen *sufficiently little* greater than γ — if we assume at the same time, as we did in the previous communication, that α and β *increase and decrease continuously* with each other. Let further $L_k(\gamma')$ be the maximum modulus of λ_k on the circumference of the circle (γ'). There is, on account of the completeness of P_1 and P_2 mentioned above, corresponding to any arbitrarily small chosen number ε a whole number E such that for $n > E$

$$L_n(\gamma') \leq (\beta' - \gamma' + \varepsilon)^n , \quad . \quad . \quad . \quad . \quad . \quad (76)$$

together with

$$|\mu_n| \leq (\gamma - \alpha + \varepsilon)^n , \qquad \text{if} \qquad |x| \leq \alpha \quad . \quad . \quad . \quad (77)$$

Further we have in the domain (α) for all integral not negative values of i and k

$$\left|\frac{\lambda_k^{(i)}}{i!}\right| < \frac{\alpha L_k(\gamma')}{(\gamma'-\alpha)^{i+1}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (78)$$

We now suppose m to be chosen greater than $2E$ and on that supposition divide the double sum (75) into the following four parts, which we denote for brevity by their limits only,

$$s_1 = \sum_{E}^{m-E}{}_k \sum_{0}^{\infty}{}_i \ , \quad s_2 = \sum_{0}^{E-1}{}_k \sum_{0}^{\infty}{}_i \ , \quad s_3 = \sum_{m-E+1}^{m}{}_k \sum_{E}^{\infty}{}_i \ , \quad s_4 = \sum_{m-E+1}^{m}{}_k \sum_{0}^{E-1}{}_i \ .$$

Further we assume ε, *after* γ', to be so chosen that $\gamma + \varepsilon < \gamma'$, say $\gamma' = \gamma + \varepsilon + \delta$. Then we find for the first three sums by means of the inequalities (77) and (78)

$$|s_1| < \frac{\alpha}{\delta} \sum_{E}^{m-E}{}_k \, m_k \, L_k(\gamma') \, (\gamma - \alpha + \varepsilon)^{m-k}$$

$$|s_2| < \frac{\alpha}{\delta} \sum_{0}^{E-1}{}_k \, m_k \, L_k(\gamma') \, (\gamma - \alpha + \varepsilon)^{m-k}$$

$$|s_3| < \frac{\alpha}{\delta} \left(\frac{\gamma-\alpha+\varepsilon}{\gamma'-\alpha}\right)^E \sum_{m-E+1}^{m}{}_k \, m_k \, L_k(\gamma') \, (\gamma - \alpha + \varepsilon)^{m-k}.$$

In the first and third sums moreover the inequality (76) can be applied, and thus we find for s_1

$$|s_1| < \frac{\alpha}{\delta} \sum_{E}^{m-E}{}_k \, m_k \, (\beta' - \gamma' + \varepsilon)^k (\gamma - \alpha + \varepsilon)^{m-k} < \frac{\alpha}{\delta} \sum_{0}^{m}{}_k <$$

$$< \frac{\alpha}{\delta} (\beta' - \gamma' + \gamma - \alpha + 2\varepsilon)^m < \frac{\alpha}{\delta} (\beta' - \alpha + 2\varepsilon)^m$$

For s_3 we find by analogous reductions

$$|s_3| < \frac{a}{\delta}\left(\frac{\gamma-\alpha+\varepsilon}{\gamma'-\alpha}\right)^E (\beta'-\alpha+2\varepsilon)^m$$

$$< \frac{a}{\delta}\left(\frac{\gamma-\alpha+\varepsilon}{\gamma-\alpha+\varepsilon+\delta}\right)^E (\beta'-\alpha+2\varepsilon)^m < \frac{a}{\delta}(\beta'-\alpha+2\varepsilon)^m$$

From this it may be inferred

$$\overline{\lim_{m=\infty}}\, |s_1|^{\frac{1}{m}} \leq \beta-\alpha \quad , \quad \overline{\lim_{m=\infty}}\, |s_3|^{\frac{1}{m}} \leq \beta-\alpha,$$

since ε and $\beta'-\beta$ may be supposed arbitrarily small.

As for s_2, in this sum we cannot assign a majorant-value for the quantity $L_k(\gamma')$. But the number of terms of s_2 is a *fixed* one *not depending on m*. Thus we need only calculate, according to the lemma at the beginning of this paragraph, the required limit for each term separately and then for the whole that limit is in any case not greater. In no one of the terms the factor $L_k(\gamma')$ depends on m, so that this factor gives the amount 1 for the required limit and, therefore, does not influence it.

If we further notice that for a given value of k not depending on m the limit for $m=\infty$ of $m_k^{\frac{1}{m}}$ is also 1, and that, finally, ε may be chosen arbitrarily small, we infer that

$$\overline{\lim_{m=\infty}}\, |s_2|^{\frac{1}{m}} \leq \gamma-\alpha.$$

Lastly we consider s_4; substituting $k=m-k'$, and then omitting again the accent at the letter k, we find

$$s_4 = \sum_{0}^{E-1}{}_k\, m_k \sum_{0}^{E-1}{}_i\, \frac{\mu_{k+i}\, \lambda^{(i)}_{m-k}}{i\,!}$$

The double summation extends over a finite number of terms, which number is independent of m; each of the terms may be identified by fixed values of i and k, likewise independent of m, so that it is sufficient for our purpose to consider the terms separately. To such a term we may apply the inequalities (76) and (78), giving

$$\left|\frac{m_k\, \mu_{k+i}\, \lambda^{(i)}_{m-k}}{i\,!}\right| < \frac{a\,|\mu_{k+i}|\, m_k(\beta'-\gamma'+\varepsilon)^{m-k}}{(\gamma'-\alpha)^{i+1}}$$

By remarks analogous to those made with regard to the preceding sum we infer from this

$$\overline{\lim_{m=\infty}}\, |s_4|^{\frac{1}{m}} \leq \beta-\gamma$$

None of the four limits is, therefore, greater than $\beta - \alpha$, since γ is at least equal to α and at most equal to β. Hence for the whole sum (75) the limit in question is not greater either than $\beta - \alpha$. Thus the radius corresponding to α is for the resulting series P at most equal to $\alpha + (\beta - \alpha) = \beta$; the required result has therefore been established.

33. We may say that with the foregoing developments our original object has been performed: to find a symbolic formula expressing the coefficients of the *resultant* of two complete transmuting series in the coefficients of these two; to fix the domain of validity of this formula; finally to derive from it the statement that the resultant transmutation is likewise complete; as to the last point, we found the same result with regard to the dependence between two corresponding domains as was the case in the proof we gave of the formula of BOURLET.

Before, however, finishing our considerations on the subject we wish to establish a few other formulae constituting with those already found a sort of closed system. In the first place we have in view the generalization of the formulae found in N°. 31 for more than two transmutations. It will appear to be sufficient if we take only *three* transmutations, represented by the series P_1, P_2 and P_3. We thereby assume that it is possible to assign *four* numbers: α_3, α_2, α_1, α_0 such that P_1 is complete in a circular domain (α_1) with corresponding domain (α_0), P_2 in a domain (α_2) with corresponding domain $\alpha_1)$, P_3 in a domain (α_3) with corresponding domain (α_2). Let the coefficients of the series be denoted respectively by $a_{1,m}(x)$, $a_{2,m}(x)$, $a_{3,m}(x)$, those of the resultant P_{II} of P_1 and P_2 by $a_{II,m}(x)$ and those of the total resultant P_{III} by $a_{III,m}(x)$. Then we have

$$a_{II,m} = (\{a_1\}_{x+a_2} + a_2)^m \quad . \quad . \quad . \quad . \quad . \quad . \quad (79)$$

valid in (α_2); further, since the series P_{II} is complete in (α_2), with a corresponding domain not greater than (α_0), we also have

$$a_{III,m} = (\{a_{II}\}_{x+a_3} + a_3)^m \quad . \quad . \quad . \quad . \quad . \quad . \quad (80)$$

valid in (α_3), and P_{III} is complete in the domain (α_3), with a corresponding domain which is at most equal to (α_0); all this is to be inferred from (73) and what has been stated about this formula. The statement that the resultant P_{III} is *complete* so that as a domain of completeness comes into account that of the *last* component, the corresponding domain being at most equal to that which for the *first* component corresponds to its domain of completeness, is thus easily proved, the generalisation for n components being at once evident. It remains only to combine the two preceding formulae, so that the resulting coefficient $a_{III,m}$ may be expressed in terms of the coefficients $a_{1,m}$, $a_{2,m}$, $a_{3,m}$.

We consequently work out the right-hand member of (80) according to the rule at the end of N°. 31, and then find the following new symbolic formula which answers to the earlier form (70) of formula (73),

$$a_{III,m} = \sum_{0}^{m} k \; m_k \, a_3^{\,m-k} \, a_{II,k}\,(x+a_3) \quad . \quad . \quad . \quad . \quad (80')$$

The right-hand member must be developed according to ascending powers of a_3; that this power-series is entirely determined follows from our detailed investigation in N°. 31, according to which the function $a_{II,m}\,(x)$ is regular in the domain (a_2) and therefore also in (a_3). Further the exponents of a_3 have to be replaced by indices; the so obtained aggregate converges, according to the investigation mentioned, absolutely and uniformly in the domain (a_3) and the same holds for each of the $m+1$ aggregates that can separately be derived from the members of the sum (80′): this latter assertion, moreover, corresponds to an *earlier* stage of the interpretation of (70). The function $a_{II,m}\,(x)$ has been considered in this as a whole, but now it must be determined by means of (79), making formula (80′) pass into

$$a_{III,m} = \sum_{0}^{m} k \; m_k a_3^{\,m-k} \left\{ \left(\{a_1\}_{x+a_2} + a_2 \right)^k \right\}_{x+a_3} \quad . \quad . \quad . \quad . \quad (81)$$

with the same signification, provided the factor between braces be interpreted as a whole, according to the rule prescribed for the working out of (79), substituting *at the end* of the process $x+a_3$ for x [1]). The latter formula we may write in a more simple manner thus

$$a_{III,m} \doteq \sum_{0}^{m} k \; m_k a_3^{\,m-k} (a_1 + a_2)^k \quad . \quad . \quad . \quad . \quad . \quad (81')$$

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[1]) By the words *at the end* we mean: *after the function* $a_{II}\,(x)$ *having been constructed as a whole.* Formula (81) would not express the same thing as (80′) if we interpreted it such that *in each element* of the infinite aggregate that is, in general, obtained from (79) for $a_{II,k}$, x were to be replaced by $x+a_3$. Meanwhile we may observe that the latter mode of calculating $a_{II,k}$ might really be applied, as it will be seen on noticing that the preceding investigations remain intact if the coefficients a_1, a_2, a_3 be replaced by their natural majorants. This remark may be of use in theoretical questions.

this form having the same meaning as the foregoing. But the latter formula suggests the idea that we shall finally have

$$a_{III,m} = (a_1 + a_2 + a_3)^m \equiv [\{(a_1)_{x+a_2} + a_2\}_{x+a_3} + a_3]^m, \quad . \quad . \quad (82)$$

the last member of which points out the signification in a more detailed manner. This is: expand the trinomial $(a_1 + a_2 + a_3)^m$, as if a_1, a_2, a_3 were numbers; replace in each individual term of that expansion, viz.

$$C a_1{}^g a_2{}^h a_3{}^i, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (83)$$

where C is a whole number only depending on the exponents g, h, i, the exponent g by an index and in $a_{1,g}(x)$ the letter x by $x + a_2$; then expand every expression

$$a_2{}^h a_{1,g}(x + a_2)$$

in a series of powers of a_2 and replace the exponents of a_2 by indices; in the functional expression represented by the now obtained aggregate, or in the aggregate itself, [1]) replace the letter x by $x + a_3$; expand the product of the latter expression by $a_3{}^i$ in a power-series of a_3 and replace the exponents of a_3 by indices; then there results an aggregate that, together with those obtained from the other terms arising, like (83), from the trinomial (82), represents the required function $a_{III,m}$ in the domain (α_3).

We can easily see that the transition from (81) to (82) is allowed. For looking into the matter thoroughly we see that the interpretation of (82) may be obtained from that of (81) by changing the latter only so far as to consider, in the development of the trinomial in question, all terms involving the same power of a_3 as an irresoluble whole. *It is not at all a matter of course* that the two points of view agree, nor even that the aggregates corresponding separately to each of the terms mentioned *converge*. But we may again state that the whole reasoning remains valid if we substitute for the functions a_1, a_2, a_3 their *natural majorants* $\overline{a}_1, \overline{a}_2, \overline{a}_3$, and thus we infer that there is no difference in the results afforded by the two points of view.

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[1]) See the foregoing footnote.

Mathematics. – *"Some Considerations on Complete Transmutation".* (Seventh communication). By Dr. H. B. A. BOCKWINKEL. (Communicated by Prof. L. E. J. BROUWER).

(Communicated in the meeting of April 27, 1917).

34. As a counterpart of the formula that expresses the coefficients of the resultant of a certain number of complete transmuting series in terms of the coefficients of the components we shall treat of the formula which expresses the quantities ξ for the resultant in terms of the same quantities for the components. It is convenient always to consider a neighbourhood of the origin; or, if not, to appropriate the symbol ξ_m to the transmuted of $(x-x_0)^m$, instead of to that of x^m, x_0 being the common centre of the circular domains considered; if, in the latter case, we assume $x-x_0$ as a new variable, all is reduced to the former.

We denote the functions mentioned for the series $P_1, P_2, \ldots$ respectively by $\xi_{1,m}$ $\xi_{2,m}, \ldots$, the resultant for two, three, ... series by $\xi_{II,m}, \xi_{III,m}, \ldots$; further we retain the notations and suppositions of the preceding paragraph. Taking first two components, we have

$$\xi_{II,m} = P_2\, P_1\, (x^m) = P_2\, (\xi_{1,m})$$

The function $\xi_{1,m}\,(x)$ belongs to the circle (α_1), thus we have by (67) in the domain (α_2)

$$\xi_{II,m} = \xi_{1,m}\,(x + a_2) \equiv \sum_{0}^{\infty}{}_{i}\, \frac{a_{2,i}\, \xi^{(i)}_{1,m}}{i\,!}, \quad . \quad , \quad . \qquad (84)$$

where the last member expresses the proper meaning. Now a_{2i} can be expressed by means of the functions $\xi_{2,m}$ according to the symbolic formula (23), giving

$$\xi_{II,m} = \sum_{0}^{\infty}{}_{i}\, \frac{(\xi_2 - x)^i\, \xi^{(i)}_{1,m}\,(x)}{i\,!} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (84')$$

provided the substitution of $(\xi_2—x)^i$ for $a_{2,i}$ provisionally does not mean any other thing than a *definite mode of calculation* of $a_2,^i$ considered as a whole. We now notice again that a corresponding formula, *with the very same domain of validity,* applies in the case that arises from the present one by replacing the functions

$a_{1,m}, a_{2,m}, \ldots$ by their natural majorants; and we infer from (23) that the functions $\xi_{1,m}, \xi_{2,m}, \ldots$ are in this case also replaced by majorant-functions[1]), though, these need not be the *natural* majorants of the former functions. In order to avoid reproductions of formulae taking up much room we propose to imagine for a moment that formula (84) relates to the last-mentioned case. But even so we are not justified yet in considering the aggregate arising from the expansion of each binomial of the series in (84′) as an absolutely converging one, if x lies in the domain (α_2), because it is not known whether we may replace $-x$ by x in that expansion. We therefore make an estimate of the magnitude of the sum

$$\sum_{0}^{i}{}_{h}\, i_h\, x^h\, \xi_{2,\, i-h}\,,$$

which we may denote by the symbol $(\xi + x)^i$, and to this purpose remark in the first place that the quantities ξ_m for a *complete* transmutation satisfy the same characteristic property as the coefficients a_m of the corresponding series: to be smaller, as to their moduli, than the m^{th} power of a number which is independent of m. This follows from formula (23). For from and after some value of k ($k = E$) we have in a domain of completeness (α), $|a_k| < (a + \varepsilon)^k$, where a depends on α, and ε may be chosen arbitrarily small. Then by (23) and supposing $m \overline{>} E$

$$\begin{aligned} |\xi_m| &\leq \sum_{0}^{E-1}{}_{k}\, m_k\, \alpha^{m-k}\, |a_k| + \sum_{E}^{m}{}_{k}\, m_k\, \alpha^{m-k}\, (a + \varepsilon)^k \\ &\leq \longleftrightarrow + \sum_{0}^{m}{}_{k} \longleftrightarrow \\ &\leq \longleftrightarrow + (a + \alpha + \varepsilon)^m \end{aligned}$$

The latter right-hand member consists of a finite number of terms, viz. $E + 1$, which is independent of m; thus it is sufficient, in order to calculate a majorant value for $\overline{lim}\, |\xi_m|^{\frac{1}{m}}$, to determine the corresponding limit for each of the terms individually: the greatest among these limits will, according to the lemma of N°. 32, be a majorant value as required. Now for each of the first E terms the limit in question is clearly not greater than α; and for the last term it is $a + \alpha = \beta$; thus β is a majorant-value as was required. We have now proved the following proposition:

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[1]) This, if an arbitrary point x_0 is the centre of the domains, is valid only if by ξ_m be denoted the transmuted of $(x - x_0)^m$.

If in all points x of a domain (α), centre the origin

$$\overline{\lim_{m=\infty}} \, |a_m|^{\frac{1}{m}} \leq a \, , \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (85)$$

where a_m for every integral value of m is a regular function of x in that domain, then in the same points we have for the symbolic binomial expression $(x + a)^m$.

$$\overline{\lim_{m=\infty}} \, |(x + a)^m|^{\frac{1}{m}} \leq \alpha + a \quad . \; . \; . \; . \; . \; . \; . \quad (86)$$

And as a consequence of this:

If a transmutation be complete in a circular domain (α), centre the origin, and the domain corresponding to (α) be (β), then the upper limit,

$$\overline{\lim_{m=\infty}} \, |\xi_m|^{\frac{1}{m}}$$

of the m^{th} root of the modulus of the transmuted ξ_m of x^m is not greater than β. [1])

The same proposition holds if for ξ_m the above mentioned majorant-function be substituted, the value of β being unaltered as it follows from the lemma in the last section of N°. 23 (5th communication).

If, now, we apply the preceding result to the above case, we find that in all points x of a domain (α) not greater than (α_2)

$$\overline{\lim_{m=\infty}} \, |\xi_{2,m}|^{\frac{1}{m}} \leq \alpha + (\alpha_1 - \alpha_2) \quad . \; . \; . \; . \; . \; . \; . \quad (87)$$

since α_1 is the number that for the series P_2 corresponds to α_2 and the difference between corresponding radii α and β does not increase if α diminishes. Further it may be inferred from (87) in the same way as (86) followed from (85)

$$\overline{\lim_{m=\infty}} \, |(\xi_{2,m} + x)^m|^{\frac{1}{m}} \leq 2\alpha + \alpha_1 - \alpha_2 .$$

This is the inequality we wished to obtain. It follows that the series arising from (84′) by substituting the sign $+$ for $-$ in $(\xi_2 - x)^i$, and by replacing the functions $\xi_{1,m}$ and $\xi_{2,m}$ by the above mentioned majorant-functions, converges in any domain (α) when

$$3\alpha + \alpha_1 - \alpha_2 < \alpha_1 \, , \quad \text{or} \quad \alpha < \tfrac{1}{3} \alpha_2 \, ,$$

because $\xi_{1,m}$ is regular in (α_1); a further explanation may be superfluous since it would be a repetition of what has been stated more

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[1]) For a domain of an arbitrary point x_0 the same holds as to the transmuted of $(x - x_0)^m$.

than once in the course of our developments of the theory of complete series. Thus the aggregate arising after the expansion of the symbolic binomial in every term of the series in (84′) will converge *absolutely* in a domain (a) satisfying the foregoing condition; and in this case it may therefore be ordered arbitrarily. We do this in such a way that elements involving symbols $\xi_{2,i}$ with the same index i — or symbolic powers $\xi_2{}^i$ with the same exponent i, if exponents have not yet been replaced by indices — are collected into one, and then may find the new arising coefficient in a simple manner. First we remark that, if ξ_2 denoted a certain *number*, the right-hand member of (84′) would represent the formal expansion of the magnitude $\xi_{1,m}(\xi_2)$ at the point $\xi_2 = x$. Now it is a well-known truth in the theory of functions that the *formal* development of the function $f(y)$ at the point $y = 0$,

$$f(y) = \sum_{0}^{\infty}{}_{i} \frac{y^k f^{(k)}(0)}{k!}$$

may be obtained from that at the point $y = x$,

$$f(y) = \sum_{0}^{\infty}{}_{i} \frac{(y-x)^i f^{(i)}(x)}{i!},$$

by expanding every binomial expression in the latter series and then collecting in the so obtained aggregate terms involving the same power y^k of y: the resulting coefficient of y^k is the *formal* expansion of

$$\frac{f^{(k)}(0)}{k!}$$

at the point x. We intentionally speak of *formal* expansion, because it may happen that there is no value of y for which the two series converge, the circles of convergence of $f(y)$ lying wholly outside each other. Or, if there does exist a value as indicated, it may happen that the just-mentioned power-series for the expression $f^{(k)}(0) : k!$ diverges because the circle of convergence of the function $f(z)$ for the value $z = x$ does not contain the point $z = 0$. The statement applies in any case since the *general form* of the expansions is independent of the particular character of the function in question and functions do occur, viz. the integral transcendental functions, for which the two series converge in the whole plane.

If now we apply the foregoing considerations in the present case, we infer that the required coefficient of $\xi_2{}^k$ is the formal expansion in a power-series of $\xi_{1,m}^{(k)}(0) : k!$ at the point x. But this *formal* expansion is here also *essential* if x lies in the domain $(\frac{1}{3}\alpha_2)$, for the convergence of the series is in this case included as a special result in the one just obtained viz. that the aggregate in question is absolutely converging in $(\frac{1}{3}\alpha_2)$. The coefficient we treat of is therefore equal in value to $\xi_{1,m}^{(k)}(0) : k!$, so that we may write

$$\xi_{II,m} = \{\xi_{1,m}\}_{\xi_2} = \sum_0^\infty{}_k \frac{\xi_{2,k}(x)\xi_{1,m}^{(k)}(0)}{k!}, \quad . \quad . \quad . \quad . \quad . \quad (88)$$

where the last member shews the meaning of the symbolic second member: in $\xi_{1,m}(x)$ we have to replace x by ξ_2, then to expand the expression $\xi_{1,m}(\xi_2)$, as if ξ_2 were a number, in its power-series of MAC-LAURIN and finally to substitute indices for exponents in the symbolic powers of ξ_2. As already remarked the formula is valid in any domain (a) the radius of which is not greater than $\frac{1}{3}\alpha_2$. The series in the last member, however, converges in the whole domain (α_2), since the limit in the left-hand member of (87) for $a = \alpha_2$ is, according to the very same inequality, less than α_1 and $\xi_{1,m}(x)$ is regular in the closed domain (α_1); further explanation may, as above in an analogous case, be omitted. The convergence of the series is also *uniform* in (α_2), its terms are regular functions of x in that domain, hence its sum represents also a regular function there. This latter must be identical with $\xi_{\text{II},m}(x)$ since this function is also regular in (α_2) and the two functions agree already in a finite part of (α_2) viz. $(\frac{1}{3}\alpha_2)$. Thus, finally, the symbolic formula (88) is valid in the same domain as its counterpart (73).

The generalization of (88) is at once obvious. Evidently we have

$$\xi_{III,m} = \{\xi_{II,m}\}_{\xi_3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (89)$$

valid in the domain (α_3), because $\xi_{\text{II},m}$ belongs to the domain (α_2). In connection with (88) this gives

$$\xi_{III,m} = \{\{\xi_{1,m}\}_{\xi_2}\}_{\xi_3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (90)$$

meaning: $\xi_{1,m}(x)$ has to be developed according to powers of x and x^i must be replaced by $\xi_{2,i}(x)$, then the resulting series, which is absolutely and uniformly convergent in (α_2), represents the function $\xi_{\text{II},m}(x)$, which is regular in the same domain; again expand this function according to powers of x and replace x^i by $\xi_{3,i}(x)$; the resulting series, which is absolutely and uniformly converging in the domain (α_3), represents the function $\xi_{\text{III},m}(x)$, regular in that domain.

It will be convenient to observe that the interpretation given just now does not at all differ from the one corresponding to the pair of formulae (88) and (89) before they are replaced by (90), so that there is nothing to be proved in doing this. Matters would be different if we wanted to effect the final operation of the given rule on the infinite series which we have obtained for $\xi_{II,m}(x)$, and not on this function considered as a whole. But that the second interpretation will do as well, follows again from the consideration of the natural majorants of $a_{1,m}$, $a_{2,m}$, ...

35. In applying the preceding symbolic formulae to particular cases special reductions are often necessary which have to be justified individually. Only in order to call the attention to this point we shall discuss one or two examples, but for the rest further explanations by means of examples of the symbolic formulae may be omitted after the detailed consideration of examples with the transmutations D^{-1} and S_ω in the previous communication, the more so as the formulae in question provide the same *general* results relative to resultant magnitudes as the formula of Bourlet.

We first take the case $T_1 = T_2 = S_\omega$. Here

$$a_{1,i} = a_{2,i} = (\omega - x)^i .$$

Applying (73) we successively obtain

$$a_{II,m} = \sum_0^m {}_k m_k \, a_2^{m-k} a_{1,k} (x + a_2) = \sum_0^m {}_k m_k \, a_2^{m-k} [\omega (x + a_2) - (x + a_2)]^k$$
$$= [\omega (x + a_2) - x]^m .$$

Here the reduction of the last member but one to the last is allowed in consequence of the particular form of the given transmutation. Nevertheless the exactness of the reduction is included in the *general* considerations according to which analytic reductions are permitted. The resulting expression must now be expanded in a power-series of a_2, which must really exist, according to the general theory, if x lies in the domain (a_2); this is in fact the case since the circle of convergence of ω is greater than a domain of completeness (a_1) of S_ω and thus a fortiori greater than (a_2). We may also obtain the power-series in question by raising that for $m = 1$ to the m^{th} power, according to the common rule for the involution of an infinite series; this is meant, when we write

$$a_{II,m} = \left[\sum_0^\infty {}_i \frac{a_2^{\,i} \, \omega^{(i)}(x)}{i\,!} - x \right]^m , \quad . \quad . \quad . \quad . \quad . \quad . \qquad (91)$$

where the term $-x$ has to be combined with the term corresponding to $i = 0$ under the sign of summation.

When the involution has been performed we have to replace a_2^i by $a_{2,i}$. The peculiar thing to be noticed is now that we may invert the order of the last two operations; this is caused by the fact that here a_2^i is not only a symbolic but a real i^{th} power, viz. of the number $\omega - x$, which is independent of i. In consequence of this the proper meaning of the product of two symbolic powers is in the present case equal to the product of their proper meanings and this has the effect that a_2^i may be replaced by $a_{2,i}$ before performing the involution [1]). The result is

$$a_{II,m} = \left[\sum_{0}^{\infty}{}_i \frac{(\omega - x)^i \, \omega^{(i)}(x)}{i\,!} - x\right]^m$$

The infinite series within the brackets (without the term $-x$) is clearly a formal expansion of the expression

$$\omega[\omega(x)]$$

and if N°. 26 of the preceding communication is consulted, especially formula (60), it will be evident that the present expansion does represent the last mentioned quantity in the domain (a_2). Finally we have therefore

$$a_{II,m} = [\omega\,[\omega\,(x)] - x]^m\,,$$

which of course could be derived in a much simpler manner (observing that $S_\omega S_\omega$ is itself an operation of substitution in which the function $\lfloor \omega[\omega(x)]$ has to be substituted for x).

In the second place we take $T_1 = S_\omega$, $T_2 = D^{-1}$. Here $a_{1,i}$ is the same as in the preceding case and

$$a_{2,i} = \frac{(-1)^i x^{i+1}}{i+1}\,,$$

as we have already utilized several times (see for instance N°. 16, 3rd communication). Since the first component is the same as in the first case, formula (91) will apply here as well. But the order of the involution and the replacement of a_2^i by $a_{2,i}$ must not be inverted now, the symbol a_2^i not being the i^{th} power of a number independent of i. However, an other reduction is permitted. For we also have

$$a_{2,i} = \int_0^x (t - x)^i \, dt$$

so that $a_{2,i}$ is at least the *integral* of an i^{th} power. We may therefore give as a further rule for the reduction of the power-series in the right-hand member of (91) the following one: replace a_2^i by

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[1]) It is for the same reason that an exact result is obtained if in applying formula (24), that is $a_m = (\xi - x)^m$, to the operation of substitution, ξ is replaced by ω before the expansion of the binomial.

$(t-x)_i$ and integrate every term from $t=0$ to $t=x$. *This* replacement may now be performed *before* the involution, on the same ground as above, so that we may say: the power-series in $(t-x)$

$$\sum_0^\infty {}_i \frac{(t-x)^i\,\omega^{(i)}(x)}{i\,!} - x\,, \quad \ldots\ldots\ldots \quad (92)$$

where the term $-x$ must be taken together with the term under the sign of summation that corresponds to $i=0$, must be involved to the m^{th} power according to the common rule for the involution of infinite series, and the result is to be integrated term by term between the limits $t=0$ and $t=x$. Now the infinite series in (91) (without the term $-x$) is a formal expansion of $\omega(t)$ and if N°. 28 of the preceding communication is consulted, from which it appears that the radius of convergence of $\omega(x)$ is greater than $2\alpha_2$, we infer that the expansion is *essential* for values of x in the domain (α_2). The involution to the m^{th} power therefore leads to a power-series in $(t-x)$ which, for the x-values mentioned, represents the function $[\omega(t)-x]^m$ and since the convergence of the latter power-series is *uniform* in the integration-interval, its integration term by term produces the integral of the function represented by it. Thus we finally have

$$a_{II,m} = \int_0^x [\omega\,(t) - x]^m\,dt$$

which has also been found in N° 28.

In applying formula (88), in order to determine $\xi_{II,m}$, analogous peculiarities occur in either of the cases just mentioned.

36. In the two previous communications we have, in considering the resultant of two complete transmutations, been able to simplify our statements by supposing that we had to deal with the following case. If the functions $\overline{a}_m(x)$ or shortly $\overline{a}_m$ be the natural majorants of the coefficients a_m of a series P representing a transmutation which is complete in certain circular domains (α), with common centre x_0 and a radius α varying between 0 and a certain positive value A, then the maximum value $\overline{a}(\alpha)$, for the domain (α), of the upper limit

$$\overline{a}_x = \overline{\lim_{m=\infty}}\, |\overline{a}_m|^{\frac{1}{m}} \quad \ldots\ldots\ldots\ldots \quad (6')$$

is *within* the interval $(0, A)$ a *continuous* function of (α). (Cf. N°. 23). From this it might further be derived that the corresponding quantity $a(\alpha)$, belonging to the given functions a_m themselves, was equal to the first mentioned, and therefore also continuous within $(0, A)$, if we further assumed, as we have done continually, that a certain supposition were realized which we proposed to quote as *the uniformity supposition of N°. 4*. The above mentioned simplification of statements was a consequence of this identity of $a(\alpha)$ and $\overline{a}(\alpha)$. Again we intimated in No. 23 that we should perhaps recur to the question as to whether the continuity of $a(\alpha)$ represents the only possible case. We now proceed to do so.

We may for shortness of notation and without loss of generality consider a neighbourhood of the origin; further we may, as long as only the natural majorants of $a_m(x)$ are considered, denote these quantities without the lines above the letters which were used hitherto; the same thing may be done in denoting quantities connected with the first mentioned, as for example the left-hand member of (6′). This latter attains in the domain (α) its maximum value for the real positive value $x = \alpha$, so that we have

$$a(\alpha) = \overline{\lim_{m=\infty}} \, [a_m(\alpha)]^{\frac{1}{m}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (93)$$

The supposition that the series P is complete in (α) implies that all functions a_m belong to (α), that is, that they are regular within that circle and on its circumference. Let R be the upper limit of the radii α for which this holds. Then we may put the question as follows: If the function $a_m(\alpha)$ of the *real* variable α for all integral positive values of m can within the interval $(0, R)$ be developed in a power-series of α with *real positive* coefficients, to investigate the question as to whether the function $a(\alpha)$ defined by (93) is *continuous* in the same interval.

Since $a(\alpha)$ evidently increases together with α, we may at once infer the following: 1. If $a(\alpha)$ be *finite* for a certain value of α in the interval $(0, R)$, then $a(\alpha)$ is also finite for all smaller values of α, belonging to the same interval. 2. If $a(\alpha)$ be infinite for a certain value of α in the interval $(0, R)$, then $a(\alpha)$ is also infinite for all greater values of α in $(0, R)$. Thus there is in $(0, R)$ a point A forming the section between those values of the interval for which $a(\alpha)$ is finite and those for which it is infinite. The point A may coincide with $\alpha = 0$ or with $\alpha = R$; in the first case the corresponding transmutation is not complete in any domain of the origin, however small, so that this case need not be regarded. (An example is furnished by $a_m = m!\, x^m$, where $R = \infty$, but $A = 0$).

We therefore take the case that there is a certain sub-interval $(0, A)$ of $(0, R)$, which may eventually coincide with the latter, such that $a(\alpha)$ has a finite value within it; and we shall prove that *discontinuity of $a(\alpha)$ within that interval is not possible.*

We divide all power-series of $a_m(x)$, for the different values of m, into two parts, the first of which contains a number of terms proportional to m, say km; thus we write

$$a_m(\alpha) = \sum_0^\infty {}_n\, c_{m,n}\, \alpha^n = P_m(\alpha) + Q_m(\alpha)\,, \quad . \quad . \quad . \quad . \quad (94)$$

where

$$P_m(\alpha) = \sum_0^{km-1} {}_n\, c_{m,n}\, \alpha^n \quad , \quad Q_m(\alpha) = \sum_{km}^\infty {}_n\, c_{m,n}\, \alpha^n\,,$$

and k is a number, independent of m, which is at our disposal. To either of the parts P_m and Q_m there corresponds, as to their sum, an upper limit as exhibited in (93); these we shall respectively denote by the names *first limit* and *second limit,* whereas we may call *total limit* that corresponding to the whole series. We may now again use the proposition stated in N°. 32 of the preceding communication, which has already served us a few times in estimating limits such as we have to deal with here. In virtue of this the greater of the limits calculated for P_m and Q_m separately is equal to the total limit, and if the first two limits are equal, the total limit is either equal to them or less: the latter, however, cannot be realized here, since all the terms of the series are positive.

If now the total limit $a(\alpha)$ is *zero* in all internal points of $(0, A)$, then $a(\alpha)$ is also *continuous* in those points and there remains nothing to be proved then. Thus we take the case that there is a point α_1 in $(0, A)$ for which the limit in question is a certain *positive* number λ_1. We may state then: There is for the point $\alpha = \alpha_1$ a value k_1 of the above number k such that the first limit is not less than the second and thus equal to the total limit λ_1. For if we suppose for a moment that the second limit were greater than the first and thus equal to λ_1 for an *arbitrary* value of k, that limit would for a value of α in $(0, A)$ greater than α_1 be *at least* $\left(\frac{\alpha}{\alpha_1}\right)^k$ times as great as λ_1 and thus, as k could be taken *arbitrarily great,* the limit in question would be necessarily *infinite,* contrary to the hypothesis. Thus there is a value k_1 of the property mentioned.

For a point α on the left of α_1 the second limit is *for the same value* k_1 of k no more greater than the first, so that the first limit for $k = k_1$ is again equal to the total limit in such a point. In fact, the second limit is there *at least* and the first *at most* $\left(\frac{a}{a_1}\right)^{k_1}$ times as small as in the point a_1. From this it at once follows that the first limit cannot be equal to zero in any such point a nor can the total limit. Further we have for two arbitrary points a and a' of the interval $(0, a_1)$ that the ratio of the values which the first limit assumes there, lies between $\left(\frac{a'}{a}\right)^{k_1}$ and 1, and thus approaches to 1 as a' approaches to a. In other words the first limit and thus the total limit too, which is equal to it, is a continuous function of a in the interval $(0, a_1)$.

From the hypothesis that $a(a_1)$ is *finite* and *different from zero* we have thus inferred that $a(a)$ is *continuous* in the interval $(0, a_1)$ and also different from zero. But if $a(a_1)$ differs from zero the same holds for any value of $a(a)$ corresponding to a point a of the interval (a_1, A). Thus we infer that *the function $a(a)$ is continuous and different from zero in any sub-interval of* $(0, A)$ *which has the left-hand end-point in common with* $(0, A)$, *and thus shortly speaking in* $(0, A)$. The limiting values (93) are thus in the internal points of the interval $(0, A)$ either *all* equal to zero or *all* different from zero, but also in the latter case they form a function of a which is continuous within that interval. This is the result required.

As regards the endpoints of the interval $(0, A)$ the foregoing reasoning does not inform us of anything. We may with a view to these points distinguish the following cases, which all are possible as it appears from the examples added.

1st. The function $a(a)$ is continuous in both endpoints, that is to say continuous *on the right* at $a = 0$, and continuous *on the left* at $a = A$.

Examples: $a_m(x) = 1 + x^{m^2}$. The interval $(0, R)$ where the functions $a_m(a)$ can be expanded into a power-series of a, is here the interval $(0, \infty)$; the maximum value A of the a-values for which $a(a)$ is finite is equal to unity. The function $a(a)$ is in the closed interval $(0,1)$ equal to the constant value 1. As another example we may quote $a_m(x) = x^m + x^{m^2}$, for which also $R = \infty$, $A = 1$; here $a(a)$ is in the closed interval $(0,1)$ equal to x. More generally we may take

$$a_m(x) = y^m (1 + x^{m^2}),$$

where $y = f(x)$ is a function of x with a radius of convergence greater than unity, and is identical with its natural majorant. Here we again have $A = 1$ and $R =$ the just-mentioned radius of convergence of y; the function $a(a)$ is in the closed interval $(0,1)$ identical with $f(\alpha)$. An example in which $a(\alpha)$ is everywhere in the closed interval $(0, A)$ equal to zero, is provided by $a_m(x) = m^{-m} x^{m^2}$, where $A = 1$.

2nd. The function $a(\alpha)$ is discontinuous at the end-point on the left of $(0, A)$, and continuous at the end-point on the right of $(0, A)$.

Examples: $a_m(x) = x + x^{m^2}$. We have $R = \infty$, $A = 1$. For $\alpha = 0$, $a(\alpha) = 0$ and in the other points of the closed interval $(0,1)$, $a(\alpha)$ is equal to 1. More generally we may take

$$a_m(x) = c^m + y^m (x + x^{m^2})$$

where c is a positive constant and $y = f(x)$ a function of x of the same kind as above, with the restriction that it is for $x = 0$ greater than c. The function $a(\alpha)$ is then in $\alpha = 0$ equal to c, and in the other points of the closed interval $(0,1)$ it is equal to $f(\alpha)$, so that, if $f(0) = c + p$, where p is a positive number, it has a saltus at $\alpha = 0$ on the right, the amount of which is p.

3rd. The function $a(\alpha)$ is continuous at the end-point on the left of the interval $(0, A)$, discontinuous at the other end-point.

a. $a(\alpha)$ is *finite* at A; *b.* $a(\alpha)$ is *infinite* at A.

Examples of the case 3*a*. Take

$$a_m(x) = y^m + x^{m^2},$$

where $y = f(x)$ is a function of the kind considered satisfying the further condition that $f(1) < 1$; then $a(1) = 1$ and in the other points of the closed interval $(0,1)$ we have $a(\alpha) = f(\alpha)$, so that this function has a saltus at $\alpha = 1$ on the left of the amount θ, where

$$\theta = 1 - f(1).$$

If we take $y = 0$, we have the case that $f(\alpha)$ is in all points of the closed interval $(0,1)$ equal to zero, except at $\alpha = 1$, where $f(\alpha) = 1$.

Examples of the sub-case 3*b*. Take

$$a_m(x) = y^m + m!\, x^{m^2},$$

where y is a function as regarded without any further restriction being necessary. If we take $y = 0$, $a(\alpha)$ is *zero* in the whole interval $(0,1)$ except at $\alpha = 1$, where $a(\alpha)$ is infinite.

4th. The function $a(\alpha)$ is discontinuous at both end-points of the interval $(0, A)$:

a. $a(\alpha)$ is finite at $\alpha = A$.

b. $a(\alpha)$ is infinite at $\alpha = A$.

Examples of 4*a*. Take

$$a_m(x) = c^m + xy^m + x^{m^2},$$

where c is a positive constant less than 1, $y = f(x)$ a function of the kind already considered satisfying the further conditions $f(0) > c$

and $f(1) < 1$. We then have $a(0) = c$, $a(1) = 1$ and within the interval $(0,1)$, $a(\alpha) = f(\alpha)$, so that, if

$$f(0) = c + p, \quad f(1) = 1 - q,$$

where p and q are positive numbers satisfying the inequality

$$c + p < 1 - q,$$

there is at $\alpha = 0$ on the right a saltus of $a(\alpha)$ of the amount p and at $\alpha = 1$ one of the amount q.

Examples of 4*b*. Take

$$a_m(x) = c^m + xy^m + m!\, x^{m2},$$

where $y = f(x)$ is a function as considered in the preceding example, without the restriction as to the value of $f(1)$ being necessary.

In all these cases we have imagined A to be essentially different from R, but matters do not alter substantially if we suppose $A = R$. We may construct examples of this case by taking in the preceding ones the function $y = f(x)$ such that its radius of convergence is unity, and then we may either choose $f(x)$ so as to be finite for $x = 1$ or infinite.

Returning to the functional operations we remark that, in consequence of the theorem of N°. 4, such an operation is complete only in domains (α) such that $\alpha < A$, or perhaps $\alpha = A$. We therefore have only to deal with domains of the latter kind and for those $a(\alpha)$ has now appeared to be a continuous function of α, except perhaps at $\alpha = 0$ and at $\alpha = A$. The case which we supposed to have been realized, in order to simplify our statements relating to the resultant of two complete transmutations, thus appears to be the only one possible. Moreover, as regards possible discontinuity at $\alpha = 0$, this is of no interest for the complete transmutation. If the function $a(\alpha)$ at $\alpha = 0$ has a saltus from the value b_0 to the value b (the limit of $a(\alpha)$ at $\alpha = 0$ on the right), then though the series P produces for all functions belonging to the circle (b_0) and not to (b) a transmuted at $x = 0$, it does not for all functions belonging to a circle (b') greater than (b_0) but smaller than (b) produce a transmuted in a certain *domain* of x_0, however small. We therefore shall assume as the domain (β) corresponding to $\alpha = 0$ the circle of radius b instead of the circle of radius b_0 and then the discontinuity of $a(\alpha)$ at $\alpha = 0$ has been removed.

A possible discontinuity on the left of $\alpha = A$ can, however, not be removed.

From formula (7) of N°. 4

$$\beta = \alpha + a(\alpha) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

it now follows that the number β corresponding to α is also continuous within the interval of completeness $(0, A)$. That β is, moreover, monotonously increasing together with α, since $a(\alpha)$ cannot decrease as α is increasing, has already been remarked in earlier parts of the present paper.

Let us finally consider the case that the functions $a_m(x)$ do not coincide with their natural majorants, and let us write the latter with the usual lines over them. We proved in N° 23 that the identity

$$a(\alpha) = \overline{a}(\alpha) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (95)$$

is valid under the following two conditions: 1st. If the uniformity supposition of N°. 4 is satisfied; 2nd if the quantity $\overline{a}(\alpha)$ is a *continuous* function of α within the interval $(0, A)$. The latter has now been proved to be always the case, so that we may infer that the equality (95) is only a consequence of the same uniformity supposition from which we derived in N°. 4 the extended theorem of BOURLET.

37. In connection with the latter considerations it may be convenient to observe that the uniformity supposition of N°. 4 can be replaced for either of the two purposes mentioned by one of somewhat wider compass. If the reasoning in N°. 23, leading to the identity (95) be carefully examined, it appears that another, viz.

$$A(\alpha) \equiv \overline{\lim_{m=\infty}} \left[A_m(\alpha)\right]^{\frac{1}{m}} = \overline{a}(\alpha) \, , \quad . \quad . \quad . \quad . \quad . \quad . \quad (96)$$

where $A_m(\alpha)$ is the maximummodulus of $a_m(x)$ on the circumference of (α) can be derived from the *continuity* of $\overline{a}(\alpha)$ only,[1] and since the latter is always realized, the same holds for formula (96) so

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[1] We found namely that on the circumference of an *arbitrary* circle $(\alpha') < (\alpha)$ and concentric with (α)

$$\overline{a}_m(\alpha') < \frac{\alpha A_m(\alpha)}{\alpha - \alpha'},$$

thus

$$\overline{a}(\alpha') \leq A(\alpha) \, ;$$

further

$$\overline{a}(\alpha) \geq A(\alpha) \, ,$$

so that

$$\overline{a}(\alpha) = A(\alpha),$$

by the further assumption that $\overline{a}(\alpha)$ should be a *continuous* function of α. Meanwhile, now that this continuity has appeared to hold universally, the question arises if it is possible to show the latter identity in a *direct* manner, without having recourse to the continuity of $\overline{a}(\alpha)$. This may in fact be done as follows. Let us again suppose the quantity $\overline{a}_m(\alpha)$ to be divided in the manner exhibited that *for this* the uniformity supposition is superfluous. The latter however served us to prove further that also the identity

$$a(\alpha) = A(\alpha) \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (97)$$

is valid. But now we may observe that for the latter the following uniformity-supposition, wider than that of N°. 4, is sufficient:

A. Corresponding to an arbitrarily chosen number ε, as small as we please, there is an integral number N_ε such that at all points x of the closed domain (α)

$$|a_m(x)| < (a(\alpha) + \varepsilon)^m \quad \textit{for} \quad m \geq N_\varepsilon \quad . \; . \; . \; . \quad (98)$$

We shall not explain any further that this supposition is sufficient in order to deduce the equality (97): it is easy to see. From (97) however and the identity (96), which has appeared to be valid independent of any particular hypothesis, the equality (95) may be derived, so that the function $a(\alpha)$ is equal to $\overline{a}(\alpha)$ and thus continuous within the interval $(0, A)$ under the single condition denoted by *A*.

But it cannot yet be inferred from that condition that we have $\beta = \overline{\beta}$. If the latter is to be true as well we must have

$$\beta = \overline{\beta} = \alpha + \overline{a}(\alpha) = \alpha + a(\alpha),$$

that is the number β corresponding to α must be determined by formula (7), copied at the end of the preceding paragraph. This formula was obtained in N°. 4 and based upon the uniformity-supposition of that paragraph. But on examining the proof of the completeness-theorem we gave there it will appear that the formula is a consequence of the following supposition only:

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by (94) and, for a certain value of α, the integral number k so chosen that we again have (writing in this case $\overline{P}$ instead of P)

$$\overline{a}(\alpha) = \overline{\lim_{m=\infty}}\, [\overline{P}_m(\alpha)]^{\frac{1}{m}}$$

Now, for every value of n, $A_m(\alpha) > \overline{c}_{m,n}\,\alpha^n$, if $\overline{c}_{m,n}$ be the modulus of the coefficient $c_{m,n}$ in the power-series of $a_m(x)$. Let $\overline{c}_{m,p}\,\alpha^p$ be the term of maximum value in $\overline{P}_m(\alpha)$; the number p will in general vary with m, but we always have $\overline{c}_{m,p}\,\alpha^p > \overline{P}_m(\alpha) : km$ and thus also $A^m(\alpha) > \overline{P}_m(\alpha) : km$. From this it follows, since $\lim_{m=\infty} m^{\frac{1}{m}} = 1$,

$$A(\alpha) = \overline{\lim_{m=\infty}}\, [A_m(\alpha)]^{\frac{1}{m}} \geq \overline{\lim_{m=\infty}}\, [\overline{P}_m(\alpha)]^{\frac{1}{m}} = \overline{a}(\alpha),$$

and thus, since $A(\alpha)$ cannot be greater than $\overline{a}(\alpha)$, we must have $A(\alpha) = \overline{a}(\alpha)$.

B. The maximum value $a(\alpha)$ of a_x *for the whole domain* (α) is equal to that *for the circumference.*

That the series

$$Pu = \sum_0^\infty {}_m \frac{a_m u^{(m)}}{m!}$$

converges for *all* functions belonging to the circle with radius $\alpha + a(\alpha)$ and that this circle is the *minimum* circle for which this property holds, therefore follows only from the hypothesis *B*. This latter however is not sufficient to derive from it the *uniform* convergence of the above series. But this at once follows if the hypothesis *A* is added to *B*; it is not necessary to explain this further since it may easily be derived from the proof given in N°. 4. The uniformity of the convergence is, however, of interest since in this case we may be certain that the transmuted of a function which is regular in (β) is itself a *regular* function, the domain of regularity being at least (α); in other words the *transmutation* in question is then always a *regular* one for the F. F. of functions belonging to (β) and the N. F. O. (α). For this reason we shall retain the hypothesis *A* for the extended theorem of Bourlet, treated in N°. 4, and thus *substitute for the uniformity-supposition of that paragraph the two suppositions A and B, which are independent of one another.*

That the uniformity-supposition of N°. 4 is narrower than the two suppositions *A* and *B* together is proved by the following example in which both *A* and *B* are satisfied, but not the former supposition. Let

$$a_m(x) = x^n - 1 \quad , \quad n = 2^{\mathcal{E}\left(\frac{\log m}{\log 2}\right)}$$

so that n depends on m in such a way as to be equal to the highest power of 2 that is contained in the number m; thus n passes through all integral powers of 2, but after every change of its value it remains constant for a certain number of m-values. For $\alpha < 1$, $a(\alpha) = 1$, for $\alpha > 1$, $a(\alpha) = \alpha$, since n is never greater than m but is equal to m for an infinite number of m-values. The quantity A is therefore, as the quantity R (see above) *infinite,* so that the series having the above quantities $a_m(x)$ as its coefficients is complete in any domain (α), however large. If we imagine $\alpha > 1$ the circle of radius unity lies wholly in the domain (α); at points on the circumference of this circle having as their argument

$$\frac{2\pi}{2^s} \quad ,$$

where s is some integral number, all the coefficients $a_m(x)$ are zero from and after the value $m = 2^s$ and thus a_x is also zero at those points. But we can always find among these points such as satisfy the condition that, corresponding to a number Q, chosen arbitrarily great, there exists a value of $m > Q$ for which the quantity

$$|a^m(x)|^{\frac{1}{m}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (99)$$

is more than a certain fixed amount $\varkappa$ greater than the limit a_x of that quantity, which is zero: we need only choose the number s greater than Q and $m = 2^{s-1}$, and then $a_m(x)$ is equal to -2 in the points corresponding to those s-values, so that the quantity (99) has a value which is greater than unity. Thus we cannot assign a number m_ε independent of x such that in the *whole* domain (a)

$$|a_m(x)| < (a_x + \varepsilon)^m \quad , \quad \text{for } m \geq m_\varepsilon ,$$

and this was the very uniformity-supposition of N°. 4. The supposition under A however is satisfied, because $|x^n - 1|$ is at most equal to 2 for points of the circular domain of radius unity and thus less than $(1 + \varepsilon)^{m_\varepsilon}$, that is less than

$$[a(1) + \varepsilon]^m$$

where ε may be prescribed arbitrarily small, if only m be chosen large enough. Again the supposition B is satisfied: in the first place we immediately see that at all points of a circle arbitrarily little smaller than that of radius unity the quantity a_x is equal to 1 so that there are in an arbitrarily small neighbourhood of the circumference of the latter circle points where a_x is equal to the upper limit of that quantity for the closed domain of that circle: this, though not exactly the same as the supposition B, agrees with it as to its consequences, viz. the validity of formula (7), and it might therefore be substituted for the supposition B. But also for the circumference of the circle (1) itself the upper limit of a_x is equal to 1. For there corresponds to any arbitrarily chosen number ε a prime number p such that at a point on the circumference of that circle having the argument

$$\varphi = \frac{\pi}{p}$$

the argument of

$$x^{2^k} = e^{i\frac{2k\pi}{p}} , \text{ i. e. } \frac{2k\pi}{p} ,$$

that is $\frac{2k\pi}{p}$, differs from π by less than ε for an infinite number of k-values.

For this purpose, we need only choose p so great that $p\varepsilon > \pi$. The congruence

$$2^k \equiv 1 \pmod{p}$$

can be satisfied according to a theorem of FERMAT for all k-values being a multiple of $p-1$; that is, for all those values we have

$$2^k = (2l + 1)\, p + 1$$

or

$$\frac{2^k \pi}{p} = (2l + 1)\, \pi + \frac{\pi}{p},$$

so that in connection with the above choice of the number p the required condition is satisfied. Thus there are always points on the circumference of the circle (1) for which the quantity

$$|x^{2^k} - 1|$$

differs from the value 2 by less than an arbitrarily small amount so that it is for instance greater than 1. Since in $a_m(x) = x^{n-1}$ the number n assumes *all* integral powers of 2 as a value, $a_m(x)$ is in the just-mentioned points for an infinite number of m-values greater than 1, so that $a_x = 1$ in all those points.

We have thus constructed an example in which the conditions A and B are satisfied, but the uniformity-supposition of N° 4 is not satisfied. As, now, regards the two former suppositions, we should like to have an example in which either one of them or both were not realized. But we have not succeeded as yet in constructing any of the kind, nor, on the contrary, in proving that this would be impossible, in which last case A and B would hold *universally*. If there be a point on the circumference of the circle (α) such that at that point the quantity $|a_m|^{\frac{1}{m}}$ be for an infinite number of m-values, $m_1, m_2, \ldots m_n, \ldots$ equal to the maximum

$$A_m^{\frac{1}{m}}(\alpha)$$

of the same quantity on the circumference of (α), and if at the same time the upper limit of the *partial* sequence

$$A_{m_1}^{\frac{1}{m_1}}(\alpha)\ ,\ A_{m_2}^{\frac{1}{m_2}}(\alpha), \ldots,\ A_{m_n}^{\frac{1}{m_n}}(\alpha), \ldots$$

be equal to that of the *complete* one, then at the point mentioned $a_x = A(\alpha)$, and thus $a(\alpha) = A(\alpha)$ so that both A and B are satisfied. In constructing a pathological example as mentioned we should therefore take care that there is no such point on the circumference of (α). But this is by no means sufficient. For it may be possible that,

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corresponding to an arbitrarily chosen number ε there exists a point in the domain (α), where the quantity $|a_m|^{\frac{1}{m}}$ for an *infinite* number of m-values is greater than $A(\alpha)-\varepsilon$, and also in that case the upper limit $a(\alpha)$ of a_x is equal to $A(\alpha)$. The condition A is satisfied then and it will be quite an ordinary thing if the condition B is also realized, though we do no longer see the necessity of it. If, on the contrary, there can be assigned an amount $\varkappa$ such that in no point of the domain (α) the quantity $|a_m|^{\frac{1}{m}}$ is greater than $A(\alpha)-\varkappa$ for an *infinite* number of m-values, then $a(\alpha)$ is certainly not greater than $A(\alpha)-\varkappa$. The condition A is not satisfied now; for if this were the case, then, since $A_m(\alpha)$ is the modulus of $a_m(x)$ for at least one point at the circumference of (α), there would, corresponding to any arbitrarily small quantity ε, be an integer N_ε such that, from and after $m = N_\varepsilon$ we should have $A_m(\alpha) < [a(\alpha)+\varepsilon]^m$, or

$$A_m^{\frac{1}{m}}(\alpha) < a(\alpha) + \varepsilon \leq A(\alpha) - \varkappa + \varepsilon\,,$$

and this is impossible, if ε be chosen less than $\varkappa$, since $A(\alpha)$ is the upper limit for $m=\infty$, of $A_m^{\frac{1}{m}}(\alpha)$. As to the condition B it might or might not be satisfied.

We have made the preceding observations in order to elucidate a few more cases as considered here. Meanwhile such cases, if they are possible at all, may undoubtedly be regarded as pathological ones rarely occurring in practice: the observations made may be able to make this even clearer.

With this our considerations on complete transmutation have come to an end.

Astronomy. — *"The Origin of the Saros".* By Dr. A. Pannekoek. (Communicated by Prof. W. De Sitter).

(Communicated in the meeting of September 29, 1917).

The forecast of eclipses, which to the uneducated is such a convincing proof of the power and accuracy of astronomical science, is not the fruit of the highly developed modern theory, but belongs to the oldest products of human science. Greek writers tell us that the Babylonians were already able to predict the eclipses by means of a period of 18 years, which they called "saros", and which rested on the fact that 223 synodic lunar periods and 242 draconic revolutions are practically equal (both $6585^1/_3$ days), that after this period, therefore, full and new moon return to the same position relatively to the nodes.

When at a later period this meaning of the saros as common multiple of two lunar periods was once grasped, the saros itself was no longer necessary, and the eclipses could be calculated directly from the knowledge of the orbits of sun and moon. But this scientific height was reached in Seleucidic Babylon and in Greece only in the last centuries B. C. The origin of the use of the saros falls in earlier times; and the first question is, what times?

According to the theory of Hugo Winckler's school, Babylonian astronomy had reached its highest perfection as early as 2000—3000 B. C., and therefore the origin of the saros lay in such a far-off time that there is no possibility of following the road to the discovery. But Kugler's researches have proved this theory to be to a large extent ungrounded romance. Afterwards, the last champion of the great antiquity of Babylonian astronomy, Ernst Weidner, tried to prove that the saros must have been known at least 1000 B. C., but in this he was not successful [1]). Kugler's argument for the opposite opinion is undoubtedly sound:

"The most ancient Babylonian observations of lunar eclipses which are in any way serviceable, date according to Ptolemy from the years 721 and 720 B. C. The accuracy found therein in no way exceeds

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[1]) E. Weidner, "Alter u. Bedeutung der babylonischen Astronomie u. Astrallehre" (1914) p. 16. See also F. X. Kugler, "Sternkunde u. Sterndienst in Babel. Ergänzungen" p. 241.

that which we find in some of the later Assyrian texts. Of a knowledge of the saros there is not the least indication in these texts; everything indicates on the contrary that the astrologers of that period were not acquainted with the saros so as to be able to forecast a lunar eclipse with certainty some time beforehand" [1]) and he concludes from this: "Before the 8th century the eclipses of the moon (and sun) were not observed with that care which is necessary for establishing their period, which was still unknown in the 7th century." In how far the first part of this sentence is correct will be shown presently.

Kugler points out, moreover, very rightly, that the discovery of the saros is not so easy as is often thought [2]). In the first place because this period does not embrace a round number of days, but 8 hours more. If we take a list of the lunar eclipses [3]) visible at a particular spot, say Babylon, and then look up those which occur one saros period later, they will all be seen to take place 8 hours later, chiefly therefore by day: most of them will not be visible in Babylon. On the other hand there now appear a large number of eclipses the predecessors of which were not visible 18 years earlier, because they occurred before the beginning of the night.

Experience, therefore, by no means points in the direction of the saros period. To get eclipses at about the same daytime the period must be trebled; after 54 years the visible eclipses return to a great extent in the same order. If we arrange the visible eclipses in series of 54 or of 18 years, it is then not difficult to establish the existence of the saros period. But it is quite another matter to find or to discover this period. If someone who knew nothing of this period was given the task of finding from a complete list of lunar eclipses, e.g. from Oppolzer's Canon, a period after which they would return in the same manner, he would certainly find it a very difficult one. And how much more difficult the discovery must have been in Babylon may be seen when we consider the conditions that had to be fulfilled.

The first essential would be to have a complete list of all the visible eclipses. Now it is doubtful whether in the Assyrian period (8th and 7th centuries B. C.) a continuous list could be made of the eclipses observed, as, at least in all our extant reports of the astrologers, the year is not noted. Let us, however, suppose that for

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[1]) F. X. Kugler, ibid Ergänzungen, p. 132.

[2]) Ibid. Vol. II. p. 65—67.

[3]) We do not mention sun eclipses here, because for them owing to the influence of the parallax, the regularity is still much more difficult to find.

this the data were available — in later centuries they certainly were — it would first be necessary that someone should conceive the idea of compiling a continuous list of this sort and moreover of looking for a period in it, only then would he stand before a problem of the same nature, although more difficult, as the one we proposed above. "It is quite another thing, however, even to arrive at the idea that the eclipses would return periodically, and yet another to deduce the suspected law from a series of observations. The inadequate appreciation of these things amongst us is due to the fact that to-day we are so much accustomed to the discovery of new natural laws that it is difficult for us, to place ourselves in the position of those who did the first pioneer work in the field of natural science." (KUGLER p. 67). Indeed, it may be said that a superhuman genius was necessary for this, capable of conceiving, as it were from nothing, scientific aims and scientific methods in a world which did not yet know the meaning of science, and of applying them.

Looked at from this point of view it is not surprising that in the Assyrian period nothing should be known of a saros period. Indeed we should wonder that the saros could ever be discovered. But this is only true if it had to be found in the way indicated here. This cannot, however, be the way in which science arose. Practical rules and regularities were first discovered, which obtruded itself to the mind through experience, found without intention or consciousness of scientific aim; much later from this the theoretical idea of regularity and periodicity in nature was formed, and the intentional search for them. If, therefore, we do not want to regard the origin of science as a miraculous creation, such a discovery as that of finding the saros may be conceived only as a gradual process, as the outcome of many steps each of which followed naturally and spontaneously from the former and in which several succeeding generations took part.

Whenever the prediction of eclipses is under consideration, the saros is always thought of, as if it were the only means available for this purpose. But there are other simpler and less perfect rules, which could be more easily discovered, and which were therefore certainly detected and made use of before the saros was known. They may be regarded as the precursors from which in the course of time the saros was developed. This development would thus fall in the centuries during which the astrological practice of the Assyrian period gradually grew to the height of astronomical science which it reached under the Seleucides and Arsacides — that is in the Babylonian-Persian period (6th and 5th centuries B.C.).

How this development took place is illustrated by a remarkable cuneiform text in the British Museum, Sp. II 71, of which STRASSMAIER gave a transcript in 1894[1]). There it is shown that this text contains a list of lunar eclipses arranged according to saros periods. STRASSMAIER thought that he could also deduce from it that the Babylonians used the saros as the foundation for their calendar, but this proved later on to be untrue. The importance of this text lies chiefly in the fact that it shows very clearly the origin of the saros from earlier phases. To understand its construction, it is first necessary to trace this development.

II.

Astrology, which directed the gaze of men to all the heavenly bodies, caused by its great development in the Assyrian period an ever-increasing detailed knowledge of the heavenly phenomena to arise during that time. This regarded especially the moon and its eclipses. As a first regularity in the eclipses, the rule must have been noticed that after a lunar eclipse it was only 6 months afterwards that an eclipse could again occur. It is true that such an eclipse was often absent; but the reason for this could be found in the observations of the moon; *a lunar eclipse only occurs when sun and moon stand opposite to each other.*

The phenomena on the days lying around the full moon (i.e. in the middle of the month, which always began with the appearance of the sickle moon) were, on account of their astrological significance, always carefully observed: from the times of transit of sun and moon through the great circle of the horizon (the natural measuring circle of the Babylonians), from the rising and setting on those days, therefore, the moment of opposition could be ascertained with certainty. If the full moon rose in the evening before sunset or set before sunrise in the morning, the opposition was yet to come; if it was later, the opposition was past. As soon as this reason for the omission of an eclipse, namely that the opposition had taken place during daylight, has been noticed, the positive rule could be established: *when a lunar eclipse has taken place, another will follow six months later*; whether this will be visible depends upon whether the opposition takes place by day or by night.

This regularity was noticed as early as the Assyrian period. In the astrological reports notice is sometimes given of predictions that

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[1]) EPPING u. STRASSMAIER, Ein babylonischer Saros-Canon. Zeitschrift für Assyriologie Vol. VIII. p. 176. The text itself is given Z. f. A. Vol. X p. 66.

did or did not come true. In one of the reports it says: "The eclipse passes by, it does not take place. If the king asks, what omens have you seen: the gods (that is the sun and moon) have not been seen with one another; ... at the beginning of the night [the moon rose, ... therefore] the eclipse passes by, [by day the moon] with the sun will be seen".[1]) Here, therefore, is given as a reason why an apparently predicted eclipse did not take place, that in the evening the moon rose after sunset, the opposition was therefore passed and had occurred in the day-time.

The above rule, however, was not always valid. After 6 lunar months (177.18 days) the longitude of the sun, and of the full moon also, has increased in the mean by $174^\circ{,}645$; during the same time the line of nodes has receded $9^\circ{,}383$. If the distance, therefore, between the full moon and the nearest node is $L - \Omega = P$, the distance of the full moon that follows six months later to the other node is $P - 5^\circ{,}355 + 9^\circ{,}383 = P + 4^\circ{,}028$. The position of the full moon with regard to the node shifts 4° per six months. From this it follows that for a number of times the lunar eclipse will return regularly after six months; but this series will finally stop, when the distance between full moon and the node has become too great. A partial eclipse is still possible if the distance $L - \Omega$ is not greater than 10°—$12\frac{1}{2}^\circ$. If at a certain full moon the value of $P = L - \Omega = -15^\circ$, this quantity for the full moons which come each six months later is:

-15°	$-10^\circ{,}97$	$-6^\circ{,}94$	$-2^\circ{,}92$	$+1^\circ{,}11$	$+5^\circ{,}14$	$+9^\circ{,}17$	$+13^\circ 20$
no eclipse.	?	partial.	total.	total.	total ?	partial.	none.

Thus 5 or 6 eclipses follow each other regularly; the series begins with 1 or 2 partial eclipses, then follow a few total eclipses; these again are followed by 1 or 2 partial eclipses, and then the series is finished.

In such a favourable climate as that of Babylon, where, with the exception of a few winter months, every phenomenon in the heavens could be regularly observed every night, the priest-astrologers must have gradually noticed this regularity. If they observed a partial eclipse which had had no predecessor 6 or 12 months before, they knew that a fresh series had begun, and they could predict a number of coming eclipses with certainty.

SCHIAPARELLI has pointed out this simple method of prediction by

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[1]) THOMPSON, The reports of the magicians and astrologers, N⁰. 275A; KUGLER, Sternkunde etc. II, p. 64.

means of series of 5 or 6 successive eclipses, 1) and he assumes that when, in the Assyrian period, predictions are made, this rule is made use of. No proof can be given of this, as there is no text in which mention is made of the means by which the results were obtained. And a prediction is possible, as shown above, with even more primitive knowledge. That these series of 5 or 6 were known in the succeeding centuries is certain, because a knowledge of them is a station on the line that leads to the discovery of the saros.

To trace this road we must first consider what phenomena and what regularity an attentive observer would further be able to notice in the lunar eclipses. If a certain series of full moons (by a series we here mean always full moons following each other with intervals of six lunar months) recedes more and more from the nodal line, the series of full moons preceding them constantly approaches it. The longitude of the former full moon is 29°,11 smaller, a month earlier the node lay upon 1°,564 greater longitude, therefore for this previous full moon $L - ☊ = P - 29^\circ{,}11 - 1{,}564 = P - 30^\circ 67$. We will now tabulate P for both series beside each other beginning with the 4th of the above row, and beside it a few of the preceding full moons.

P two full moons back	P former full moon	P		
		+ 1.11	tot.	1st series
		+ 5.14	tot?	
	— 21.50	+ 9.17	part.	
	— 17.47	+ 13.20	—	
	— 13.44	+ 17.23	—	
	— 9.41	+ 21.26	part.	2nd series
	— 5.39		tot?	
— 32.03	— 1.36		tot.	
— 28.00	+ 2.67		tot.	
— 23.97	+ 6.70		part.	
— 19.95	+ 10.72		part?	
— 15.92	+ 14.75		—	
— 11.89			part?	3rd series

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1) G. Schiaparelli, I primordi dell' astronomia presso i babilonesi. (Scientia IV p. 36).

We now see that when the first series has expired, it is quickly followed by a new series of eclipses, which falls a month earlier than the continuation of the old series. Instead of always counting by 6 months occasionally only five months need to be passed over, and we then come to the beginning of a new series.

When, therefore, a continuous list of eclipses was composed, this at once divided into series of five or six eclipses following each other with a 6 months' interval; each succeeding series was the continuation of the preceding one if at the end of a series 5 months instead of 6 were passed over. Between two succeeding series there were always a few missing, so that eleven, seventeen or twenty-three months passed without eclipses (the full moons in our list, in which P lies between 12° and 15°). If these full moons before and after each series were added so that the series succeeded with an interval of five months, *a continuous list of eclipse moons was obtained divided into series by the five-monthly intervals*, and for which the rule was: in the middle of each series lie the total eclipses, beside them on either side the partial, and beside these, where the series join, they drop out. In such a list let us tabulate the value of P, beginning with the arbitrary value — 15°, in such a way that we pass over 5 months, as soon as P obtains a smaller value in the following series than in the preceding one.

1st series	2nd series	3rd series	4th series	5th series	6th series
— 15°.00	— 13.44	— 11.89	— 14.35	— 12.80	— 15.28
— 10.97	— 9.41	— 7.85	— 10.32	— 8.77	— 11.25
— 6.94	- 5.39	— 3.82	— 6.29	— 4.75	— 7.22
— 9.29	— 1.36	+ 0.21	— 2.27	— 0.72	— 3.20
+ 1.11	+ 2.67	+ 4.24	+ 1.76	+ 3.31	+ 0.83
+ 5.14	+ 6.70	+ 8.26	+ 5.79	+ 7.33	+ 4.86
+ 9.17	+ 10.72	+ 12.29	+ 9.81	+ 11.36	+ 8.89
+ 13.20	+ 14 75	5 m.	+ 13.84	5 m.	+ 12.92
5 m.	5 m.		5 m.		5 m. etc.

Here it shows that the successive series contain sometimes eight and sometimes seven moons. There occur sometimes 7, sometimes 6 intervals of 6 months successively, separated by intervals of 5 months.

The alternation in these figures is, however, not altogether without rule; the 6th series begins with a value for P which differs very little from that of the 1st series. *All the values of P, therefore, very nearly return after 5 series,* therefore the same alternation of long and short series will return again after five series:

8 8 7 8 7 | 8 8 7 8 7 | 8 8 7 8 7.

In the succession of the series of eclipse moons a periodicity takes place, therefore, with a period of five series. In a period of this sort $7 + 7 + 6 + 7 + 6 = 33$ intervals of six months occur, and 5 intervals of 5 months; they embrace, therefore, $33 \times 6 + 5 \times 5 = 223$ lunar months. *This period is the saros.*

III.

It is, however, not probable that this periodicity in the series of the eclipse moons taken by themselves can be easily discovered. For the Babylonian observers had not a list of values of P of this sort at their disposal; what is expressed in our list as quantity they observed only as quality: total eclipse, partial eclipse, no eclipse. As the eclipses are missing at the transition from one series to the next the position of the transition cannot be distinctly marked, and the interval of 5 months could be assumed equally well earlier or later. This would cause a disturbance in the regular repetition of long and short series, and the period was no longer so conspicuous.

The saros period has, however, another peculiarity. Not only do full moon and node return in it to the same position with respect to each other, but also *the anomaly of the moon returns to almost the same value.* In one saros period the major axis of the lunar orbit revolves a little more than twice and returns therefore to almost the same position with regard to node line and full moon, both of which have acquired 10° greater longitude. The anomaly of the full moon during an eclipse determines to a great extent the varying circumstances: velocity of the moon, diameter of the earth's shadow and diameter of the disk of the moon. Return to the same anomaly, when the position with regard to the node is also the same, means the return of the same aspect of the eclipse.

For the series of eclipses tabulated above we can calculate the anomaly and from that the external circumstances, starting from a hypothetical initial value of 0° for the first full moon. Per 6 lunar months the perigee advances by 19°,739, the following full moon, therefore, advances on the perigee by $174{,}645 - 19{,}739 = 180° - 25°{,}09$. Per 5 months these values are $145{,}54 - 16°{,}449 = 180° -$

50°,91. The distance of the full moon to the perigee $L - \pi = v$ thus decreases at each leap of 6 months by $180° + 25°,09$, and at each leap of 5 months by $180° + 50°,91$. If the apparent radii of the moon and the sun be called r and r', the lunar parallax p (the solar parallax may be neglected), the inclination of the lunar orbit i, the radius of the earth's shadow is $R = \frac{41}{40}(p - r')$ and the latitude of the moon i sin $(P - 0°,4$ sin $V)$. The distance from the edge of the shadow, where it is nearest to the moon, expressed in 12ths of the lunar equator, (this is always called the magnitude of the eclipse in inches) is the quantity which determines the external aspect and the duration of the eclipse. It is

$$m = \frac{R + r - i \sin (P - 0°,4 \sin v)}{{}^1/_6\, r} = 6 \times \frac{1,025\,(p - r') + r - i \sin (P - 0°,4 \sin v)}{r}$$

As $\frac{p}{r} = 3,67$ and $\frac{i}{r} = 20$, whereas in the syzygies

$$\frac{1}{r} = \frac{1}{r_0}\,(1 - 0,065 \cos v) \quad \text{and} \quad \frac{r'}{r^0} = 1,05$$

this gives, as $0°.4 \cos P$ may always be replaced by $0°.4$,

$$6 \times (4,76 - 1,05 + 0,065 \cos v - 20\,\{\sin P\,(1 - 0,065 \cos v) + \tfrac{0,4}{57} \sin v)\} =$$
$$6 \times (3,71 + 0,065 \cos v - 20 \sin P\,(1 - 0,065 \cos v) + 0,14 \sin v).$$

If this quantity is negative there is no eclipse; if it is smaller than 12 the eclipse is partial; if it is greater than 12 the eclipse is total, and the totality lasts longer according as the number is larger. Of course in these calculations only mean circumstances are taken into consideration; owing to the perturbations of the moon and the eccentricity of the earth's orbit the actual course will deviate somewhat from the mean.

The results of our calculation are found in Table I; under "Aspect" is given what an observer could note in the various eclipses. For partial eclipses "upper" or "lower" is given according as the N. or S. portion of the moon remains uncovered; the same is also given for total eclipses when the moon passes distinctly through the upper or lower part of the shadow ($m < 17$), and therefore at the beginning and end the N. or S. portion remains longer light; if the moon disappears only for a short time in the shadow (m between 12 and 14), this is indicated by "short". It now appears that each of the successive series has a different peculiar character in the aspect of the successive eclipses. But in the 6th series the character is exactly the same as that of the 1st series, as P and v are almost the same; similarly the 7st 8th and 9th series will correspond in character to the 2nd 3rd and 4th. This agreement is strengthened by

TABLE I.

P	v	m	Aspect	P	v	m	Aspect
— 15°.00	0°	— 6.4	—	— 14°.35	345°	—13.3	—
— 10.97	155	— 2 5	—	— 10.32	40	1.6	part. lower 2^d
— 6.94	310	9.2	part. lower 9^d	— 6.29	195	8.2	part. upper 8^d
— 2.92	105	15.1	tot. upper	— 2.27	350	18.3	tot.
+ 1.11	260	19.0	tot.	+ 1.76	145	18.6	tot.
+ 5 14	54	12.9	tot. low. short.	+ 5.79	300	10.0	part. upper 10^d
+ 9.17	209	1.3	part. upper 1^d	+ 9.81	95	2.5	part. lower 2^d
+ 13.20	4	— 2.9	—	+ 13.84	250	— 7.9	—
			5 months.				5 months.
— 13.44	133	— 7.7	—	— 12.80	19	— 2.6	—
— 9.41	288	4.0	part. upper 4^d	— 8.77	174	2.3	part. lower 2^d
— 5.39	83	10.3	part. lower 10^d	— 4.75	329	13.6	tot. upper short.
— 1.36	238	19.9	tot.	— 0.72	124	19.7	tot.
+ 2.67	33	17.8	tot.	+ 3.31	279	14.6	tot. lower
+ 6.70	188	6.9	part. lower 7^d	+ 7.33	74	8.2	part. upper 8^d
+ 10.72	343	1.4	part. upper 1^d	+ 11.36	229	— 3.3	—
+ 14.75	138	— 9.4	—				5 months.
			5 months.	— 15.28	358	— 6.9	—
— 11.89	267	— 1.7	—	— 11.25	153	— 3.2	—
— 7.86	61	5.8	part. upper 6^d	— 7.22	308	8.7	part. lower 9^d
— 3.82	216	14.0	tot. low. short.	— 3.20	103	14.5	tot. upper
+ 0.21	11	22.3	tot.	+ 0.83	258	19.6	tot.
+ 4.24	166	12.7	tot. upper short.	+ 4.86	52	13.5	tot. lower short.
+ 8.26	321	5.7	part. lower 6^d	+ 8.89	207	1.9	part. upper 2^d
+ 12.29	116	— 3.4	—	+ 12.92	2	— 2.4	—
			5 months.				

the fact that the more or less sloping direction of the movement of the moon with regard to N—S also returns after a saros period, because this direction depends upon the time of year, and the saros differs only 11 days from a year [1]).

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[1]) As the *v* shifts over 2° per saros, the initial value will be after say 10 of these periods considerably changed, and the character of the series will become

Only the fact, therefore, *that after* 5 *series the same aspect in each eclipse and the same character in the eclipse series returned* makes it comprehensible, how the periodicity could at last be discovered. The omission of a number of eclipses, which were invisible owing to daylight and cloudiness still made it difficult; it wasonly possible in the very favourable climate of Babylon. But when once the regularity of the series was discovered, in the course of centuries, when the successive eclipses were collected in lists, their periodical recurrence after five series must be noticed at last.

IV.

That the saros really came into existence in this way, can be seen from the construction of the above mentioned text. In the clearly legible portion (on the right and left columns are broken off) in 6 columns every time a number is found (which rises by one each 2 lines downwards and evidently represents the year) side by side with the name of a month (in our table on the next page indicated by Roman figures I—XII). The months follow each other with intervals of 6, except at the horizontal lines, where they follow each other with an interval of 5; after the name under the horizontal line there is always added "5 months". Sometimes the interval in other places seems to be only 5, but then a second 12th month has been inserted, and under the year-number then stands "dir" (sometimes VI dir, when a 2nd Ululu is introduced).

The year numbers begin again every time with 1, accompanied by the first syllable of a King's name; from these names it appears that the beginning of the table is year 31 of Artaxerxes II (—373), which are followed by those of Ochus (Umasu), Arses, Darius, Alexander, Philip, Antigonus, and Seleucus: the last years are continued as Seleucidean era. Epping and Strassmaier, by comparison with Oppolzer's Canon, have established the fact that the total eclipses always fall in the middle of the divisions separated by horizontal lines.

We here have, therefore, the same kind of list of eclipse moons as we supposed above; each column contains 5 series, some of 8, some of 7 names of months, which together form a saros; the columns placed next to each other are 6 successive saros periods. This text, therefore, is in the first place a proof that the Babylonians

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different from those in the examples we have taken. This is the reason why, as Schiaparelli noticed from Oppolzer's Canon, sometimes for several centuries in succession some series consist of only 4 eclipses, while after that for several centuries each series consists of 5 or 6 eclipses.

TABLE II. Saros-Canon Sp. II. 71.

X	X	dir XI	XI	XI	dir XII
32 IV	4 IV	1 Ar. IV	5 V	11 V	29 V
dir X	X	X	¹) XI	XI	XI
33 II 5 m.	5 III 5 m.	2 III 5 m.	6 III 5 m.	12 IV 5 m.	30 IV 5 m.
VIII	dir IX	IX	IX	¹) X	X
34 II	6 II	1 Da. III	1 An. III	13 III	31 IV
dir VIII	VIII	dir IX	IX	IX	dir X
35 I	7 II	2 II	2 III	14 III	32 III
VII	VIII	VIII	dir IX	IX	IX
36 I	8 II	3 II	3 II	15 III	33 III
VII	VIdirVII	VIII	VIII	dir IX	IX
XII 5 m.	XII 5 m.	4 I 5 m.	4 I 5 m.	16 I 5 m.	34 II 5 m.
37 VI	9 VI	VIa	VII	VII	dir VIII
dir XII	XII	XII	5 I	17 I	35 I
38 V	10 VI	5 VI	VIa	VII	
XI	dir XII	XII	XII	18 I	
39 V	11 V	1 A VI	6 VI	VIa	
XI	XI	dir XII	XII	XII	
40 IV 5 m.	12 IV 5 m.	2 IV 5 m.	1 Si. V 5 m.	19 V 5 m.	
dir X	X	X	dir XI	XI	
41 III	13 IV	3 IV	2 IV	20 V	
IX	dir X	X	X	dir XI	
42 III	14 III	4 IV	3 IV	21 IV	
IX	IX	dir X	X	X	
43 III	15 III	5 III	4 IV	22 IV	
dir IX	IX	IX	dir X	X	
44 I 5 m.	16 II 5 m.	6 II 5 m.	5 II 5 m.	23 III 5 m.	
VII	dir VIII	VIII	VIII	dir IX	
45 I	17 I	7 II	6 II	24 II	
VII	VII	dir VIII	VIII	VIII	
XIIa	18 I	1 Ph. I	7 II	25 II	
46 VI	VII	VII	dir VIII	VIII	
XII	XIIa	2 I	8 I	26 II	
1 U. VI	19 VI	dir VII	VII	VIII	
XI 5 m.	XI 5 m.	XII 5 m.	XII 5 m.	XIIa 5m.	
2 V	20 V	3 V	9 VI	27 VI	
dir XI	XI	XI	dir XII	XII	
3 IV	21 V	4 V	10 V	28 VI	

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[1] »dir« ought to have been given here too.

actually were acquainted with the series of lunar eclipses belonging together, as SCHIAPERELLI supposed. Further it shows that the Babylonian saros was not simply a period of 223 lunar months, but a group of 5 series, each of which consists of 7 or 8 full moons, excepting the extreme, all eclipse moons. It clearly demonstrates, therefore, that the saros must have arisen, as suggested above, from the knowledge of SCHIAPARELLI's series, which represent a more primitive stage of science, by noticing another periodicity in them. It is worth notice, that arising in this way, the fact which at first seemed to be a difficulty, viz. that the saros is 8 hours more than a round number of days, becomes of absolutely no importance. In this genesis of the saros the time of day at which an eclipse occurs plays no part at all.

The text of STRASSMAIER gives us no conclusive evidence as to the time at which the saros originated. It dates at its very earliest from the 3rd century B. C., when the Seleucidean era was already in use, and it represents a somewhat higher development of knowledge already. For in it not only the saros itself occurs, but apparently also a knowledge of the imperfection of the saros. As, after this period, the value $P = L - \Omega$ does not return to exactly the same value, the first terms of each series must after a time become the last of the previous series, the 5 lunar intervals must leap forward one interval; and in the Babylonian Canon, therefore, the horizontal lines must come down one line after a certain number of saros periods. STRASSMAIER assumes that the reason for the top line of the Canon falling in the middle of a series is, that originally there were a great number of columns on the left, that the list, therefore, began in very ancient times and that by this constant leaping over the top dividing line has come down 3 lines. This would show, again, that the compilers of this Canon already knew that the saros was not exact. The first knowledge of the saros itself, therefore, must be looked for in the previous centuries, perhaps the 4th or 5th B. C. This shows at the same time that the familiar story according to which the Greek philosopher THALES predicted a total suneclipse (that in 585 B. C.) by means of a knowledge of the saros borrowed from the Babylonions, can only be regarded as a fiction. At that time the saros was still unknown, and moreover the saros of later times referred only to the return of lunar eclipses.

Astronomy. — "*Investigation concerning the precession-constant and the systematic proper-motions of the stars*". By Dr. C. DE JONG. (Communicated by Prof. E. F. VAN DE SANDE BAKHUYZEN).

(Communicated in the meeting of September 29, 1917).

In a former paper in these Proceedings, I have given a resumé of the results of a determination of the precession-constant and the systematic proper-motions of the stars, obtained by the comparison of KÜSTNER's catalogue of 10663 stars (Veröff. der Königl. Sternwarte Bonn, N°. 10) with some zone-catalogues of the "Astronomische Gesellschaft." Since then I have continued the investigation with new material, commencing with the proper-motions deduced from a comparison of KÜSTNER's catalogue with BESSEL's zone-observations between 0° and + 15° declination. Although I have published the results of this investigation together with those previously obtained in my dissertation [1], and hope in a further extended and completed investigation (which will include all BESSEL's zone-observations in so far as they occur in KÜSTNER) to publish all these results at its full length, I beg to offer here a short resumé of the recently completed second part and of the conclusions which would appear to follow from the investigations so far accomplished. Together with this I shall give a comparison of my results with these of similar investigations by others, the latter being reduced afresh by the means at present at our disposal.

1. *Immediate comparison.*

BESSEL's coordinates (in future referred to as B. Z.) were taken from the original publication of the zones by BESSEL in the "Astronomische Beobachtungen auf der Königl. Univ. Sternwarte zu Köningsberg", Abth. VII—XVII, 1821—1833. These coordinates are reduced to 1825 by LUTHER's reduction tables (Astr. Beob. Königsberg. Abth. XXXVII, Th. II) and then, after having been freed as far as possible of errors by the corrections in Abth. XXXVII, Th. I and from RISTENPART's Fehlerverzeichniss, compared with the coordinates reduced to 1825 in KÜSTNER's Catologue (Kü). It then appeared that, with the exception of 29 double stars, which were excluded,

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[1] Onderzoekingen omtrent de praecessiekonstante en de stelselmatige Eigenbewegingen der sterren, Leiden 1917.

the catalogues had 1489 stars in common between the above mentioned declination limits, 1695 observations of which occur in B. Z. The epochs are 1823.0 and 1895.2, the difference of epoch being therefore 72.2 years.

2. *Reduction of the differences Kü — B. Z. to a Fundamental system. Determination of the constant zone-errors of B. Z.*

It is well known, that the separate zones of Bessel show in several cases fairly large systematic, usually constant, errors, the determination of which is imperatively necessary in researches as the present one. This determination can be made by comparison of B. Z. with simultaneous observations or with coordinates which have been obtained by interpolation between earlier and later observations. In this case I have used for the purpose coordinates from Auwers-Bradley, Tobias Mayer's Sternverzeichniss and Argelander's Catalogus Aboensis, which between them have almost 600 stars in common with B. Z. For these stars the differences Au Br — B. Z., TM — B. Z., and Abo — B. Z. were taken and kept separated according to Bessel's zones. After all differences Abo — B. Z. had been made to correspond to the differences Au Br. — B. Z. by the addition of the systematic difference Au Br. — Abo (the differences TM — B. Z. do this already), all differences that were shown to possess the same weight, were simply averaged for each of Bessel's zones, whereby, thus, for each zone a value of the reduction to the system Au Br was found.

In a number of zones the number of stars that occurred in the catalogues used for comparison proved to be too small for the deduced reduction to Au Br to be regarded as sufficiently accurate. I have compared these zones (by means of stars in common) with as many other zones as possible, the corrections for which had been determined with satisfactory accuracy, in order to find reliable corrections for these zones also. In most cases the result was satisfactory.

In a few cases it was possible to determine the total zone-errors in an indirect way, viz. by using the A. G. catalogues Albany and Nicolajew, in such a way that I could make myself free from the systematic changes during the difference of epoch.

Everything taken together it was possible in almost all cases to deduce a sufficiently reliable final value for the *individual* zone corrections. In only two cases the *systematic* reduction Au Br — B. Z. which I had deduced and calculated for comparison for all zones, had to be taken as total correction.

3. *Immediate results of the comparison.*

The zone-corrections found were afterwards added to the directly obtained differences Kü—B.Z. with reversed sign. From the quantities so obtained I then formed mean values in various ways.

In the first place two groups were distinguished according to magnitude (limit $8^m.50$ according to Kü): group *B* and group *F.*

Then solutions were carried out, by which in varying degree the largest P. M. are excluded:

a. *solution A'*, in which were only excluded all stars from zones 1, 2, 5, and 7 (which show great anomalies), besides those stars from other zones (13 in number), which show in one of the two coordinates a very great difference Kü—B.Z., which, as a further investigation showed, could not be explained to a large extent by P. M.;

b. *solution A*, in which a small number of the largest P. M. was excluded *as well*, viz. all differences which deviated by more than 0^s8 or $10''0$ from a "normal" value for Kü—B.Z. derived from a preliminary solution; these were 39 in number;

c. *solution E*, in which I excluded all differences which deviated by more than 0^s40 or $5''0$ from the normal; there were 315 of these.

In all these cases a deviation in one of the coordinates has caused the *complete* exclusion of the star.

Further I reduced all results to the system of Auwers' New Fundamental Catalogue, while at the same time I calculated all the reductions so as to be able to pass over to the system of Newcomb or Boss (Prel. Gen. Cat.).

4. *Determinations of the unknown quantities.*

The mean numbers are, as formerly, represented by formulae of the form

$$\Delta\alpha = a + b \sin \alpha + c \cos \alpha + d \sin 2\alpha + e \cos 2\alpha + f \sin 3\alpha + g \cos 3\alpha$$
$$\Delta\delta = a' + b' \sin \alpha + c' \cos \alpha + d' \sin 2\alpha + e' \cos 2\alpha + f' \sin 3\alpha + g' \cos 3\alpha,$$

the actual unknown quantities were deduced from the coefficients of these formulae, after corrections had again been introduced depending upon the systematic connection between the distances of the stars and the galactic latitude. The results obtained are briefly discussed below.

5. *Results for the precession.*

a. *from Right Ascensions.* The six values of the precession from the various solutions agree very satisfactorily, especially those obtained from bright and faint stars:

$$\left.\begin{array}{ll}\text{bright:} & \Delta m = -1''21 \\ \text{faint:} & \Delta m = -1''26\end{array}\right\} \text{per century.}$$

In order to investigate this point further I divided the material into three groups according to the magnitude. Again no influence of the magnitude upon Δm was perceptible. *We are therefore iustified in drawing the important conclusion that in all probability* BESSEL's *magnitude error was very small.*

The above values Δm are corrections to the values of STRUVE-PETERS; reducing to NEWCOMB we find as the mean:

$$\Delta m \text{ NEWCOMB} = -0''72 \text{ per century.}$$

b. From the Declinations. Here, also, the agreement between the solutions is satisfactory; the general mean is:

$$\left.\begin{array}{ll}\Delta n \text{ (STRUVE-PETERS)} & = -0''80 \\ \Delta n \text{ (NEWCOMB)} & = -0''27\end{array}\right\} \text{per century.}$$

To deduce a value for the precession correction *from R. A. and Decl. together,* it is simplest to calculate the correction Δp for the lunisolar precession both from Δm and Δn. As corrections of NEWCOMB's values I found:

$$\begin{array}{ll}\Delta p \text{ (from } \Delta m) & = -0''78 \\ \Delta p \text{ (from } \Delta n) & = -0''68\end{array}$$

or, if we introduce another correction to the value of Δp from the R. A. on account of the correction of the motion of the equinox N_1 which NEWCOMB [1]) considered to be probable:

$$\begin{array}{ll}\Delta p \text{ (from } \Delta m) & = -0''45 \\ \Delta p \text{ (from } \Delta n) & = -0''68\end{array}$$

which are in good agreement. If these values are combined with the weights 2 and 1, the result becomes:

$$\Delta p = -0''53.$$

A reduction of the results to the Fundamental Systems of NEWCOMB and of Boss gave practically agreeing values. Finally I assumed as the final result of this investigation:

$$\Delta p = -0''54; \quad \Delta m = -0''50; \quad \Delta n = -0''22.$$

In the first part of my investigation containing the comparison of Kü—Zone-Catalogues of the A. G. the following results were

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[1]) Precessional constant, p. 69 sqq.

obtained as means of values which agreed very satisfactorily among themselves:

$$\Delta p = + 0''50\,;\ \Delta m = + 0''46\,;\ \Delta n = + 0''20.$$

The result of my researches so far made is therefore a precession-value that deviates little from that found by Newcomb in 1896.

In the following table I give, besides my own result, those according to other investigators, as they are after the new reduction, the results for Δp from Right Ascensions and Declinations being kept separate.

RESULTS FOR THE CORRECTION OF NEWCOMBS' LUNISOLAR PRECESSION, THOSE FROM EARLIER INVESTIGATIONS ACCORDING TO A NEW REDUCTION.

Source	mean magn.	number of stars	$\Delta p\ (\alpha)$	$\Delta p\ (\delta)$	weight
Newcomb, Auwers-Bradley	5^m5	3181	−0″01	+0″02	3
Kapteyn, Auwers-Bradley and others	5. 5	3339	0.00	0.00	
Dyson and Thackeray, Greenw.-Groombr.	5. 9	1657	+1.41	+0.43	½ 1½
Dyson and Thackeray, Greenw.-Groombr.	7. 9	2050	+1.41		½
Boss, Prel. Gen. Cat.	5. 7	6188	−0.08	+0.85	3
Nyrén, Schjellerup-Bessel Zonae	8. 3	5300	−2.14	—	1
Dreyer, Schjellerup-Lalande Zonae	7. 8	3300	−0.17	—	½
Bolte, Schjellerup-Lalande Zonae	7. 8	3300	—	−0.10	1
Boss, Albany-Lalande Zonae	8. 5	1687	+0.90	+2.90	½
Boss, Albany-Bessel Zonae	8. 9	3436	+1.95	+0.70	1
de Jong, Küstner-A. G. catalogi	7.25	1259	+0.47	+0.85	1
de Jong, Küstner-A. G. catalogi	9.19	1830	+0.23	+0.70	1
de Jong, Küstner-Bessel Zonae	7.57	730	−0.47	−0.48	1
de Jong, Küstner-Bessel Zonae	9.09	910	−0.53	−0.72	1
4 determinations from bright stars	5^m7		+0″07	+0″43	
5 „ between 7^m0 and 8^m0	7. 8		+0.21	+0.24	
5 „ below 8^m0	8. 9		−0.01	+0.61	
Mean			**+0″07**	**+0″42**	

Finally I combined all the determinations in three groups according to magnitude, giving to the separate results the weights of the last column which I determined by a rough estimation.

A comparison of the results for Δp in the three magnitude-groups shows that the influence of magnitude on the precession-correction is not perceptible, while it remains uncertain whether there is a systematic difference between the results deduced from R.A. and Decl.; it is certainly not great. The exclusion of a single result has immediately a great influence upon the final value. Taking everything together there would seem to follow as final result:

$$\Delta p = +\,0''25 \text{ per century,}$$

whereby the precession constants for 1850 would become:

$$\begin{aligned} p_1 &= \text{lunisolar-precession} = 50''3709 \\ m &= \text{precession in R.A.} = 46.0734 \\ n &= \text{precession in Decl.} = 20.0521. \end{aligned}$$

6. *Results for the elements of the solar motion.*

The results of the E-solutions have no sharply defined significance for the value of the solar motion. I therefore combined in the following table the results of the groups A' and A of the comparison Kü—B.Z.; I also included in the results the A-solutions of the differences Kü—A.G.-catalogues. The values are throughout the mean of three results, obtained according to the systems of AUWERS, NEWCOMB, and BOSS.

RESULTS FOR THE SOLAR MOTION.

	BESSEL				A. G.-Catalogues	
	BA'	BA	FA'	FA	BA	FA
	magnitude 7^m 57		magnitude 9^m 05		7^m 25	9^m 19
X	+ 0″69	+ 0″71	+ 0″13	+ 0″16	+ 0″68	+ 0″62
Y	— 1.65	— 1.14	— 1.41	— 1.28	— 3.14	— 1.96
$\sqrt{X^2+Y^2}$	1.79	1.34	1.41	1.28	3.21	2.06
Z	+ 2.00	+ 1.42	+ 1.99	+ 1.83	+ 2.26	+ 3.02
$\sqrt{X^2+Y^2+Z^2}$	2″69	1″96	2″44	2″24	3″92	3″65
A	292°8	302°0	275°3	275°9	282°3	287°7
D	+ 48°3	+ 46°7	+ 54°6	+ 55°0	+ 35°2	+ 55°7

For the coordinates of the apex the E-solutions may also be of value. Their results are as follows:

	BESSEL		A. G.-Catalogues	
	BE	*FE*	*BE*	*FE*
A	300°2	283°8	283°4	284°6
D	+ 47°6	+ 49°2	+ 40°4	+ 57°1

In the following table I have combined my final results with those obtained by other investigators according to the new reduction.

RESULTS FOR THE SOLAR MOTION.

Source	mean magn.	number of stars	X	Y	$\sqrt{X^2+Y^2}$	Z	A	D
NEWCOMB, AUWERS-BRADLEY	5^m5	3181	+ 0″13	— 2″54	2″54	+ 1″68	272°9	+ 33°5
KAPTEYN, AUWERS-BRADLEY and others.	5. 5	3339	—	—	—	—	272.2	28.4
WEERSMA, AUWERS-BRADLEY and others.	5. 6	3616	—	—	—	—	267.7	31.4
DYSON and THACKERAY, Greenw.-Groombr.	5. 9	1657	— 0.09	— 2.61	2.61	+ 1.38	268.0	27.9
DYSON and THACKERAY, Greenw.-Groombr.	7. 9	2050	+ 0.11	— 1.77	1.77	+ 1.46	273.6	39.5
BOSS, Prelim. Gen. Catalogues	5. 7	6188	— 0.16	— 3.14	3.14	+ 2.17	267.1	34.6
NYRÉN, SCHJELLERUP-BESSEL Zonae . .	8. 3	5300	— 1.37	— 2.02	2.44	—	235.8	—
DREYER, SCHJELLERUP-LALANDE Zonae.	7. 8	3300	— 0.16	— 0.83	0.85	—	259.1	—
BOLTE, SCHJELLERUP-LALANDE Zonae .	7. 8	3300	—	—	—	(— 0.15)	—	—
BOSS, Albany-LALANDE Zonae	8. 5	1687	+ 0.02	— 1.34	1.34	+ 1.71	270.9	51.9
BOSS, Albany-BESSEL Zonae	8. 9	3436	— 0.58	— 1.36	1.48	+ 1.44	246.9	44.2
DE JONG, KÜSTNER-A. G.-Catalogues. .	7.25	1259	+ 0.68	— 3.14	3.21	+ 2.26	282.3	35.2
DE JONG, KÜSTNER-A. G.-Catalogues. .	9.19	1830	+ 0.62	— 1.96	2.06	+ 3.02	287.7	55.7
DE JONG, KÜSTNER-BESSEL Zonae . . .	7.57	730	+ 0.69	— 1.65	1.79	+ 2.00	298.3	47.5
DE JONG, KÜSTNER-BESSEL Zonae . . .	9.09	910	+ 0.13	— 1.41	1.42	+ 1.99	275.3	54.6
4 determinations from bright stars . .	5^m7		— 0″03	— 2″79	2″79	+ 1″82	269°0	+ 31°7
5 „ between 7^m0 and 8^m0 .	7. 8		+ 0.36	— 1.96	1.99	+ 1.84	280.4	42.8
5 „ below 8^m0	8. 9		— 0.26	— 1.65	1.67	+ 2.09	261.0	51.4

From the arrangement of these determinations into three groups, it becomes clear that the Z-component certainly does not decrease with decreasing magnitude of the stars. The equatorial movement $\sqrt{X^2+Y^2}$ changes with the magnitude in a way that is to be expected. The results may be represented by the formula:

$$\sqrt{X^2+Y^2} = 2''83 \times (0.846)^{m-5.7}$$

to which the values correspond very accurately. For the magnitude-factor KAPTEYN (Gron. Publ. 8) found the value 0.78 and UNTHANK (Mon. Not. R.A.S., April 1916) 0.847; from my two determinations of $\sqrt{X^2+Y^2}$ followed 0.79 and 0.90 respectively.

For Z a constant value of $+1''88$ can best be taken; according to my determinations Z would increase rather than decrease in the direction of the smaller magnitudes.

From the ratio between Z and $\sqrt{X^2+Y^2}$ it immediately follows that the apex-declination D must also become abnormal. It increases greatly with decreasing magnitude.

WEERSMA, also (A determination of the Apex of the solar motion etc. Groningen 1908), found a high value for D, from TAYLOR's stars, viz. $8^m0 \ldots D = +51°8$, while COMSTOCK (Astr. Journ. 655) found for very faint stars:

$$11^m.1 \ldots\ldots D = +71°.$$

Although we should not perhaps attach too much weight to the latter results, they all point to deviations in the same direction, so that one feels inclined to seek other than accidental causes for them.

The Z component, upon which the apex-declination depends, reveals itself as a constant term in the star declinations, that is one not dependent upon the R. A. We should, therefore, in the first place look for the cause of an abnormal behaviour of Z and D in constant errors of the declinations, and it would even be natural to account for them by these, as long as we confine ourselves to a determination of the apex coordinates. If such errors exist and work in the *same* direction as the Z-component of the solar motion, when we proceed to fainter stars the equatorial component will decrease in a greater degree than the one at right angles to it, and we shall thus find a higher apex-declination.

If, on the other hand, we consider the Z-component itself, we shall continue to find that it decreases and even always by the same absolute amount, unless, when bright and faint stars are taken from different catalogues, the δ-errors are larger in the second case, and when the same catalogue is used for both, a magnitude-error in declination enters into it. As in my two investigations the value of Z certainly did not decrease towards the faint side, and as this does not seem to be the case either in the mean of the other material examined, so that in the various cases declination-errors would have to be assumed largely varying in a direction favourable to the phenomenon, such an explanation is for the present open to grave suspicion.

If, however, we do not accept an explanation by declination errors, we must attribute it to some cosmic phenomenon, the simplest being to assume a general streaming of the bright stars with relation to the faint ones, which would take place in a direction about at right angles to the equator.

At the same time it is clear that on this supposition we should proceed from an assumption for which there is not a single ground a priori, viz. that the equatorial motion may be regarded in its entirety as a purely parallactic one, or else that for the equatorial motion bright and faint stars may be considered as a homogeneous whole differing only in distance. We should, therefore, also lose the right to regard $\sqrt{X^2 + Y^2}$ as a measure for the distance.

It will be unnecessary to say that such farreaching conclusions must not be ventured upon until the foundation for them has been more firmly laid.

7. *Terms with 2α and 3α.*

The values for the coefficients of these terms ($d \sin 2\alpha + e \cos 2\alpha + f \sin 3\alpha + g \cos 3\alpha$ in the R. A. and the same with dashes in the Decl.) that were found from my two researches are as follows:

HIGHER TERMS PER CENTURY.

Source	*d*	*e*	*f*	*g*	*d′*	*e′*	*f′*	*g′*
Kü-A.G.-Cat.	−0″04	−0″38	−0″36	−0″28	−0″07	+0″64	+0″15	+0″15
Kü-B.Z.	+0.15	+0.33	−0.15	−0.34	−0.17	+0.22	+0.13	−0.14
Mean	+0.06	−0.02	−0.26	−0.31	−0.12	+0.43	+0.14	0.00

or, expressed in another way;

in R.A. mean : $+ 0''06 \cos 2 (\alpha - 50^\circ) + 0''40 \cos 3 (\alpha - 73^\circ)$

in Decl. ,, : $+ 0''45 \cos 2 (\alpha - 172^\circ) + 0''14 \cos 3 (\alpha - 30^\circ)$

These results must be regarded as very uncertain.

GYLDÉN (V. J. S. **9**, 180) found, from a few groups of stars with large P.M.:

in R.A.: $+ 1''27 \cos 2 (\alpha - 99^\circ) + 0''94 \cos 3 (\alpha - 45^\circ)$

and OPPENHEIM (A.N. **188**) from Groombridge-stars:

in R.A.: $+ 0''52 \cos 2 (\alpha - 74^\circ) + 0''17 \cos 3 (\alpha - 23^\circ)$.

A certain agreement may perhaps be traced here.

8. *Rotation according to* SCHÖNFELD.

SCHÖNFELD's equations (V. J. S **17**, 256) show that the values of the precession-constant p, deduced from α and δ, are vitiated about equally by a general rotation of the stars with the axis of rotation at right angles to the plane of the galactic, so that a comparison of these results cannot teach us anything on this point. It appears that the rotation constant dl can then only be determined by a comparison of the values of Y deduced from α and δ. In this way I have calculated the values for dl which would follow from the material at our disposal.

The results are the following:

RESULTS FOR THE ROTATION CONSTANT dl

Astr. Ges.-catalogues			BESSEL	
Catal.	*BA*	*FA*	*BA′*	*FA′*
Lei I	+ 0″26	+ 0″64		
Be A	— 0.38	+ 0.23		
Be B	+ 0.29	+ 0.60		
Leiden	+ 0.84	— 0.34		
Mean	+ 0″18	+ 0″24	— 0″42	+ 0″33

Nothing can therefore be deduced with any probability from the material here collected.

Besides the supposition of a constant dl, that of a dl depending upon the distance, might be also examined.

9. *The systematic "proper-motions of the* 2nd *kind."*

It is my intention to investigate these in the following way. From the total P.M. of the separate stars the systematic "P.M. of the 1st kind" discussed above is subtracted. The differences thus obtained will then be examined as to preferential directions, etc.

I have already made a beginning with this investigation, according to a method partly graphical and partly analytic. I hope soon to be able to publish the results.

Mathematics. — *"A relation between the polytopes of the* C_{600}*-family".* By Dr. W. A. WIJTHOFF. (Communicated by Prof. J. CARDINAAL).

(Communicated in the meeting of June 30, 1917).

1. In the manuscript left by the late Prof. Dr. P. H. SCHOUTE which served as basis for the fifth section of his last memoir on polytopes [1]), we find with reference to the form of the coordinate-symbols of the polytope $ce_1\,e_3\,C_{600}$ the following remark: *"Result much resemblance to* $ce_1\,e_2$*."*

The resemblance consists not only in the total number of vertices being the same, but also in the fact that this number is distributed over the same number of symbols which agree with each other in the number of zeros and in the number of vertices represented by each symbol.

Again, in the same manuscript the author makes this remark about the coordinate-symbols of $ce_1\,e_2\,C_{600}$: *"Much resemblance to Elte p.* 26, *but not the same. Has to be further investigated."* [2])

The time for making this further investigation, however, was lacking, neither did the writer observe a similar resemblance which exists between several other members of the said family as also between members of other families.

In this paper I intend to explain the existence of these resemblances.

2. Let each of the 600 regular tetrahedra, the limiting bodies of the C_{600}, be divided into 24 other tetrahedra by the 6 planes each going through one of the edges and the middle point of the opposite edge. Project these planes from the centre on the circumscribed hypersphere.

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[1]) P. H. SCHOUTE. Analytical treatment of the polytopes regularly derived from the regular polytopes (Section V). Verh. der Kon. Akad. v. W. te Amsterdam. Eerste sectie, Deel XII, N⁰. 2.

J. CARDINAAL. Mededeeling over een nagelaten arbeid van wijlen Prof. P. H. SCHOUTE. Verslagen, Deel XXIV 2, p. 1077—1079; These Proceedings XVIII p. 1173—74.

[2]) This remark refers to the dissertation of Dr. E. L. ELTE: "The semiregular polytopes of the hyperspaces." But the page is incorrectly quoted, and should be p. 25. See the footnote to § 7.

The whole hypersphere is thus divided into 14400 double rectangular[1]) hyperspherical tetrahedra, which I shall call fundamental tetrahedra, the vertices of which are:

the vertices of the C_{600} (points 0);

and the projections of:

the middle points of the edges (points 1);

the centres of the faces (points 2);

the centres of the limiting bodies (points 3).

The vertices of each fundamental tetrahedron are a point 0, a point 1, a point 2 and a point 3.

The elements of the fundamental tetrahedron may be easily calculated from the 6 dihedral angles. We know indeed, that the dihedral angle at the edge 01 is $= \frac{1}{5}\pi$, each of those at the edges 23 and 03 $= \frac{1}{3}\pi$, each of the others $= \frac{1}{2}\pi$.[2])

3. Deducing from the polytope C_{600}, in the manner as described in the memoir of Prof. SCHOUTE already quoted firstly the polytopes $ce_1\,C_{600}$, $ce_2\,C_{600}$, and $ce_3\,C_{600}$, and then by combination the other polytopes of the family, we easily see:

1. that the vertices of these primitive polytopes are projected on the hypersphere respectively in the points 1, 2, and 3;

2. that the vertices of the polytopes each derived from two of these primitive ones are projected in definite points of the corresponding edges of the fundamental tetrahedra;

3. those of the polytopes derived from three of the primitive ones are projected in definite points of the corresponding faces of the fundamental tetrahedra;

while finally the vertices of the polytope $e_1e_2e_3C_{600}$ obtained by combination of the four primitive ones are each projected in a definite point within one of the fundamental tetrahedra.

4. Taking one of the faces of a fundamental tetrahedron we produce the spherical surface to which it belongs through the three edges.

Since at every edge an even number of dihedral angles come together, we find, that in the produced part of one of the faces there lie three faces of other fundamental tetrahedra. Hence we see,

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[1]) W. A. WYTHOFF. The rule of NEPER in the four-dimensional space. These Proceedings IX 1, p. 529–534; Verslagen, Vol. XV 2, p. 492—497.

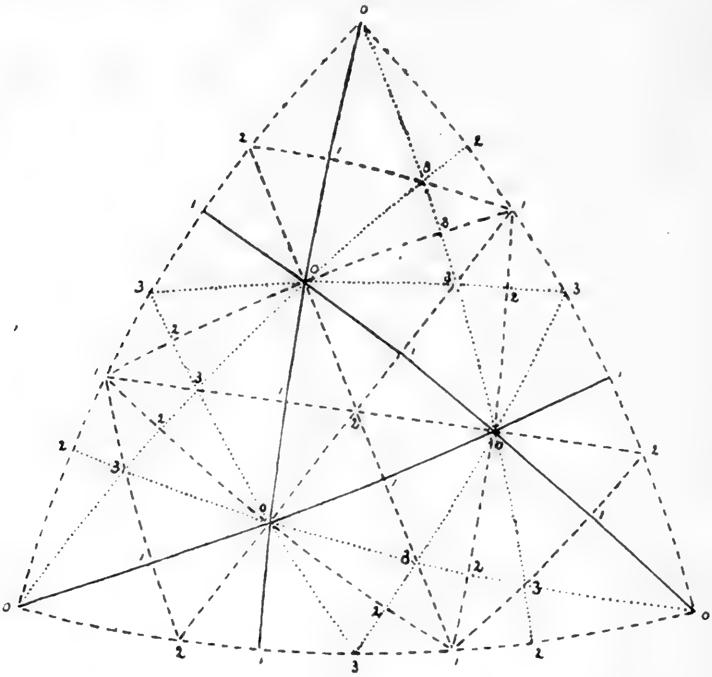
[2]) This tetrahedron is treated as an example by P. H. SCHOUTE, Mehrdimensionale Geometrie I, § 9, N⁰. 133, Aufgabe 302.

The vertices 0, 1, 2, 3 are called here A_1, A_2, A_3, A_4.

that the whole spherical surface is filled by faces of fundamental tetrahedra.

5. It is not difficult to conceive in what manner such a spherical surface is divided into faces of fundamental tetrahedra, all rectangular spherical triangles.

The figure given here represents the eighth part of such a spherical surface in stereographic projection.



The full circles in the figure divide the sphere into 12 regular spherical pentagons and 20 equilateral spherical triangles arranged like the faces of the polyhedron ce_1I or ID. Of these the figure represents three half pentagons, one whole and three half triangles. The dashes connect the vertices in the said pentagons and triangles with the middle points of the opposite sides. The dotted lines are the diagonals of the pentagons.

We see that the figure contains triangles of all four kinds, viz. 012, 013, 023 and 123. It follows therefore, since each triangle of a certain kind is equivalent in C_{600}, that all the spherical surfaces met with in the division of the hypersphere here considered are divided into triangles in the same manner.

6. Let us now look first of all at the dotted circles in the figure. These circles contain successively vertices 0 3 2 3 0 etc. They

consist of arcs measuring $\frac{1}{6}\pi$, each of which is composed of an edge 03 and an edge 23. On the former part lies the projection of a vertex of $e_3 C_{600}$, on the other that of a vertex of $ce_2 e_3 C_{600}$. Hence, viewed in this manner, the vertices of these two polytopes correspond one-for-one to each other.

Since the whole C_{600} considered possesses the symmetry of the pentagonal (= parallel or pyritohedral) hemihedrism of the regular crystal system, we know that also the said arcs measuring $\frac{1}{6}\pi$ can be classed into groups possessing that symmetry, each set of corresponding points of which may be represented by one symbol. Hence it is clear that also the coordinate-symbols correspond one-for-one to each other, and that the corresponding coordinate-symbols represent the same number of vertices.

They likewise agree in the number of zeros. For besides the vertices the said arcs have no point in common with a coordinate-space unless when lying entirely in it.

Two corresponding symbols do not always agree in the number of equal coordinates. It is found indeed, that some groups of the said arcs are intersected by spaces bisecting the angles of the coordinate-spaces exactly in the projections of the $e_3 C_{600}$-vertices. In this case the $e_3 C_{600}$-symbol has two equal coordinates and the indication of the hemihedrism can be omitted in it, while this is not the case with the corresponding $ce_2 e_3 C_{600}$-symbols.

7. We shall next consider the dashed lines in the figure.

These contain in succession the vertices 0 2 1 3 1 2 0, etc. They are found to consist of arcs measuring $\frac{1}{4}\pi$, each of which contains an edge 02, an edge 12, and an edge 13.

We conclude from this, that a relation similar to that which connects $e_3 C_{600}$ and $ce_2 e_3 C_{600}$, likewise exists between the three polytopes $e_2 C_{600}$, $ce_1 e_2 C_{600}$ and $ce_1 e_3 C_{600}$ [1]).

The full lines contain no other edges than 01. Hence the polytope $e_1 C_{600}$ stands alone.

8. We shall now proceed to consider the division of the sphere into spherical triangles.

The dashed lines divide the sphere into rectangular spherical

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[1]) The set of points whose coordinate-symbols are mentioned on p. 25 (Table B) in Dr. Elte's dissertation, are projected in the foot-points of the perpendicular arcs drawn in each triangle 012 from the vertex 1 to the opposite side 02. It is evident that these symbols must show a similar resemblance to those of the polytopes $e_3 C_{600}$, $ce_1 e_2 C_{600}$ and $ce_1 e_3 C_{600}$ as exists between these three polytopes.

triangles, each consisting of four parts. These four parts are found to be triangles respectively of the kind 012, 013, 023 and 123.

Hence a relation as the one described is shown to exist between the four polytopes $e_1e_2C_{600}$, $e_1e_3C_{600}$, $e_2e_3C_{600}$ and $ce_1e_2e_3C_{600}$.

The polytope $e_1e_2e_3C_{600}$ obtained by composition of the four primitive forms of course stands alone.

9. I finally remark that a similar investigation as has been carried out here relative to the C_{600}-family, may be undertaken in the same manner with regard to other polytope-families in fourdimensional and in other spaces.

In the icosahedron-family for instance a relation as the one described will be found to exist between the polyhedra e_1I, e_2I and ce_1e_2I.

In the C_{24}-family it exists:

1. between e_2C_{24}, $ce_1e_2C_{24}$ and $ce_1e_3C_{24}$,
2. between $e_1e_2C_{24}$ and $e_1e_3C_{24}$,
3. between $e_2e_3C_{24}$ and $ce_1e_2e_3C_{24}$.

In consequence of the small number of coordinate-symbols the property is here much less striking than in the case of the C_{600}-family.

Physiology. — "*On the Behaviour of Megakaryocytes in the Spleen of the young Kitten*". By J. J. L. De Neve. (Communicated by Prof. J. Boeke).

(Communicated in the meeting of December 23, 1917.)

I. *Introduction.*

It is a well known fact, though hardly any mention is made of it in the literature, that in the normal mammalian spleen the giant-cells (which have been named mega- or megalokaryocytes by W. H. Howell, 1890) of embryonic life persist after birth. Nay, several French investigators, notably Jolly and Rosello (1909), and especially de Kervily (1912) have discovered that giant-cells are demonstrable in the normal full-grown spleen of most mammals.

They are, however, far more numerous in the embryonic than in the adult spleen. To find out the way in which this diminution in number takes place, has been the object of my research. I set myself the question: in what way and when do megakaryocytes disappear from the spleen of the cat. I presume to have found the answer to this question.

II. *The Literature.*

Giant-cells in the liver and in the umbilical vesicle have been oftener described than those in the embryonic spleen, because most investigators, writing about this subject, have studied the youngest stages of hematogenesis. Now the spleen is of comparatively late growth, when the bloodrelations are already so complicate as to present insuperable difficulties for a study of the formation of the blood. Besides, another difficulty is met with in the complicate structure- and tissue-relations that soon arise in the spleen itself. This, however, does not interfere with our study, since it regards only one special type of cells.

Assuming the giant-cells to perform some function, the disappearance of the cell must in one way or other be connected with this function. We, therefore, deemed it interesting to consider the views adopted by others.

Van der Stricht is among the first to discuss this point (1888 and

1891). He ascribed to the giant-cells a variety of functions, the principal of which was no doubt a phagocytic one. Flemming is more decided in his opinion, asserting that the giant-cells, wherever they appear, are pathological products, that are without a special function and even succumb. Von Kostanecki (1892) maintains that he has seen the giant-cells break up into smaller cells, and also admits a phagocytic function. On this point he is, as he himself says, at variance with Ranvier and Kuborn who hold the giant-cells to be "cellules vasoformatives". Afterwards this "vasoformative function" turned out to be merely a degeneration process. Von Kostanecki also refuses to agree with van der Stricht, who asserts that the nuclei of the phagocyted cells themselves tend to enlarge the nucleus of the giant-cells, and he, therefore, maintains that there is no reason for the conclusion that the giant-cells have been made up of different smaller cells. Finally the says about this function: "Meinen Erfahrungen nach muss ich aufs entschiedenste die Ansicht vertreten, dass die Riesenzellen der embryonalen Leber — für andere blutbildende Organe der Säugetiere wird die Ansicht auch von manchen Autoren verfochten —, so auffällig und interessant sie in ihrer Form and in ihren Lebenserscheinungen auch sein mögen, bezüglich ihrer Funktion und ihres Verhältnisses zum Vorgang der Blutbilding völlig nebensächlich und bedeutungslos sind".

Saxer (1896) discusses this point at greater length, saying that some appearances led him to suppose that the giant-cells could take up smaller cells as well as be broken up into others. He disputes Flemming's view that the giant-cells are only an "abgeartete und ausgeartete" form of cells. Saxer believes there is a certain relation to hematogenesis, but he leaves this point undecided. In the end he says: "Sodass es in der That unmöglich erscheint aus den verschiedenen Erscheinungsphasen einen einheitlichen Vorgang zu konstruieren".

Maximow (1908) supposes that the giant-cells occur where erythropoësis and granulopoësis take place, though he never observed any splitting into smaller mononuclear cells.

As early as (1901) Sophie Lifschitz had already pointed in her thesis to a parallelism between erythropoësis and the number of megakaryocytes in the spleen of the human fetus.

It appears, then, that many observers admit a relation to the formation of white and red bloodcorpuscles, but the most recent view is that of Wright (1906) and Ogata (1912) that the giant-cells form and split up thrombocytes or bloodplatelets.

We see, then, that the theories regarding the fate of megakaryocytes may be grouped under the following headings : 1o. degeneration; 2o. relation to erythropoësis ; 3o. formation of mononuclear, white bloodcorpuscles ; 4o. formation of thrombocytes.

III. *Material and Methods.*

Provided the material be good, giant-cells, when present, are always demonstrable by every proper staining method. To bring out details well the sections should not be made too thick, because the megakaryocytes are large bodies (some, indeed, measure 40μ or more). This is why some experimenters attach value only to sections of 3μ at the most. Some of them achieve the best results with paraffin-sections, but MAXIMOW e.g. asserts that only celloidin material, treated in his own complicated way yields results of any value at all. Others recommend the most various fixation-fluids and dyes.

In course of time the various fixatives have been abandoned, ZENKER's fluid being now used by most researchers, either in its original composition, or slightly modified.

The dye now generally adopted in staining methods is hematoxylin (EHRLICH)-eosin and in special cases iron-hematoxylin (HEIDENHAIN)-eosin, besides the various ROMANOWSKI-variations.

It seemed to me to be best to try these various methods, in order to select the one best suited to my purpose.

IV. *Personal Observations.*

The animal. I experimented upon cat-embryos, young kittens and adult specimens. In view of the problem I wished to solve and the facts known, I confined myself chiefly to young kittens.

Tissue. Of those kittens which were killed with chloroform and opened immediately (still warm), the spleen, the liver, and the bone-marrow of the femur were excised and put in different fixation-fluids.

Fixation. With cats ZENKER's fluid at ± 37° C. gave most satisfactory results. It yields beautifully fixed preparations and causes the least shrivelling.

Embedding. Initially the embedding was done in paraffin as well as in celloidin, but when the latter method proved ineffective, it was abandoned and only paraffin was used.

Sections. These were cut, at a venture, 7μ thick, which proved quite effectual, though I also cut some of 3μ, which, however, opened up no fresh point of departure.

Staining. After trying various staining methods subsequent to different fixations, I achieved the best results after ZENKER fixation with the iron-hematoxylin (HEIDENHAIN)-eosin stain. I proceeded as follows:

1. xylol; 2. alcohol 100 $^{0}/_{0}$; 3. alcohol 96 $^{0}/_{0}$; 4. alcohol 70 $^{0}/_{0}$; 5. water; 6. for six hours in $2^{1}/_{2}$ $^{0}/_{0}$ iron-alum used as mordant; 7. washing in water; 8. allowing to overstain in iron-hematoxylin after HEIDENHAIN; 9. washing in water; 10. differentiating in iron-alum; 11. washing and examining under the microscope; repeating 10 and 11 till the differentiation is sufficient and after this 12 washing for one hour in tapwater. Lastly after treatment with the successive alcohols, the usual after-staining with eosin.

Fig. 1. Spleen of cat, 2 weeks. ZENKER. Iron-hematoxylin-eosin, ± 250× natural size. *a.* megakaryocyte degenerating in a vein with only slightly stainable protoplasm-loop. *b.* dissolving protoplasm-filaments. *c.* pyknotic nucleus of a megakaryocyte, some protoplasm still surrounding it. *d.* megakaryocyte protruding into the vein. *e.* normal megakaryocyte against the vessel-wall. *f.* vessel-wall.

To secure a good set of animals I killed some young kittens of the same litter, one of a day's lifetime, the following after 1, 2, 3 and 4 weeks. To fill up the number I took another kitten, 46 days old, from another litter. Afterwards, of course, also some control-animals.

When studying a preparation from a one-day-old kitten we see in the spleen numerous giant-cells, mostly in groups of 2, 3 or 4, and often close to the trabeculae, so also close to the large vessels that enter the trabeculae. This typical arrangement is even more conspicuous in a preparation from a kitten of 8 days. Here a slight alteration in the appearance can also be noted. Whereas the giant-cells were on the first day comparatively quiescent and the compound nuclei formed beautiful rings and horseshoes, on the 8th day a general mobility is discernible. The number of giant-cells is, indeed, about as large as before, but the nuclei present fewer ring- and horseshoe-shapes and stain rather more deeply than their surroundings. This statement does not apply to every individual cell. Likewise the cytoplasm often stains more intensely with eosin.

Fig. 2. Spleen of cat, 1 week. ZENKER-Ironhematoxylin-eosin, 950 × natural size.
a. megakaryocyte in a mitotic stage. *b*. wall of a vein.

One single mitosis is also noteworthy. (Fig. 2). Here the cell-body is stained more basophilic and granular. In a similar preparation of a kitten of two weeks I was struck, on cursory inspection, by the location of a giant-cell in a large vein. (Fig. 1). This discovery gives a clue to the whole process as in nearly every large vein appeared several cells similarly disposed. It also soon became clear how they got there, when I encountered cells protruding crosswise through the vessel-wall (Fig. 3 *a* and *b*). On closer investigation the following process can be deduced from the various preparations: A giant-cell displaying great amoeboid activity (SCHRIDDE 1905) sends out, when about to vanish, pseudopods in all directions, which process is attended with all the typical features of degeneration. The nucleus shrinks, gets rounded, presents no longer a distinct appearance, gets much more deeply stained, in a word: pyknotic. The protoplasm of this cell-body at first stains slightly deeper with eosin, rather brownish, but in a more developed stage even the cytoplasm is no longer stainable, only the blurred periphery being visible. (Fig. 4 *c* and *d*). The cytoplasm of the cell now protrudes through the vessel-wall either actively or passively and the nucleus is carried along with it. On watching the process closely we are under the impression that it is rather a passive act than a manifestation of vital activity. The

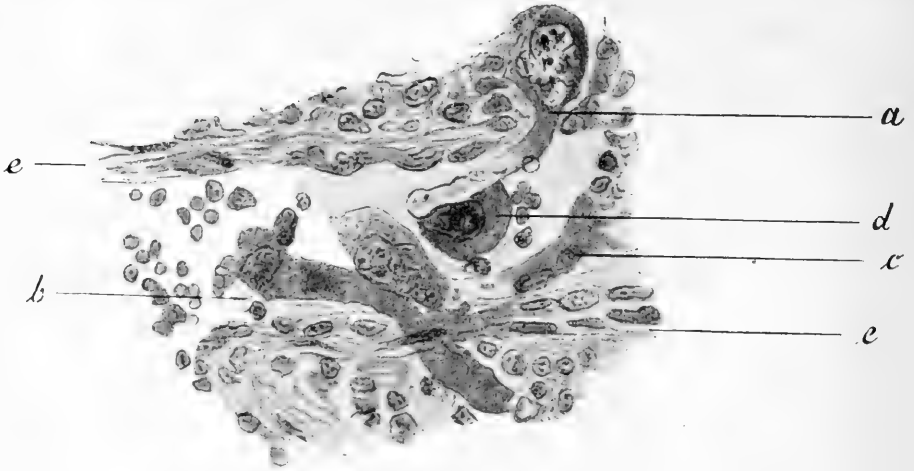

Fig. 3. Spleen of cat. 2 weeks. ZENKER Iron-hematoxylin-eosin. ± 580 × natural size. *a*. megakaryocyte with a long protoplasm-filament in the lumen of the vein, no longer stainable at the extremity. Nucleus pyknotic; still lying in the spleen-pulpa. *b* like *a*, but the shrivelled nucleus lying just in the lumen of the vessel. *c*. degenerating megakaryocyte lying entirely in the vein. *d*. megakaryocyte with easily stainable cytoplasm, without filaments, but with markedly pyknotic nucleus. *e*. vessel-wall.

filaments of the protoplasm (they can no longer be called pseudopods) extend invariably in the same direction in the same bloodvessel (Fig. 3 *a* and *b*). It would seem then that the protoplasm is carried along by the blood-stream, when once it has entered into the lumen of the vessel. Now it is still a subject of dispute whether the vessel-wall in the spleen is closed or whether there are openings in it. When assuming the latter, the limp, inert, degenerating giant-cells, lying close to the vessel-wall, are supposed to be pressed through the openings in the vessel-wall, when the spleen contracts. This squares with the appearances presented by the megakaryocytes in these preparations.

For, if it were an active diapedesis we should see a pseudopod, after it had once protruded through the bloodvessel, lodged against the opposite side of that vessel; now this we do not see anywhere. Contrariwise we see that a protoplasm-process is gradually prolonged in the lumen of the vessel and dissolves. If sometimes a whole cell is seen in the lumen of the vessel, all sorts of irregular protoplasmic strands are seen to form (Fig. 1 *a* and *b*), while the detached pyknotic nuclei are occasionally noted in the lumen entirely deprived of cytoplasm. I take it, therefore, that here we have to do with an unmistakable degeneration.

Further research revealed similar degenerations already in the first week. Also in the spleen-pulpa groups are found here and there presenting an appearance exactly like those afforded by the degenerating giant-cells in the lumen or in the wall of the vessel. The process, then, does not depend entirely on the protrusion into the vessels, which lends additional support to the conception that this protrusion is of a passive, not of an active nature.

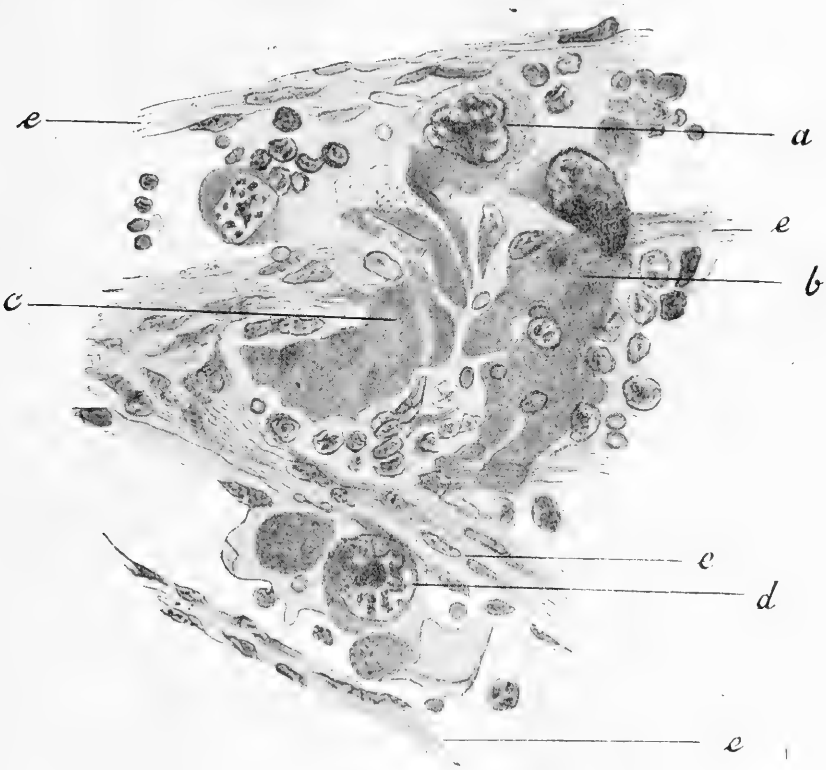

Fig. 4. Like fig. 3. 570 × natural size.
a. megakaryocyte with pyknotic nucleus, reaching only with a narrow point the vessel-wall. *b* and *c* degenerating megakaryocytes, especially *c* is poorly stained. *d.* degenerating megakaryocyte. *e.* vessel-wall.

Not in one instance did I see in the lumen of the vessel a normal giant-cell with a beautifully horseshoe-shaped nucleus, which confirms our conception of the whole process. Now if we observe the preparations from a kitten of three weeks, we note a considerable decrease in the number of giant-cells. This does not surprise us in the least, considering the extensive dissolution. In the fourth week only few giant-cells are distinguishable, for the greater part, to all appearance, normal. Generally the number is not larger than DE KERVILY (1912) assigns for the spleen of the normal adult cat. He gives the numbers 2 per cm^2 to 20 mm^2, so it seems to be rather fluctuating.

Quite the same may be noted in the spleen of a kitten 46 days old. The number of the giant-cells seems to be rather smaller in this case. Anyhow the appearance does not differ from that of an adult cat's spleen. It is evident, then, that the giant-cells do not disappear completely, as indeed DE KERVILY has been able to demonstrate for the spleen of the majority of adult mammalia. In the Leyden laboratory I also have been enabled to detect giant-cells in all sorts of mammals except man.

The answer to the original question was now found: the giant-cells disappear from the cat's spleen through degeneration and dissolution, especially in the 2nd week after birth and in the large veins.

This does not fit in with WRIGHT's view. As early as 1906 WRIGHT described a process of extrusion also in the spleen of the kitten, but this process he holds to be a formation of blood-platelets. The process described by me is certainly not formation of bloodplatelets but a degeneration. May it be possible that WRIGHT has observed what I have seen, but that he gives a different interpretation? The process described by me leaves no room for another explanation.

WRIGHT's description is about as follows:

The megakaryocytes form pseudopods, which they send out into the small capillaries. These pseudopods are stained less intensely at the margin and have a granulated appearance. Small pieces are now constricted off, they are the thrombocytes. WRIGHT does not mention the age of the kitten upon which he experimented. Of this process he gives fourteen microphotographs, which are anything but clear, but still they resemble my findings too closely to conclude that they are widely different from WRIGHT's. He obtains these appearances by means of a special staining method of his own device with the exclusion of all others. As far as I know, OGATA (1912) is the only one who corroborates WRIGHT's histological findings, in spite of SCHRIDDE's failure (1907) to demonstrate a similar process in man. This, OGATA asserts, was because SCHRIDDE was obliged to work with post mortem material, whereas WRIGHT and himself were enabled to work with "lebenswarm fixierten Präparaten". As to the latter I was in the same condition. In this connection it seems strange that OGATA could not get good preparations when using WRIGHT's method, and succeeded when applying the SCHRIDDE-azure II-eosin method. Furthermore the drawings of Ogata also show that the process observed in the bone-marrow bears no resemblance whatever to Wright's. In other respects their assertions are also clashing. Let us, however, dismiss Ogata's process from further discussion, considering that this worker confined himself to bone-marrow, and let us give our attention more particularly to Wright's publication.

In the sections examined by myself, and they are many, I have never been able to detect a process in agreement with Wright's conception. Wright may be mistaken. He may have drawn a far-reaching conclusion from a superficial similarity. I do not think it improbable. The process described by me is so perfectly evident that anyone who examines the spleen of a kitten of two weeks, must see it. I, therefore, believe that he has also observed it, but I am positive that it is not a formation of thrombocytes. This assertion is based on the following considerations:

First and foremost the process is demonstrable with any proper fixation- and staining method, also with those that do not show thrombocytes in any portion of the preparation. Protoplasm filaments are, indeed, vaguely discernible as well as those that are still properly stained. Moreover the thickness of 3 μ, which Wright prescribes for the sections, is as little imperative as his special staining method. As stated above I took the trouble to cut sections of 3 μ, but they could not alter my opinion. The diameter of the protoplasm filaments also is many times larger than that of a thrombocyte.

Did Wright overlook the extrusion of entire degenerating cells just in the large veins (not in the capillaries)? Again, how does Wright account for the thrombocyte-formation when it should appear that also in mammalia the thrombocytes are nucleated cells? This question has not been solved as yet. H. G. Langemeyer (1916) e.g. again arrives at this conclusion. In that case the thrombocytes, which, according to Ogata, originate only from the marginal zone (Heidenhain 1907), could not possibly contain any karyoplasm. And would it be likely that the giant-cells ever continue to form thrombocytes? How then does Wright account for those detached pyknotic nuclei that occur too frequently to be considered as mere casual phenomena? They are lying in the vessels as well as in the spleen pulpa.

It is true, Wright has reserved some clinical adhesion latterly, e.g. from Hal Downey, Bunting (1909), Selling (1910) and above all from E. Frank (1915). But histologically this support is not well-founded, and besides highly debatable. It is out of place here to enter into further discussion on this point.

Now about the giant-cells in the cat's liver. These are quite similar to those of the spleen but at birth they are less abundant. After birth they disappear gradually, so that only few remain in the 4th week. However, never did I find an instance of degeneration as is encountered in the spleen.

Fig. 5. Embryo guinea-big 9 cm. long. Spleen nearly 1000 × natural size. *a*. megakaryocyte lying in the lumen of the vessel, with a process still in the wall. *b*. lumen of the vessel. *c*. vessel wall.

Because I would, not limit my research to one species of animal, I also studied the guinea-pig in quite the same way as I did the cat. The result was nearly the same. In the guinea-pig the critical moment does not occur after, but before birth. This, no doubt, is explained by the fact that the cavia has already before its birth reached a stage of development that is attained by the cat only much later. The extrusion-process showed itself most distinctly in the veins of an embryo 9 cm long (from head to caudal bend) (Fig. 5,*a*) I would have studied earlier stages of suitable caviae, had any been at my disposal. The maximal number of giant-cells of the spleen is smaller in the guinea-pig than in the cat. In guinea-pigs I did not detect such a typical degeneration accompanying the extrusion as in cats. My failure in finding the most appropriate stage is perhaps responsible for this. In a cavia of 17 days the number of giant-cells has already diminished to a number not larger than is assigned by DE KERVILY for the adult cavia, viz. 3 per 25 mm^2.

SUMMARY.

1. In the cat's spleen a process is most distinctly demonstrable in the second week after birth, in which the megakaryocytes protrude into the large veins and break up into pieces through degeneration.
2. This process is not a formation of thrombocytes.
3. A similar process takes place in guinea-pigs shortly before birth.

I would take this opportunity of thanking Dr. A. B. DROOGLEEVER

Fortuyn for his interest taken in my research as well as for his valuable help and suggestions offered me.

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Chemistry. — *"On the replacement, in amides, of an alkylanilino-group by an aniline group, by heating with aniline hydrochloride."* By G. DE BRUIN. (Communicated by Prof. P. v. ROMBURGH).

(Communicated in the meeting of December 29. 1917).

For the preparation of certain other carbamide derivatives, which I hope to describe later, I required a quantity of methylcarbanilide $(CH_3)(C_6H_5)N . CO . NH . C_6H_5$. GEBHARDT[1]) first obtained the latter compound from methylaniline and phenylisocyanate. Since, however, a quantity of methylphenylcarbamic chloride $(CH_3)(C_6H_5)N . CO . Cl$ was already available, I attempted to utilise it for the preparation of the desired substance. Accordingly one grammolecule of the chloride was dissolved in ether and two grammolecules of aniline were added. On warming under a reflux condenser aniline hydrochloride at once separated. Since the precipitate caused violent bumping it was found desirable to add a little water, in which it dissolves. In order to complete the reaction boiling for about 30 hours was necessary. After separation of the two layers, the ether was evaporated and the crystalline residue was ground up with dilute hydrochloric acid in order to remove adherent aniline. After one recrystallisation from alcohol a pure product, melting at 104° C., was obtained in practically theoretical yield.

Since the reaction proceeds so smoothly, an attempt was made to obtain the same result more rapidly by heating aniline with the chloride without dilution with ether and at a higher temperature. Two molecules of aniline were melted on a sand bath with one molecule of methylphenylcarbamic chloride and heated to about 190°. Copious fumes of hydrochloric acid were evolved and the liquid smelt strongly of methylaniline. After heating for two hours the mass was poured into cold water and the liquid was acidified with hydrochloric acid. The crystals were sucked off and recrystallised twice from alcohol. The product melted at 236° C. and was therefore not methylcarbanilide. A complete analysis gave the following result:

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[1]) B. B. 17. 2093 (1884).

C = 73.21 %   H = 5.62 %   N = 13.11 %.

These figures agree fairly closely with those calculated for symm diphenylcarbamide, which melts at 235°.

C = 73.58 %   H = 5.66 %   N = 13.21 %.

There was no depression of the melting point of a mixture of the substance with symm. diphenylcarbamide, so that the formation of the latter was established. Probably in this reaction methylcarbanilide was formed as an intermediate product, from which and from the aniline hydrochloride, also formed in the reaction, the diphenylcarbamide would be formed by elimination of methylaniline. Therefore an experiment was made to see whether symm. diphenylcarbamide is formed by heating methylcarbanilide with aniline hydrochloride. This was indeed found to be the case; the reaction product was identified as such.

Now GEBHARDT[1]), who observed analogous transformations with arylthiocarbamides already showed that methylcarbanilide breaks up on distillation into methylaniline and phenylisocyanate. We might therefore assume that in the present case phenylisocyanate is first formed and that this substance subsequently combines with aniline to form diphenylcarbamide. Against this view, however, is the fact that methylphenylthiocarbamide, which on boiling with aniline also gives diphenylthiocarbamide[2]), furnishes phenylthiocarbamide and methylaniline already by heating with alcoholic ammonia to 100°. Moreover the above mentioned transformation reminds one of the displacement, described by VAN ROMBURGH[3]) and Miss WENSINK, of the dimethylamino group by NH_2 or $NHCH_3$ in 2:4-dinitro- 1:3-phenylenediamine derivatives, with the aid of ammonia or methylamine; in that case fission of the original product is pretty well excluded.

A few further examples of this reaction were investigated.

Dimethylcarbanilide $CO[N(CH_3)(C_6H_5)]_2$, prepared according to MICHLER[4]), and heated with 2 molecules of aniline hydrochloride, again yielded symm. diphenylcarbamide.

Diethylcarbanilide $CO[N(C_2H_5)(C_6H_5)_2]$ also prepared according to MICHLER[5]) gave the same product when similarly heated with aniline hydrochloride.

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[1]) B. B. 17. 3036. (1884).

[2]) loc. cit. pag. 3034.

[3]) These Proceedings. XVII p. 1034. In the organic chemical laboratory of Utrecht other analogous cases have been demonstrated with aliphatic amines; they will be examined systematically.

[4]) B. B. **12**. 1166 (1879).

[5]) B. B. **9**. 712 (1876).

That the transformation is not, however, limited to carbamide derivatives, is shown by the following case.

Dimethyloxanilide, $C_2O_2[N(CH_3)(C_6H_5)]_2$, was heated with aniline hydrochloride. On treatment in the manner described above, a product was formed which melted at 245° C. This melting point agrees with that of oxanilide $C_2O_2(NH . C_6H_5)_2$. Mixed with the latter substance there was no depression of the melting point, so that we have here another example of the replacement of the group $— CO . N(CH_3)(C_6H_5)$ by the group $—CO . NH . C_6H_5$.

Investigations in this direction are being continued with other aromatic amines.

Ouderkerk a/d, Amstel, December 1917.

Physics. — "*The viscosity of liquefied gases.* X. *The viscosity of liquid hydrogen.*" By Prof. J. E. VERSCHAFFELT. Communication N°. 153*b* from the Physical Laboratory at Leiden. (Communicated by Prof. H. KAMERLINGH ONNES.)

(Communicated in the meeting of November 24, 1917).

A preliminary value having been obtained from the experiments described in Comm. N°. 151*g*, a new series of measurements was undertaken in order to find a more accurate value of the quantity in question. The construction of the apparatus to be used for this purpose was based on the considerations referring to similarity in the oscillatory motion of a sphere in a viscous fluid (comp. Comm. N°. 148*e* and N°. 151*f*). Taking in view the materials available for this construction it appeared to be possible to construct the apparatus in such a manner, that the motion in liquid hydrogen would be approximately similar to the motion in carbon disulphide[1]) which was studied on a former occasion (Comm. N°. 151*d* § 3), using a sphere of the same radius (2 cm.). In this case by equation (2) of Comm. N°. 151*f* (comp. also § 4 and the table), assuming for liquid hydrogen the preliminary value of $\eta = \cdot 00011$, the oscillating system had to be given a moment of inertia of about 30 c.g.s. units and a time of oscillation of about 40 secs.; this would correspond to a logarithmic decrement of $\cdot 07$ and a value of $\psi = \frac{T-T_0}{T_0} = \cdot 011$ [2]) (therefore $T-T_0 = \cdot 44$).

In these new experiments a hollow exhausted glass sphere was used (weight 8.9 grms), blown by Mr. KESSELRING, glass-blower of the laboratory, as nearly accurately round as was possible[3]). The sphere ended in a tube which had served in the blowing and was

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[1]) Originally similarity was tried with the motion in liquid air or ether (comp. Comm. N°. 151*f* § 4), but of all the substances included in this study of a possible similarity carbon disulphide was the most advantageous, because it made it possible to reach a comparatively high logarithmic decrement, without its being necessary to use too high a time of oscillation (comp. Comm. N°. 151*f*, § 4); the higher decrement is then obtained by a smaller moment of inertia.

[2]) The value of this quantity given in Comm. N°. 151*f* in the table is too small.

[3]) The diameters measured in different directions did not differ from the mean by more than $^3/_{1000}$.

also made use of for the suspension, the axis of the tube representing the axis of revolution of the sphere. The equatorial radius of the sphere was 2.0049 ± 0.0025 cms. The sphere with its tube was sealed (accurately centred) to an oscillating system, consisting of a glass tube and a copper tube with disc, as in Comm. N°. 149*b* § 2, only thinner and lighter (weight 26.5 grms). This oscillating system to which also, as in the previous experiments, a hollow cylinder belonged which was put round the copper tube and rested on the disc was suspended from a thin manganin wire (·04 mm. thick and 55 cms. long). For the rest the apparatus was the same as in the preliminary experiments with liquid hydrogen.

As in the experiments with liquid air two hollow cylinders were used. The one (C_1) made of brass weighed 33.125 grms ($=m$) and had the dimensions R_e (external radius) $= 1.1915 \pm 0.0017$, R_i (internal radius) $= 0.3970 \pm 0.0019$, h (height) $= 1$ cm. about. Its moment of inertia was therefore

$$K_1 = \tfrac{1}{2}\, m\, (R_u^2 + R_i^2) = 26.12 \pm 0.07 .$$

The other one (C_2) was made from retort-coal[1]) and covered with a thin layer of varnish; it had very nearly the same dimensions as the former, weighed 7.26 grms. and had a moment of inertia $K_2 = 5.77 \pm 0.01^5$. This value was not as in the previous case derived directly from the dimensions and the mass, but was determined by oscillation-experiments in which the oscillating system, leaving out the sphere, was first loaded with the one and then with the other cylinder. The times of oscillation were measured with a stop-watch with an accuracy of at least $^1/_{1000}$ [2]). These same experiments gave for the moment of inertia of the system without cylinder or sphere $K_0 = 3.95 \pm 0.01$. Similarly by the oscillation-method the moment of inertia of the sphere at room-temperature was found to be $K_b = 20.26 \pm 0.05^5$. [3])

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[1]) This material was selected on the ground of its having the desired specific gravity (see further on).

[2]) The chronometer had been previously compared with the standard-clock and was found to fully guarantee an accuracy of $^1/_{1000}$ in the time-readings.

[3]) The times of oscillation at 15° C. and in vacuo were as follows;

System	without	sphere	and	without	cylinder	(weight : 26.5 gr.)	$T = 14.46$ sec.
„	„	„	„	with	„ C_1	(„ 59.6 „)	40.16 „
„	„	„	„	„	„ C_2	(„ 33.8 „)	22.75 „
„	with	„	„	without	„	(„ 35.4 „)	35.89 „
„	„	„	„	with	„ C_1	(„ 68.5 „)	52.05 „
„	„	„	„	„	„ C_2	(„ 42.7 „)	39.97 „

These observations give as previously (comp. Comm. 149*b* IV § 5) a small

The oscillating system with sphere and cylinder C_2 had therefore very nearly the same moment of inertia (and thus also the same time of swing) as the system without the sphere but with cylinder C_1. [1])

The oscillations of the system when immersed in liquid hydrogen were found to be actually strongly damped ($\delta = 0.1$ about): but the regularity of the damping left much to be desired, so much so that for a somewhat accurate calculation of δ only a few small portions selected from the series of observations could be utilized. [2]) The most regular portion obtained, which was exclusively used, was as follows: the elongations α, expressed in radiants, were observed to the right (+) and to the left alternately; in the table are also given the naperian logarithms of the absolute values of the elongations.

$\alpha = +\ 0.05180$	$\log \alpha = -\ 2.9599$
$-\ \ 4925$	3.0108
$+\ \ 4685$	3.0607
$-\ \ 4460$	3.1101
$+\ \ 4230$	3.1632

From these results it follows that

$$\delta = 0.1012 \pm 0.0003.$$

This is, however, not yet the logarithmic decrement δ_0 for infinitely small amplitudes; in order to find it, we utilize equation c of Comm. N°. 151*d*, § 3 found for carbon disulphide, which equation shows, in what manner the maximum-elongation depends upon the

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diminution of the moment of the torsional couple M with increasing load (the change amounts to $\cdot 00035\ M$ per gramme); M_0 (couple without load = $\cdot 752 \pm \cdot 002$). From a few observations at different temperatures it appeared moreover, that with rising temperature the couple M diminished by $\cdot 0008\ M$ per degree.

[1]) This was the condition laid down in making cylinder C_2 (comp. Comm. 149*b*, IV, § 5); for this reason this cylinder was made of retort-coal (see note on previous page), after cylinder C_1 had been made before. In this case the condition, laid down before, that the system with or without sphere should have about the same weight, was not fulfilled; as a matter of fact it had been found, that the weight of the system has but very little influence on the torsional moment, and this was confirmed by the present experiments (comp. previous note).

[2]) Indeed, with very small elongations no damping could be observed at all: the oscillating system did not come to rest, but continued to oscillate to and fro over a few scale divisions, sometimes more and sometimes less. This must undoubtedly be partly ascribed to not entirely avoidable convection-currents in the liquid, which was found to receive radiation to a not inconsiderable degree: when the apparatus was closed (as before the liquid was under constant pressure), the vapour-pressure rose at the rate of about 2 cms. mercury per minute, corresponding to a temperature-rise of $\cdot 1^\circ$.

time, and therefore also how the logarithmic decrement $\delta = -T\frac{d \log a}{dt}$ changes with the amplitude of the oscillations. For $a = 0.05$ approximately we thus find $\delta = 1.052\ \delta_0$, and hence

$$\delta_0 = 0.0962$$

with an accuracy which may be taken at 1°/₀.[1]

The time of swing of the oscillating system with the sphere immersed in liquid hydrogen (temperature of the liquid about 20° K, the temperature of the room and thus also of the wire being 8.7° C) was

$$T = 40.20 \pm 0.10\ ^{2})$$

so that (see above)

$$T_0 = 40.20 - 0.44 = 39.8\ ^{3}).$$

The hydrogen was under a pressure of 76.9 cms mercury; thus according to the latest data about the vapour pressure of hydrogen[4]), the temperature of the liquid was: $20.39 + \frac{9}{200} = 20^\circ.43$ K. At that temperature the sphere (according to the data supplied in Comm. N°. 85) had a radius of 2.002 cms, and therefore a moment of inertia of $20.26 \times \left(\frac{2.002}{2.005}\right)^2 = 20.23$, so that the total moment of inertia of the oscillating system in the experiment in liquid hydrogen was

$$K = 3.95 + 5.77 + 20.23 = 29.95.$$

For the moment of the couple exerted on the sphere by the viscosity of the liquid we now find (by equation 28′ of Comm. N°. 148*b*).

$$L'_1 = \frac{2\delta_0 K}{T_0} = 0.1448.$$

In order to determine the couple of the frictional forces on the not-immersed part of the oscillating system the sphere was, as previously, removed and cylinder C_2 was replaced by cylinder C_1.

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[1] This logarithmic decrement is considerably larger than what was expected from the similarity with carbon disulphide (see above). But it should be borne in mind that on account of the complicated structure of the oscillating system there can only be question of similarity in a very rough sense; for as regards the part which is not immersed in the liquid there is no question of similarity at all. For this reason the reduction of δ to δ_0 is not so accurate as might have been the case in the case of perfect similarity.

[2] As the decrement of the swings, this time of swing was subject to irregular variations (of a few tenths of a second).

[3] This value agrees closely with that which is to be deduced from those mentioned above taking into account the changes in the temperature of sphere and wire.

[4] Comp. Comm. N°. 152*a*.

Under these conditions the result was

$$\delta_2 = 0.0091$$ [1]

which gives (as $T = 40.06$ and $K = 3.95 + 26.12 = 30.07$)

$$L'_2 = 0{,}0137,$$

so that the torsional moment of the friction on the sphere alone amounts to:

$$L' = L'_1 - L'_2 = 0.1311.$$

The density of liquid hydrogen at the temperature of observation (20°.43 K) being 0.0708 [2], it follows, according to equation (*a*) of Comm. N°. 148*b*, that

$$\eta = 0.000130$$

with a degree of accuracy which may be placed at about 1°/₀.

A further experiment was made from which a preliminary value could be derived for the viscosity of the hydrogen vapour. The oscillations of the system carrying the sphere and cylinder C_2 were observed, while only a little liquid hydrogen was left on the bottom of the vessel, the sphere thus being just above the liquid surface in the vapour. In these circumstances a damping was found with a decrement of 0.0128, whereas the system without the sphere and with cylinder C_1 gave $\delta_2 = 0.0093$ (at the same room-temperature of 18° C and the same pressure of 76.9 cms). The decrement due to the friction on the sphere alone is therefore $\delta_1 = 0.0035$; hence with $T = 40$, $K = 30$ and $\mu = 0.00119$.

$$\eta = 0.000010$$ [3]).

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[1] This result was obtained from a few experiments at different room-temperatures and different pressures, so that a reduction could be made to the same temperature (8.7° C.) and pressure (76.9 cms.) as in the experiment with the sphere in the liquid. These experiments indicated a small increase of δ with the temperature, the change with pressure being insensibly small.

[2] Comp. Comm. 137*a*.

[3] This preliminary value agrees well with that found by H. KAMERLINGH ONNES, C. DORSMAN and S. WEBER by the method of transpiration (Comm. N0. 134*a*). The accuracy of this result was further tested by a few observations made in air (at 10° C.). These gave for the system with cylinder C_2 and sphere (the latter suspended in a large vessel): $\delta = .0448$ and for the system without sphere, but with cylinder C_1: $\delta = .0154$, so that for the sphere by itself: $\delta = .0294$, giving ($K = 30$, $T = 40$, $\mu = .00126$) $\eta = .000177$, in good agreement with known data (comp. Phys. Rev., 8, p. 738, 1916).

Physics. — *"On the measurement of low temperatures.* XXVII. *Vapour-pressures of hydrogen in the neighbourhood of the boiling point and between the boiling point and the critical temperature."* By P. G. CATH and H. KAMERLINGH ONNES. (Communication No. 152*a* from the Physical Laboratory at Leiden).

(Communicated in the meeting of June 30, 1917.)

§ 1. *Introduction.* Having obtained in the hydrogen-vapour cryostat [1]) the means of keeping temperatures constant for a considerable time between the boiling point of hydrogen and the melting point of oxygen, we were able to carry out a long desired determination of the vapour-pressure of hydrogen above the boiling point. Our investigation on this subject was in connection with the determination of the critical point of hydrogen and extends as far as this point. Accordingly a few of our results falling in the immediate vicinity of the critical temperature have already been published in the paper on the latter subject. [2])

In order to obtain a connection with the measurements of KAMERLINGH ONNES and KEESOM of the vapour-pressure below the boiling point, a few determinations were made in the neighbourhood of this point by means of the cryostat with liquid hydrogen and the vapour-pressure apparatus which were also used by the authors mentioned.

As we shall see in § 5 the agreement at the boiling point of hydrogen is not so close as we thought we might expect, considering the degree of accuracy of the measurements in both cases; consequently a renewed investigation with a view to establishing the boiling point of hydrogen with the accuracy required remains much to be desired.

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[1]) H. KAMERLINGH ONNES, Comm. No. 151*a*. These Proceedings XIX (2) p. 1049.

[2]) H. KAMERLINGH ONNES, C. A. CROMMELIN and P. G. CATH, Comm. No. 151*c* These Proc. XX (1) p. 178. The present communication we have included in the series "On the measurement of low temperatures", as it forms an immediate continuation of Comm. XXIII of that series. It is, however, also closely connected with the series "Isothermals of di-atomic substances etc.", of which the communication just quoted froms N⁰. XIX.

§2. *The helium-thermometer.* For our measurements we made use of a new gas-thermometer. In its main points it resembles the type of instrument last used by KAMERLINGH ONNES and HOLST[1]). We have introduced the improvements which had proved appropriate in the case of the thermometer for measuring temperatures to be obtained with liquid helium,[2]) as well as others which were suggested by the experience gathered in various measurements in the laboratory. One of the latter consists in the tube of the manometer which is on the side of the thermometer-bulb (the adjustment-tube) and the one which is turned the other way no longer being connected by a rubber tube,[3]) but by a glass tube) which involves the adjustment of the mercury by tap $K\Theta_1$ and $K\Theta_5$ (fig. 3 and fig. 4)[4]) being obtained by means of a mercury vessel D connected to the tube by a rubber tube, air-traps (near tap $K\Theta_{10}$ and tap $K\Theta_3$) being interposed[5]). The various points are shown[6]) in fig. 1 which gives a general aspect of the thermometric apparatus and which must now replace Pl. I of Comm. No. 95*e*, and in figg. 3 and 4 which give diagrammatic views of the arrangement of the apparatus for our

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[1]) Comm. N⁰. 141*a*. These Proc. XVII (1) p. 501 This type was described for the first time in Comm. No. 27, Zitt. Versl. Akad. Amsterdam, May 1896 as type *b*. It was used by KAMERLINGH ONNES and BOUDIN Comm. No. 60. (these Proc. III. p. 299) and afterwards, with only small modifications, by KAMERLINGH ONNES and BRAAK (Comm. No. 95*e*; these Proc. IX. p. 367.) Particulars may be found in the Communications quoted. In putting together the drawings from Comm. 27, 60 and 95*e* it should be noted, that even before Comm. No. 60 instead of *m* fig. 1 Pl. II. Comm. No. 27 for the purpose of filling the thermometer a tap like $K\Theta_g$ in fig. 3 of the present communication had been fitted to the apparatus (comp. Comm. No. 60; these Proc. III p. 299, the end of § 21).

[2]) Comm. No. 119. These Proc. XIII, (2) p. 1093 and Comm. 124*b*. These Proc. XIV (2) p. 678.

[3]) The pressure-difference between the inside and outside of the tube in the measurement of the temperatures which are obtained with liquid hydrogen amounts to about 1 atmosphere and at this pressure a rubber tube, even when intended for a high vacuum and of a good quality, transmits air. Moreover the mercury becomes soiled in the long run, a treatment of the tube with caustic soda does not guard against this altogether. And finally the elastic time-action may be troublesome when adjusting to a definite pressure.

[4]) An additional figure: fig. 4 has been introduced in the translation.

[5]) In this figure some accessories have been introduced in the translation. It remains to be noted that the glass tubes connecting the different apparatus are given diagrammatically and some joints therefore are not shown in the figure.

[6]) When the adjustment is accomplished, the tap $K\Theta_5$ is closed. Any further small changes in the adjustment are carried out as in Comm. No. 124*b* by means of the adjusting screw fitted for this purpose (comp. fig. 1 and σ_b and σ_c on Pl. I Comm. No. 119 and fig. 3 of Comm. No. 124*b*.)

investigation regarding the vapour-pressure of hydrogen above the boiling point; if the communications quoted above are consulted, fig. 1 and, as regards the thermometer, figg. 3 and 4 will not require much explanation.[1]

At pressures above and below one atmosphere the mercury surface can now be read in one and the same tube $l'a_1$ [2]. By tap $K\Theta_8$ a vacuum can be produced above $l'a_1$ which is kept up by means of DEWAR's method of charcoal cooled in liquid air with $K\Theta_8$ and $K\Theta_{95}$ open and $K\Theta_9$ closed. For pressure-readings above one atmosphere $K\Theta_8$ is connected with a barometer and a space of constant and practically atmospheric pressure in the same manner as with the previous thermometer. (See fig. 1).

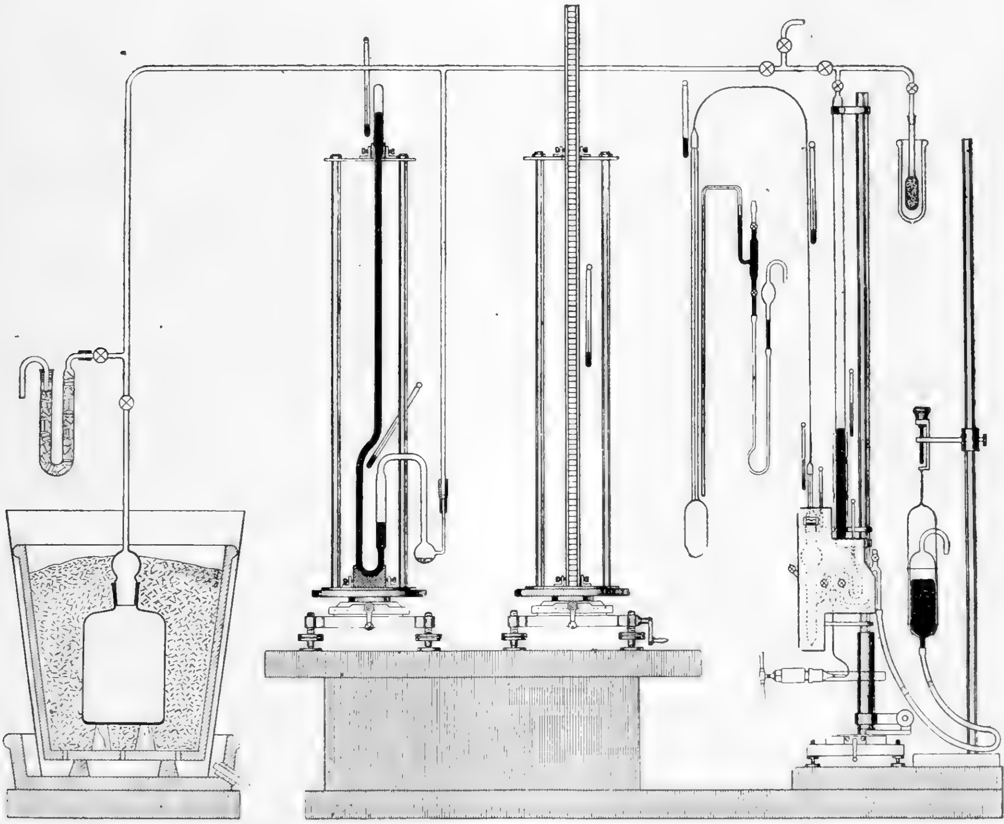

Fig. 1.

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[1] The letters in fig. 3 are the same as those in the previous communications; modified parts are indicated by a dash or an additional dash.

[2] At the time when the previous type of thermometer used in the latest experiments (Comm. 141 Proc. May 1914) was constructed, it was not possible to provide for a practically complete vacuum, with the same degree of certainty as at present.

In the connection of the adjustment-tube of the manometer 1) with

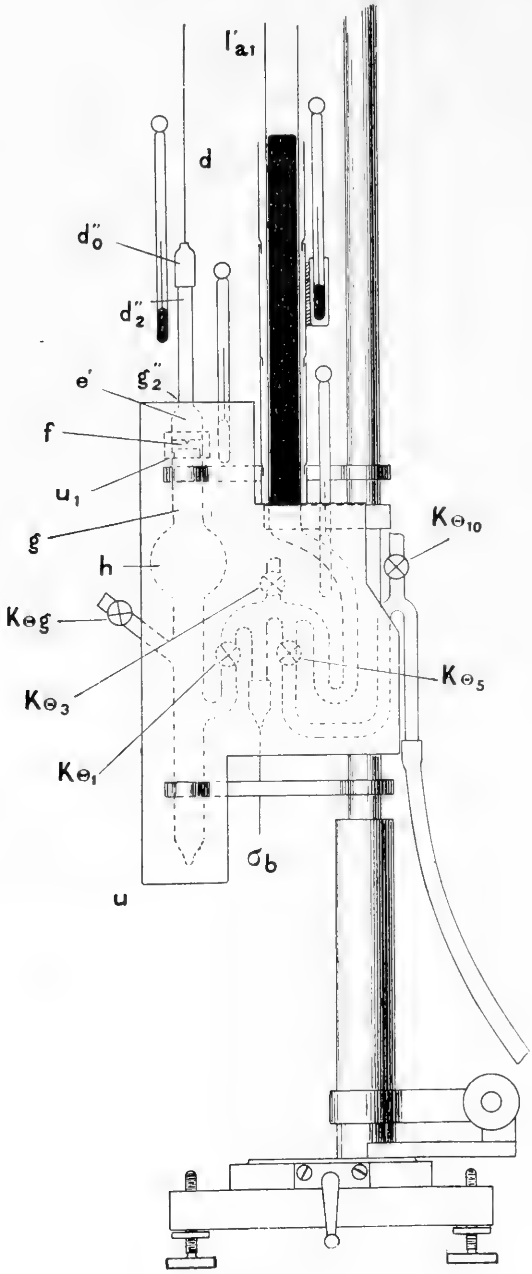

Fig. 4.

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1) In the same way as previously the thermometer is provided with a wider part *h* below the adjustment-tube, in order that the thermometer may be filled at the ordinary pressure and that the zero-pressure may be raised to that of the international thermometer (1000 mms) by forcing up the mercury to close to the adjustment-point. A mark on the adjustment-tube makes it possible to obtain the desired filling at the ordinary temperature and pressure with sufficient accuracy.

the capillary of the thermometer the use of cement is now entirely avoided,. in the same way as this was effected in the latest thermometer for helium-temperatures, (c.f. Comm. No. 124*b*). In the previous type of thermometer the cement between the steel cap and the glass had evidently been a constant cause of absorption of gas which showed itself in a change of the zero-pressure. [1])

With the thermometers constructed as they are at present we have not found any change of the zero-pressure which may not be deemed to fall within the limits of accuracy of the measurements (probable error of ·02 mm.).

At the top above the space where the adjustment is made the tube is finished off hemispherically (g_2'' figg. 3 and 4) and is blown to a capillary d_2'' which in its turn is soldered to a steel capillary connectingtube Th_2'' according to CAILLETET's method which is used in the construction of a great many apparatus in the laboratory. [2]) In order to reduce the "noxious space" as much as possible the small bored out steel adjustment-piece e' of Comm. No. 124*b* is now ground into the hemispherical space g_2''. [3])

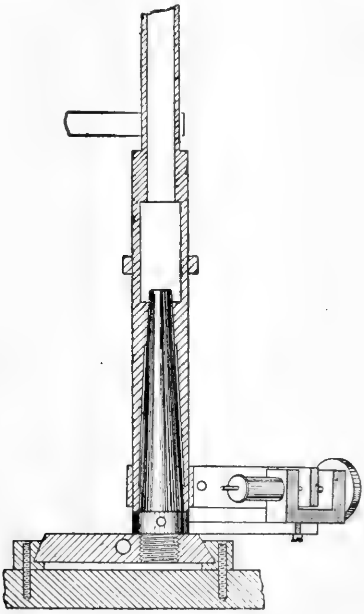

Fig. 2.

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[1]) In the case dealt with in Comm. No. 95*e* this change amounted to ·61 mm. in 19 months and in another case ·18 in 4 months. CHAPPUIS found in his thermometer ·1 mm. in 3 months.

[2]) On the platinized extremity of the glass capillary a layer of copper is deposited electrolytically. Over this the copper cap is soldered into which the steel capillary has been previously soldered.

[3]) The small piece is raised into the adjusting-space from below. The tube

The adjustment-tube was manufactured from a tube which was as free as possible from optical errors: it did not give a difference in the reading, when a kathetometer was pointed on the adjustment-point *f* and the tube was interposed or removed. The steel piece is taken long enough to be sure that the tube on the level where the adjustment is made is not injured by the heating, when the capillary is sealed on.

To ensure uniformity of temperature of the mercury in the *U*-tube of the manometer — another condition for extreme accuracy — the glass-part of the thermometer which contains mercury is now so constructed [1]) that it may be conveniently surrounded by a closed [2])

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which is attached to the thermometer for this purpose is sealed in a manner which may be seen in figg. 1, and 3 and 4 after the piece has been brought into its proper position. In the thermometer for helium-temperatures the operation could be performed by means of a ground joint (see fig. 3 Comm. No. 124*b*).

The piece is attached to the glass in its proper position by means of a very small quantity of sealing wax which has been previously freed of all vapours by exhaustion with liquid air. The bottom face of the piece is ground flat at right angles to the tube. It carries a point ·75 of a mm. long.

[1]) For the thermometer of helium-temperatures where very small differences of pressure had to be measured a pasted down cardboard chamber, fitted on the inside with copper screens, had been improvised for the same purpose. With the present arrangement the difference in the readings of two thermometers placed at different points in the chamber was less than ·05 of a degree. The change during a day was below ·1 of a degree.

[2]) A characteristic of this type of thermometer is that the whole can be easily moved to a different position. This requirement has lost the special importance which it had at the time, when the first type was developed and it was still so much more difficult to obtain absolutely constant low temperatures.

At the same time it has still its great advantange and it was possible to retain it in the new type. A movement up and down of the adjustment-space has become unnecessary by the special arrangement for pressures below one atmosphere.

The stand shown in previous drawings has been replaced by a more satisfactory one. Its base is now connected permanently with the levelling table on which it is mounted. The rod has been detached from the base and rests on it in the manner of the cone of the axle of a theodolithe resting in its conical seat (see fig. 2). This arrangement is especially convenient, when several thermometers are in use which it is desired to put by ready for use. Each thermometer then has its own rod, to which it remains attached while being kept; when it is to be used the rod is placed on to the common foot-stand.

In order that the kathetometer needs only be turned to focus both tubes of the manometer with equal sharpness, the manometer — like the barometer and standard-metre — is mounted on a slide which can be moved in the direction of the kathetometer-axis and the rod to which both arms of the manometer are attached can turn about the cone which is attached to the slide. (For the finer adjustment in this respect consult details in fig. 2).

copper chamber *U*, after the thermometer having been attached to its support. (See fig. 1 and 4). On the level of the adjustment-space it is provided with a slit for illumination and one for reading *u*, (see fig. 4). The former is closed by a piece of ordinary glass. The reading-slit is provided with a slide holding a thin plan-parallel piece of glass. The rising

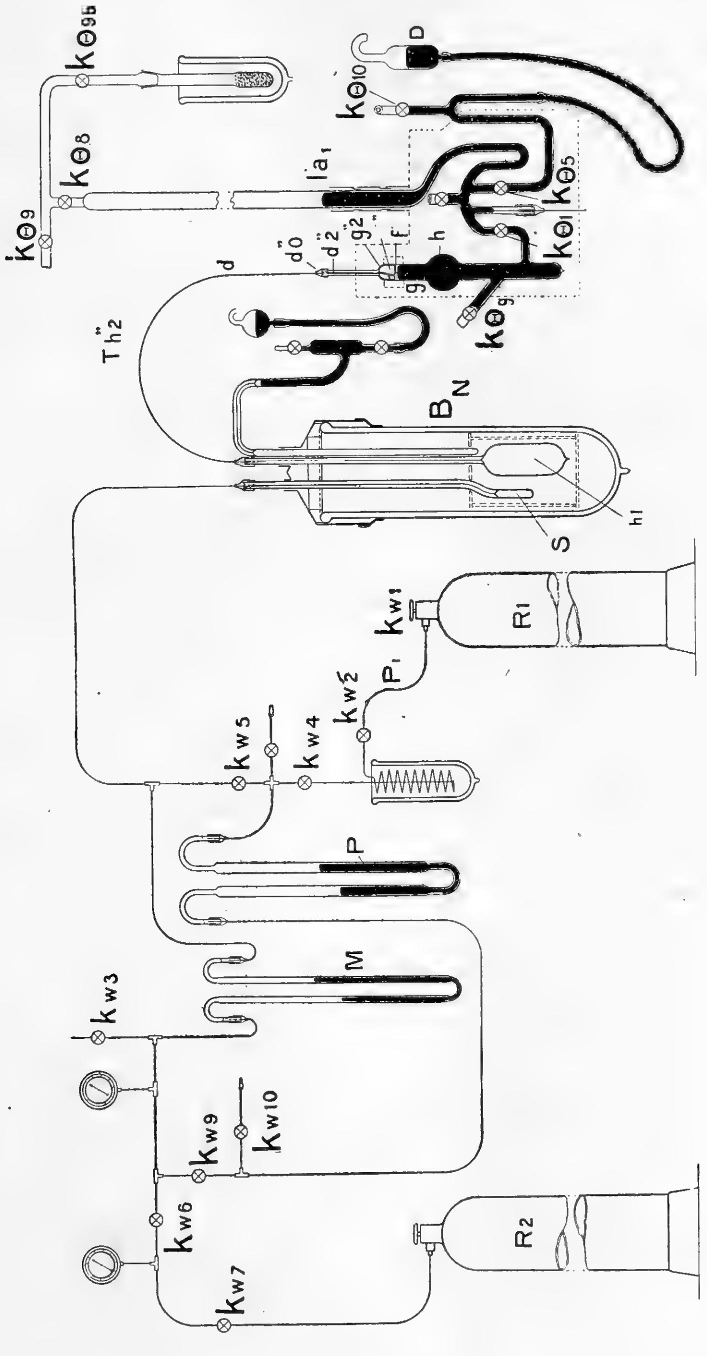

Fig 3.

manometer-tube is surrounded (see fig. 4) by a copper enclosure forming the continuation of the chamber by piling up, as far as the mercury reaches, a number of mutually fitting pieces of copper tubing consisting of hinging halves. One of them carries a thermometer in a copper tube soldered to the main piece.

It is of special importance to know the noxious volume with great accuracy [1]). All its parts are calibrated in the manner detailed on previous occasions [2]), whereas the spaces at the soldering places of the steel capillary to the glass capillary are known by measuring and estimating the various dimensions. Moreover a volumenometric determination was made by sealing the steel capillary at the thermometer-end, filling the adjustment-space and capillary with dry air and utilising the calibration-tap $K\Theta g$ mentioned in note 1 p. 992. [3]) The two results, by the direct measurement and weighing and by this volumenometric determination of the volume of the adjustment-space between a horizontal plane through the adjustment-point and the sealing-place of the thermometric capillary agree within a few mm^3.

The measurements with the present thermometer [4]) are carried out in the same manner as before with the previous types of instrument (see particularly Comm. N°. 141*a*).

The correction for the capillary of the thermometer is obtained by mounting the wider capillary of an auxiliary thermometer beside the capillary in question, according to the method used by CHAPPUIS. (See fig. 1).

§ 3. *Apparatus and method.* Fig. 3 gives a general aspect of the

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[1]) It is possible in the manner followed by HENNING, Ann. d. Phys. (4) 40 1913) p. 635 to free the measurements of the temperature from an error which (as in his measurements) remains in the determination of the noxious space, by not using the real pressure-coefficient in calculating a certain range of temperatures, but that one which is found by calculation with the incorrect value of the noxious space, but it is very much preferable to prevent errors of that kind by a careful determination of the noxious space, which as a matter of fact does not involve any special difficulties.

[2]) In computing the space between a horizontal plane passing through the adjustment-point and the surface of the mercury the formulae of LOHNSTEIN and of SCHEEL and HEUSE were used. For the determination of the section of the tube at the place of adjustment, a temporary glass tap was blown to the lower end of the adjustment-tube.

[3]) In this measurement the steel capillary was surrounded by a copper tube to make sure of the temperature.

[4]) The data of the thermometer are as follows: Volume of thermometer-bulb 108·31 cm^3; volume of glass capillary ·040 cm^3; noxious space ·767 ± ·003 cm^3 diameter manometer-tube 1·473 cm.; change of volume of bulb for 1 atm. change of pressure ·0051 cm^3.

apparatus for determining vapour-pressures. In the experimental space inside the vacuum-glass B_N of the hydrogen-vapour cryostat beside the helium-thermometer (§ 2) and the auxiliary capillary of CHAPPUIS is placed the small vessel S, which is meant to receive the condensed hydrogen and whose volume is known [1]). The high-pressure reservoir R contains the pure hydrogen which has been obtained by distillation. A sufficient quantity of it is collected in the calibrated arm (in the drawing on the right hand side) of a pressure tube P, which is half full of mercury by means of a pipette P_1 (between the stop-cocks K_{IV_1} and K_{IV_2}), using a differential manometer M placed in parallel with it. The pressure-equilibrium which is changed by the admission of hydrogen is re-established by the admission of compressed air, which is contained under high pressure in the supplycylinder R_2. In this manner the pressure in the vapour-pressure apparatus is gradually raised to the equilibrium pressure between liquid and vapour, corresponding to the temperature in the cryostat. K_{IV_4} is then closed and a known quantity of gas present in the pressure-tube P can be carried over into S as liquid. [2])

S is calibrated. When a known quantity of hydrogen has been added, K_{IV_5} is closed and the operations are stopped until the equilibrium of temperature which is disturbed by the adiabatic compression of the gas has been restored, as shown by the indication of the differential manometer M. When at the same time the temperature in the cryostat, as checked by means of two platinum thermometers, has become constant, the pressure-measurements can be started. For this purpose the air-side of the differential manometer is connected along K_{IV_3} with the open standard gauge of the laboratory (Comm. N°. 44). Concurrently with the determination of the pressures in the different parts of the apparatus one or more readings of the gas-thermometer are taken. In this manner determinations were made for a series of temperatures; in a few cases moreover measurements were made at one and the same temperature with different quantities of liquid, in order to make sure that the equilibrium pressures with little and with much liquid in the vapour-pressure bulb S were the same at the same temperature, as must be the case, if the temperature is uniformly distributed and the gas is pure.

(To be continued).

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[1]) For the cryostat and the arrangement of the various apparatus inside it, as also for the method of regulating the temperature, we refer to fig. 1 of Comm. N°. 151*a*. For simplicity the auxiliary capillary of CHAPPUIS has there been left out.

[2]) For this purpose the right-hand arm of the pressure-tube P is calibrated. The density of hydrogen is obtained by estimation by means of the law of the straight diameter from the data of Comms, N°. 137*a* and N°. 151*c*.

Physics. — *"The specific heat at low temperatures.* IV. *Measurements of the specific heat of liquid hydrogen. Preliminary results on the specific heat of solid hydrogen and on the heat of fusion of hydrogen."* By W. H. KEESOM and H. KAMERLINGH ONNES. Communication N°. 153*a* from the Physical Laboratory at Leiden. (Communicated by H. KAMERLINGH ONNES).

(Communicated in the meeting of June 24, 1916).

§ 1. After the measurements of the specific heat of solid and liquid nitrogen (Comm. N°. 149*a* Jan. 1916) others were undertaken concerning liquid and solid hydrogen. The apparatus used were those described in Comm. N°. 149*a*.

In order to obtain a temperature as nearly as possible uniform in the experiments in which the calorimeter was surrounded by solid hydrogen, the calorimeter-vessel in the last three series of measurements was surrounded by a cylinder of copper which was closed at the top by tin-foil [1]). As the cryostat did not provide sufficient room for both the copper cylinder and the stirrer, the latter was removed in this case.

In the measurements of Oct. 23 (table II) and of Nov. 3 (table I), as it appeared afterwards, an error has crept in the measurement of the tension of the heating current at the terminals of the constantin wire of the central heating-core [2]). For these measurements, therefore, the number of Joules supplied was calculated from the current and the resistance of the heating-wire. This resistance which includes that of the platinum supply-wires which reach outside the calorimeter-vessel is a little different according to the vacuum in

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[1]) Cf. p. 1004 note 1.

[2]) For the purpose of accurate measurements of the specific heat of metals and alloys (e.g. of nickel and copper in connection with the magnetic properties of these alloys, comp. Suppl. No. 36 *c*, May 1914, p. 110 note 1) a new heating-core has been constructed in which the thermometer wire is suspended freely in a gas-filled space in order to get rid of the changes to which the thermometer-wire is subject when it lies enclosed in enamel-paint.

the calorimeter-vessel being more or less, owing to the difference in the temperature which the supply-wires assume during the heating. These differences in the resistance of the heating-wire did not amount to more than 0.9 % in those cases where the resistance could be derived from the tension and current during the heating. They fall within the limits of accuracy which could be reached in these measurements [1].

§ 2. *Specific heat of liquid hydrogen.* The correction to be applied for the evaporation of hydrogen during the calorimetric experiment is somewhat larger than in the case of nitrogen (Comm. N°. 149*a* § 5): the largest value was 4.5 % of the heat supplied.

The results are combined in table I and represented in fig. 2.

In this figure we have also included the results of Eucken's measurements [2] (indicated by △△) concerning the specific heat of liquid hydrogen between 17.3° and 21.2° K. Our results, which, however, reach down to lower temperatures, agree at these higher temperatures with those of Eucken within the limits of accuracy of the measurements.

It is represented in Fig. 1. *Au* is the spirally wound wire of the purest gold to be obtained; diameter 0.05 mm., resistance at room-temperature 1,2 Ω. The spiral is suspended between two insulated supply-wires of gold (1 m.m.) which protrude through glass tubes through the lid of the copper chamber *Ka*. The glass tube *g* serves for filling the chamber with gas (H_2 or He). A constantin wire with platinum supply-wires was wound round the chamber in exactly the same manner as described in Comm. N°. 143 § 2, to which we here refer. Special tests showed that at the temperature of liquid hydrogen the resistance of a spiral of the same gold wire, suspended in a space filled with gaseous hydrogen, could be measured with a current of 35 milli-ampères, without the Joule-heat raising the temperature of the wire by more than 0.01 of a degree above that of the bath.

The resistance of the gold wire which after winding is heated to 300° C. does not attain constant values, until it has been cooled in liquid air several (say 5 or more) times and brought back to room-temperature.

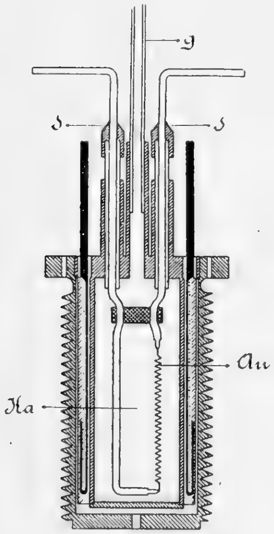

Fig. 1.

[1] Our thanks are again due to Mr. J. M. Burgers for his assistance in the temperature-measurements.

[2] A. Eucken, Verh. d. physik. Ges. 1916, p. 4.

TABLE I.

Specific heat of liquid hydrogen.

Number		Quantity of hydrogen in gr.	Mean temp.	Rise of temp.	Heat-capacity of hydrogen + flask and block K_{III} in Joules p. degree K.	Heat-capacity of flask + block in Joules p. degree K.	Specific heat of hydrogen in cal._{15} per degree K.	Atomic heat in cal._{15} per degree K.
Febr. 12 '16		2.89	16.03^5	2.64	24.06	1.48	1.87	1.88
April 12	VI	3.17	15.81	0.54	23.98	1.43	1.70	1.71
	VII	„	16.80	0.50	26.05	1.63	1.84	1.85^5
May 11	II	3.52	14.71	0.54	25.84	1.22	1.67	1.68
	III	„	15.38	0.51	27.58	1.34^5	1.78	1.79
Nov. 3 [1]	I	3.50	14.82^5	1.24	26.73	1.24	1.74	1.75
	II	„	16.30	1.12	28.69	1.53	1.85	1.87
	III	„	17.63	1.04^5	30.97	1.81	1.99	2.00
	IV	„	18.67	0.61	32.54	2.06	2 08	2.09
	V	„	19.41	0.58	34.24	2.26	2.18	2 20
	VI	„	20.11	0.56	35.30	2.45	2.24	2.26
Sept. 11 '17 [1]	II	3.51	15.24^5	1.01	28.28	1.33	1.84	1.82
	III	„	16.37	0.95	29.49	1.54	1.90	1.89
	IV	„	17.37^5	0.92	30.98	1.75	1.99	1.98
	V	„	19.07	0.58	34.07	2.17	2.17	2.16

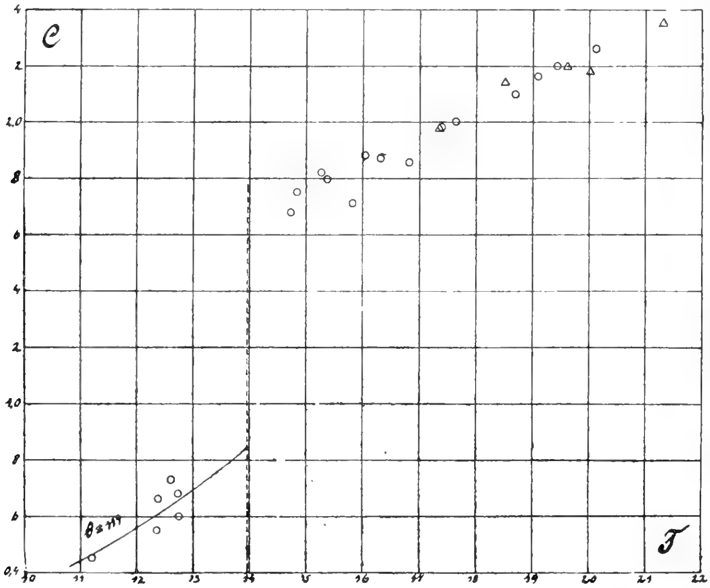

Fig. 2.

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[1] These measurements were carried out after this communication had been presented to the R. Academy and are here included.

§ 3. *Preliminary results of measurements on the atomic heat of solid hydrogen.* In connection with our measurements on the specific heat of liquid hydrogen we also carried out some calorimetric experiments on solid hydrogen at temperatures which could be reached by reducing the pressure as far as possible. As we have not had an opportunity to calibrate the thermometer-wire at these temperatures we can only give some preliminary results. They are based on the assumption that the linear dependence on the temperature which holds for the constantin wire between 14.16° and 20.48° K. with great accuracy (Comm. N°. 143, Oct. 14, Table I) remains valid for this wire down to 10.5° K. (or at least: down to 11.8° K., if the measurement of Oct. 23 I which is less certain on account of an insufficient vacuum in the calorimeter-vessel is excluded).

These results are also shown in fig. 2.

TABLE II.

Atomic heat of solid hydrogen. Preliminary results.

Number	Quantity of hydrogen in gr.	Mean temp.	Rise of temp.	Heat-capacity of hydrogen + flask and block K_{III} in Joules p. degree K.	Heat-capacity of flask and block K_{III} in Joules per degree K.	Atomic heat of hydrogen in cal$_{15}$ per degree K.
April 12 '16 I	3.17	12.61^{5}	1.31	10.51	0.87	0.73
II	„	12.72^{5}	1.39	9.84	0.89	0.68
May 11 I	3.52	12.34	1.11^{5}	8.89	0.83	0.55
Oct. 23 I	3.19	[11.20^{5}	1.47	6.69	0.68	0.45]
II	„	12.73^{5}	1.07	8.91	0.89	0.60
Sept. 11 '17 [1] VI	3.51	12.36^{5}	0.61	10.41	0.83	0.66

In cooling the solid hydrogen for the object of these measurements, as also in a few special experiments made for this purpose, cooling curves (comp. Comm. N°. 149*a* fig. 5) were recorded. In several of them small irregularities occurred; as these did not show themselves at the same temperatures in all the curves, and even in two of them taken under the same circumstances were completely absent, we ascribe these bends to disturbances (such as changes of the galvano-

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[1] Comp. p. 1002 note.

meter-zero, change of temperature by radiation [1]), in the measurements of Febr.—May 1916 possibly owing to the formation of gaseous spaces in the bath of solid hydrogen by the evaporation) and we conclude from the whole of eight cooling-curves, that our experiments do not give an indication of the existence of a point of transformation for solid hydrogen between the triple point solid-liquid-vapour and 10°.6 K. [2])

Taking as a mean for the specific heat of solid hydrogen at $T = 12.55°$ K. the value 0.64, we find (neglecting the difference $C_v - C_{sat}$) in DEBIJE's formula for hydrogen $\theta = 111$ (comp. the curve in fig. 2). To an even higher degree than in the case of nitrogen this value is smaller than the one which would follow from LINDEMANN's formula (Comm. N°. 137*a* § 5), viz. $\theta = 212$. As for nitrogen (Comm. N°. 149*a* § 4) we conclude that presumably hydrogen in the solid condition is more-atomic in the sense given there.

§ 4. *Heat of fusion of hydrogen.* We have made two experiments (May 11 1916 and Sept. 11 1917) for the determination of the heat of fusion, by measuring the heat which had to be supplied to heat the flask with hydrogen from 0.2° and 0.7° respectively below to 0.9° and 1,6° respectively above the triple-point-temperature. These experiments gave for the heat of fusion per gramme of hydrogen: 13.3 and 16.4 cal._{15} respectively, with a mean value of 15 cal._{15}.

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[1]) It was also partly to obviate the radiation referred to here, which is due to the walls at a higher temperature, that the calorimeter-vessel was afterwards surrounded by a copper cylinder (comp. § 1).

[2]) On the linearly extrapolated temperature-scale of $Const_{c3}$ (comp. the beginning of this §).

Anatomy. — "*Short history of the head of Vertebrates.*" By Dr. H. C. Delsman. (Communicated by Prof. J. Boeke).

(Communicated in the meeting of Dec. 23, 1917).

In preparing a second edition of my theory on the origin of Vertebrates I was led to indulge once more in a number of questions relating to their structure and development. Applying to these the principles of my theory I arrived again at several new points of view. As the completion of the more elaborate article will probably be delayed for some time, I wish to give here a short summary of the views arrived at concerning the structure of the head of Chordates, though I will not add now a complete account of the considerations which induced me to embrace certain conceptions and to reject others. It seems to me that with the records now at hand we have approached considerably nearer to the solution of this problem than one would be inclined to conclude from a more superficial acquaintance with the chaos of divergent and contradictory opinions of older and of more recent investigators.

The history of the head of Vertebrates is closely related to that of the animal pole of the egg and of the blastula in the animal kingdom. Already in *Volvox* we find a contrast between two opposed poles of the colony, expressed by the stronger development of the red stigmata characteristic of Flagellates at the one pole and of the plasmodesms, serving for the transport of food between the cells, at the other. In the development, too, which begins with an egg cleavage reminding one of the spiral type, the contrast between the two poles becomes evident. The colony swims with the animal pole forward, rotating round the main axis (for literature cf. Janet, 1912). The same holds for the free-swimming blastula — "the animal Volvox" as Huxley (1877, p. 678) called it — of different groups of marine animals, the planula of Coelenterata and other pelagic larvae. The animal half of the blastula as a rule develops into a sensory and nervous centre, the so-called apical plate of larvae like the trochophore, arising from the four animal cells of the eight-celled stage (1st quartet of micromeres). The animal pole and the prae-oral lobe or prostomium, to which the apical plate gives rise, as a rule continue to indicate the anterior end of the body in free-moving animals such as Annelids, Molluscs, Arthropods, Chordates and Enteropneusts, as is the case already in *Volvox* and the pelagic blastulae and larvae.

Sessile forms such as Coelenterates, Echinoderms, Ascidians (Willey, 1894, p. 329), however, often attach themselves with the anterior end. Then the prae-oral lobe loses its significance as a sensory and nervous centre. This is equally the case in burrowing animals like the earth-worm, *Amphioxus* and *Balanoglossus*.

In Polychaetous Annelids the whole segmented soma, ecto-, ento- as well as mesoderm, takes its origin from the four vegetative cells of the eight-celled stage, i.e. from the vegetative half of the blastula. Prostomium and soma are met with again in Chordates. With Annelids and Anthropods one or some of the anterior segments unite with the prostomium to form the head; this is equally the case in Vertebrates.

In *Amphioxus*[1] we can hardly speak of anything of the kind. The Annelidan stomodaeum has grown out in a backward direction and has become the medullary tube (Delsman, 1913a, p. 649), which even surpasses the soma in length (formation of the tail, Delsman, 1917b, p. 1271). The mouth, situated in Annelids ventrally just behind the limit of prostomium and first segment (peristomium), is found again in *Amphioxus* as the neuropore on the corresponding place (Delsman, 1913b), viz. dorsally, at the boundary of prostomium and soma, just in front of the first mesodermic segment, which is the mandibular segment of van Wijhe (1893, p. 157), the "collar-cavity" of MacBride (1898, p. 599). The fore-end of the notochord is originally situated right under the neuropore and equally indicates the limit of prostomium and soma. Sense organs and ganglia have been lost or become indistinct in *Amphioxus*. The brain vesicle corresponds to the deuterencephalon (Kupffer, 1905) of Craniotes. The somites from the foremost up to the last develop uniform myotomes constituting together the voluntary longitudinal trunk musculature. The first pair sends out a "rostral prolongation" (Kopffortsatz) into the prae-oral lobe in which also muscle fibres develop which, however, subsequently disappear.

In the larva the gill-clefts regularly alternate with the myotomes (Willey's figures, 1891, Hatschek, 1892, p. 145), so there is eumetamerism. Only secondarily, after the "critical stage" (Willey, 1891, p. 202), it gets lost. The left gill-cleft between the first and the second somite becomes the larval mouth, its antimere is the club-

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[1] The reader is invited to compare the following descriptions with the plate.

shaped gland (van Wijhe, 1893, p. 153, cf. also His, 1887, p. 429); they represent the second pair of gill-slits. The first pair of gill-pouches, at the limit of prostomium and first segment, is represented by the bilaterally symmetrical "anterior entoderm pockets" or "head-cavities" (Hatschek, 1892, p. 144) of which only the left one still gets an opening, known as Hatschek's pit, to the exterior; the right one does not open, but gives rise to the so-called praeoral coelome.

To each somite a dorsal nerve belongs, to the first one, however, two, situated close to each other and compared by Hatschek (1892) to the two parts of the trigeminus in Craniotes, which commonly is considered as a double nerve, by me, however, with Balfour (1878, p. 214) as a single one which sometimes may be split into two (cf. facialis and acusticus) and belonging to the first or mandibular segment (v. infra). The same holds for both the anterior spinal nerves of *Amphioxus*, which accordingly I designate together as no 1. No distinction can be made as yet between cranial and trunk nerves, dorsal and ventral roots remain separated along the whole body. Here already, however, the fourth nerve (Hatschek's 5th, 1892, p. 143), the future vagus, is distinguished by its strong development and it is especially this nerve which communicates with the longitudinal plexus supplying the gills (ramus branchio-intestinalis vagi of Craniotes). Thus the first four somatic segments evidently correspond to the trigeminus-, the acustico-facialis-, the glossopharyngeus- and the vagus-segment of Craniotes (v. infra).

The prostomium does not contain any mesoderm of itself, its mesoderm is derived from the first somite, as is also found very generally in Annelids (cf. e.g. Meyer, 1890, p. 299). The proper, ectodermal, so-called primary or larval, mesenchyme (Conklin, 1897, p. 151) of the prostomium of Annelids, a last remnant of the mesenchyme of the primary body-cavity of flatworms (Meyer, 1890), has evidently disappeared in Vertebrates, together with the so-called head-kidney of the trochophora, a last rest of the protonephridia of flat-worms which, however, have been preserved in certain Annelids and in *Amphioxus* (Goodrich, 1902).

The asymmetrical origin of the mouth of *Amphioxus* gives us the key to the interpretation of the larval asymmetry.

Petromyzon is distinguished from *Amphioxus* by the possession of a praechordal brain, the palaeocranium of Kupffer, which together with the inverted eyes has arisen from the dorsal half of the surface of the prostomium, so that the neuropore, also in the other Craniotes, is not situated any longer dorsally, and above the anterior end of the notochord, but terminally, far in front of the end of the notochord and close to the animal pole (Delsman, 1916, 1917). *Petromyzon* resembles *Amphioxus,* however, by the complete segmentation of the mesoderm (Hatschek, 1910) and the separate course of the dorsal and ventral roots of the spinal nerves. The series of well-developed somites continues forward as far as the prostomium. As is the case in most of the Annelids that possess them (Fauvel, 1907, p. 110), the static vesicles are situated in the second segment of the soma, being the hyoid- or facial-acoustic-segment, but more in the hinder half of it. Thus in *Petromyzon* and in all Craniotes two pro-otic segments may conveniently be distinguished, the mandibular or trigeminus-segment and the hyoid- or acustico-facialis-segment. Behind the ear-vesicles then follow the segment of the glossopharyngeus, that of the primary vagus, that of the first spinal nerve, the ganglion of which in Gnathostomes fuses with that of the vagus ("spinalartiger Vagusanhang", Hatschek, 1892, p. 156) and those of the subsequent spinal nerves. The first pair of somites again send out each a prolongation into the prostomium, the so-called praemandibular somite (Hatschek, 1910, p. 481), comparable to the "Kopffortsatz" in *Amphioxus,* and thus not to be considered as a proper somite. The two pro-otic somites do not contribute any more to the formation of the longitudinal trunk musculature, but together with the so-called "praemandibular somite" give rise to the eye-muscles. The post-otic somites all form regular myotomes, constituting the longitudinal trunk-musculature. This, consequently, unlike in other Craniotes, also holds for the glossopharyngeus- and the primary-vagus-somite, though in both a beginning of reduction manifests itself in the breaking up of the internal, deeper parts of these myotomes during development (Koltzoff, 1901, p. 329). Evidently this is caused by the strong development of the auditory capsule, which extends backward into the first and the second post-otic segment, the myocommata between the first and the second and between the second and the third myotome as a consequence attaching themselves to the auditory capsule and the first free neural arch being situated between the third and the fourth myotome (cf. e.g. Goodrich, 1909, p. 40). Superficially, however, the first and the second myotome do not differ from the subsequent ones.

As in *Amphioxus* branchiomerism and mesomerism correspond, the eight gill-pouches, of which the first, the spiracular one, does not break through, alternate with the 1st—9th somite (Neal, 1897, p. 447, Koltzoff, 1901, p. 432). In front of the first somite lies a median mouth. That the mouth of Craniotes corresponds to two fused gill-slits is a supposition (Dohrn, 1875) which, though not supported in a convincing way by embryology, is yet rendered probable by a comparison with *Amphioxus*. The mouth of Craniotes is at any rate a different one from that of *Amphioxus* which corresponds to the left spiracle (VAN WIJHE, 1907 [1]), while that of Ascidian-larvae is again another one. It breaks through at the place of the neuropore so that even the anterior part of the medullary tube, the former stomodaeum, contributes towards the formation of the new entrance to the gut (HUNTSMAN, 1913). The secondary nature of the Vertebrate mouth is thus clearly shown.

While in *Amphioxus* the endostyle arises as a ventral bulging out of the gut just in front of the mouth, in the first somatic segment, in *Ammocoetes* it originates, like the rudiment of the thyroid gland in Craniates, in the same segment but consequently just behind the mouth (VAN WIJHE, 1907, p. 75).

From the anterior post-branchial myotomes (NEAL, 1897, p. 444, KOLTZOFF, 1902, p. 304) ventral buds grow out, similar to those which in Gnathostomes produce the musculature of the paired limbs. Growing down behind the last gill-slit and then forward they give rise to the hypobranchial musculature which is supplied by the ventral roots of the same post-branchial myotomes (7th—12th post-otic somite after NEAL) that have produced the muscles, as holds equally for the musculature of the limbs. The original eumetamerism of gill-slits and somites afterwards, as in other Craniates, gets lost, by a backward extension of the branchial sac, by which the gill-slits are caused to disperse and the originally post-branchial myotomes 7—12 now come to lie epibranchially. The ventral roots of these somites continue to pass behind the last gill-slit on their way to the hypobranchial muscles and during the elongation of the branchial basket they unite one by one to a common horizontal stem, which springs from six roots and bends downward behind the last gill-slit. This stem we can call the hypoglossus or plexus cervicalis (fig. 1). (See following page).

The primarily epibranchial somites give rise to the epibranchial musculature, supplied by the corresponding ventral roots in front of the hypoglossus.

The 5th, 7th, 9th and 10th cranial nerves of Craniotes innervate the primordial branchial musculature (Musculi constrictores) which, though striated, must be counted with the visceral musculature

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[1] Regarding the mouth of *Amphioxus* we now come to a conclusion somewhat different from that reached formerly (1913, *b*). An interpretation of the praemandibular cavity of Craniotes diverging from that of VAN WIJHE (1882) leads us to a confirmation of VAN WIJHE's conclusions on another subject.

(Van Wijhe, 1882, p. 41), since it originates from the lateral plate.

The vagus is considered by Hatschek (1892, p. 152) to be a primarily single nerve belonging to the second post-otic somite but which has collected in its ramus lateralis the lateral dorsal branches of all the spinal nerves behind it and in the same way in its ramus branchio-intestinalis (with the rami post- and praetrematici), the corresponding ventral branches of as many subsequent nerves as it supplies more than one gill-slit ("partial polymerism" of the vagus). The ramus branchio-intestinalis, which may be compared to the epibranchial plexus of *Amphioxus*, no doubt, like the latter also owes its origin to the process of dispersion of the gill-slits.

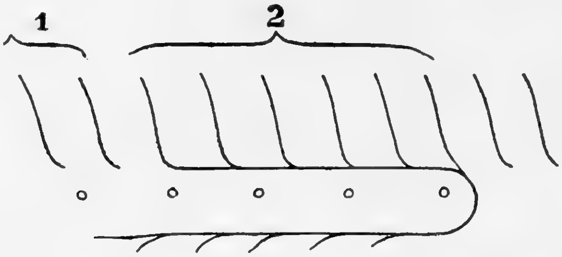


Fig. 1. The Hypoglossus of *Petromyzon*, after Neal (1897)
1 primarily epibranchial ventral roots.
2 secondarily " " " (hypoglossus).

For the first time we meet in *Petromyzon* the beginning of a cartilaginous skull, comparable to the head cartilage in Cephalopods and arising, like the latter, round the central part of the nervous system and the main sense-organs. It contains, besides the prostomium (which Hatschek does not distinguish from the first segment, designating it together with the latter as the acromerite, 1910), only two segments (Hatschek, 1892, p. 159), as the skull ends with the auditory capsule. The roof remains for the greater part membranous and membranous walls reach from the auditory capsules to the first neural arch, situated between the third and the fourth post-otic myotome. Through this membranous wall the glossopharyngeus and the vagus pass.

Attention must be drawn to the fact that the hypoglossus lies far behind the skull and also far behind the vagus. It does not appear from any publications on the subject that the spinal ganglia following behind the vagus are, under the influence of the latter, less developed than those situated more backwards, as may be noted regularly in Gnathostomes.

In many respects the Amphibians more closely resemble the Cyclostomes than the Selachians which, though exhibiting several primitive features, yet in other respects remind one more of the Amniotes. This holds e.g. for the earliest stages of development of the egg, which are nearly identical in *Petromyzon* and Urodelans, and also for the origin of the hypophysis in front of the mouth involution (in Selachians and Amniotes from the roof of the latter), for the structure of the brain which still lacks a developed metencephalon in *Petromyzon* and Amphibians, for the presence of horny teeth round the mouth (in Amphibians at least in the larval stage) and for the development of the cranial muscles (EDGEWORTH, 1911, p. 292). It holds equally for the backward extension of the skull.

Into the last the first three post-otic somites have now been incorporated, together with the first free neural arch of *Petromyzon* which in ontogeny appears as the so-called occipital arch, bounding behind the foramen vagi (STÖHR, 1879, 1881). The occipital region of Amphibians accordingly contains only one vertebral rudiment (SEWERTZOFF, 1897, p. 262). Between the occipital arch and the auditory capsule in early ontogeny three somites can be recognized (MISS PLATT, 1897, p. 448, for *Necturus*, SEWERTZOFF 1897, p. 260, for *Pelobates*, possibly also for *Siredon*, cf PLATT, 1898, p. 450) the last of which lies over the space between the 4th and the 5th gill-slit (MISS PLATT, 1897, MARCUS for Gymnophiones, 1910). These three are the somites of the glossopharyngeus, of the primary vagus, and of the first spinal ganglion which fuses with the latter ("spinalartiger Vagusanhang", HATSCHEK, 1892, p. 158). Of this fusion indications were observed in ontogeny by MISS PLATT, (1897, p. 448) and MARCUS, (1910, p. 378). To the last head-segment in young stages a ventral root, a so-called occipital nerve (FÜRBRINGER, 1897, p. 353), was observed in a few cases (FÜRBRINGER, 1897, p. 486, PETER, 1898, p. 42, DRÜNER, 1901, 1904, OSAWA, 1902, MARCUS, 1910, p. 376) which, however, during further development disappears. Of the three post-otic somites only the posteriormost in Urodelans and Gymnophiones still produces a regular myotome, which gives rise to the anterior segment of the longitudinal trunk musculature and, like the 3d post-otic myotome of *Petromyzon*, is inserted at the auditory capsule. Both the anterior post-otic myotomes, the deeper parts of which already in *Petromyzon* showed reduction, have been suppressed, evidently by the extension of the auditory capsule; only the second may still produce a few muscle fibres (MISS PLATT, 1897, p. 447, MARCUS, 1910, p. 430). In phaneroglossan Anurans, however, the 3d and the 4th post-otic myotome (SEWERTZOFF, 1895, p. 269) also disappear, together with the ventral root of the latter two, being that of the first free spinal nerve.

The gill-pouches, five in number in *Necturus*, the foremost corresponding to the spiracle, again regularly alternate with the 6 anterior somites (Platt, 1894, 1897). Thus the five head somites are all epibranchial, the occipital arch lying right above the last gill-slit (Platt, 1897, p. 452). By the restricted backward extension of the cranium the sphere of influence of the vagus causing the suppression of the subsequent spinal ganglia extends beyond the craniovertebral limit; in Amphibians as a consequence the first free spinal ganglion is always absent. Since in phaneroglossan Anurans also the ventral root gets lost (see above), the first spinal nerve is wholly absent here. The vagus does not supply more than three gill-slits, being that of the primary vagus, of the "spinalartiger Vagusanhang" and the first free spinal nerve which either has no dorsal root or is wholly absent.

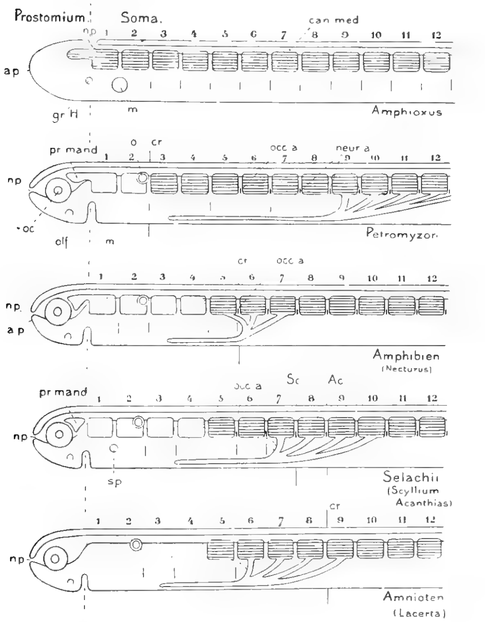
The hypobranchial musculature is formed in Urodelans from ventral buds of the anterior two post-branchial (4th and 5th post-otic somite) and the last epibranchial myotome (3d post-otic somite) (Miss Platt, 1897, p. 452) and innervated by the ventral roots of both the former somites, being the first two free spinal nerves, which in Urodelans together constitute the wholly post-cranial hypoglossus. The latter now partly lies within the sphere of influence of the vagus which causes the anterior root to have no dorsal ganglion. The ventral root of the last epibranchial myotome gets lost, as was mentioned above.

Epibranchial musculature is absent.

The olfactory grooves, situated in Annelids dorsally on the prostomium just in front of the limit between prostomium and first segment, are found in Craniotes at the corresponding place, viz. ventrally on the prostomium just in front of the mouth. Only in *Petromyzon* has a secondary monorhinism been established.

In Selachians as well as in Amniotes the enormous yolk-contents of the egg has in a corresponding way influenced the course of the earliest development, in both these the metencephalon has developed and the pituitary body does not originate any longer in front, but from the roof of the mouth evolution. In both the skull has annexed a number of vertebral elements constituting the regio occipitalis. In *Acanthias* the rudiments (or at least the indication, Sewertzoff, 1899) of four vertebrae may be observed (Hoffmann, 1894, p. 638), the foremost of which corresponds to the occipital arch of Amphibians (Sewertzoff, 1895, p. 260) and, like the latter, is separated by three somites from the auditory capsule. Thus three segments have been added to the skull; in *Scyllium* and *Pristiurus*

Diagrams of the head of the main groups of Vertebrates.



The shaded squares are somites producing myotomes, the blank squares, somites not doing so. The hypoglossus roots on the whole correspond to the myotomes from which the hypobranchial musculature is formed; the root belonging to the last epibranchial somite, however, never forms part of the hypoglossus.

a. p. animal pole, *can. med.* medullary tube, *cr.* cranio-vertebral limit. (*Sc.* in *Scyllium*, *Ac.* in *Acanthias*), *gr. H.* groove of HATSCHEK, *m.* mouth, *neur. a.* neural archs, *np.* neuropore, *o.* auditory vesicle, *oc.* eye, *occ. a.* occipital arch., *olf.* olfactory groove, *pr. mand.* "praemandibular somite", *sp.* spiracle.

where the skull is one segment shorter (HOFFMANN, 1894, p. 638, SEWERTZOFF, 1899, p. 302) than in *Acanthias*, only two. Thus in the latter forms the skull contains, besides the prostomium, seven segments (equalling the number of visceral arches [1]), of which one is post-branchial, in *Acanthias* eight, of which two are post-branchial, in *Hexanchus* and *Heptanchus* probably still a few more (v. infra). Mesomerism and branchiomerism again correspond (VAN WIJHE, 1882, ZIEGLER, 1908, 1915). As in Amphibians the two anterior post-otic somites no longer develop myotomes, but the second somite still forms a rudimentary one. From the remaining occipital somites, however, myotomes are still developed.

From these the epibranchial musculature is formed (DOHRN, 1885, p. 446, HOFFMANN, 1898, p. 265), which in all other Gnathostomes, and also in rays already, is absent. It still reaches its strongest development in the primitive *Hexanchus* and *Heptanchus* (FÜRBRINGER, 1897, p. 416). The hypobranchial musculature (Musculi coraco-arcuales) originates from the ventral buds of the last epibranchial and the first four post-branchial myotomes, being the 4th—8th post-otic somite, according to NEAL (1897, p. 450), and only of the latter four, all post-branchial, according to HOFFMANN (1898, p. 263). It is supplied by the ventral roots of these latter four myotomes, which partly lie within the range of the skull, partly behind it, forming together the plexus cervicalis. The gill-slits here too dispersing afterwards in a backward direction, this plexus cervicalis again comes to run in a curve round behind the last gill-slit; by the strong elongation of

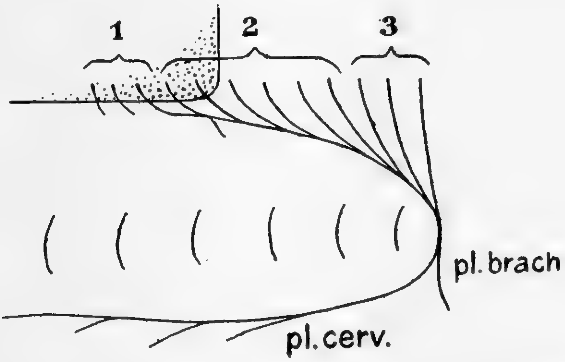


Fig. 2. Plexus cervico-brachialis of *Heptanchus*, after FÜRBRINGER. (1897).
1. primarily epibranchial ventral roots.
2. plexus cervicalis (hypoglossus). 3. plexus brachialis.

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[1] Accordingly two less than the well-known number given by VAN WIJHE (1882), who considered the praemandibular cavity as the first somite and counted the hyoid segment as two.

the branchial basket it is even united with the plexus brachialis into a common stem, the plexus cervico-brachialis, which only distally splits into a branch to the hypobranchial musculature and one to the pectoral fin (fig. 2). This common plexus was seen by HOFFMANN (1901, p. 39) to form during development in exactly the same way as is described by NEAL for the hypoglossus of *Petromyzon.* The skull in *Acanthias* containing 8 segments, of which 6 post-otic, we may expect that of the four roots of the hypoglossus or plexus cervicalis after HOFFMANN's statements the anterior two (5th and 6th post-otic somite) will pass through the skull. This indeed proves to be the case (FÜRBRINGER, 1897, p. 362). In no Selachian is the number of intra-cranial (occipital) hypoglossus roots, as far as has been definitely stated, more than 2 (FÜRBRINGER, l.c. p. 404), often one or none. From this the approximate number of post-branchial segments incorporated into the skull may be derived. Provisionnally we must say "approximate" since we have insufficient evidence as to the participation or non-participation of the last epibranchial somite. NEAL (1897, p. 461) supposes that a transitory crowding forward of the posterior gill-slits causing the last one to lie under and not behind the last epibranchial myotome, as observed by himself in *Acanthias* and by Miss PLATT (1897, p. 458) in *Necturus,* sometimes allows the ventral growth of a myotome, which otherwise would be prevented.

In front of the occipital hypoglossus roots (*y, z,* of FÜRBRINGER) a few ventral occipital nerves may still be found which either remain independent (*Hexanchus, Heptanchus*) or at first may join the plexus, but supplying only epibranchial musculature (*x, w, v,* of FÜRBRINGER), and therefore are comparable to the ventral roots of the first six post-otic myotomes of *Petromyzon* for which the same holds. Thus of the occipital myotomes and nerves only the anterior ones are primarily epibranchial, the one or two posterior ones are of post-branchial origin, but have become epibranchial only secondarily by the dispersion of the gill-slits.

In *Hexanchus* and *Heptanchus* where the number of gill clefts is greater, but the number of occipital hypoglossus roots not less than in pentanch sharks, the skull accordingly may be expected to comprise at least one or two segments more than in *Acanthias.* This is also confirmed by what follows. The sphere of influence of the vagus on the dorsal roots following it in *Scyllium* etc. reaches to a little behind the cranio-vertebral limit, the second free spinal ganglion only is again nearly normal, the first as a rule being absent (FÜRBRINGER, l.c. p. 392). In *Acanthias* there is formed in the last head-segment a more or less developed spinal ganglion, which however no longer produces a dorsal root. In *Hexanchus* and *Heptanchus*, however, the last occipital nerve is provided with a well-developed dorsal root with a ganglion, so here the influence of the vagus no longer reaches quite to the end of the skull, which in these forms passes more or less gradually into the vertebral column. This holds still more for the Chondrostei, where in several species a considerable number of dorsal and ventral roots leave the skull (FÜRBRINGER, 1897, p. 450) and where even the whole brachial plexus can have been incorporated into it (ibid., p. 457). For the supposition that in *Hexanchus* and *Heptanchus* the longest skull among Selachians is found, we may also point out the well developed primarily epibranchial nerves (*v*, *w*, *x* of FÜRBRINGER). In pentanch sharks the hindmost of these roots, as a consequence of the decreasing number of gill-slits, pass into post-branchial or hypoglossus-roots. The same holds for the development of the epibranchial musculature.

On the other hand in rays the cranium appears to be shorter, the number of occipital hypoglossus-roots is mostly 0, never more than 1 (FÜRBRINGER, 1897, p. 404), and the influence of the vagus reaches part of the way behind the cranio-vertebral limit, both the anterior spinal ganglia being absent (FÜRBRINGER, p. 392). There is no epibranchial musculature or occipital nerves supplying it. All this points much more to a phylogenetic decrease than to an increase of the length of the skull in the Selachians.

So in Elasmobranchs we have on the whole a partly intracranial, partly post-cranial hypoglossus the anterior roots of which as a rule are lying within the sphere of influence of the vagus and hence lose their dorsal glanglia. GEGENBAUR (1871, p. 521) called the occipital nerves "ventral vagus roots", which is right, if we consider the vagus as a partially polymeric nerve and moreover bear in mind that in Selachians we could better speak of a vago-accessorius. With the already bivalent vagus-ganglion the rudimentary ganglion of the 6th segment still fuses (NEAL, 1898, p. 238), so that the vagus ganglion is now trivalent, fused from one normal and two rudimentary ganglia.

In Amniotes the number of occipital myotomes observed during ontogeny nearly corresponds to that of Selachians. The number of intracranial (occipital) hypoglossus roots is very generally stated to be 3, thus one more than in *Acanthias*, but the number of gill-slits (5) being one less than in *Acanthias*, we must conclude that the cranio-vertebral limit almost corresponds to that of the latter form and that the skull here too contains some eight segments. Both in Selachians and

Amniotes the first pronephric tubule is found as a rule in the third somite behind the skull (FRORIEP, 1905, p. 119). The relation between myotomes and gill-slits in this case is not so evident as in lower Chordates. If, however, we take as an example the instructive figure given by VAN BEMMELEN (1889, p. 254) for the head of *Lacerta*, I think we may deduce from it as the most probable conception that the anteriormost occipital myotome figured corresponds to the first somite following behind that of the primary vagus, being accordingly the third post-otic (just as in Amphibians and Selachians), and the last epibranchial one.

This is the first of the five myotomes, which after VAN BEMMELEN give rise to the hypobranchial or tongue-musculature, while CORNING (1895, p. 165) denies the participation of this first one, only the four anterior post-branchial myotomes according to his statements contributing to it. Of these, three belong to the head, while the fourth corresponds to the atlas. Indeed, the tongue musculature is supplied by a hypoglossus with three occipital roots uniting with the first free ventral root (FÜRBRINGER, 1897, p. 506) to a plexus cervicalis which, however, in this case does not fuse with the plexus brachialis, which in Amniotes often shoves backwards pretty far from the head.

Thus the hypobranchial resp. tongue-musculature of Vertebrates in its origin and innervation wholly agrees with that of the paired limbs. To the hypobranchial musculature also the rule formulated by FÜRBRINGER (1879, p. 389) for that of the extremities is applicable, that the structure and innervation is not bound to distinctly numbered segments but that the place and number of the latter depends upon the situation and extension of the "segmental level" from which the organ takes its origin. Displacement in forward and backward direction, extension and reduction is possible without the myotomes themselves moving, dividing or fusing, or new myotomes being intercalated or others falling out. The anterior limit of the hypoglossus region is always determined by the situation of the last gill-slit and so by the number of gill-slits. Possibly also the hindmost epibranchial myotome can participate, though among the observations cited above there is only one concerning this point (Miss PLATT, 1897) which has not met opposition, nor does the non-participation of the corresponding ventral root exclude every possibility of doubt.

It also depends on the number of gill-slits whether the anterior hypoglossus roots come to lie within the sphere of influence of the vagus, and, together with the primarily epibranchial nerves, may be considered with some right as ventral roots of the partially polymeric vagus (vago-accessorius) or not (Gegenbaur). In *Petromyzon* e.g. this is not the case.

On the number of gill-slits and on the backward extension of the skull depends whether the anterior hypoglossus-roots are incorporated into the skull as occipital nerves. In *Petromyzon* the skull is short and the number of gill-slits great: the hypoglossus accordingly lies far behind the skull. Directly behind the skull we find the hypoglossus roots in Amphibians and most rays; partly in the skull and partly behind it in sharks; for the greater part in the skull in Amniotes, wholly in the skull in Chondrostei, where moreover the plexus brachialis may have been incorporated into it, as is the case in *Acipenser* (Fürbringer, 1827, p. 457).

From the above considerations the following conclusions may be drawn:

1. Froriep's (1882—1887) sub-division of the head of Vertebrates into a primarily unsegmented "cerebral" part, comprising besides eye and nose also the auditory vesicles and the gill-slits, and a segmented "spinal" part (regio occipitalis) is false. Gegenbaur's division of the skull into a prae-chordal "evertebral" and a chordal "vertebral" part is the right one, though the anterior part of the latter, as far as the occipital arch, has not formed from vertebrae, but has originated simultaneously with the latter (cf. *Petromyzon*, Gegenbaur, 1887, p. 77, van Wijhe, 1889). Branchiomerism and mesomerism correspond.

A primarily unsegmented head mesoblast (Froriep, 1887, "Urmesoderm" of de Lange, 1913, p. 250), in which we could speak only of branchiomerism, does not exist; the prostomium no longer contains primordial mesoderm (cf. *Amphioxus* and the "proammion" of Amniotes).

2. Froriep's (1882 etc.) conception of a secondary invasion of trunk segments into the primarily unsegmented "cerebral" head and Fürbringer's (1897, p. 440) opinion on a "stetiges Vorrücken" and breaking down of these myotomes with their ventral roots in the occipital region are false. Froriep's argument that rudimentary dorsal roots discovered by him belonging to these myotomes would indicate that they cannot belong to the vagus and must be of postbranchial origin, loses its value by the conception of the vagus as a partially polymeric nerve, which would lead us naturally to expect just such rudimentary dorsal roots. The argument of Fürbringer, that the occipital nerves of the sharks unite with the anterior free spinal nerves to form the plexus cervicalis, is wholly deprived of its value by the above considerations. Froriep and Fürbringer, not making a difference between primarily and

secondarily epibranchial myotomes, as we can do so clearly in *Petromyzon*, and taking them all for secondarily epibranchial and of post-branchial origin, arrived at erroneous conclusions. No "heiszer Kampf der Theile" (FRORIEP, 1901, p. 372): peace and rest are reigning in the occipital region.

3. FÜRBRINGER's (1897) conception of the metameric structure of the Amphibian cranium, and as a consequence also that concerning the skull of Amniotes, is false. The Amphibian skull does not, as FÜRBRINGER (l. c. p. 485) assumes a priori, contain as many segments as that of Selachians, but less (SEWERTZOFF, 1897, p. 410). The single occipital nerve which sometimes may be observed in early stages of development of Amphibians, was accounted for above. It is not to be considered as a last remnant of more occipital nerves (x, y, z) corresponding to those of Selachians, it is accordingly not z, but x.

The occipital hypoglossus roots of Amniotes do not owe their presence to a second annexation of free segments, but correspond to those of Selachians, not to the anterior free roots of the latter. The oldest conception (GEGENBAUR, 1871, p. 532) once more proves to be the right one here. Occipito-spinal nerves (a, b, c, etc. after FÜRBRINGER) do not exist, at least not in Amniotes. Only if with FÜRBRINGER (l. c. p. 362) one designates the last occipital nerve of *Acanthias* as a, we ought to do so equally with Amniotes. The "ganglion hypoglossi", discovered by FRORIEP (1882) in the last head segment of the sheep, evidently corresponds to the ganglion in the last head segment of *Acanthias*. The sharp distinction between proto- and auximetameric neocranium must be left out, at any rate the Amphibians have no protometameric neocranium (which is of equal length to that of Selachians), nor have the Amniotes an auximetameric neocranium (longer than that of Selachians) in FÜRBRINGER's sense. Only in Notidanidae and Chondrostei could one speak of an auximetameric neocranium and of occipito-spinal nerves.

Surely the hypoglossus has originally nothing to do with the vagus, as *Petromyzon* teaches us; that in higher Chordates it approaches the latter so closely that we may partly designate them with GEGENBAUR as ventral vagus roots, (i.e. of the partially polymeric vago-accessorius, better still of the accessorius which originates in closest connection to the anterior, rudimentary ganglia behind the vagus and in Selachians has not yet separated from the latter), is not to be accounted for by a forward movement of the myotomes and ventral roots themselves, but by a displacement of the "segmental hypoglossus level" in forward direction as a consequence of the decrease of the number of gill-slits (shortening of the branchial level), in accordance with the principle formerly put forward by **Fürbringer** (1879) himself. This principle, together with the admirable anatomical investigations of **Gegenbaur** and **Fürbringer**, and those of others in embryological direction, now opens to us the prospect to a better understanding of the structure of the head of Chordates.

The doubt whether the structure and the history of the Vertebrate head will ever be elucidated may, I think, make place for hope. "The recent rehabilitation of *Amphioxus* as an ancestral type by **Delsman** ('13)", **Neal** (1914, p. 138) wrote some years ago, "seems to justify the hope that the ancestral history of the head may yet be known and general agreement among morphologists be attained". May this expectation be realized and further investigations complete and rectify the results reached in this first attempt.

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Physics. — "*On a method of determining spectral intensities by means of photography*". By Dr. G. Holst and Dr. L. Hamburger. (Communicated by Prof. H. Kamerlingh Onnes).

(Communicated in the meeting of September 29, 1917).

1. *Introduction.*

The problem of the influence of small amounts of impurities upon the emission of light by a gas, made us look for a method of obtaining in a simple way a general survey of the alterations in the emission of light. Through this means we arrived at a working method very similar to the well known wedge-method [1] used for determining absorption spectra. Photographs obtained by the latter method are very simple to read [2], the height of the spectrum serving as a measure for the absorption in the region of wave-lengths under consideration. In analogy with this we arranged our spectrograph [3] in such a way that the height of the spectral line forms a measure for the intensity of the incident light. For this purpose we introduced a rotating sector with a radially increasing opening close in front of the slit. The time of illumination increases therefore along the spectral line from below upwards, and with it the blackness. The most intense lines will become the longest on the plate [4].

2. *Arrangement of the apparatus.*

The sector must be so constructed that there is a simple connection between length of spectral line and the intensity of the incident light. For this it is of the greatest importance to know, how the blackness of the photographic plate depends upon the intensity I and the duration of the illumination t. Under normal circumstances, according to Schwarzschild, the blackness is measured by the product It^p, where p is about 0.8 [5]. With intermittent light the illumination must be longer to produce the same blackness. The exponent becomes,

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[1] H. Kayser. Handbuch der Spectroscopie, III. p. 58.

[2] Comp. for instance the atlases of absorption spectra by H. S. Uhler and R. W. Wood, or by C. E. Kenneth Mees.

[3] Hilger quartz-spectrograph C.

[4] A similar arrangement for sensitometric purposes is used by E. Belin. Brit. Journ. Phot. (53) 630. 1906.

[5] H. Lux (Zeitsch. f. Bel. wesen 1917 p. 83) finds that in Wratten and Wainwright plates, which we used also, p may vary from ± 0,83 to 6,3 while according to L. Vegard Ann. d. Phys. (39) 111, 1912 $p = 0{,}89$.

therefore, greater and under certain circumstances may, it seems, become 1., so that Roscoe and Bunsen's law would become valid. According to measurements made by A. E. Weber [1]) this would be the case when the number of revolutions of the sector is more than 120 per minute. A similar result was obtained by F. Eckert and R. Pummerer [2]) and also by H. E. Howe [3]). A. Odencrants [4]) also finds an increase of p, but he does not consider that Bunsen and Roscoe's law may be applied. There is thus still some uncertainty as to the foundations upon which our method rests. This is, moreover, increased by the fact that we determine the ends of the line that are still just visible, and in consequence measure in the field of sub-illumination. For the sake of simplicity we have, therefore, in constructing the sector assumed the validity of Bunsen and Roscoe's law, and subsequently investigated by measurements in how far this assumption is borne out.

The first sector was so constructed, that the length of the spectral line was proportional to the intensity of the light. The intensity-variations along the line were then, however, too small to be easily measurable. We therefore proceeded to make a sector with which the length of the lines becomes proportional to the logarithm of the intensity (fig. I), with this we obtained satisfactory results. For the sector used

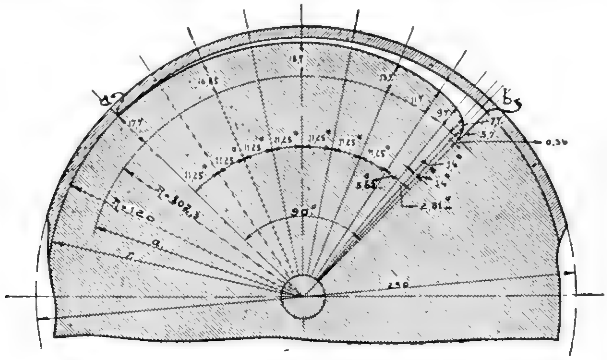

Fig. 1.

$$log \frac{I_2}{I_1} = 1,5\,(l_2 - l_1)$$

where l means the length of the spectral line [5]).

3. *A few measurements.*

In order to test the reliability of our method we carried out

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[1]) Weber. Ann. d. Phys. (45) 801. 1914.

[2]) Eckert and Pummerer. Zeitsch. f. physik. Chem. (87) 605. 1914.

[3]) Howe. Phys. Rev. 8, 674, (1916).

[4]) Odencrants Zeitsch. f. Wissensch. Phot. (16) 69, 111. 1916.

[5]) For further particulars cf. Hamburger, Diss. Delft 1917.

various checking measurements, some of which made with a mercury arc lamp we shall detail here [1]). The arrangement of the apparatus is shown in fig. 2.

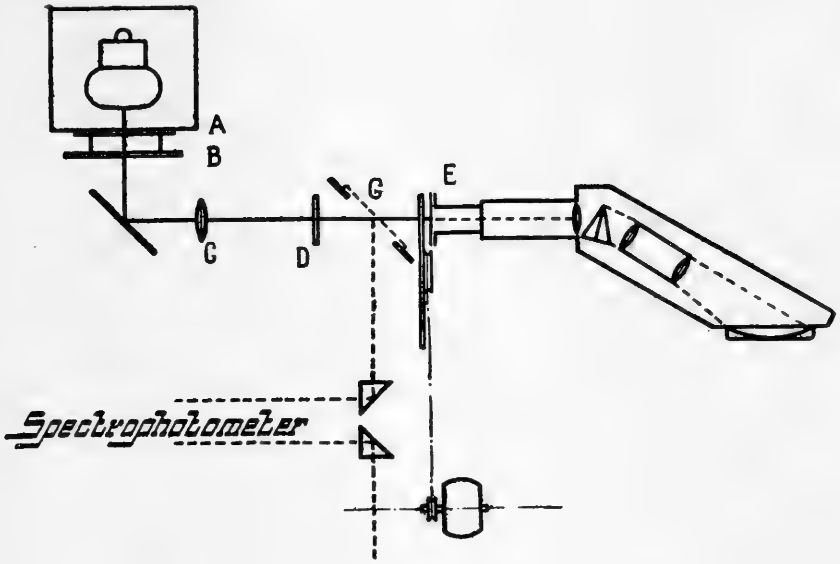

Fig. 2.

Under the mercury lamp a plate of ground-glass was introduced. A second similar plate *B* was illuminated by *A*. The illumination of *B* was completely uniform. An image of *B* was thrown upon a 3rd ground glass plate *D* by means of a lens *C*. The slit in the HILGER-quartz spectrograph was completely uniformly illuminated by *D*. The sector was placed close in front of *E*. At *G* a mirror was placed which could be removed. By means of a spectro-photometer of the KÖNIG-MARTENS type the illumination of *D* could be measured in the light of the green mercury line.

The measurements proceeded as follows. When the mercury lamp had become constant, the illumination of *D* was determined by the spectro-photometer. Then the mirror *G* was removed and the plate illuminated for three minutes. After this *D* was again measured with the photometer. The illumination was then changed by reducing the lens *C* with a diaphragm. The measurements were repeated and a new photograph taken.

In this way we made several series of readings with different intensities.

The differences in length of the spectral lines were measured by placing the negatives on top of each other and shifting them until

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[1]) Other checking measurements carried out previously may be found in Diss. HAMBURGER p. 87, 91, 92, 96, 108 and 113.

the tops of the lines to be compared were in a line beside each other. The distance between the dark lower ends was then measured. As in this way the two lines could be compared over a fairly great length and both appear on the same back ground, the difference in length can be pretty correctly measured. Not more accurately however than to about 0.2^5 mm.

With intensity-ratios of 1 : 3,95 : 8,80 as measured by the spectro-photometer we found in the mean differences of length of 3,7 and 6,5 mm. Calculating from these the intensity-ratios we find $1:3{,}6:9{,}4^6$. These deviations correspond to difference in length of $0{,}2^7$ and 0,2 mm. and are therefore of the order of accuracy with which the length of the lines can be determined. We could not find a systematic difference for the various lines; the lines measured were 5191—5170, 5461, 4359, 4047 and 3650.

We also tried the sector for much greater intensity-ratios, for instance 1 : 60 and 1 : 100. In these cases the short line was always found too short. It is certainly not surprising that the sector is not correct in the extreme fields. For some investigations, however, these are not of importance; in which cases the method may be usefully applied. The method [1]) was not sufficiently accurate for us to be in a position to decide as to the validity of BUNSEN and ROSCOE's law.

To summarize the advantages and disadvantages of this method we may say: it gives on a single photographic plate an easily read image of the spectral intensity including the ultraviolet part of the spectrum; it enables us to determine quantitatively changes in the intensity, the measurements do not cost much time and require only a few photographic plates. The accuracy of the method, however, is not more than about 10 %. Greater intensity-ratios than about 1 : 20 cannot very well be determined by it. These characteristics of the method make it very suitable for preliminary investigations, which can subsequently be worked out in detail by a less rapid and usually less easily readable method. Researches of this kind concerning the emission of light by gases and their mixtures, have been made by one of us which have demonstrated its usefulness.

It remains to express our sincere thanks to Dr. G. L. F. PHILIPS for the friendliness with which he enabled us to carry out this research.

Eindhoven. *Laboratories of Philips's Incandescent lamp factories.*

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[1]) As a matter of fact all photographic methods have only a relatively small accuracy. A. E. WEBER (l. c.) says for instance that if abnormal values are excluded a possible error of about 8 % must be reckoned with.

Physics. — *"On the sparking-potential of argon-nitrogen mixtures"*. By Dr. G. Holst and A. N. Koopmans. (Communicated by Prof. H. Kamerlingh Onnes).

(Communicated in the meeting of September 29, 1917).

1. *Introduction.* It is well known that the so-called halfwatt-lamps are filled with a gas at a pressure of about an atmosphere in order to obviate the evaporation of the tungsten wire. In lamps with a relatively thick wire, in which the loss of heat to the gas is comparatively small as against the radiated heat, nitrogen is commonly used, whereas argon is used in lamps with thin wires as its conductivity for heat is very much smaller. The substitution of argon for nitrogen produced a new difficulty: the occurrence of electric discharges through the gas. By the addition of small quantities of nitrogen it has been possible to raise the sparking-potential by a considerable amount. [1])

Our wish to learn more of this influence of nitrogen on the sparking-potential was the origin of the present investigation. A few measurements on this subject have been made by Bouty [2]); he also discovered the great influence of traces of admixtures.

2. *Experimental method and apparatus.* The method used by us is based on the drop of potential at the terminals of a condensor connected in parallel to the discharge tube at the moment of passage of the spark (fig. 1). The discharge tube was a large bulb of 12 cms in diameter, with two silver electrodes attached to stiff brass wires. The electrodes had a diameter of 36 mms and their distance was about 6,7 mm. [3]) The ends which were turned towards each other were spherical with a radius of 10.8 cm. A side-tube was blown to the bulb on which a quartz window was cemented. Through this window a small spark between aluminium electrodes could be

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[1]) A. E. G. D. R. P. 289543.

[2]) E. Bouty, Journ. de Phys 4. série, 1904, p. 489, 593.

[3]) Owing to a displacement of the electrodes by change of pressure it was necessary to measure the distance of the electrodes each time with a kathetometer.

focussed on the kathode by means of a quartz lens in order to prevent retardation in the discharge.[1])

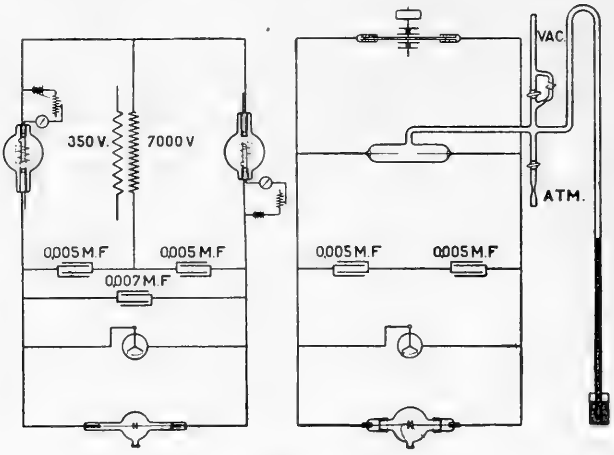

Fig. 1.

In the case of pure argon we used a discharge tube of 7 cms diameter with electrodes of 13 mms at a distance of 6,31 mm. The radius of curvature of the opposed surfaces was 7 cms; the centre of curvature coincided with the sealing places of the supply-wires, so that changes in the position of the electrodes had no influence on the distance.

The discharge tube was connected to a LANGMUIR condensation pump and to the apparatus in which the gases were kept and purified. The argon-nitrogen mixtures were prepared from fractionated argon and nitrogen[2]): the composition was determined by means of a baroscope. The last remaining traces of oxygen were burnt out by means of a glowing tungsten wire. The gases were freed from water-vapour, mercury-vapour and carbon dioxide by means of cooling tubes in liquid oxygen. Pure argon was prepared in a potassium-cell according to GEHLHOFF[3]); the smaller discharge-tube was used in this case in order not to use too much gas. The pressure in the discharge-space was measured with a mercury gauge; every now and then we used a MAC LEOD gauge. The potential was

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[1]) E. WARBURG, Ann. d. Phys. (62), 385, 1897. When an arc between iron electrodes was used, irregular results were obtained, probably in consequence of an effect of the heat.

[2]) Our thanks are due to Mr. H. FILIPPO Jzn. for the preparation of the mixtures.

[3]) We made use of the same large potassium-cell as was used by Dr. L. HAMBURGER (Diss. Delft 1917).

measured with an electrometer according to WULF[1]), which could be used up to 40000 Volts. It was calibrated with a compensation-apparatus according to BROOKS. The ratio between the various measuring ranges was checked with a sparking-gap between needle-points.

The condensers were glass tubes 1 metre in length and 8 cms in diameter which were silvered both on the inside and outside. At the ends and at the bottom the glass was locally thickened in order to prevent the spark piercing the glass[2]). They were blown for us in PHILIPS' glass-factory[3]). Their capacity was about 0,005 of a micro-farad. When rectified alternating current was used (see below), an additional condenser of 0,007 m.F. had to be connected up in parallel in order to obtain a sufficiently constant potential. With the higher potentials the condensers were charged with a Wimshurst machine. In order to make the charging take place very slowly, a discharge-tube with pointed electrodes was put in parallel to the machine. The air-pressure in this tube could be varied. During the measurements air was slowly admitted. Hereby the tension at the pointed electrodes was gradually raised. As soon as the sparking potential in the discharge-tube which is filled with the mixture is reached and passed, the discharge takes place and the potential falls to a smaller value. The highest reading of the electrometer was taken.

With the smaller tensions this arrangement was not found satisfactory. For this reason we used in this case for charging the condensers a 7000 volt-transformer with rectifiers joined up as described by GREINACHER[4]). Instead of cells of the GRAETZ-pattern we used thermo-ionic rectifiers of our own manufacture, so called kenotrons[5]). By a regulation of the heating current any charging-speed desired could be obtained.

In all our experiments we have taken care that electrostatic disturbances were kept down as low as possible.

3. *Measurements.* The apparatus were first checked by a series of determinations of the sparking-potentials in air. The air was freed of carbon dioxide and water vapour.

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[1]) TH. WULF, Phys. Zeitschr. (11), 1090, 1910.

[2]) J. MÓSCICKI. E. T. Z. 1904, 527.

[3]) Our thanks are due to Mr. P. J. SCHOONENBERG, who superintended the manufacture of the condensers.

[4]) H. GREINACHER, Phys. Zeitschr. (15), 412, 1914.

[5]) J. LANGMUIR, Electrician 1915. LXXV, 240.
A. W. HULL, Electrician 1916. LXXVII, 220.

These observations are put together in table 1; p stands for the pressure in mms mercury, S for the distance of the electrodes in cms.

Our results lie within the limits of those obtained by other observers and differ but little from the values given in the "Standardization rules of the A. I. E. E." [1]) for the sparking potentials between spheres of a diameter of 62.5 mms (compare fig. 2).

TABLE 1

Sparking-potentials in dry, CO_2-free air.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
496.4	21300	301.6	13683	122.9	6657	7.83	1017
465.8	19931	264.3	12428	97.6	5587	3.80	687
441.0	18893	267.7	12335	71.6	4303	3.06	640
412.0	17851	235.2	11140	52.2	3427	3.16	616
403.6	17367	207.7	10134	40.9	2911		
338.7	15018	165.6	8374	24.8	1998		

In figs 3—5 our observations on argon, nitrogen and mixtures of argon and nitrogen are plotted. Table 2 contains the results of our measurements.

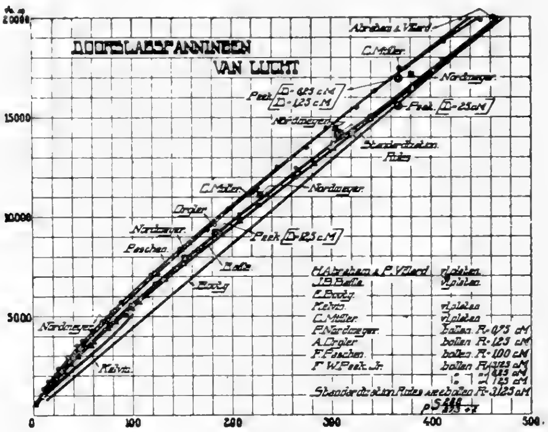

Fig. 2.

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[1]) Standardization rules, Edition of July, 1, 1915, p. 50.

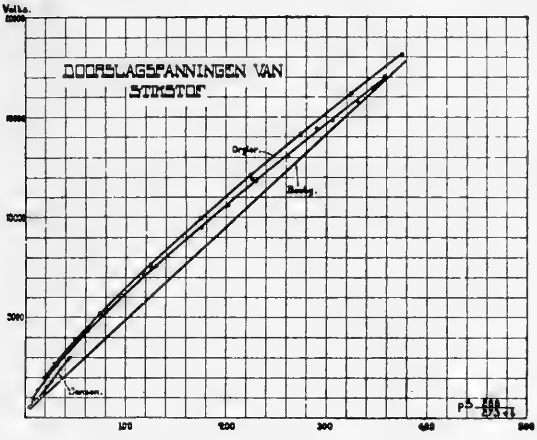

Fig. 3.

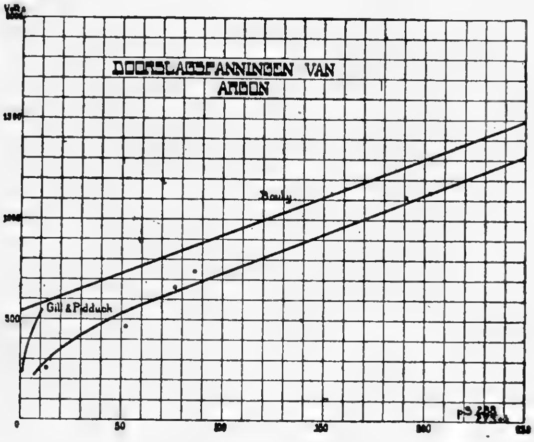

Fig. 4.

The figures found for nitrogen lie in between the potentials determined by **Orgler** [1]) and the values found by **Bouty** for the "champ critique", a quantity which is practically identical with the sparking-potential. [2]) (See fig. 3).

At lower pressures **Jensen** [3]) has also made some measurements. Supposing his measurements were also made at 15°, his results are lower than ours; but the temperature is not mentioned by him.

Pure argon has been investigated by **Bouty** [4]) and at low pressures

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[1]) A. **Orgler**, Ann. d Phys. 1900, 1, 159.

[2]) E. **Bouty**, Journ. de Phys. 4. série III. 1904, 489, 593.

[3]) J. C. **Jensen**, Phys. Rev. Vol. VIII, 433, 1916.

[4]) E. **Bouty**, Ann. de Chim. et de Phys. 8. série. Tome 23 (1911) p. 1.

by Gill and Pidduck [1]). We found a lower value for the potential at which the discharge occurs. (See fig. 4). The direction of our curve is practically the same as Bouty's curve at high pressures. As the direction of this curve according to Bouty is a test of the purity of the argon, and the curve given in the figure corresponds to Bouty's purest sample we may conclude that our argon satisfied very high demands. Similarly to Bouty we found it exceedingly difficult to obtain series of observations which give properly corresponding results, as small quantities of gas — probably liberated from the electrodes by the sparks — raised the sparking-potentials considerably. We found it therefore necessary to purify the argon anew for the determination of each point.

TABLE 2.

Sparking-potentials for argon, nitrogen and mixtures of argon and nitrogen.
Pure nitrogen.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
358.02	17033	229.78	11771	118.45	7162	22.53	2074
346.12	16460	226.05	11870	97.68	6052	12.97	1377
330.50	15799	202.91	10561	80.20	5248	8.16	954
306.13	14868	175.40	9459	63.32	4313		
290.54	14430	130.38	7548	47.96	3577		
261.03	13057	125.95	7402	32.93	2717		

Argon-nitrogen mixture 80.9 % N. 19.1 % A.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
386.39	15963	232.84	10401	129.50	6692	53.80	3442
336.27	14270	199.82	9320	102 40	5714	38.79	2731
271.20	11741	163 23	7894	68.57	4117	27.18	2143
						13.88	1342

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[1]) E. W. B. Gill and F. B. Pidduck, Phil. Mag. (6) 16, p. 280, 1908; 23, p. 837, 1912.

TABLE 2.

Argon-nitrogen mixture 44,6 % N. 55,4 % A.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
333.65	11283	201.78	7522	89.36	3988	17.57	1264
315.78	10899	196.82	7355	62.13	3058	11.15	927
293.93	10234	167.65	6538	52.35	2664	4.96	190
274.36	9669	134.99	5499	41.26	2224		
247.81	8954	121.30	5031	31.78	1892		
223.52	8253	103.45	4472	25.41	1637		

Argon-nitrogen mixture 22.3 % N. 77.7 % A.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
379.24	10131	270.25	7841	159.60	5340	89.35	3420
364.50	9879	261.62	7752	150.86	5020	72.81	2924
337.16	9313	235.35	7041	124.54	4348	41.36	1907
301.63	8577	192.52	6006	118.11	4189	24.14	1378
296.68	8392	186.38	5938	98.91	3635	16.30	1064

Argon-nitrogen mixture 11,2 % N 88,8 % A.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
394.33	7922	251.31	5469	132.34	3243	39.07	1405
320.96	6667	232.64	5227	114.39	3017	21.52	965
296.51	6294	191.53	4482	97.65	2801		
292.47	6213	162.77	4021	62.06	2005		

Argon-nitrogen mixture 6,6 % N 93,4 % A.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
373.67	6248	259.41	4733	132.27	2813	58.54	1642
341.92	5774	227.85	4211	112.63	2517	46.19	1326
306.66	5298	198.20	3798	98.09	2336	29.16	888
282.54	4972	164.74	3368	79.00	1988		

Pure argon.

$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts	$pS\frac{288}{273+t}$	Potential in Volts
202.48	1138	86.23	740	63.34	678	12.85	255
190.57	1113	76.44	660	52.25	466		

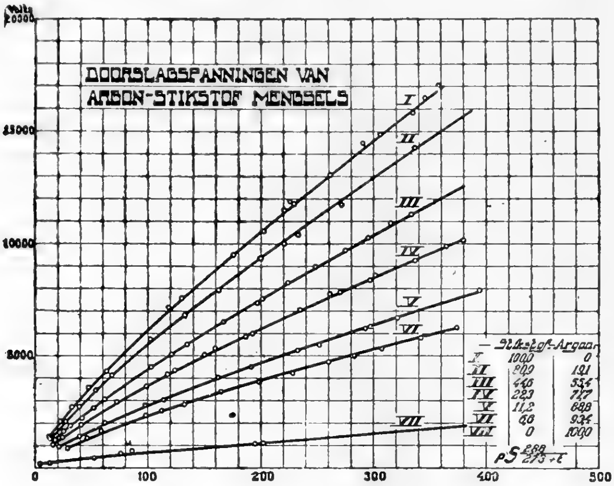

Fig. 5.

Our results on argon, nitrogen and the mixtures are combined in figs 5 and 6. Fig. 6 shows the large influence of small admixtures of nitrogen on the sparking-potential, especially at the higher pressures. Qualitatively our measurements on mixtures agree with those of Bouty.

4. *Discussion of the results.* The exceedingly low sparking-potential for argon can be made intelligible by TOWNSEND's theory. [1]) According

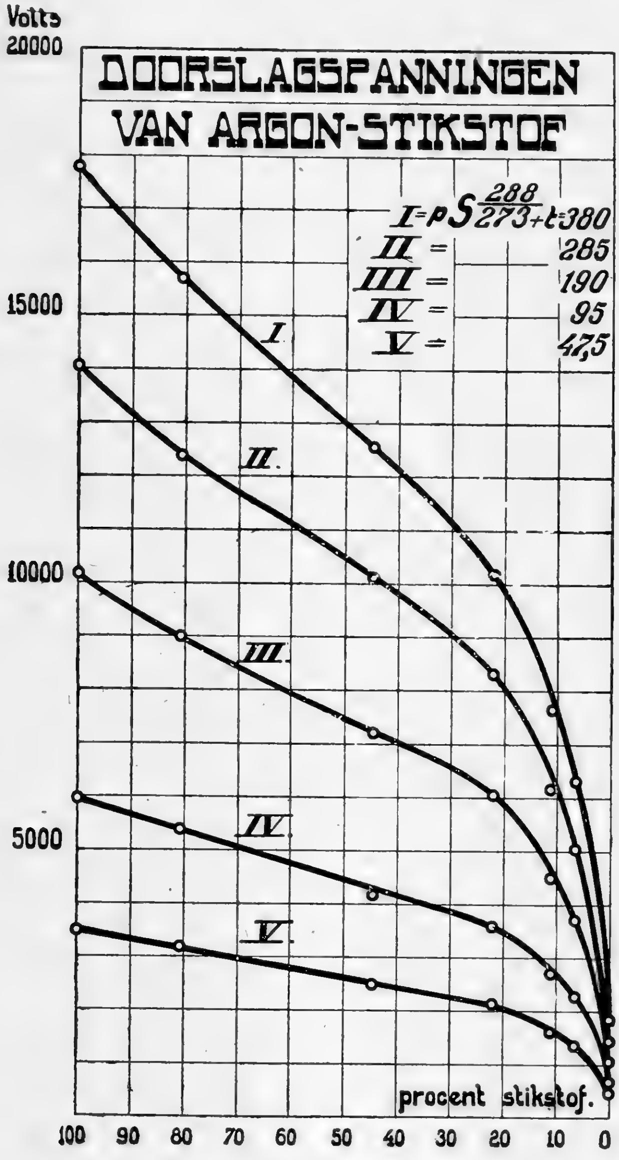

Fig. 6.

to him the distance S (in cms) between the plates at which the discharge passes with a given gas-pressure and potential-difference is given by

$$S = (\alpha - \beta) \log \left(\frac{\alpha}{\beta}\right)$$

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[1]) See for instance TOWNSEND, Electricity in gases, p. 323.

where α represents the number of ionising collisions per cm. of path described in the direction of the field for a negative ion, β the same quantity for a positive ion. S will thus be large, if α is large and if β — which is always small as compared to α — does not become relatively large. Argon belongs to the so-called "elastic" gases in which the negative ions in collisions, with molecules which do not give rise to ionisation, do not lose any energy. For these gases K. T. COMPTON [1]) has recently calculated $\frac{\alpha}{p}$ ($p =$ gas-pressure) as a function of the ionisation-potential V_0, the mean free path l and the intensity of the field per unit pressure $\frac{X}{p}$. He finds $\frac{\alpha}{p}$ much larger for the elastic gases than for the non-elastic gases. Substituting in the table given by him the values of V_0 and l for argon, neon and helium (the value of l being taken proportional to the value calculated according to the kinetic theory) α is found largest for argon. As there is no obvious reason for assuming that β would be much larger for the elastic gases than for the non-elastic ones, the large value of α can make us foresee that the sparking-potential of argon should be particularly small.

As regards the sparking-potential of the mixtures we may again refer to a calculation of COMPTON's [2]). This calculation was not carried to the end, but it indicates, that for a mixture of an elastic and a non-elastic gas α should have about the character of α for non-elastic gases. Now nitrogen is not altogether un-elastic. K. T. COMPTON and J. M. BENADE [3]) have proved, that the loss of energy in a collision of an electron with a nitrogen molecule depends on the smallest distance of their respective paths, more particularly, that it is inversely proportional to the 10th power of this distance. In this way the problem certainly becomes very complicated.

The other limiting case of a mixture of two completely elastic gases has been discussed by FRANCK and HERTZ [4]), although in a somewhat different connection. In that case the gas with the smaller ionisation-potential practically completely assumes the conduction. For nitrogen the ionisation-potential is considerably smaller than for argon. The part played by nitrogen will therefore be in the first place a reduction of α by its un-elastic nature, whereas on the other hand

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[1]) COMPTON, Phys. Rev. (7). 517, 1916

[2]) loc. cit. p. 516.

[3]) Phys. Rev. (8) 449. 1916.

[4]) FRANCK and HERTZ, Ber. D. phys. Ges. (18) 213. 1916.

when the current passes, the nitrogen will be principally ionised. The spark will therefore show the nitrogen-spectrum. This was actually observed by BOUTY [1]) and was recently confirmed by HAMBURGER's measurements. [2])

In conclusion we wish to express our sincere thanks to Dr. G. L. F. PHILIPS for the opportunity given to us of carrying out this investigation.

Eindhoven.

Physical Laboratory of
Philips's Incandescent lamp-factory.

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[1]) BOUTY, Journ. de Phys. (1904), p. 605.
[2]) HAMBURGER, Diss. Delft 1917.

Physics. — *"The brightness of the black body and the mechanical equivalent of light."* By Dr G. HOLST and J. SCHARP DE VISSER. (Communicated by Prof. H. KAMERLINGH ONNES.)

(Communicated in the meeting of Sept. 29, 1917).

Introduction. For various investigations it is of great importance to know the brightness of the black body at various temperatures. Direct measurements were made by LUMMER and PRINGSHEIM[1] and by NERNST[2]. NERNST sums up his results in the formula $\log H = 5{,}367 - \frac{11230}{T}$ ($H =$ intensity of 1 cm² in Hefner-candles, $T =$ absolute temperature). The curve corresponding to this formula passes closely through LUMMER and PRINGSHEIM's points. Recently, however, doubts have arisen as to these measurements. Various investigators starting from the spectral distribution of energy, the sensitiveness of the eye and the mechanical equivalent of light have calculated the brightness of the black body and all of them come to much higher values than were found experimentally. The following table shows the results:

	formula $C_2 = 14600$	$C_2 = 14600$	$C_2 = 14392$	$C_2 = 14400$	$C_2 = 14370$	$C_2 = 14450$
T	NERNST	LUMMER PRINGSHEIM	LANGMUIR [3]	PIRANI MIETHING [4]	IVES KINGSBURY [5]	FOOTE FAIRCHILD [6]
1300	0.054	0.052	0.0784	0.095	0.0700	0.086
1400	0.222	0.219	0.300	0.340	0.262	0.333
1500	0.759	0.760	0.960	1.04	0.825	1.051

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[1] LUMMER and PRINGSHEIM. Phys. Zeitschr. (2) 97, 1901.

[2] NERNST. Phys. Zeitschr. (7) 380, 1906.

[3] LANGMUIR. Phys. Rev. (7) 302, 1916.

[4] PIRANI and MIETHING. Verh. D. Phys. Ges. 219, 1915.

[5] IVES and KINGSBURY. Phys. Rev. (8) 323 1916 and mech. equiv. $M = 0.00159$ Phys. Rev. (8) 254, 1916.

[6] FOOTE and FAIRCHILD. Scient. papers Bur. Stand. n⁰. 270 and $M = 0.00159$.

Langmuir has made a series of measurements on tungsten-lamps from which by means of the mean emissive power the brightness of the black body may be calculated. At the higher temperatures the results of these measurements are in good accordance with the calculated values. At the lower temperatures the measured values are considerably smaller. Langmuir ascribes this deviation to the influence of the Purkinje-effect in the photometric determinations. If the values found by Nernst and by Lummer and Pringsheim are reduced to the temperature-scale used by Langmuir, the results of their measurements are found to be very nearly in a constant ratio to those of Langmuir's; Langmuir finds an intensity which is 1.15 times higher. It is, therefore, probable that in the measurement of the former investigators also the Purkinje-effect must have played a part. Ives and Kingsbury [1]) have also pointed this out. It thus seemed to us worth while determining the intensity of the black body once more, arranging the method in such a manner that the Purkinje-effect cannot arise. We made our measurements in the neighbourhood of the melting-point of gold seeing that this temperature is sufficiently well ascertained to enable us from the intensity as measured, the known energy-distribution of the black body and the luminosity curve for the human eye to calculate the mechanical equivalent of light.

Method and instruments.

The arrangement of the method is rendered in fig. 1. Through the telescope A, the photometric prism B and the two totally reflecting prisms C and D, a small disc of alundum blackened with oxydes and placed in the furnace E and a plate of ground glass F are seen immediately beside each other. F is provided with a diaphragm the opening of which is measured with great accuracy. F is illuminated by a second plate of ground glass G and a projection-lamp H, which is placed behind it. The three prisms B, and C and D are mounted on one plate I. They may be removed in a body and replaced in exactly the same position. When I is removed the temperature of the furnace may be measured with the Holborn and Kurlbaum-pyrometer J and the candle power of the ground-glass F with the Brodhun-sector photometer K.

In front of F an additional rotating sector L was placed. This sector rotated during the comparison between furnace and ground-glass but was stationary in the photometric measurement of F in

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[1]) Ives and Kingsbury, Phys. Rev. (8) 177, 1916

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order to obtain a greater intensity of light. Moreover a potassium bichromate filter *M* was placed in front of *F* by means of which

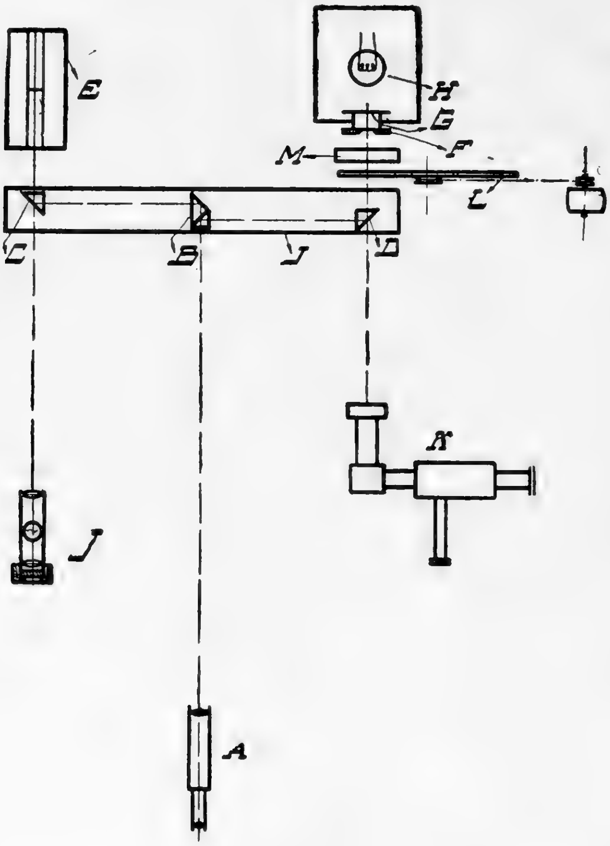

Fig. 1.

in the comparison of furnace and ground-glass the colour could be matched.

The measurement was made as follows. The plate *I* with the prisms was removed and the temperature of the furnace was checked until it was properly constant. *I* was then replaced and with the sector *L* in motion the brightness of the ground-glass *F* as seen through the telescope *A* was made equal to that of the alundum disc in the furnace. This was attained by varying the current in the projection-lamp. As soon as equality was obtained the prisms *BCD* were once more removed; the furnace-temperature was then again determined and the ground-glass *F* was measured photometrically with the sector open. In this measurement the intensity was $\frac{200}{70}$ times greater than in the comparison furnace-ground glass. The photometric measurement of *F* with the bichromate filter in front was difficult owing to the great difference in colour. For this reason F was measured without the filter. From the brightness obtained in this way and the absorption of the filter the brightness of F with the filter in front could be calculated. The absorption of the filter was determined separately with a flicker-photometer for the same spectral energy-distribution of the light-source. For this measurement the intensity of the ground glass was too small, however; according to Ives[1]) in order to obtain satisfactory results with a flicker-photometer the illumination of the photometer-fields must be about 25 metre-candles. This higher intensity was obtained by cementing together the two plates of ground glass with a drop of canada-balsam[2]). They became transparent thereby and the absorption in the bichromate filter could now be determined at a suitable brightness of the photometer-fields. Fig. 2 shows the ratio of the intensities with and without filter at various current-strengths through the projection-lamp. The measurements both with the flicker-photometer and the Brodhun-photometer are given in the figure. Although the determinations with the Brodhun-photometer will be seen to deviate mutually more than those with the flicker-photometer, the two series do not appear to give any systematic difference. This is not to be wondered at considering that all the measurements were made at such intensities of the photometer-fields that the Purkinje-effect could not have any influence.

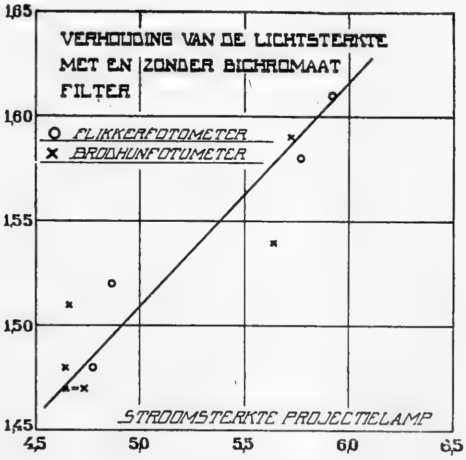

Fig. 2.

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[1]) Ives. Phil. Mag. (24) 1912.

[2]) The glass-plates which we used were not completely free of colour. Otherwise they might have been simply removed.

The measurements.

The pyrometer which was used for measuring the temperature had been very carefully standardized at the melting-point of gold. The greatest deviation in six measurements was 1° C. The active wave-length of the red glass filter (Schott and Gen. N°. 2745) had been determined by the method of Hyde, Cady and Forsythe [1]) with a spectro-photometer. By means of photometric measurements the relation was established between the current in the pyrometer-lamp and the ratio of the intensity as measured at a definite temperature through the red filter to the intensity at the melting point of gold.

The constant of the Brodhun-photometer was determined by means of a set of lamps which had been standardized in the Phys. Techn. Reichsanstalt.

The set of three prisms $B\ C\ D$ was standardized separately. The determination of the absorption of the bichromate-filter was made by six observers. The results of four of these agreed to within about two percent. The other two obtained larger deviations. As with them similar irregularities had been noticed on a former occasion, their results were rejected.

The following results were thus arrived at:

T	H candles/cm².	T	H candles/cm².
1296	0.0743	1298^5	0.0743
1301^5	0.0716	1301	0.0682
1307	0.0784	1427	0.369
1307	0.0820	1415^5	0.324

By means of a plot of the form $log\ H = f\left(\frac{1}{T}\right)$ we found at the melting point of gold $T = 1336°\ H = 0{,}119^{HK}/_{\text{cm}^2}$.

The values found by us agree well with those calculated by others, but deviate very far from the numbers obtained by Nernst and by Lummer and Pringsheim. It is, therefore, very probable that in their measurements the Purkinje-effect must actually have played a part. Nernst used rods of an area of 10 mm². The intensity in his measurements was, therefore, very small indeed.

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[1]) Hyde, Cady and Forsythe Astrophys Journ. (42) 294, 1915.

The mechanical equivalent of light.

According to our results the brightness of the black body at 1336° K would be 0,119 $^{Hk}/_{cm^2}$. The light-flux is therefore 0,119 π $H^{lumen}/_{cm^2}$ But this flux is also equal to:

$$\frac{1}{M}\int_0^\infty E_\lambda V_\lambda d\lambda = \pi\, 0{,}119$$

where M is the mechanical equivalent of light i. e. the number of watts required to obtain a light-flux of 1 Hlumen of the wave-length for which the sensitiveness of the eye for light is a maximum; $E\lambda$ being the intensity of radiation of the black body in the wave-length λ at 1336°, V_λ the sensitiveness of the eye for light of wave-length λ.

Calculating M for the determinations of the luminosity curve of a number of observers we found using the measurements of

Coblentz and Emerson (125 persons)[1]	$M = 0{,}00154$ $^{Watts}/_{H.\ Lumen.}$
Ives and Kingsbury (61 persons)[2]	0,00151
Nutting (21 persons)[3]	0,00135.

The constants in Planck's formula[4]) were taken as follows:

$$c_1 = 3{,}704\ 10^{-12}\ ^{Watts}/_{cm^2}$$
$$c_2 = 1{,}4300 \qquad \text{genomen.}$$

With 1 watt a mean spherical intensity of 51.7 H-Candles may thus be obtained.

We add the results of a few more observers.

Buisson and Fabry[5])	$M = 0{,}00130$ $^W/_{HL}$
Nutting[6]) (acetylene flame)	$M = 0{,}00108$
Langmuir[7]) (tungsten-lamp luminosity curve according to Nutting)	$M = 0{,}00109$
Ives and Kingsbury[8]) (various methods)	$M = 0{,}00143$
Pirani and Miething[9]) (measurement on lamps)	$M = 0{,}00123$

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[1] Coblentz and Emerson. Ref. El. World (69) 1117, 1917.

[2] Ives and Kingsbury. Phys. Rev. (6) 319, 1915.

[3] Nutting. Phil. Mag. (29) 301, 1915.

[4] c_2 was taken in accordance with the new temperature-scale of the Ph. T. R. This choice further involves $h = 6{,}55\ 10^{-27}$, $N = 6{,}05\ 10^{23}$, $\sigma = 5.75\ 10^{-12}$ $^{watts}/_{cm^2}$ in good agreement with the most recent measurements.

[5] Buisson and Fabry. Compt. Rend. (153) 254, 1911. Comp. also Ives, Coblentz and Kingsbury. Phys. Rev. (5) 269, 1915.

[6] Nutting. Phil. Mag. (29) 301, 1915.

[7] Langmuir. Phys. Rev. (7) 302, 1916.

[8] Ives and Kingsbury. Phys. Rev. 1915 and 1916, various articles. Final value in (8) 254, 1916.

[9] Pirani and Miething. Verh. D. Phys. Ges. 219, 1915.

The agreement with Ives [1]) is thus pretty close. A large source of uncertainty is due to the sensitiveness of the eye. By selecting observers with normal eyes in the manner proposed by Ives and Kingsbury [2]) it will undoubtedly be possible to obtain results which are in better mutual agreement.

Eindhoven. *Physical Lab. of Philips's*
Incandescent lamp-factory.

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[1]) With $c_2 = 14350$ we find $M = 0,00143$ W/HL. (Note added in translation).

[2]) Ives and Kingsbury. Trans. Ill. Eng. Soc. (X), 317, 1915.

Compare particularly Richtmeyer and Crittenden. Ill. Eng. Soc. New-York. Febr. 10 and 11, 1916.

Physics. — "*On the light emission of gases and mixtures of gases by electric discharges.*" By Dr. L. HAMBURGER. (Communicated by Prof. H. KAMERLINGH ONNES.)

(Communicated in the meeting of December 29, 1917).

§ 1. *Method of investigation.*

Researches on the influence of different factors in the emission of light of gases by electric discharges have revealed phenomena of so very different nature, that we strongly felt the desirability of systematic methods of investigation and of objective measuring methods that are free from the imperfections of the human eye.

As however in spectroscopy and the measuring of intensities which is closely connected with it, great difficulties arise, we should have to wait a very long time for results if we wished to measure the intensities of each emitted wavelength (spectral line or band) for the different gases and mixtures and then still under different circumstances. Therefore on entering this difficult and vast domain of research our first work can only be a first exploration of the new field of investigation, which work renders a method desirable which allows a quick survey of what is observed under different conditions. Such a method is the photographic one, which can also be used in the ultraviolet part of the spectrum and which has been described in a recent publication of G. HOLST and L. HAMBURGER. [1])

In the two next paragraphs of this short paper a survey is given of the most important results of an investigation made in this way on the influence of different factors on the emission of light of gases and mixtures by electric discharges. The lightning of the gases was excited by a continuous current discharge apparatus the construction of which is based upon the direction lines determined by H. KONEN and W. JUNGJOHANN [2]) and in which (comp. J. SCHWEDES [3])) streaming gases are used. By the application of streaming gases several phenomena could be investigated which up till now had not or hardly been observed.

The sector method alone, described in the publication of G. HOLST

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[1]) These Proceedings, p. 1021.

[2]) Verh. d. D. phys. Ges. (12) **3**, 128—144 (1910).

[3]) Zeitschr. f. wiss. Phot. **11**, 199 (1913).

and L. Hamburger, does not allow to compare the intensities of the radiations of different wavelengths. This can be done however by determining the selective action of the optical arrangement and of the photographic plate.

For this purpose it proved in some definite cases desirable to illuminate the sensitive plate, whilst using the rotating sector, with known temperature radiators, which work at a very high temperature and the energy-curve of which is known. The sensibility curve arising after the development (fig. 1, plate I) was then compared with the energy-curve following from the well-known laws of radiation. In this way the change of sensibility of the plate with the wavelength could be determined.

Those who want to know more about the subject, must be referred to my dissertation [1]), where moreover a collection of objectively reproduced emission spectra of a considerable number of gases and their mixtures will be found, which at first sight give an impression of the distribution of intensities, such at least as has been represented on the photographic plate. Such a collection was wanting until now in the literature on the light emission by gases.

§ 2. *Measurements of intensity for pure gases* [2]).

Investigations were made on the following gases: nitrogen, hydrogen, argon, neon, helium. As an example we shall first give a more detailed discussion of the result found with nitrogen. We shall however begin with referring to the spectrograms represented in fig. 2. plate I. The first one is obtained by the action of light emitted by nitrogen, when in front of the slit of the spectrograph a wedge of smoked glass has been placed. The other one has been made with the rotating sector. This reproduction is a good illustration of the very strong selective power of the wedge of smoked glass.

A. *Nitrogen.*

It may be allowed to mention in this place only the conclusions we got from the observations on the sharp edges of the bands of the second positive nitrogen group:

1. The measurements can be reproduced. 2. The influence of the time of illumination on the length of the edges of the bands is in agreement with the calculations. 3. The emissive power of nitrogen

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[1]) L. Hamburger. Diss. Delft. 1917. In one chapter of this dissertation my experiments in the phenomena shown by *Tesla*-discharges in gases have been described.

[2]) For comparison with the measurements known in the literature see my diss.

HAMBURGER: "ON LIGHT-EMISSION OF GASES AND MIXTURES OF GASES CAUSED AT ELECTRICAL DISCHARGES"

Fig. 1.

Spectrum of glowing Tungsten obtained with the aid of the rotating sector - disc etc. on Wratten & Wainright's panchromatic - plate (Copper - comparison spectrum).

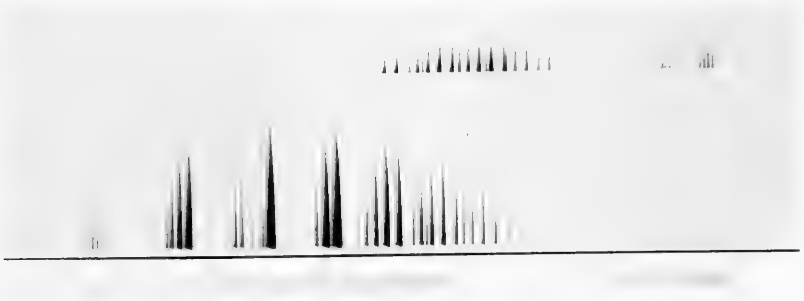

Fig. 2

Comparison of the nitrogen-spectrograms, obtained with the aid of the rotating sector-disc, resp. the wedge-method.*)

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*) In the visible part of the wedge spectrogram (spectrum 2, fig. 2) some bands there occur that are wanting in the sector-spectrogram. The time of exposition however was in the latter case much shorter.

HELIOTYPE, VAN LEER, AMSTERDAM

is proportional to the supplied energy. 4. As to the pressure of the gas the following conclusions are drawn from the measurements.

a. For constant current density the intensity of the light decreases with a decrease of the pressure for the investigated range of pressure.

b. At the same time the maximum of the emitted energy is shifted towards the shorter wave-lengths.

c. At a change of the pressure of the gas the bands belonging to the negative pole show a characteristic difference in behaviour with the bands of the second positive nitrogen group which surround them.

d. The potential gradient in the positive column decreases with decrease of pressure and also with increase of intensity of the current.

e. At the side of the cathode the passage of a current gives rise to a decrease in pressure, which for high values of the current density is relatively great; the value of this pressure effect proves to be (approximately) proportional to the intensity of the current. This effect is opposite to the very small pressure increase found by A. WEHNELT and J. FRANK [1]) under special conditions; it has been observed for the first time here in a spectral tube and it offers — especially when the dimensions of the apparatus would be chosen with a view to this purpose — a means to learn something about the properties (mass) of the gas particles that conduct the current. It has been proved, that we have to do here with a physical effect and we may directly draw the conclusion that the negative ions are no electrons but electric particles with a relatively large mass. From a more detailed investigation we learn that to this phenomenon there corresponds an increase in pressure at the anode. The pressure effect is smaller in gases with lower molecular weight. It is apparently possible to bring these different things into connexion with the difference in mass, number, velocity of the negative and positive electrons (mass-transport by the electric current).

B. *Hydrogen, neon, helium.*

The observations made on these gases are in so far in agreement with those on nitrogen, that here we may draw the general conclusion, that within the limits of possible observation errors (and within the domain of investigation) the intensity of the spectral lines or bands of all gases investigated is proportional to the supplied energy and that with decrease of pressure the maximum of energy is shifted towards the ultra-violet. Moreover we learn from the observation

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[1]) Verh. d. D. phys. Ges. 12, 444, 1910. See also J. STARK, BOLZMANN Festschr. 399, 1904.

that in the method used we have a very good means to discern between lines or bands belonging to different "carriers".

C. *Argon.*

The spectral properties of this gas are widely different from those of the preceding elements. It is a well known fact that Argon gives a red and a blue spectrum. The representatives of the red and the blue spectrum show a totally different behaviour with respect to the supplied energy as is evident from the graphical representation in fig. 3 [1]) of the connexion between the intensities of the light and of the current for the red and the blue spectrum of argon.

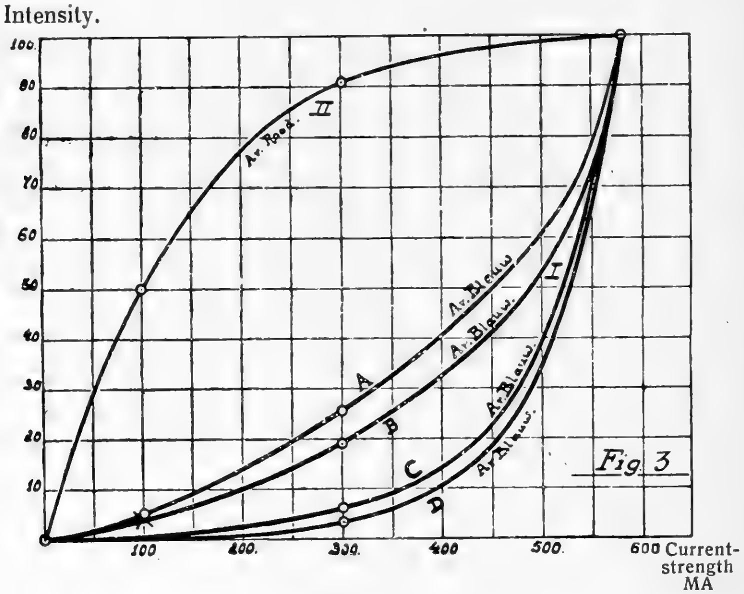

Fig. 3.

Also by changing other variables we find back this difference in behaviour.

§ 3. *Measurements with binary mixtures.*

The observations on gaseous mixtures show a pronounced difference between those mixtures, between the components of which reactions *can* take place and those for which this is *not* the case.

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[1]) This figure is a good confirmation of the visual observations of P. G. NUTTING and O. TUGMAN (Bull. Bur. of Stand 7, 62 (1911).

A. *Mixtures of gases which do not give reactions.*

Investigations were made with the binary systems argon-nitrogen, neon-nitrogen, argon-hydrogen, neon-helium, and argon-mercury.

While visual measurement by former investigators had founded the opinion that for gaseous mixtures the spectral behaviour is by no means an additive property with respect to the components, our research has rendered a slight change of these conceptions desirable. Let us first stipulate however, that the conceptions following from the above mentioned non-objective investigations doubtlessly contain a fundamental truth, especially there where the ionisation tensions of the components are widely different [1]) resp. where we have to do with highly electro-negative gases. In the last case however there occurs complication by chemical reactions.

Our investigations have taught us however to be careful not to exaggerate the conception, that the spectral behaviour of gaseous mixtures is by no means additive with respect to the components. At a definite total pressure the mixing of gases gives rise to a lower (partial) pressure of the components. And it proves to be a general phenomenon, that a decrease in pressure causes a shift of the light-emission towards the ultra-violet. With other words when gases are mixed visual observations will give the *impression* that the spectrum of one of the two gases strongly vanishes especially when the emissive intensities of the components are very different.

B. *Mixtures of gases which react with each other.*

The systems nitrogen-oxygen, nitrogen-hydrogen and nitrogen-carbon-monoxide were investigated. In each of these three cases the reaction took place by means of an electric discharge. Each time this chemical reaction was found to be accompanied by the occurrence of new bands in the spectrum. As for the system N_2—H_2 this had never been sharply proved in a spectral tube [2]). We subjected it to a detailed investigation, which supported the conception that the "electric ammonia-synthesis" is preceded by a disintegration of the reacting molecules into atoms. Further it proved possible to indicate conditions under which the ammonia-yield was many times higher

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[1]) Comp. J. **Franck** and G. **Hertz**, Verh. d. D. phys. Ges. **18**, 213 (1916)

[2]) Already in 1891 J. M. **Eder** mentioned the occurrence of new bands in the neighbourhood of $\lambda = 3359$ Å. in the spectrum of the ammonia-oxygen-flame. (Anzeiger d. k. Ak. d. Wiss. Wien, 1891. See also Denkschr. d. kais. Ak. d. Wiss. Wien **60**, 5, 1893.

than had ever been found. Photographs of discharges through flowing ammonia-gas confirmed the spectral part of the investigation of this system. For a more detailed discussion of the obtained results in connexion with the well-known theoretical considerations of J. FRANCK and G. HERTZ and J. STARK we must refer to our dissertation. Some points, both photographic and spectral, will be further discussed in a later paper of the author.

The author gladly accepts this occasion to express his gratitude to Dr. G. L. F. PHILIPS who kindly enabled him to make this investigation and also his great indebtedness to Prof. W. REINDERS and Prof. L. H. SIERTSEMA for their helpful interest in the work and the preparation of the writer's dissertation, from which this short paper is extracted.

Eindhoven. *Laboratory of the N.V.*
Philips's Incandescent-lamp-factory.

Physics. — *"The definiteness of macroscopic processes and the kinetic theory"* [1]. By Mrs. T. Ehrenfest—Afanassjewa. (Communicated by Prof. J. P. Kuenen).

(Communicated in the meeting of December 29, 1917).

§ 1. Let us suppose a definite quantity of gas to be a system containing a very great but finite number N of molecules which obey certain conservative laws of motion. Then all successive states of the system are for all times perfectly determined by $6N$ "microscopic coordinates" e. g. the $3\ N$ space-coordinates and the $3\ N$ velocity-components of all molecules at a definite but arbitrarily chosen moment t_0.

These quantities however are not directly observable. It is only possible to observe certain statistical mean values of these quantities: the "macroscopic coordinates", which at a definite moment give by far not enough determining quantities, so that we can by no means derive the microscopic quantities from the combination of the macroscopic ones.

Though therefore the microscopic state of the gas at one moment perfectly determines the progress of the successive "micro-" and also "macroscopic" states, the macroscopic state at one moment is not sufficient to determine the progress of the microscopic state, nor of the successive macroscopic states.

In this way we come to the following conclusion: though the kinetic theory of gases involves fundamentally a well-defined determination of the processes in a gas, it teaches us at the same time that it is practically impossible to obtain any knowledge of this determination and that we cannot but treat the succession of macroscopic states of a gas as the successive drawings of a lottery: after a definite given state we may expect all possible different states and only certain considerations on probability are at our disposal to confine our expectations somewhat.

The same is true when we want to extend the kinetic conceptions to the whole world.

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[1] The considerations given in this paper were induced by discussions with Prof. Ph. Kohnstamm and Prof. J. D. van der Waals Jr. in connexion with their publications.

Yet, in the establishing of physical laws and in applying them, every physicist practically behaves as if he were perfectly convinced of all macroscopic processes being determined by the macroscopic coordinates alone.

Now we may ask: this intuitive way of thinking, which always proved to be very fruitful, is it in incompatible antagonism with the kinetical conception — which on the other hand has also proved to be useful — or may a closer investigation prove these two conceptions to be reconcilable?

The following short discussion may indicate the way in which such a reconciliation between the two conceptions may be approached.

§ 2. The following has only to be remarked. We have never contented ourselves with describing the state of the world macroscopically for *one single* moment. On the contrary, we have been studying it since a very long time by means of many conscious and still more unconsious observations and the laws we establish must agree with all those observations.

Applied to our gas this runs as follows: Principally it is always possible to consider the values M_1^i, $M_2^i \ldots\ldots M_m^i$ of the macroscopic quantities M_1, $M_2 \ldots\ldots M_m$ at a moment t_i as functions of the 6 N unknown values x_1^0, $x_2^0 \ldots\ldots x^0{}_{6N}$ of the microscopic coordinates at the moment t_0 and of the moment t_i

$$\left.\begin{aligned}
M_1^1 &= \varphi_1 (x_1^0, x_2^0, \ldots\ldots x^0{}_{6N}; t_1)\\
M_1^2 &= \varphi_1 (x_1^0, x^2{}_0, \ldots\ldots x^0{}_{6N}; t_2)\\
&\ldots\ldots\ldots\ldots\\
M_m^1 &= \varphi_m (x_1^0, x_2^0, \ldots\ldots x^0{}_{6N}; t_1)\\
M_m^2 &= \varphi_m (x_1^0, x_2^0, \ldots\ldots x^0{}_{6N}; t_2)\\
&\ldots\ldots\ldots\ldots
\end{aligned}\right\} \quad \ldots\ldots (A)$$

However small the number m of the observable quantities may be compared with the number 6 N, by an increase of the number of moments of observation the number of equations A may be made to approach the number necessary for a perfect definiteness of the quantities $x_1^0, \ldots\ldots x^0{}_{6N}$.

But then the values of M_h are also determined for all moments, so that finally equations of the following form are obtained

$$\left.\begin{aligned}
M_1^i &= \Phi_1 (M_1^1, M_1^2, \ldots;\ M_m^1, M_m^2, \ldots;\ t_1, t_2, \ldots;\ t_i)\\
&\ldots\ldots\ldots\ldots\ldots\ldots\\
M_m^i &= \Phi_m (M_1^1, M_1^2, \ldots;\ M_m^1, M_m^2 \ldots;\ t_1, t_2, \ldots;\ t_i).
\end{aligned}\right\} \quad \ldots (B)$$

Principally it is therefore not necessary to be able to measure microscopic coordinates nor to determine the form of the equations A. The equations B are sufficient which only contain macroscopic variables and macroscopic instantaneous values.

The equations which have to be made up in physics can thus be considered as the elimination results of the equations B or, *strictly speaking*, as *approximations of these.*

§ 3. So the contradiction between the kinetic-microscopic and the physico-macroscopic conception has been removed by transferring all observations necessary for the perfect determination of the microscopic processes to the macroscopic domain. This however does not yet make it sure that the laws of nature worked out in physics until now are not open to the same objections as before our remark. Indeed: we have by no means made as many observations as are requested by the number of molecules by which the world is built up.

It may be allowed to answer this objection with an analogy. Let a part of an algebraic curve of extremely high degree be drawn and suppose we were asked to continue approximately its course to a certain finite extent. If it were possible to determine the coordinates for a sufficient number of points of the given part of the curve we should be able to indicate the whole further course of the curve. But let this be practically impossible because of the high degree of the curve, so we must content ourselves with a much smaller number of accurately determined coordinates. Must we then quite give up the solution of the problem? Firstly, the relatively few points will serve to confine somewhat the multitude of possible curves from which we have to choose. But we get still further if we take into consideration all we *see* in the part of the curve that is drawn out: that will give us many *qualitative* data, which enable us to exclude a much greater number of curves than would be possible by means of the accurately determined points and in this way we may be able — at least to a certain finite extent — to give a very good extrapolation of the curve.

It might be possible that in the same way in the establishing of physical laws the meaning of *qualitative* observations must not be estimated too low. Continuing the line of these considerations, the daily confirmation of the "intuitively" formed physical laws will perhaps no longer appear like an astonishing accidentality in face of the fundamental ideas of kinetics.

§ 4. We must pay attention to one other objection: what has been said above on the extrapolation of the curve may possibly hold very well for a curve with not too many ups and downs. It quite loses its validity when we have to do with a zigzag line. And the kinetic conception exactly assumes in general such a zigzag course of the different macroscopic quantities — this course being always compared with one that would be obtained by means of a *lottery drawing*.

This objection brings into prominence a characteristic of the kinetic theory which does not belong to its fundamental properties, but to the desperate auxiliary methods of calculation. The foundations of the theory teach us that the course of the processes takes place according to mechanical laws. In order to calculate some results however we suppose that some elements of the calculation may be replaced by certain formulae borrowed from the theory of probability. In every separate case the different intuitive images have to make clear which formulae from this theory must be taken. We may directly add that these formulae give just as regular natural laws as those obtained in the ordinary way, so that until now the *practical* kinetic theory does not show any deviations from the other physical theories.

In *theoretical* discussions however the applicability of the theory of probability is generalized so far as to request that all possibilities must be expected which would occur when at any moment the values of the above mentioned $6N$ microscopic quantities would be determined by means of an ideal lottery.

Now we can immediately give examples which are in contradiction with this demand.

A closed vessel be divided into two by a wall. One part may contain a gas in which MAXWELL's partition law is fulfiled; the other part be empty. At a definite moment a rather large opening be made in the wall. Then it is *impossible that* the second part of the vessel remains always empty in the following time.

It is also excluded, that at an arbitrary moment determined *by lottery* the molecules entered in this latter part will all have crept back through the opening: the *mechanical* laws demand, that there exists an inferior limit for the time which these molecules want to proceed to the walls of the vessels and to return to the opening, this limit being determined by the velocities and their directions of these molecules, by the form and the extension of the vessel.

Intuitively we are inclined to draw still much farther reaching conclusions as to the further distribution of the gas over the two halves of the vessel. On the other hand no one has ever calculated in how far it is mechanically possible that e.g. after a homogeneous distribution such as might result from the above mentioned primary conditions there may occur such a sudden reversion of the velocities that soon afterwards the gas is compressed in a definite corner of the vessel, a case which can hardly be imagined by our fantasy.

As long as similar questions are not solved the last mentioned objection neither decides for or against our way of conception.

§ 5. If our point of view were right, the physical laws accepted just now would at all events prove to be only approximations, which in the course of time would have to make place for other laws, which again would prove to be good approximations for a limited time. This is demanded by Poincaré's law, according to which the course of mechanical processes of a finite system must be quasi-periodic.

Mathematics. — *"On Satellite Points on Curves, Given by the Equations:* $x = at^p$, $y = bt^q$". By Prof. W. A. Versluys. (Communicated by Prof. J. Cardinaal).

(Communicated in the meeting of September 29, 1917).

§ **1.** If p and q are positive integers, having no common factor, q being greater than p, the equations

$$x = at^p \quad , \quad y = bt^q \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

represent for any value of a and b, if t is supposed variable, a curve of order q, which will be indicated as a curve $C(p,q)$.

The curve is completely determined by the point $P(a,b)$, but any other point of the curve, for which the parameter t is not zero or infinite also determines the curve completely.

If a or b or both are given all possible values, the equations (1) represent a pencil of curves of order q. Let the curve of the pencil determined by the point P be the curve $C_P(p,q)$.

§ **2.** The tangent in a point $P(at^p, bt^q)$, to the curve $C_P(p,q)$ has the equation:

$$\frac{x - at^p}{p\,at^{p-1}} = \frac{y - bt^q}{q\,bt^{q-1}} .$$

Let S be a point in which this tangent intersects the curve $C_P(p,q)$ and let vt be the parameter of this point S, v is then a root of the equation:

$$\frac{a\,(vt)^p - at^p}{p\,at^{p-1}} = \frac{b\,(vt)^q - bt^q}{q\,bt^{q-1}} .$$

or after simplification

$$\frac{v^p - 1}{p} = \frac{v^q - 1}{q} . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

This equation is of order q in v, but possesses a double root $v = 1$, so that the $q-2$ remaining roots correspond to $q-2$ intersections or satellite points of P.

It is to be seen at once that the equation (2) possesses at most 3 real roots, if q is odd and 2 real roots at most, if q is even.

If therefore q is odd and v the real root of the equation (2), different from 1, the point with parameter vt is the only real satellite point of P; this will henceforth be indicated as the satellite point S of P.

If q is even and consequently p odd, none of the $q - 2$ roots of the equation (2) different from 1 is real and a point P does not possess real satellite points.

For any imaginary satellite point corresponding to a complex root of equation (2) the same properties apart from reality hold good as for a real satellite point.

In the equation (2), t, a, and b occur no more, the value v, therefore, is independent of t, of a and of b. For any point P on each of the curves $C\,(p, q)$ the parameter of the satellite point is found by multiplying the parameter t of P by the same number v, which is a root of equation (2) different from 1.

§ **3.** Let l be an arbitrary straight line:

$$Ax + By + C = 0,$$

and P_i (x_i, y_i) an arbitrary point of l.

The satellitepoint S_i of P_i has the co-ordinates:

$$x_i v^p \quad \text{and} \quad y_i v^q.$$

and lies therefore on the straight line:

$$\frac{A}{v^p}x + \frac{B}{v^q}y + C = 0.$$

This straight line l' is called the satellite of l.

If P_i describes the straight line l, S_i describes l' and conversely. And as to each point P_i corresponds *one* point S_i and conversely, the straight lines $P_i\,S_i$ envelop a conic. As both the x-axis and the y-axis, as well as the line at infinity, are positions of $P_i\,S_i$, we have the proposition:

The tangents in the points P_i of an arbitrary straight line l drawn to the curves $C\,(p, q)$ passing through those points envelop a parabola, touching the axes of co-ordinates, the straight line l and its satellite l'.

The coefficient of direction m of l is $-\frac{A}{B}$; the coefficient of direction m' of l' is: $-\frac{A}{B}\,v^{q-p}$. We have therefore

$$m' = m\,v^{q-p}.$$

Consequently there exists a constant ratio between the coefficients of direction of a straight line and its satellite.

§ **4.** Let S_1 be the intersection of l with its satellite l'; as S_1 is a point of l, the satellite point of S_1 lies on l', and as the satellite point cannot coincide with S_1 (v unequal to 1), l' is the tangent in S_1 to the curve $C(p, q)$ passing through S_1.

The co-ordinates of the intersection S_1 are:

$$-\frac{C\,v^p(v^q-1)}{A(v^q-v^p)}\ ,\quad \frac{C\,v^q(v^p-1)}{B(v^q-v^p)}$$

or, by taking into account that v is a root of the equation (2):

$$-\frac{C\,v^p\,q}{A(q-p)}\ ,\quad \frac{C\,v^q\,p}{B(q-p)}.$$

By comparing the equation of the straight line l with the equation of the tangent in the point P_1 to $C_P(p, q)$, it is found that l touches only in *one* point a curve $C(p, q)$. This point of contact is the point P_1:

$$-\frac{Cq}{A(q-p)}\ ,\quad \frac{Cp}{B(q-p)}.$$

The intersection S_1 is the satellite point of the point P_1, in which l touches a curve $C(p, q)$ and l and l' are consequently tangents to the same curve $C(p, q)$ in the point P_1 and its satellite point S_1.

§ **5.** Of the points in which $C(p, q)$ intersects a straight line l not passing through one of the singular points (O and Y_∞) of $C(p, q)$ 3 are at most real, if q is odd and 2 at most, if q is even.

Let q be odd, and let P_2, P_3 and P_4 be the 3 real intersections of the straight line l with an arbitrary curve $C(p, q)$, their satellite points S_2, S_3 and S_4 lie then according to § 3 on the satellite l' of l. And conversely if the straight line l' intersects the curve $C(p, q)$ in 3 points S_2, S_3 and S_4, the points of which S_2, S_3 and S_4 are the satellite points are on a straight line.

The satellite l'' of l' touches the curve $C(p, q)$, which touches l', i. e., the curve $C(p, q)$ which touches l etc, so that we have:

The straight line l, its satellite l', the satellite l'' of l', the satellite of the latter and so on, are all tangents to the same curve $C(p, q)$.

From the similitude of the series of points P_1 on l with the series of points S_1 on l' ensues:

$$P_1P_2 : P_2P_3 : P_3P_4 = S_1S_2 : S_2S_3 : S_3S_4.$$

The straight line l, the three tangents in the intersections of l with the curve $C(p, q)$, the satellite l^1, the line passing through the two singular points (O and Y_∞) and the tangents in the two singular points touch the same conic.

If q is even and if the straight line l intersects the curve $C(p, q)$ in two real points, the tangents in these points to the curve, the straight line l, and the sides of the triangle of co-ordinates touch the same conic.

If we take $q = 2$, the curve $C(p,q)$ is a parabola of which O and Y_∞ are ordinary points and we have as a special case the well-known proposition:

Let a quadrangle be inscribed in a conic, the tangents in the vertices and a pair of opposite sides touch a conic.

§ **6.** The tangent in the point $P(x_1 y_1)$ to the curve $C_P(p,q)$ has the equation:

$$\frac{x-x_1}{px_1} = \frac{y-y_1}{qy_1}.$$

The points of contact of tangents from a point $L(\lambda, \mu)$, to the curves of the pencil $C(p,q)$ are lying on a conic k_L:

$$\frac{\lambda - x}{px} = \frac{\mu - y}{qy}.$$

or:

$$(q-p)\,xy + p\mu x - q\lambda y = 0.$$

The conic k_L passes through the three vertices of the triangle of co-ordinates and through the point L. And as v is a root of the equation (2) k_L passes also through the point L_1, of which the co-ordinates are:

$$\frac{\lambda}{v^p}\ ,\ \frac{\mu}{v^q}.$$

The curve $C_L(p,q)$ touches k_L in L and intersects k_L in L_1. This point L_1 is the point of the curve $C_L(p,q)$ that has the point L as satellite point.

If the point L is chosen on one of the sides of the triangle of co-ordinates, k_L degenerates into two straight lines. *One* of these straight lines is the side of the triangle of co-ordinates on which L lies, the second straight line passes through the opposite vertex. Of the tangents to a curve $C(p,q)$ that touch in ordinary points, only two or one are real, if k_L degenerates.

§ **7.** Let φ be an arbitrary curve of order n, each intersection Q of φ with k_L not being a vertex of the triangle of co-ordinates possesses then the property that the tangent in Q to $C_Q.(p,q)$ passes through L. If therefore in any point of φ the tangent is drawn to the curve $C(p,q)$ passing through this point, these tangents envelop a curve of class $2n-k$, if k is the number of intersections of φ with k_L that coincide with the vertices of the triangle of coordinates.

If φ is in particular a straight line the tangents to the curves $C(p,q)$ in the points of φ envelop a conic. If the straight line passes through a vertex of the triangle of co-ordinates the tangents to the curves $C(p,q)$ in the points of the straight line pass through *one* point.

If φ is a curve $C(p,q)$ this curve is the envelop of those tangents, and as $C(p,q)$ passes p times through O, and $q-p$ times through Y_∞ and does not touch k_L in these points, $2q-p-(q-p)=q$ intersections fall outside the vertices of the triangle of co-ordinates, and the curve $C(p,q)$ is therefore of class q.

The parameters t of the points Q, in which the curve $C(p,q)$ passing through $P(a,b)$ intersects the conic k_L, i.e. the parameters of the points of contact of the tangents out of $L(\lambda,\mu)$ to $C_P(p,q)$ are the roots of the equation:

$$\frac{q\lambda}{at^p}-\frac{p\mu}{bt^q}=q-p.$$

This equation is in $\frac{1}{t}$ of order q, and it is easy to see that, for q odd three of the roots at most, for q even two of the roots at most, are real.

Consequently we have:

If q is odd three real tangents may at most be drawn out of an arbitrary point to a curve $C(p,q)$. The three points of contact of these tangents lie on a conic passing through the vertices of the triangle of co-ordinates, through L and through L_1, in which L_1 is the point of $C_L(p,q)$ that has L as satellite point.

If q is even, only two real tangents may be drawn out of an arbitrary point L to a curve $C(p,q)$. The points of contact of these two tangents lie on a conic passing through the vertices of the triangle of co-ordinates and through the point L.

§ **8.** The equation of a curve $C(p,q)$ is:

$$y^p=c\,x^q.$$

The first polar of a point $L(\lambda,\mu)$ is:

$$\lambda c q x^{q-1}-p\mu y^{p-1}-(q-p)\,y^p=0,$$

Consequently the points of contact lie on the curve:

$$x\{\lambda c q x^{q-1}-p\mu y^{p-1}-(q-p)\,y^p\}-q\lambda(c x^q-y^p)=0,$$

or after reduction

$$y^{p-1}\{(q-p)\,x\,y+p\mu x-q\lambda y\}=0,$$

so that the points of contact lie on the x-axis, the line at infinity, and on the conic k_L; and as the line at infinity and the x-axis intersect the curve $C(p,q)$ each q times in one of the singular points, which singular points are no points of contact for the arbitrary point L, it has been again proved that the points of contact lie on the conic k_L. As the equation of k_L does not contain the constant c, which determines the curve $C(p,q)$ the points of contact of the tangents from L to the curves of the pencil lie on the conic k_L.

If $q=3$, the first polar is of the second order and passes through the cusp of the cubic. The intersections of the first polar and k_L are the cusp and the three points of contact.

§ **9.** The tangent at the point $P(at^p, bt^q)$ to the curve $C_P(p,q)$ has the equation:

$$\frac{qx}{at^p}-\frac{py}{bt^q}=q-p\,.$$

The pole of this tangent with regard to the conic f:

$$qx^2-py^2=q-p\,,$$

is the point $R\left(\frac{1}{at^p},\frac{1}{bt^q}\right)$.

If P describes the curve $C(p,q)$, the locus of R is a curve $C(p,q)$ too. The reciprocal polars of the curves of the pencil $C(p,q)$ with regard to the conic f are therefore curves of the same pencil $C(p,q)$. The straight lines through $L(\lambda,\mu)$ are the reciprocal polars of the points of a straight line l. The points of contact of the tangents from L to the curves $C(p,q)$, are the reciprocal polars of the tangents at the points of l to the curves $C(p,q)$ and as these points of contact lie on the conic k_L, the tangents in the points of a straight line envelop a conic k_l, as was formerly proved. And as k_L passes through the vertices of the triangle of co-ordinates that is autopolar for f, k_l touches the sides of the triangle of co-ordinates. The conic k_L passes through the point L and through the point L_1, which has L as satellite point. The conic k_l touches therefore the polar lines l and l_1 of L and L_1. As L and L_1 lie on the same curve $C(p,q)$ l and l_1 touch the same curve $C(p,q)$, and as L lies on the tangent in L_1, the point of contact of l_1 lies on l and l_1 is consequently the satellite of l.

As the straight line l, if q is odd, intersects a curve $C(p,q)$ in three real points at most, and if q is even in two real points at most so, if q is odd, three of the tangents from L to a curve $C(p,q)$ are at most real, and only two at most, if q is even.

§ **10.** Let q be odd, and $P_i(x_i, y_i)$ a point of the curve φ of order n:

$$\varphi(x; y) = 0$$

the satellite point S_i $(x_i v^p, y_i v^q)$ will be on the curve φ' of order n

$$\varphi\left(\frac{x}{v^p}, \frac{y}{v^q}\right) = 0.$$

If P_i describes the curve φ, S_i describes the curve φ', hence the proposition:

If a curve $C(p,q)$ intersects an arbitrary curve φ of order n, the satellite points of the intersections lie on a curve of order n. As the transformation of P_i in S_i is an affine transformation, φ' is affine with φ.

If q is odd the satellite points of the points of k_L lie on the conic $k_{L'}$

$$(q-p)\,xy + p\mu v^q x - q\lambda v^p y = 0.$$

The tangents in the points of $k_{L'}$ pass through the point $(\lambda v^p, \mu v^q)$. This point L' is the satellite point of L, and for any curve $C(p,q)$ — (q *odd*) — the propositions hold:

I. If P_1, P_2, P_3 are the points of contact of the tangents from L to a curve $C(p,q)$, the tangents in the satellite points of P_1, P_2, P_3, pass through the satellite point L' of L.

If in the satellite points of the satellite points the tangents to $C(p,q)$ are drawn, they pass again through a point L''; this process can be continued ad infinitum; the points L', L'' etc. lie on $C_L(p,q)$.

II. If P_1, P_2, P_3, are the points of contact of the tangents from L to $C(p,q)$, the three straight lines passing through P_1, P_2 and P_3, which touch $C(p,q)$ outside these points, pass through *one* point viz. L_1.

The Hague, September 1917.

Physiology. — *"A contribution to the physiology of the fresh-water sponges (Spongillidae)"*. By H. VAN TRIGT. (Communicated by Prof. C. A. PEKELHARING).

(Communicated in the meeting of November 24, 1917).

In this paper I propose to give a short preliminary account of the results obtained in the course of an investigation, covering many years, into the chlorophyll, the movement of water (in the canal system) the ingestion of food, and the defecation and excretion in the fresh-water sponges. A detailed account of this investigation will shortly appear in the Tijdschrift der Nederl. Dierkundige Vereeniging. The work was carried out in the Zoötomic Laboratory at Leyden during the lifetime of Professor VOSMAER, and is in part a continuation of his well known treatises "On the ingestion of food by sponges" and "Observations on Sponges" published in collaboration with Professor PEKELHARING in 1898 in the Verhandelingen of this Academy.

A. *The Chlorophyll.*

I. In 1882—1883 there arose polemics between RAY LANKESTER [1]) and BRANDT [2]) on the question whether the chlorophyll, that is found in the amoebocytes of fresh-water sponges in the form of oval corpuscles 2—3 μ large, was of animal origin, so chlorophyll formed by the sponge itself as an inherent part of its cells, or whether it belonged to unicellular algae which would be associated to the sponge in symbiosis. LANKESTER held the first opinion, BRANDT the second. BRANDT's arguments were rather strong, but not so conclusive as to enable him to convince LANKESTER. Nevertheless later-on BRANDT's conclusion is almost generally acknowledged to be exact in literature, although in English handbooks LANKESTER's view is still maintained, as for instance by SOLLAS in the Cambridge Natural History (1906).

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[1]) Quart. Journal of Microsc. Science, 1882.

[2]) Arch. für Anat. u. Physiol., Physiol. Abth., 1882.
Mitth. a. d. Zoolog. Station zu Neapel, IV, 1883.

It was therefore very desirable definitely to settle this point of difference. First of all, however, I had to complete the investigations of Brandt and of Lankester, who had concerned themselves with the identity in a physical sense of the chlorophyll in freshwater sponges with that in plants, but had left the physiological identity out of consideration. I have been able to prove this latter identity by showing experimentally, that the green chlorophyll-corpuscles of the sponges in light produce O_2 and assimilates (oil), but not in the dark. And with regard to the controversy as to the origin of this chlorophyll, I have been able to prove by stronger and more convincing arguments than Brandt furnishes, that his conclusion was indeed right: The chlorophyll-corpuscles in the sponges are composed of protoplasm and chloroplast (also established by Brandt and Lankester); they perhaps contain a nucleus (which Brandt says he was able to show positively, whilst Lankester denies its existence; I do not consider Brandt's proof to be decisive); they are surrounded by a cell-wall, which I was able to make visible by means of plasmolysis (Brandt could not show it with certainty); in cultures the green chlorophyll-corpuscles, when separated from the sponge-tissue, remain normal and alive for 6 months and even longer, and multiply vigorously (Brandt mentions only a survival for 3—4 weeks, whilst their multiplication could not be established); the green chlorophyll-corpuscles of the sponges occur also *free* in nature, where they also multiply (not observed by Brandt); finally the fact, that colourless sponges can be transmuted permanently into the green form by infection with isolated green chlorophyll-corpuscles (not demonstrated by Brandt either; he only mentions — although with incomplete proof — a case of "grafting" the chlorophyll-corpuscles from spongilla on a stentor). These results therefore prove conclusively that the chlorophyll-corpuscles of the fresh-water sponges are *algae* associated to the sponge in "symbiosis".

II. The investigators who hold Brandt's view all agree, that this symbiotic alga belongs to the genus *Chlorella*, e.g. Beyerinck [1]), Oltmanns [2]), Wille [3]). The mode of reproduction of these algae, which I have been able to determine, has however shown me, that in the case of the sponges investigated here, we are by no means dealing with a member of the genus Chorella, but with a form

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[1]) Botanische Zeitung, 1890.

[2]) Morphologie und Biologie der Algen, 1904—05.

[3]) In Engler und Prantl: Natürl. Pflanzenfamilien. Nachträge zu I^2, 1911.

probably very closely related to *Pleurococcus*. For reproduction takes place not at all by "freie Zellbildung" (cell division preceded by free nuclear condition), but by simple vegetative division of the whole mother-cell.

III. Generally speaking the spongillidae are green in the light and colourless, i.e. cream-coloured in the dark (in twilight); whilst green sponges generally become colourless in the dark, and colourless ones green in the light. LANKESTER (l.c.) found that in colourless sponges the otherwise green chlorophyll-corpuscles were colourless; and he then concluded that these colourless corpuscles are either directly transformed by the influence of sunlight into chlorophyll granules or that during their development, by sunlight, instead of yielding the colourless form they pass into the green type. In the same way BRANDT (l.c.) says, when speaking about these colourless corpuscles: "eben so gut wie die Chlorophyllkörper von höheren Pflanzen, können doch aber auch die Chlorophyllkörper von Algen bei mangelhaftem Lichtzutritt blasser werden"; and elsewhere "dass die Chlorophyllkörper der Zoochlorellen ihre grüne Farbe im Dunkeln einbüssen ist selbstverständlich." Both authors, therefore, imagine conformity of the behaviour in the dark of the chlorophyll-corpuscles of spongillidae to that of the chloroplasts of higher plants, and so they explain the above mentioned facts by analogy to the fact known for angiospermae, for instance, that chlorophyll cannot be produced in the dark. This I found to be quite inexact. Want of light is indeed the cause why the colourless sponges remain colourless or the green sponges become colourless in the dark, but for a wholly different and much more complicate reason than BRANDT and LANKESTER think of; as will now be shown.

I established that the isolated green symbiotic algae, cultivated in water, can certainly produce chlorophyll in the dark; whilst I also found, that these green algae, when cultivated in the light and in the dark, alike in poor and in rich organic feeding media, remain alive and normal (green) for months and multiply, but that the isolated colourless algae under similar conditions sooner or later disappear from the culture, and never pass into the green form. It proved therefore quite impossible, that these green sponge-algae should pass into colourless ones, and the colourless into green ones by the combined influence of darkness or light and some or other feeding medium, such as might perhaps have been expected in analogy to the results obtained by BEYERINCK, (l.c.), Artari [1]), Grintzesco [2]), Radais [3]) with various other algae. I established, however, that the green symbiotic algae can become colourless *only by dying*; in order to pass subsequently and gradually from colourless algae with well-defined internal structure through various "stages of dissolution" (colourless ones with a shadowy internal structure, colourless ones without internal structure, and shades of colourless algae) and finally to disappear from the sponge-tissue or from the culture.

After I had stated this, I made analyses of the intrinsic amount of the various (green and colourless) stages of the symbiotic algae in the tissues of a large number of green spongillidae, from the light and of colourless ones from the dark. The results are too numerous to be given here in extenso; briefly, it may be said, that a green sponge in light contains an excess of green living algae and a smaller number of colourless dead algae; on the other hand a colourless sponge in the dark contains an excess of colourless dead ones and a smaller number of green living algae.

I then studied the factors, which regulate the number of symbiotic algae in the sponge-tissues, each one separately (per unit of time and per unit of volume of the sponge). These factors are 6 in number: 1st. The import (*i*) of algae from the water into the sponge, a powerful factor in nature and as active in light as in darkness. 2nd. The export (*e*) of the algae from the sponge into the surrounding water, an uncertain, but probably not important factor. 3rd. The reduction (*r*) of the sponge-tissue to a smaller volume, a factor which in nature only occurs in autumn and then might cause an increased concentration of the algae in the tissue. 4th. The growth (*g*) of the sponge-tissue, which in the long run must lower the concentration of the algae and which in green sponges in the light is more vigorous than in colourless ones in the dark. 5th. The intensity of multiplication (*mu*) of the algae, which in sponges at equal concentration of the algae is much greater in light than in darkness, in light at high concentration greater than at low concentration, but in the dark in both cases pretty well zero. 6th. The mortality (*mo*) of the algae, which in sponges at equal concentration is much greater in the dark than in light, and at low concentration is in the dark less than, but in light as great as that at high concentration.

With the aid of these data concerning the 6 factors we can now

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[1]) Berichte Dt. Botan. Ges., 1902.

[2]) Revue génér. de Botanique, T. 15, 1903.

[3]) C. R. Acad. d. Sc. Paris, T. 130, 1900.

prove: 1^{st}. Why spongillidae in nature must possess in light and in darkness an amount of the different stages (green and colourless) of the symbiotic algae, as we have found by analysis (see above). 2^{nd}. In what way these sponges maintain their "colour" (green or colourless). 3^{rd}. In what way the two "colour" types pass into one another. It would be beyond the scope of this paper to work out these proofs here. I will only remark, that what happens under certain conditions with the number of green algae of a sponge, in other words, how the "colour" of the sponge is affected, depends wholly on the value, which each of the above 6 factors in these conditions assumes in the formula

$$i + mu + r \gtreqless e + g + mo.$$

Thus, if the left side equals the right, then the number of green algae, so the colour of the sponge, remains constant; if the left side is greater, then their number increases and the sponge becomes greener; if it is smaller, then the number decreases and the sponge becomes colourless.

IV. The general conception (of course apart from that of LANKESTER etc.) of the symbiotic relation of fresh-water sponge and alga is, that it is one probably based upon mutual usefulness. So spongilla counts as almost as classic an example of symbiosis, as the lichens. Yet on the mutual relation of "host" and "guest" but very few experiments, and those not conclusive, have been made by BRANDT (l.c.), as BIEDERMANN[1]) rightly observes.

a. I myself have become convinced, by a comparison of the behaviour of the "symbiotic" algae when cultivated (in light) in sponge-tissue and isolated in water, that the "symbiotic" association of sponge and alga offers to the alga more advantage than a free existence in the water. I have been more especially led to this view by comparing the intensity of multiplication of the algae, as a measure of the favourableness of, briefly, the feeding medium, and by comparing the total increase or decrease of their whole culture, as a measure of the favourableness of all the conditions together. That advantage, however, only consists in the fact that the sponge *protects* the alga against foes. The feeding medium on the other hand is in the sponge *certainly not* more favourable to the alga than in the water. Since further we know that the algae are also continually being destroyed inside the sponge, although less so than in the

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[1]) In WINTERSTEIN's Handb. d. Vergleich. Physiologie, II^1, 1911.

water, we are forced to the conclusion, that from the point of view of usefulness to the alga the association with the sponge can *certainly not* be called a symbiosis in the sense of that found in the lichens.

β. Concerning the question of the advantage to the sponge of this association with the alga, I have been able to establish a very large number of facts, too numerous to give here. With the aid of these I came to the following view on this question:

It is either the need of food for the part of the sponge or (and) the "toxic" action of harmful metabolism-products of the sponge (to be considered as a defensive reaction against an intruder), which continually destroys the green "symbiotic" algae in the amoebocytes; and exactly those algae whose power of resistance has been weakened already by some cause or other. All the algae so destroyed serve the sponge as *food*; because this one digests and dissolves them entirely either free in the protoplasm of its amoebocytes or in food-vacuoles, retains the decomposition-products, and builds up its own cell parts with them, for instance oildrops and carbohydrate globules. These oildrops and carbohydrate globules in their turn are the source of the great quantity of energy, which the sponge transforms in the movement of the flagella in the chambers.

At present I cannot give a decision as to the exact significance to the life of the sponge of the oxygen, which the living green algae produce within its tissue in the light. It is possible that this oxygen has a *very great* significance; so great, indeed, that the katabolic phase of metabolism in a green sponge in light takes on this account a quite different course — namely in giving a relatively larger quantity of energy to the sponge — than in the sponge in the dark. Some indications were found for this.

Finally I came to the conclusion that direct transfer of assimilates (carbohydrates, fats, proteins) from the living green algae into the sponge tissue does, most probably, *not* take place at all.

If one now inquires what may really be the "symbiotic" relation between sponge and green alga considered from the point of view of its usefulness to the sponge, this question cannot be answered very well before the above problem of the significance to the sponge of the oxygen produced by the algae has come to solution:

If the significance of the oxygen is in fact so great as was indicated above, then, notwithstanding the fact that the sponge continually destroys and digests the algae in great numbers and in spite of all other phenomena which do not agree very well with symbiosis, we are nevertheless obliged to conclude, that the relation between sponge and green alga, considered from the point of view of the advantage to the sponge, is *indeed* a symbiosis, although by no means so complete as that of the lichens.

If on the other hand the significance of the oxygen produced by the alga is only of subsidiary importance, then, whatever may be the real cause of the death of the algae in sponge-tissue, whether it be the need of food of the sponge or (and) the "poisoning" of the algae by the products of metabolism of the sponge, in either case we may, indeed we *must* conclude that, practically speaking, this so called symbiotic relation of sponge and alga is really nothing but simply a *nutrition-process* of the sponge, or, if one likes, a very first transition from a nutrition-process into a symbiosis. This holds good in any case for a sponge in the dark.

For I have established the following:

The sponge continually imports green algae from the surrounding water into its amoebocytes; here those algae — this should be explicitly stated — are killed and digested by the sponge only for a part, when circumstances are favourable, while the rest can live on, photosynthesise and multiply. (And will give to the sponge-tissue their oxygen formed in light — the *only* argument that can be brought up in favour of the conception of symbiosis!) This favourable case is only realized in sponges growing in light, and then not even always. When circumstances are however somewhat less favourable — as is the rule with sponges in darkness, and as sometimes also happens when they are in light — then all imported algae (and all that might be present already) are continually and unavoidably destroyed and digested by the sponge.

γ. Instead of being considered as a classic example of symbiosis in the sense of the mutualism of the lichens, the association of sponge and alga should therefore be called *at most a transition from a nutrition-process* (of the sponge) *into a still wholly imperfect symbiosis.*

B. *The Movement of Water through the Canal System.*

I have discovered a method, which makes it possible to observe for hours *wholly intact normally living* sponge-tissue with an oil-immersion, on many consecutive days. It was by the aid of these living microscopic preparations that I was able to determine the mode of movement of the flagella of the choanocytes in the flagellated chambers, that is the actual cause of the water-current in the canals, as well as the way in which the food-particles in the sponge are captured and the manner in which defecation and excretion take place (see below).

As to the cause of the water-current the investigation and the theory of VOSMAER and PEKELHARING[1]) are now almost generally acknowledged to be decisive. According to these, the motion of the flagella of the choanocytes is irregular, *going to and fro* (as fig. 1*d*), for which reason the water-current in the flagellated chambers is not a regular stream, but a *whirling* movement. In consequence of this motion of the flagella the water-pressure on the inside of the wall of the flagellated chambers is constantly changing, at one time it is higher than outside the chamber, at another time lower. With a rise of pressure the outflow of water through the prosopyles is checked by the choanocytes acting as valves; if on the other hand the pressure is lowered, the water will easily enter the flagellated chambers by these openings between the choanocytes. The sponge must therefore suck up water by its incurrent canals, and this flows out again by the osculum.

I have myself now been able to establish, not only with my normally living microscopic preparations and therefore with choanocytes in wholly intact flagellated chambers, but also with isolated choanocytes in ravel preparations of living tissue: 1st. That the above described flagellar movement is not the normal one, but an abnormal caused by exhaustion. 2nd. That the normal movement takes place in *spiral- or wave-line*, just as occurs in the Flagellates, in the Choanoflagellates for instance; which movement consists in a very rapid succession of waves of small amplitude, passing along the flagellum from the base to the apex; by this means a current of water is generated *straight through the axis of the flagellar spiral* and similarly in the direction from base to apex, whilst the water comes to the base laterally (fig. 1*a*, 2). By exhaustion wholly different flagellar movements arise, with abnormal current of water, which are best illustrated by some drawings (fig. 1*a*—*e*) made directly from the living object. The figure shows the successive stages of movement of the flagellum of an isolated choanocyte, from immediately after the isolation (1*a*) till it came to rest 20 minutes later (1*e*). It may be clearly seen how the movement of the flagellum and the water-current, which immediately after isolation (1*a*) were still normal, have already much degenerated 5 minutes later (1*c*); and that after 15 minutes they take the form which was observed by VOSMAER and PEKELHARING: a relatively slow and irregular beating to and fro of the flagellum *without* waves, whilst the water is moved to and fro

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[1]) Proc. Kon. Acad. v. Wetensch. Amsterdam, 26 Maart 1898.
Verhandelingen Kon. Acad. v. Wetensch. Amsterdam, 2e Sect., VI, 3, 1898.
Arch. f. Anat. u. Physiol., Physiol. Abth., 1898.

and is *no* longer driven away (fig. 1*d*). Still 5 minutes later and all movement has ceased, the flagellum is extended (1*e*). As I have already said, I have also observed this same succession in intact living flagellated chambers, when in consequence of very prolonged experimenting the choanocytes had got into an unfavourable condition.

I should mention, that the isolated choanocyte represented here (fig. 1) did not show a collar; this was therefore evidently wholly contracted. In the intact flagellated chambers the collars were however very plainly visible as long cylinders, which left the flagellum uncovered for only a relatively short apical portion (fig. 2—5).

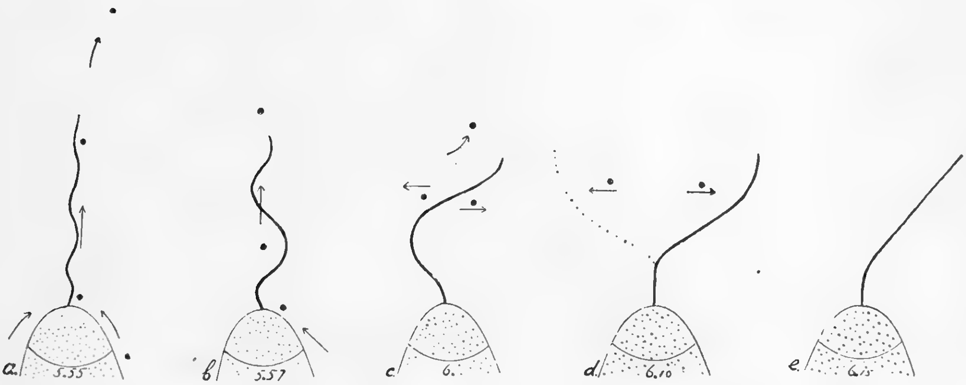

Fig. 1. Successive stages of the flagellar movement of an isolated choanocyte. The cell-body is still connected with several other choanocytes, the collar is retracted. *a* immediately after isolation. The arrows indicate the direction of the water-current, the dots floating particles; the moment of observation is given in each case. At *e* the flagellum has finally come to rest. Magnification about 1770 times.

The whole water-current in a flagellated chamber is of course the resultant of the water-displacements caused by each individual flagellum; it is *rapid and regular*, as can best be made clear by a diagrammatic figure (fig. 2). It is seen, how from the prosopyles (one only is here indicated, but there are 2—5 in one flagellated chamber) the water must flow between the cell-bodies and the collars of the choanocytes, rapidly and regularly, towards the base of the flagella (here really the opening of the collars), to be forced hence by the flagellar movement towards the middle of the flagellated chamber, thence to stream away through the apopyle. In order that a powerful, steady current may be maintained by the chamber and that, therefore, the water may enter rapidly and exclusively at the prosopyles and flow out by the apopyle, the structure of the

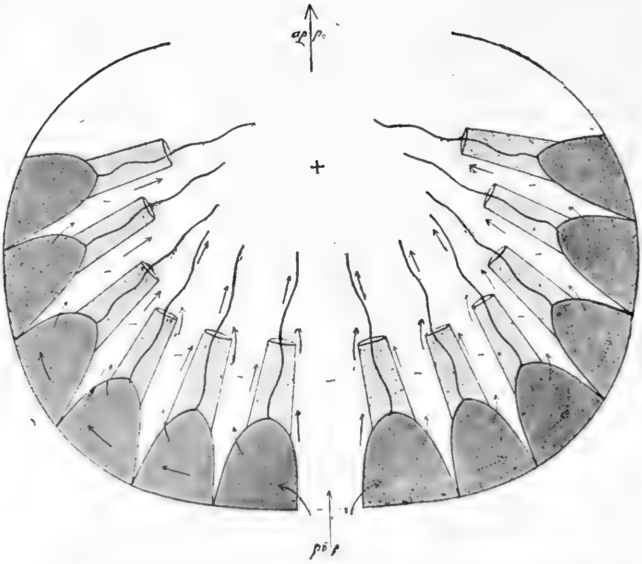

Fig. 2. Diagrammatic representation of the water-current inside a flagellated chamber. pr. p. = prosopyle, ap. p. = apopyle. The arrows indicate the direction of the current; + and — refer to the water-pressure.

flagellated chamber must comply with definite requirements. To discuss these here would take too long.

Also in the remaining portion of the canal-system (outside the flagellated chambers) there are of course a number of subsidiary arrangements, which help to bring about a good circulation of water. I shall not describe these here; more especially, as they have already been dealt with in detail by Vosmaer and Pekelharing, and my results on this point add nothing new.

C. *The Ingestion of Food.*

Most closely connected with the problem of the water-movement is that of the ingestion of food. On this question also there is a fairly general agreement with Vosmaer and Pekelharing (l. c.), whose careful investigation showed more precisely, that the collar cells would be the principal organs by means of which particles suspended in the water are transferred into the tissue of the sponge, and more especially into the cells of the parenchyma. Next these investigators consider that the ingestion of food-particles by cells lining the canals is not excluded, but that it is in any case a subsidiary method. Minchin [1]) holds a somewhat different view, namely that of Metschnikoff [2]).

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[1]) In Lankester's Treatise on Zoology, II, 1900.

[2]) Leçons sur la pathologie comparée de l'inflammation, 1892.

Minchin considers that in the simplest forms of sponges the choanocytes would be the principal "eating organs"; whilst, according as we observe sponges with a more complicate canalsystem, we see this function of ingestion performed more and more by the cells of the canalwall, especially in the case of food-particles too large to be absorbed by the collar cells. It is further self-evident that also the method by which the particles are caught by the collar cells, was conceived in complete agreement with the theory of the water-movement, put forward by Vosmaer and Pekelharing. Thus these investigators say that the flagella, by means of the whirling movement they produce in the water of the flagellated chambers, secure that the food-particles come as much as possible *into* the collars of the choanocytes, where they then can be taken up by the protoplasm.

Now, since I had been able to state in my normally living preparations a mode of motion of the flagella and the water inside the flagellated chambers wholly different from that described by the latter investigators, the way along which the choanocytes take up the food-particles was also bound to prove wholly different. These particles are not at all taken up inside the collars, but rather *outside and between* the collars (especially at their base) or between the bodies of the choanocytes themselves; exactly, therefore, in the way known for

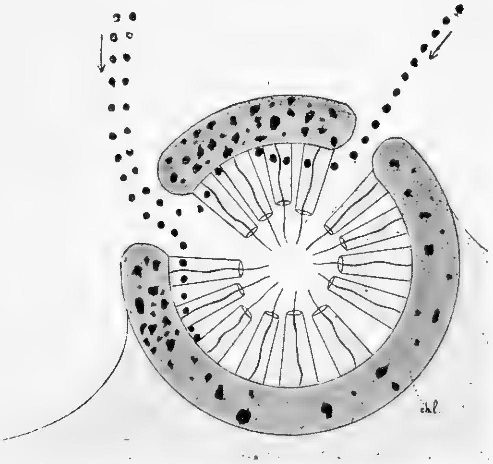

Fig. 3. The capture of (food) particles within a flagellated chamber with 2 prosopyles, namely at the base between the collars of the choanocytes. The path taken by the particles is indicated by dots. The individual cells in the choanocytes-layer (ch.l.) are not drawn; the layer contains numerous particles that have been captured. (Magnification about 1000 times).

Choanoflagellates. We see at once that this must necessarily be the case of fig. 2, the diagrammatic representation of the water-current in a flagellated chamber as the resultant of the smaller currents caused individually by each flagellum. For the bodies and collars of the choanocytes must as it were *filter* the water, circulating between them, *free* from floating particles. A representation of the way followed by the food-particles from an incurrent canal through the prosopyles inside a flagellated chamber is seen in fig. 3; it was drawn from the living object, but diagrammatized as regards the choanocytes-layer. It is seen that the particles — as is generally the case — pass by the layer of choanocytes (evidently this layer is too compact and the open spaces between the cell-bodies do not admit of a vigorous circulation of water). At the level of the bases of the collars the particles however immediately deviate aside, to be soon captured — still at the bases of the collars.

I established that the food-particles are next taken up within the choanocytes, to be fairly soon expelled by these again into the mesogloea; a phenomenon that I have also been able to observe in my living preparations. Finally the particles reach the amoebocytes, which probably have taken them up out of the mesogloea.

In addition to this method of capture by the choanocytes of — what I found to be almost exclusively small — food-particles, I was able to observe yet a second method of ingestion — of coarser particles — not within the flagellated chambers. That this method should exist was indeed to be foreseen. For the width of the ostia in living fresh-water sponges may be, according to my measurements, even 63 × 84 μ, whilst the prosopyles usually only measure 3—4 μ. It is therefore obvious, that with the current of water there will enter by the ostia numerous particles, which are too large to pass the prosopyles. These particles would therefore threaten to stop up permanently all the prosopyles of a sponge, if the latter had no means at its disposal of removing them again.

The sponge is indeed furnished with very efficient means, as I have observed in my living microscopic preparations. It is known, that the choanocytes-layer of the flagellated chambers is covered on the side of the incurrent canal with a thin layer of tissue. When living, this layer proved to me to consist of apparently undifferentiated protoplasm (up to about 3 μ in thickness), and to be simply continued from the lining of the incurrent canal over the flagellated chamber. In it I observed all sorts of corpuscles, such as oil-droplets or food (carmine) particles caught by the choanocytes, being very often slowly carried on by protoplasm current, and so displaced considerable distances (e. g. one quarter of the outer over surface of a flagellated chamber) (fig. 4). Thus it is seen, for instance, that by this current carmine-particles are carried off aside of the flagellated chamber into the parenchyma (fig. 4). It is evident that this layer of streaming plasma leaves the prosopyles uncovered. Now, if there comes with the water-current through the incurrent canal a particle that is too large to pass a prosopyle, and therefore remains sticking in it, nothing is seen to happen in the first 10 minutes; but then a movement arises and the particle is, *outside* along the flagellated chamber — therefore between choanocytes and incurrent canal —, very slowly carried off *aside into the parenchyma*. So this is brought about by the above-mentioned layer of streaming plasma, so that the prosopyle again become accessible (fig. 5). It needs no proof that, if possible, the particles taken up in this way will also serve as food to the sponge.

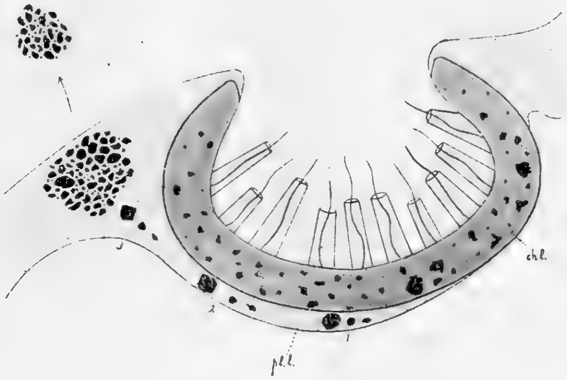

Fig. 4. Semi-diagrammatic representation of a flagellated chamber with the layer of circulating protoplasm (pl.l.) lying against the base of the choanocytes on the side of the incurrent canal. The choanocytes-layer (ch.l.) is drawn as one whole. It lodges numerous captured carmine-particles. Similar particles are carried along in the protoplasm-layer (1—2—3) to a deposit place, from which now and then a large (fecal) conglomerate is extruded. (Magnification about 1000 times).

In any case, therefore, the fresh-water sponge has 2 different methods of taking-up food (possibly even more). Which method is most utilised, will depend, in my opinion, on the size of the particles of food present; seeing, that the ingestion by the collar cells is used especially for smaller particles and that by the plasmic layer for larger ones.

D. *The Defecation and Excretion.*

Very little is as yet known about these phenomena, particularly because observation and hypothesis are so much intermingled in the literature. Masterman [1]) found amoeboid cells loaded with feces (carmine), which cells were on the point of being expelled at the internal or external surface of the sponge, or had already been expelled. Masterman says of these: "We have here an example of a process of intracellular excretion for the removal of waste solids". Cotte [2]) describes defecation by the choanocytes and expulsion of (and by means of) "cellules sphéruleuses".

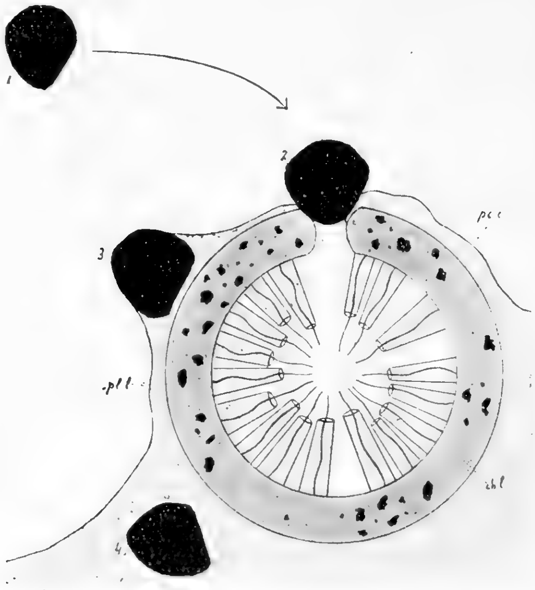

Fig. 5. The capture (2) and the carrying aside (3, 4) of a coarse (food-) particle that remained sticking in the prosopyle (1,2), by means of the layer of streaming plasma (pl.l.). The individual cells of the choanocytes-layer (ch.l.) are not drawn; it contains numerous particles that have been taken up. (Magnification about 1000 times).

I have been able to establish in my living preparations that defecation takes place on a large scale — and probably at the same time also excretion of dissolved matter — by means of *vacuoles* situated in cells of the canal-walls, whilst these cells themselves remain behind in the wall of the canal (fig. 6*a*, *b*). The feces must

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[1]) Ann a. Mag. of Nat. History, Ser. 6 Vol. 13, 1894.

[2]) Bull. scient. d. l. France et d. l. Belgique, T. 38, 1904.

necessarily only be expelled into excurrent canals. The vacuoles, in order to attain this purpose, show a remarkable phenomenon; but for briefness' sake I will not describe it here. These vacuoles can be found, in my opinion, anywhere at arbitrary points of the excurrent canal walls.

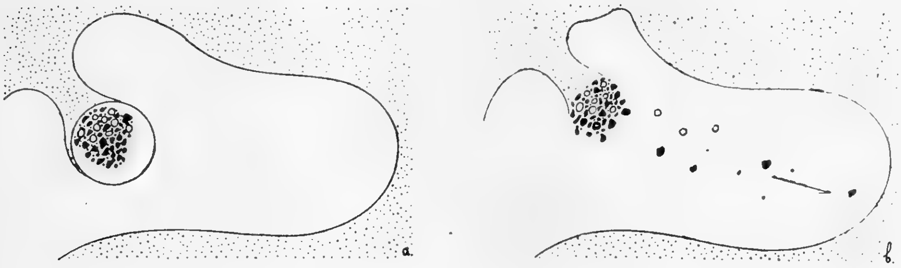

Fig. 6. Defecation and excretion by a vacuole situated in the canal-wall. The cell in which the vacuole occurred, is not drawn. (Magnification about 400 times).

In addition to these defecation-places, definite deposits of feces are to be found very often in the immediate neighbourhood of the apopyle of the flagellated chambers. Whilst on the one hand, as I have pointed out, new feces (carmine for instance) are continuously brought to such a deposit by means of the layer of circulating protoplasm situated on the prosopylar side of the flagellated chamber (fig. 4: 1—2—3), a large fecal conglomerate on the other hand is occasionally seen to be extruded from this place of deposit into the excurrent canal (fig. 4). If we now further consider what was said above concerning the capture and the removal, by this protoplasmic layer, of coarse particles which stop up the prosopyles, we obtain an insight into a (indispensable!) *system of cleansing* of the sponge, which acts very rapidly.

Physics. — *“On the mass of a material system according to the gravitation theory of* Einstein.” By Dr. G. Nordström. (Communicated by Prof. H. A. Lorentz).

(Communicated in the meeting of December 29, 1917).

§ 1. In this paper some formulae will be deduced for the mass of a material system according to Einstein’s gravitation theory. The principal purpose of these formulae is to express the mass firstly by a volume integral over the material system and secondly by a surface integral over a surface surrounding the system.

First I shall indicate in this paragraph the general formulae which will be used further on. The following calculations are principally based upon Einstein’s paper: “Hamiltonsches Prinzip und allgemeine Relativitätstheorie” [1]) (further cited as: Einstein, Hamiltonsches Prinzip). His article: “Die Grundlage der allgemeinen Relativitätstheorie” [2]) (further denoted by Einstein, Grundlage) will also be referred to.

In the first paper Einstein points out that the formulae in his gravitation theory can be deduced from a variation principle of this form:

$$\delta\int\!\!\int\!\!\int\!\!\int(\mathfrak{G}^* + \varkappa\,\mathfrak{M})\,dx_1\,dx_2\,dx_3\,dx_4 = 0, \quad . \;\; . \;\; . \;\; . \;\; . \quad (1)$$

where the first part $\mathfrak{G}^*$ of the integrand refers to the gravitation field and the second part $\varkappa\mathfrak{M}$ to the matter (inclusively the electromagnetic field), $\varkappa$ is the gravitation constant, which in Einstein’s paper has been put equal to 1. $\mathfrak{G}^*$ is a function of $g^{\mu\nu}$ and

$$g_\alpha{}^{\mu\nu} = \frac{\partial g^{\mu\nu}}{\partial x_\alpha}.$$

$\mathfrak{M}$ is a function of $g^{\mu\nu}$ and of several parameters which determine the state of the matter.

The components $\mathfrak{T}_\sigma^\nu$ of the stress-energy-tensor for the matter are represented by the following expression (formula (19) Einstein, Hamiltonsches Prinzip):

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[1]) A. Einstein, Berl. Ber. 1916, p. 1111.

[2]) A. Einstein, Ann. d. Phys. **49**, p. 769, 1916.

$$\mathfrak{T}_\sigma^{\nu} = - \sum_{\mu} \frac{\partial \mathfrak{M}}{\partial g^{\mu\sigma}} g^{\mu\nu} \,. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

According to its behaviour with respect to transformations $\mathfrak{T}$ is a mixed volume tensor, $\mathfrak{M}$ a volume scalar, $\mathfrak{G}^*$ is no volume scalar, but this quantity is formed from the volume scalar $-\sqrt{-g}\,G$ (where G is the *total curvature* of the four-dimensional continuum) by elimination of the second derivative $\frac{\partial^2 g^{\mu\nu}}{\partial x_\alpha \, \partial x_\beta}$ by partial integration. We have[1])

$$-\mathfrak{G}^* = \sqrt{-g}\, G - \sum_{\tau} \frac{\partial \mathfrak{A}_\tau}{\partial x_\tau} , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where $\mathfrak{A}$ is a four-fold vector in the sense given to it in the special theory of relativity of EINSTEIN-MINKOWSKY. $\mathfrak{A}$ is thus covariant for LORENTZ transformations. The sign of $\mathfrak{G}^*$ and for $\mathfrak{M}$ has been chosen in such a way that the expression (2) gives the density of energy of the matter with the right sign. For $-\mathfrak{G}^*$ and $\mathfrak{A}_\tau$ we have the expressions:

$$\left.\begin{aligned} -\mathfrak{G}^* = \sqrt{-g} \sum_{\substack{\alpha\beta\mu\\ \nu\sigma\tau}} g^{\mu\nu} g^{\sigma\alpha} g^{\tau\beta} \left(\begin{bmatrix}\mu\tau\\ \alpha\end{bmatrix} \begin{bmatrix}\nu\sigma\\ \beta\end{bmatrix} - \begin{bmatrix}\mu\nu\\ \alpha\end{bmatrix} \begin{bmatrix}\sigma\tau\\ \beta\end{bmatrix} \right) - \\ - \sum_{\mu\nu\sigma\tau} g^{\sigma\nu} \begin{bmatrix}\mu\nu\\ \sigma\end{bmatrix} \frac{\partial}{\partial x_\tau} (\sqrt{-g}\, g^{\mu\tau}) - \sum_{\mu\nu\sigma\tau} g^{\sigma\tau} \begin{bmatrix}\mu\nu\\ \sigma\end{bmatrix} \frac{\partial}{\partial x_\tau} (\sqrt{-g}\, g^{\mu\nu}), \end{aligned}\right\} . \quad (4)$$

$$\mathfrak{A}_\tau = \sqrt{-g} \sum_{\mu\nu\sigma} (g^{\mu\tau} g^{\nu\sigma} - g^{\sigma\tau} g^{\mu\nu}) \begin{bmatrix}\mu\nu\\ \sigma\end{bmatrix} , \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where the CHRISTOFFEL symbols

$$\begin{bmatrix}\alpha\beta\\ \gamma\end{bmatrix} = \frac{1}{2} \left(\frac{\partial g_{\alpha\gamma}}{\partial x_\beta} + \frac{\partial g_{\beta\gamma}}{\partial x_\alpha} - \frac{\partial g_{\alpha\beta}}{\partial x_\gamma} \right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

are used. According to the equations[2])

$$\frac{\partial \sqrt{-g}}{\partial x_\alpha} = - \frac{1}{2} \sqrt{-g} \sum_{\mu\nu} g_{\mu\nu} \frac{\partial g^{\mu\nu}}{\partial x_\alpha} , \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\frac{\partial g_{\mu\nu}}{\partial x_\alpha} = - \sum_{\sigma\tau} g_{\mu\sigma} g_{\nu\tau} \frac{\partial g^{\sigma\tau}}{\partial x_\alpha} , \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

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[1]) Because of equation (3) and as at the limits of the domain of integration all variations are taken equal to zero, the variation principle (1) is equivalent with the variation principle expressed by the following equation

$$\delta \iiiint (-\sqrt{-g} \,.\, G + \varkappa \, \mathfrak{M}) \, dx_1 \, dx_2 \, dx_3 \, dx_4 = 0 \,, \quad . \quad . \quad (1a)$$

from which equation EINSTEIN originally started.

[2]) EINSTEIN, Grundlage, equations (29) and (32).

we find that $\mathfrak{G}^*$ is a homogeneous quadratic function of the quantities $g_\alpha^{\mu\nu}$, so that we have

$$\sum_{\mu\nu\alpha} \frac{\partial \mathfrak{G}^*}{\partial g^{\mu\nu}_{\alpha}} = 2\,\mathfrak{G}^* \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (9)$$

The stress-energy-components of the gravitation field $\mathfrak{t}_\sigma^\nu$ introduced by Einstein are connected with $\mathfrak{G}^*$ by the formula [1])

$$\varkappa\, \mathfrak{t}_\sigma^\nu = \frac{1}{2}\left(\mathfrak{G}^* \delta_\sigma^\nu - \sum_{\mu\tau} \frac{\partial \mathfrak{G}^*}{\partial g_\nu^{\mu\tau}}\, g_\sigma^{\mu\tau}\right), \quad . \; . \; . \; . \quad (10)$$

where $\delta_\nu^\nu = 1$, $\delta_\sigma^\nu = 0$ for $\sigma \neq \nu$. For the diagonal summation we find because of (9)

$$\varkappa \sum_\nu \mathfrak{t}_\nu^\nu = \mathfrak{G}^*. \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (11)$$

For the diagonal summation of the material stress-energy tensor we have again [2])

$$\varkappa \sum_\nu \mathfrak{T}_\nu^\nu = \sqrt{-g}\, G. \quad . \; . \; . \; . \; . \; . \; . \; . \quad (12)$$

By summation we find taking (3) into account

$$\varkappa \sum_\nu \left(\mathfrak{T}_\nu^\nu + \mathfrak{t}_\nu^\nu\right) = \sum_\tau \frac{\partial \mathfrak{A}_\tau}{\partial x_\tau} \quad . \; . \; . \; . \; . \; . \quad (13)$$

An equation of quite the same form is obtained from the following formula of Einstein [3])

$$\varkappa \left(\mathfrak{T}_\sigma^\nu + \mathfrak{t}_\sigma^\nu\right) = -\sum_{\mu\tau} \frac{\partial}{\partial x_\tau}\left(\frac{\partial \mathfrak{G}^*}{\partial g_\tau^{\mu\sigma}}\, g^{\mu\nu}\right) \quad . \; . \; . \; . \; . \quad (14)$$

We thus find that the four-fold vector $\mathfrak{A}$ and the four-fold vector, the components of which are $-\sum_{\mu\nu} \frac{\partial \mathfrak{G}^*}{\partial g_\tau^{\mu\nu}}\, g^{\mu\nu}$ have the same divergency; the notations "four-fold vector" and "divergency" have here the meaning ascribed to them in the special theory of relativity. From a private correspondence with Einstein I learned that he has proved that these two vectors are really identical, at least when the system of coordinates is thus chosen that $\sqrt{-g} = 1$.

Now all general formulae necessary for the following have been cited. We still remark that not yet anything has been said about the units in which the quantities are expressed. In order to obtain the stress- and energy-density in the desired units it may therefore be necessary to introduce in the expressions (2) and (10) a constant

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[1]) Einstein, Hamiltonsches Prinzip, equation (20).

[2]) See e.g. J. Droste. Het zwaartekrachtsveld van één of meer lichamen volgens de theorie van Einstein. (Diss. Leiden 1916) p. 8 and 12.

[3]) Einstein, Hamiltonsches Prinzip, equation (18).

factor depending on the system of units (comp. the next §, equation (15)).

§ 2. *Energy of a stationary system.*

We shall now consider a material system of finite dimensions and especially one for which there exists (at least) *one system of coordinates in which the gravitation field is stationary.* Let us first consider what must be understood by the mass of the system. The material system having finite dimensions it is evident that its gravitation field may be considered as being caused by a material point, the mass of which has a definite meaning, and all that holds with greater accuracy according as the distance to the system is greater. The best way of defining the mass of the system is based on the properties of the created gravitation field at points at a great distance. According to the theory of relativity however the mass of the system is equal to its total energy when at rest divided by the square of the universal constant c which represents the velocity of light in natural units. If according to our assumption we use a system of coordinates in which the gravitation field is stationary we find for the energy at rest the expression

$$\iiint (\mathfrak{T}_4^4 + \mathfrak{t}_4^4)\, dx_1\, dx_2\, dx_3$$

where the integration has to be extended over the whole three-dimensional space. Possibly a universal constant factor has to be added in order that we may obtain the energy expressed in the desired units (comp. § 1 end). It is easy to see whether this is necessary. First of all we choose the time-coordinate in such a way that *at an infinite distance* g_{44} *gets the value* c^2. Of course the value of the universal constant c depends again on the system of units, which can be chosen thus that $c = 1$. Further we remark, that together with a change of the unit of the time-coordinate the numerical values of $\sqrt{-g}$ and of all $\mathfrak{t}_\mu^\nu$'s changes proportionally to the numerical value of c. The energy having the dimensions ML^2T^{-2}, it is now evident that the factor c must be added to our integral expression in order that it may express the energy independently of the choice of the unit of time in the corresponding unit. We thus have for the energy at rest E:

$$E = c\iiint (\mathfrak{T}_4^4 + \mathfrak{t}_4^4)\, dx_1\, dx_2\, dx_3, \quad . \quad . \quad . \quad . \quad . \quad (15)$$

integrated over the whole three-dimensional space.

This expression gives the total energy at rest for a definite material system when this is the only one within the domain of integration.

Dividing by c^2 we then obtain the mass of the system and in § 4 we shall find that this mass is identical with the one we obtain by considering the gravitation field at points at a very great distance.

As has been said, the integral in (15) must be extended over the whole infinite space. It is however desirable to express the mass of a material system by an integral taken over the material system itself and we shall now show how this can be done. According to a law of v. LAUE we have for the energy E also this expression

$$E = c \iiint \sum_{\nu} (\mathfrak{T}_{\nu}^{\nu} + \mathfrak{t}_{\nu}^{\nu})\, dx_1\, dx_2\, dx_3, \quad . \quad . \quad . \quad . \quad . \quad (16)$$

integrated over the whole three-dimensional space. We subtract this equation from equation (15) after having multiplied the latter by 2. As in a stationary field $g_4^{\mu\tau} = 0$, we have because of (10) and (11)

$$\varkappa\, \mathfrak{t}_4^4 = \tfrac{1}{2}\, \mathfrak{G}^* = \tfrac{1}{2}\, \varkappa \sum_{\nu} \mathfrak{t}_{\nu}^{\nu}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and we obtain

$$E = c \iiint (\mathfrak{T}_4^4 - \mathfrak{T}_1^1 - \mathfrak{T}_2^2 - \mathfrak{T}_3^3)\, dx_1\, dx_2\, dx_3 \quad . \quad . \quad . \quad (18)$$

The integrand being zero at every point outside the material system, the integral here has only to be extended over the material system itself.

By means of formula (18) we have expressed the mass of a material system by a space-integral extended over the material system. This space-integral can again be transformed into a surface integral extended over a surface enclosing the material system. This may be made evident in the following way. From formula (14) we see that $\varkappa (\mathfrak{T}_4^4 + \mathfrak{t}_4^4)$ can be expressed as the divergency of a three-dimensional quasivector $\mathfrak{B}$:

$$\varkappa (\mathfrak{T}_4^4 + \mathfrak{t}_4^4) = \sum_{\tau} \frac{\partial \mathfrak{B}_{\tau}}{\partial x_{\tau}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where

$$\mathfrak{B}_{\tau} = - \sum_{\mu} \frac{\partial \mathfrak{G}^*}{\partial g_{\tau}^{\mu 4}}\, g^{\mu 4}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19a)$$

Multiplying (19) by 2 and subtracting (13) from this product while also (17) is taken into account, we obtain

$$\varkappa (\mathfrak{T}_4^4 - \mathfrak{T}_1^1 - \mathfrak{T}_2^2 - \mathfrak{T}_3^3) = \sum_{\tau} \frac{\partial}{\partial x_{\tau}} (2\, \mathfrak{B}_{\tau} - \mathfrak{A}_{\tau}) = \sum_{\tau} \frac{\partial \mathfrak{C}_{\tau}}{\partial x_{\tau}}. \quad . \quad (20)$$

According to this equation the application of GAUSS's law to (18) gives

$$E = \frac{c}{\varkappa} \int \mathfrak{C}_n\, df. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

integrated over a surface f enclosing the material system. Therefore the mass of the system is also expressed by a surface integral over a surface enclosing the system. Unfortunately the quasivector $\mathfrak{C}$, the normal component $\mathfrak{C}_n$ of which occurs in the integral expression is not covariant, even not with respect to LORENTZ-transformations.

§ 3. *Application to a field with spherical symmetry.*

In our discussion on a system with spherical symmetry we shall principally introduce the same notation as J. DROSTE in his article: Het zwaartekrachtsveld van een of meer lichamen volgens de theorie van EINSTEIN (further cited as: DROSTE, Het zwaartekrachtsveld) chapter II § 1. In contradiction with DROSTE we shall however consider also the field within a material body. Introducing as space-coordinates the polar coordinates r, ϑ, φ we can at any rate represent the line-element ds by the same expression as DROSTE viz.:

$$ds^2 = w^2\, dt^2 - u^2\, dr^2 - v^2\, (d\vartheta^2 + \sin^2 \vartheta\, d\varphi^2), \quad . \quad . \quad . \quad (22)$$

where u, v, w are functions of r only. Here the time-coordinate $x_4 = t$ has thus been chosen that everywhere

$$g_{14} = g_{24} = g_{34} = 0,$$

which is always possible in a stationary field with spherical symmetry [1].

Instead of the polar coordinates r, ϑ, φ we shall now introduce as space-coordinates the corresponding orthogonal coordinates

$$\left.\begin{aligned} x_1 &= r \cos \vartheta \cos \varphi, \\ x_2 &= r \cos \vartheta \sin \varphi, \\ x_3 &= r \sin \vartheta, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

while we keep the same time-coordinate as DROSTE. We put

$$v = r\, p. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

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[1] Because of the spherical symmetry $g_{\vartheta 4}$ and $g_{\varphi 4}$ must be zero. The system of coordinates may however be chosen in such a way that g_{r4} is not zero. We then have

$$ds^2 = w^2\, dt^2 + 2 g_{r4}\, dt\, dr - u^2\, dr^2 - v^2\, (d\vartheta^2 + \sin^2 \vartheta\, d\varphi^2). \quad . \quad (22a)$$

If however the time-coordinate is transformed in the following way, while r is left unchanged:

$$dt = d\bar{t} + \psi(r)\, dr,$$

we obtain

$$ds^2 = w^2 d\bar{t}^2 + 2(g_{r4} + \psi w^2) d\bar{t}\, dr - (u^2 - \psi^2 w^2 - 2\psi g_{r4}) dr^2 - v^2\, (d\vartheta^2 + \sin^2 \vartheta\, d\varphi^2).$$

If now the function $\psi(r)$ is defined thus that

$$g_{r4} + \psi w^2 = 0,$$

g_{r4} will be zero in the new system of coordinates.

for the components of the fundamental tensor we then have the following expressions

$$\left.\begin{aligned} g_{\mu\nu} &= -\frac{x_\mu x_\nu}{r^2}(u^2-p^2) \quad \text{for} \quad \mu \neq \nu, \\ g_{\mu\mu} &= -p^2 - \frac{x_\mu^2}{r^2}(u^2-p^2),\ g_{\mu 4} = 0,\ g_{44} = w^2, \end{aligned}\right\} \quad . \quad . \quad (25)$$

where $\mu,\ \nu = 1,\ 2,\ 3.$

For the components of the contravariant fundamental tensor we have:

$$\left.\begin{aligned} g^{\mu\nu} &= -\frac{x_\mu x_\nu}{r^2}\left(\frac{1}{u^2}-\frac{1}{p^2}\right) \quad \text{for} \quad \mu \neq \nu, \\ g^{\mu\mu} &= -\frac{1}{p^2} - \frac{x_\mu^2}{r^2}\left(\frac{1}{u^2}-\frac{1}{p^2}\right),\ g^{\mu 4} = 0\ g^{44} = \frac{1}{w^2}. \end{aligned}\right\} \quad . \quad . \quad (26)$$

As we consider also the field inside the matter, the material stress-energy-tensor $\mathfrak{T}_\mu^\nu$ occurs now too in our formulae. Because of the spherical symmetry we can write for its components:

$$\left.\begin{aligned} \mathfrak{T}_\mu^\nu &= \frac{x_\mu x_\nu}{r^2}(\mathfrak{T}_r^r - \mathfrak{T}_p^p) \quad \text{for} \quad \mu \neq \nu, \\ \mathfrak{T}_\mu^\mu &= \mathfrak{T}_p^p + \frac{x_\mu^2}{r^2}(\mathfrak{T}_r^r - \mathfrak{T}_p^p),\ \mathfrak{T}_\mu^4 = \mathfrak{T}_4^\mu = 0, \\ \mu,\ \nu &= 1, 2, 3. \end{aligned}\right\} \quad . \quad . \quad (27)$$

That here $\mathfrak{T}_\mu^4 = \mathfrak{T}_4^\mu = 0$ rests on our assumption that the energy of the system remains constant. No radial energy-current can exist then.

Now we shall deduce formulae for the gravitation field from the variation principle of the form (1*a*). We chose this form of the variation principle with a view to a better correspondence with the article of J. DROSTE.

By a right choice of the limits of integration the equation (1*a*) becomes:

$$4\pi\,\delta\int_{t_1}^{t_2} dt \int_{r_1}^{r_2} (-\sqrt{-g}\,G + \varkappa\,\mathfrak{M})\, r^2\, dr = 0$$

or by division by $4\pi\,(t_1 - t_2)$

$$\delta\int_{r_1}^{r_2} \sqrt{-g}\,G\, r^2\, dr = \varkappa\,\delta\int_{r_1}^{r_2} \mathfrak{M}\, r^2\, dr \quad . \quad . \quad . \quad . \quad . \quad (28)$$

The integral on the lefthand side, which multiplied by 4π evidently gives the space-integral $\int \sqrt{-g}\, G\, dV$ over an empty spherical space V, has been calculated by DROSTE. He used polar coordinates, but, the integral multiplied by $4\pi\,(t_2 - t_1)$ giving a scalar, the result is independent of the choice of the space-coordinates. First DROSTE finds for G, which evidently is also a scalar, (see DROSTE, "Het zwaartekrachtsveld" p. 16)

$$G = \frac{2}{v^2} - \frac{2\,v'^2}{u^2 v^2} - \frac{4\,v'w'}{u^2 vw} - \frac{4\,v''}{u^2\,v} + \frac{4\,u'v'}{u^3 v} - \frac{2\,w''}{u^2 w} + \frac{2\,u'w'}{u^3 w}, \quad . \quad (29)$$

where $u'\ v'\ w'$ are derivatives with respect to r. Further DROSTE finds:

$$\int \sqrt{-g}\, G\, r^2\, dr = 2 \int \left\{ -\frac{d}{dr}\left(\frac{v^2 w' + 2vwv'}{u}\right) + \frac{wv'^2 + 2\,vv'w'}{u} + uw \right\} dr. \quad . \quad (30)$$

All variations being taken zero at the limits $r = r_1$ and $r = r_2$, we have

$$\delta \int_{r_1}^{r_2} \sqrt{-g}\, G\, r^2\, dr = 2\,\delta \int_{r_1}^{r_2} \left\{ \frac{wv'^2 + 2\,v\,v'w'}{u} + uw \right\} dr\,. \quad (31)$$

This is now our expression for the lefthand side of equation (28). Now we must consider the righthand side of this equation, and we shall begin by proving the following relations:

$$\left.\begin{aligned} \sum_{\mu\nu} \frac{\partial \mathfrak{M}}{\partial g^{\mu\nu}} \frac{\partial g^{\mu\nu}}{\partial u} &= \frac{2}{u}\, \mathfrak{T}_r^{\,r}, \\ \sum_{\mu\nu} \frac{\partial \mathfrak{M}}{\partial g^{\mu\nu}} \frac{\partial g^{\mu\nu}}{\partial v} &= \frac{4}{v}\, \mathfrak{T}_p^{\,p}, \\ \sum_{\mu\nu} \frac{\partial \mathfrak{M}}{\partial g^{\mu\nu}} \frac{\partial g^{\mu\nu}}{\partial w} &= \frac{2}{w}\, \mathfrak{T}_4^{\,4}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

where $\mathfrak{T}^r_r$ and $\mathfrak{T}_p{}^p$ are connected with the tensor $\mathfrak{T}$ in the way indicated by the equations (27). In order to prove the validity of the equations (32), we first remark that because of the spherical symmetry both the lefthand and the righthand side depend on r only. If the equations hold for an arbitrary point on the X_1-axis ($x_1 = r$, $x_2 = x_3 = 0$), they are always valid.

According to (26) and (27) we have for points on the X_1-axis:

$$g^{11} = -\frac{1}{u^2}, \quad g^{22} = g^{33} = -\frac{1}{p^2} = -\frac{r^2}{v^2}, \quad g^{44} = \frac{1}{w^2}, \qquad (33)$$

$$\mathfrak{T}_1^{\,1} = \mathfrak{T}_r^{\,r}, \quad \mathfrak{T}_2^{\,2} = \mathfrak{T}_3^{\,3} = \mathfrak{T}_p^{\,p} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

All quantities $g^{\mu\nu}$ and $\mathfrak{T}_{\mu}^{\nu}$ for which $\mu \neq \nu$ are equal to zero. Consequently we have for points on the X_1-axis

$$\frac{\partial g^{11}}{\partial u} = \frac{2}{u^3}, \quad \frac{\partial g^{22}}{\partial v} = \frac{\partial g^{33}}{\partial v} = \frac{2r^2}{v^3}, \quad \frac{\partial g^{44}}{\partial w} = -\frac{2}{w^3}; \quad . \quad . \quad (35)$$

the other derivatives of $g^{\mu\nu}$ with respect to u, v, w are zero. According to the formulae (33) equation (2) gives

$$\mathfrak{T}_1^{\,1} = -\frac{\partial \mathfrak{M}}{\partial g^{11}} g^{11} = \frac{\partial \mathfrak{M}}{\partial g^{11}} \frac{1}{u^2}, \quad \mathfrak{T}_2^{\,2} = -\frac{\partial \mathfrak{M}}{\partial g^{22}} g^{22} = \frac{\partial \mathfrak{M}}{\partial g^{22}} \frac{r^2}{v^2} \, (= \mathfrak{T}_3^{\,3}),$$

further we have because of (35)

$$\sum_{\mu\nu} \frac{\partial \mathfrak{M}}{\partial g^{\mu\nu}} \frac{\partial g^{\mu\nu}}{\partial u} = \frac{\partial \mathfrak{M}}{\partial g^{11}} \frac{2}{u^3}, \quad \sum_{\mu\nu} \frac{\partial \mathfrak{M}}{\partial g^{\mu\nu}} \frac{\partial g^{\mu\nu}}{\partial v} = \left(\frac{\partial \mathfrak{M}}{\partial g^{22}} + \frac{\partial \mathfrak{M}}{\partial g^{33}} \right) \frac{2r^2}{v^3}.$$

All these equations hold for points on the X_1-axis and consequently the two first equations (32) are valid for these points. The general validity of the equations follows from the above. The proof for the third formula (32) is given in the same way; this latter proof directly holds for points not on the X_1-axis, as everywhere $g^{14} = g^{24} = g^{34} = 0$.

Because of the equations (32) the righthand side of (28) can be written in the form

$$\varkappa \delta \int_{r_1}^{r_2} \mathfrak{M} \, r^2 \, dr = 2\varkappa \int_{r_1}^{r_2} \left(\mathfrak{T}_r^{\,r} \frac{\delta u}{u} + 2 \, \mathfrak{T}_p^{\,p} \frac{\delta v}{v} + \mathfrak{T}_4^{\,4} \frac{\delta w}{w} \right) r^2 \, dr. \quad . \quad (36)$$

Introducing the expressions (31) and (36) for both sides of equation (28) and dividing by 2 the variation principle for a field with spherical symmetry finally becomes

$$\delta \int_{r_1}^{r_2} \left\{ \frac{w v'^2 + v v' w'}{u} + u w \right\} dr = \varkappa \int_{r_1}^{r_2} \left(\mathfrak{T}_r^{\,r} \frac{\delta u}{u} + 2 \, \mathfrak{T}_p^{\,p} \frac{\delta v}{v} + \mathfrak{T}_4^{\,4} \frac{\delta w}{w} \right) r^2 dr. \, (37)$$

As the variations $\delta u, \delta v, \delta w$ are independent of each other, and as u, v, w, v', w' are not varied at the limits r_1 and r_2, we find (comp. Droste, Het zwaartekrachtsveld, equations (24) which hold for the field outside the matter)

$$\left. \begin{aligned} -\frac{w v'^2 + 2 v v' w'}{u^2} + w &= \frac{r^2}{u} \varkappa \, \mathfrak{T}_r^{\,r}, \\ -\frac{w v'' + v' w' + v w''}{u} + (v w' + w v') \frac{u'}{u^2} &= \frac{r^2}{v} \varkappa \, \mathfrak{T}_p^{\,p} \\ -\frac{2 v v'' + v'^2}{u} + u + 2 v v' \frac{u'}{u^2} &= \frac{r^2}{w} \varkappa \mathfrak{T}_4^{\,4} \end{aligned} \right\} \quad . \quad . \quad (38)$$

These equations are the fundamental formulae for a gravitation field with spherical symmetry. We can easily deduce from it the following equation

$$r^2\left(\frac{u'}{u}\mathfrak{T}_r^r + 2\frac{v'}{v}\mathfrak{T}_p^p + \frac{w'}{w}\mathfrak{T}_4^4\right) = \frac{d}{dr}(r^2\,\mathfrak{T}_r^r), \quad . \; . \; . \quad (39)$$

which can also be found immediately by applying formula (22) of EINSTEIN (Hamiltonsches Prinzip) to our case. Formula (39) expresses that the spherically symmetrical material system is in equilibrium when the gravitation is taken into consideration[1]).

Starting from equation (18) we shall now deduce a formula for the energy and the mass of the system. We put

$$\Psi = \mathfrak{T}_4^4 - \mathfrak{T}_1^1 - \mathfrak{T}_2^2 - \mathfrak{T}_3^3 = \mathfrak{T}_4^4 - \mathfrak{T}_r^r - 2\mathfrak{T}_p^p \quad . \; . \quad (40)$$

and calculate $r^2\varkappa\Psi$. Putting for $r^2\varkappa\mathfrak{T}_4^4$, $r^2\varkappa\mathfrak{T}_r^r$, $r^2\varkappa\mathfrak{T}_p^p$ the expressions following from (38), we find that most terms neutralize each other and we obtain

$$r^2\,\varkappa\,\Psi = 4\frac{v\,v'w'}{u} + 2\frac{v^2w''}{u} - \frac{2\,v^2\,w'u'}{u^2},$$

$$r^2\,\varkappa\,\Psi = 2\frac{d}{dr}\left(\frac{v^2\,w'}{u}\right) = 2\frac{d}{dr}\left(\frac{r^2\,p^2\,w'}{u}\right) \quad . \; . \; . \; . \quad (41)$$

Outside the material system is $\Psi = 0$ and we thus have for $r > R$ (R being the radius of the body)

$$2r^2\frac{p^2\,w'}{u} = constant \qquad (r > R) \quad . \; . \; . \; . \; . \quad (42)$$

The meaning of the constant will be examined later on.

Equation (41) suggests a connexion with our former equation (20) and we shall directly see that this really exists.

Excluding the theoretically possible case that $\dfrac{p^2w'}{u}$ is ∞ at the centre of the system we find by integration of (41) from $r = 0$ to an arbitrary upper limit r

$$\int_0^r r^2\,\varkappa\,\Psi\,dr = 2\,r^2\frac{p^2\,w'}{u}.$$

---

[1]) If we put

$$u = w = 1 \quad , \quad v = r,$$

viz. if we neglect the gravitation (39) becomes

$$2r\,\mathfrak{T}_p^p = \frac{d}{dr}(r^2\,\mathfrak{T}_r^r),$$

which equation expresses the equilibrium between the ponderomotoric forces given by the stress-tensor $\mathfrak{T}$ for the case that there is no gravitation.

For the volume integral $\int \varkappa \Psi d\, V$ over a spherical space with radius r we obtain

$$4\,\pi\,\varkappa \int_0^r r^2\; \Psi\; dr = 4\,\pi\, r^2 \,\frac{2\, p^2\, w'}{u}.$$

If we integrate over the same sphere and apply the law of GAUSS, equation (20) gives again

$$4\,\pi\,\varkappa \int_0^r r^2\; \Psi\, dr = 4\,\pi\, r^2\, \mathfrak{C}_r,$$

where $\mathfrak{C}_r$ is the component of the quasi-vector $\mathfrak{C}$ directed radially outward. In consequence of the spherical symmetry there does not exist a component of $\mathfrak{C}$ perpendicular to the radius. Thus we have

$$\mathfrak{C}_r = \frac{2\, p^2\, w'}{u} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (43)$$

In our orthogonal system of coordinates we have as component in the direction of the X_τ-axis

$$\mathfrak{C}_\tau = \frac{x_\tau}{r}\,\frac{2\, p^2\, w'}{u} \qquad \tau = 1, 2, 3 \quad . \; . \; . \; . \quad (44)$$

Combined with our former formula (18) or with (21) our last formulae give also an expression for the total energy at rest and for the mass of the system. Taking r greater than the radius R of the material body we obtain

$$\varkappa\, E = 4\,\pi\,\varkappa c \int_0^r r^2\; \Psi\, dr = 4\,\pi\, c\, r^2\; \mathfrak{C}_r = 4\,\pi\, c\, r^2\, \frac{2\, p^2\, w'}{u},$$

$$E = \frac{4\pi c r^2}{\varkappa}\,\frac{2 p^2\, w'}{u} \qquad (r > R) \quad . \; . \; . \; . \; . \quad (45)$$

This formula expresses the mass of the body by means of the gravitation field outside the body. This shows at the same time the meaning of the constant on the right-hand side of equation (42).

In our considerations of this § we assumed the field to be thus, that there exists at least one system of coordinates in which the field is stationary and to have spherical symmetry; and our formulae hold for such a system of coordinates that has its origin in the centre of symmetry of the material system and that has such a time-coordinate that $g_{14} = g_{24} = g_{34} = 0$. If however there exists one system of coordinates of the above mentioned properties there exists an infinite quantity of such systems of coordinates, and our formulae hold for all these systems. Not alone the directions of the X_1-, X_2-, X_3-axes can be chosen in an infinite number of ways, but we are still free to chose the method of measurement for the length ot the radius vector in space. Without destroying the validity of our formulae we may thus pass from a system of coordinates x_1, x_2, x_3, x_4 to an other one $x'_1\, x'_2\, x_3'\, x_4$ with the same time coordinate, but for which

$$\frac{x_1'}{x_1}=\frac{x_2'}{x_2}=\frac{x_3'}{x_3}=\frac{r'}{r},$$

where $r'=\sqrt{x'^2_1+x'^2_2+x'^2_3}$ is a function of r (comp. DROSTE, Het zwaartekrachtsveld p. 16). For such a transformation of coordinates u, p, w change of course. If therefore we have to calculate u, p, w (which according to (25) determine all $g_{\mu\nu}$'s) we must first fix the system of coordinates. This may e. g. be chosen in this way that everywhere $p=1$ (corresponding to $v=r$ of DROSTE). If then still the unit of time is chosen so that the universal constant c has the value 1 the system of coordinates is determined except as to the directions of the three axes in space, which for spherical symmetry are of no importance. For the thus specially fixed system of coordinates we have outside the body (see DROSTE, Het zwaartekrachtsveld, p. 18)

$$w^2=\frac{1}{u^2}=1-\frac{\alpha}{r} \quad , \quad p=1 \qquad (r>R). \quad . \quad . \quad (46)$$

where α is a constant.

That these formulae are right can easily be verified from the formulae (38); they are also found more directly from more general formulae which will be deduced in a following paper. The constant α must of course be connected with the mass of the body. Formula (45) gives for this relation, c being equal to 1,

$$m=E=\frac{4\pi\,\alpha}{\varkappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46a)$$

In this special system of coordinates we have according to the formulae (25) outside the body [1])

$$\sqrt{-g}=1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46b)$$

Inside the body however this value of $\sqrt{-g}$ need not hold. If

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[1]) This is seen most clearly by considering a point on one of the axes of coordinates. We then find first $\sqrt{-g}=u\,w\,p^2$.

the system of coordinates is fixed by the condition that everywhere $\sqrt{-g} = 1$, then we have $p \neq 1$.

§ 4. *Generalization of the obtained result.*

In the preceding § we have chosen the time-coordinate so that everywhere $g_{14} = g_{24} = g_{34} = 0$. Now we shall show how the formulae (41)— (45) can be generalized, so that they also hold when this condition is not fulfilled. Because of the spherical symmetry we can write

$$g_{\mu 4} = \frac{x_\mu}{r}\, g_{r4}, \qquad \mu = 1, 2, 3, \quad . \; . \; . \; . \; . \; . \quad (47)$$

where g_{r_4} has the same meaning as in formula (22a) of the note on p. 1081. To generalize one formulae to the case $g_{r_4} \neq 0$ we must evidently transform the time-coordinate in the opposite way as in the note on p. 1081. The quantities referring to the original four-dimensional system of coordinates, in which $g_{r_4} = 0$, will now be denoted by letters with a dash over them. The expression of the line-element in polar coordinates from which we start becomes then :

$$\overline{ds}^2 = \overline{w}^2\,\overline{dt}^2 - \overline{u}^2\,\overline{dr}^2 - \overline{p}^2\,\overline{r}^2\,(\overline{d\vartheta}^2 + \sin^2\overline{\vartheta}\,\overline{d\varphi}^2).$$

We transform the time-coordinate by putting

$$\overline{dt} = dt - \psi\,(r)\,dr,$$

while $\overline{r} = r$, $\overline{\vartheta} = \vartheta$, $\overline{\varphi} = \varphi$ are left unchanged.

ds^2 being an invariant, we obtain by substitution

$$ds^2 = \overline{w}\,dt^2 - 2\psi\,\overline{w}^2\,dt\,dr - (\overline{u}^2 - \psi^2\,\overline{w}^2)\,dr^2 - \overline{p}^2\,r^2\,(d\vartheta^2 + \sin^2\vartheta\,d\varphi^2).$$

The components of the fundamental tensor are then transformed according to the formulae

$$w^2 = \overline{w}^2, \quad g_{r_4} = -\psi\,\overline{w}^2, \quad u^2 = \overline{u}^2 - \psi^2\,\overline{w}^2, \quad p^2 = \overline{p}^2.$$

These formulae firstly give

$$\overline{u}^2\,\overline{w}^2\,\overline{p}^4 = (u^2\,w^2 + g_{r4}{}^2)\,p^4.$$

This equation shows that the determinant g of the components $g_{\mu\nu}$ is not changed by our transformation of the time-coordinate. We have namely

$$\overline{u}^2\,\overline{w}^2\,\overline{p}^4 = -\overline{g} \quad , \quad (u^2\,w^2 + g_{r4}{}^2)\,p^4 = -g, \quad . \; . \; . \quad (48)$$

where $\overline{g}$ and g denote the above mentioned determinant for the *orthogonal* system of coordinates (which through the formulae (23) is connected with the polar system of coordinates) before and after the transformation of the time-coordinate. That both members have the meaning we ascribed to them, is evident from the consideration of a point on one of the axes of coordinates.

We shall now transform formula (41). In the original four-dimensional system of coordinates this is after a slight variation

$$\bar{r}^2 \varkappa \bar{\Psi} = 2 \frac{d}{dr} \frac{\bar{r}^2 \bar{p}^4 \bar{w} \bar{w}'}{\sqrt{-\bar{g}}} .$$

Now we shall prove that the lefthand side remains covariant at the transformation of the time-coordinate. As also the righthand side remains invariant, the formula holds in this form also in the four-dimensional system of coordinates. According to (40) we have for every system of coordinates

$$\Psi = 2\ {}_4^4 - \Sigma \mathfrak{T}_\mu^\mu .$$

$\mathfrak{T}$ being a mixed volume-tensor, $\mathfrak{T}_4^4$ is transformed according to this formula:

$$\mathfrak{T}_4^4 = \frac{\sqrt{-g}}{\sqrt{-\bar{g}}} \sum_{\alpha\beta} \frac{\partial x_4}{\partial \bar{x}_\beta} \frac{\partial \bar{x}_\alpha}{\partial x_4} \bar{\mathfrak{T}}_\alpha^\beta . \quad \ldots\ldots \quad (49)$$

If we consider a point on the X_1-axis, then $dx_1 = dr$. At our transformation of the time-coordinate $\frac{\partial \bar{x}_4}{\partial x_4} = 1$ is the only one of all $\frac{\partial \bar{x}_\alpha}{\partial x_4}$ which is not zero. Of all $\frac{\partial x_4}{\partial \bar{x}_\beta}$ only $\frac{\partial x_4}{\partial \bar{x}_4} = 1$ and $\frac{\partial x_4}{\partial \bar{x}_1} = \psi$ are different from zero. As further $g = \bar{g}$ we find

$$\mathfrak{T}_4^4 = \bar{\mathfrak{T}}_4^4 + \psi\, \bar{\mathfrak{T}}_4^1 .$$

It $\bar{\mathfrak{T}}_4^1$ was not zero this would mean that there existed a radial energy-current and the energy of the system would change continually. As we assumed the field to be stationary, we have $\bar{\mathfrak{T}}_4^1 = 0$ and therefore $\mathfrak{T}_4^4 = \bar{\mathfrak{T}}_4^4$. As $\sum_\mu \mathfrak{T}_\mu^\mu$ is a volume-scalar and as the determinant g does not change by the transformation, $\sum_\mu \mathfrak{T}_\mu^\mu$ does not undergo a change by the transformation either. Thus at the transformation Ψ remains invariant.

$$\bar{\Psi} = \Psi .$$

As r, p, w too remain constant, we thus obtain for the new four-dimensional system of coordinates also

$$r^2 \varkappa \Psi = \frac{d}{dr} \left(\frac{r^2 p^4}{\sqrt{-g}} \frac{dw^2}{dr} \right) \quad \ldots\ldots\ldots \quad (41a)$$

We here have found a generalization for formula (41) which also holds when $g_{r4} \neq 0$. It must still be remarked that g_{r4} occurs in the expression for $\sqrt{-g}$ (see the last formula (48)).

The more general formulae for (43), (45) can easily be obtained in the same way as above.

$$\mathfrak{E}_r = \frac{p^4}{\sqrt{-g}} \frac{dw^2}{dr}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43a)$$

$$E = \frac{4\pi c r^2}{\varkappa} \frac{p^4}{\sqrt{-g}} \frac{dw^2}{dr} \qquad (r > R). \quad . \quad . \quad . \quad . \quad (45a)$$

In this § and in the preceding one we have confined our discussion to bodies with spherical symmetry. If we have a body of finite dimensions, which does not possess spherical symmetry, the corresponding gravitation field is different from that belonging to a body of the same mass but with spherical symmetry. We see however, that the greater the distance from the body in question becomes, the more the two fields must become equal. Therefore we can *define* the mass m of a finite material system of arbitrary form by the formula

$$m = \frac{4\pi}{c\varkappa} \lim_{r=\infty} \left(\frac{r^2 p^4}{\sqrt{-g}} \frac{dw^2}{dr} \right) = \frac{4\pi}{c\varkappa} \lim_{r=\infty} \left(\frac{r^2 g_{pp}{}^2}{\sqrt{-g}} \frac{dg_{44}}{dr} \right) \quad . \quad (50)$$

In the last expression we have introduced $-p^2 = g_{pp}$ analogous to the notation in formula (27). In order that formulae (50) may have a definite meaning, the limit on the right-hand side must of course have the same value for any direction in which we move towards the infinite. Formula (50) supposes therefore the system of coordinates to be chosen in such a way that at an infinite distance the field possesses spherical symmetry.

For the case we are considering formula (43a) gives

$$\lim_{r=\infty} r^2 \mathfrak{E}_r = \lim_{r=\infty} \left(\frac{r^2 p^4}{\sqrt{-g}} \frac{dw^2}{dr} \right), \quad . \quad . \quad . \quad . \quad . \quad (51)$$

and as formula (21) in § 2 is also valid for a stationary field, which has no spherical symmetry, this equation gives together with (50)

$$E = c^2 m, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (52)$$

as is demanded by the theory of relativity. Thus we have shown that the calculation of the mass of a stationary system by means of formula (50) from the field at points at a great distance and the calculation of the mass by means of formula (52) from the total energy at rest give the same result also for bodies without spherical symmetry. From our considerations it also follows, that E/c^2 has the same value in every arbitrary system of coordinates in which the field is stationary and possesses spherical symmetry in the infinite. The mass m is thus a scalar.

In a following article the gravitation field for an electrically charged centre will be calculated by application of the result found in this paper for a field with spherical symmetry.

Further it will be proved, that the density of energy of the gravitation field $\mathfrak{t}_4^4$ outside the body is everywhere zero, when the system of coordinates is thus chosen that $\sqrt{-g} = 1$ (or constant).

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CONTENTS.

Meteorology. — *"Periodicity of Winter Temperatures in Western Europe, since A.D. 760"*. By Dr. C. EASTON, Amsterdam. (Communicated by Dr. J. P. VAN DER STOK).

(Communicated in the meeting of February 24, 1917.)

In two previous communications on fluctuations in the solar activity and in the climate ¹) KÖPPEN's historical data on hard winters in Western and Western Middle-Europe ²) formed the climatological foundation. For a closer investigation of a possible periodicity in the climate of Western Europe it seemed desirable to collect these historical data afresh and as completely as possible, to submit them to criticism and then to connect them with the later scientific observations of temperature. In this way it might be possible to obtain over some twelve centuries an uninterrupted (albeit incomplete and only approximate) series of such an important climatological element covering a considerable area. Even if the investigation of the periodicity should prove to be unsuccessful, this material would still retain its value.

Before publishing the full amount of this very extensive material, and before giving the discussion in all its details, — which under the present unfavourable circumstances may require a considerable amount of time — it may be useful to publish a summary of the results which it has yielded.

Historical data. The historical data concerning abnormal winters have been revised from a threefold point of view:

1. Restriction of the area to the West-European climatic province ³),

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¹) Oscillations of solar activity and the climate. Two communications. Proc. Royal Acad. of Sciences, Amsterdam, 1904. Nov. 26, 1905 June 24, Vols. VII, VIII.

²) W. KÖPPEN, "Ueber mehrjahrige Perioden der Witterung. Die strengen Winter Europas u.s.w. *Zeitschrift d. Oesterr. Ges. f. Met. XVI* (1881). (On weather periods extending over many years; the hard winters of Europe, etc.).

³) The borders of the climatic province proper were drawn over Rouen, Angers, Bordeaux, Toulouse, St. Etienne, Berne, Constanz, Stuttgart, Hanover, Hamburg and Helgoland, so that the greater part of France, N. W. Switzerland, Belgium, the Netherlands, and Western Germany are included. In France the borderline of this climatic province approximately follows the "ampl. 15° de la variation annuelle" then the Cevennes and further the ampl. 19° as far as Geneva. (ANGOT, "Climat de la France", T. I. pl. X).

2. Increased accuracy of the material, by collecting all available data concerning the winters in this district and its periphery, with a critical comparison of the data.

3. Consideration of the data on mild winters also, since these were not included in KÖPPEN's material.

So I tried, although restricting myself to the cold season, to obtain for the area mentioned above a survey of the whole course of winter-temperature for a very long interval of time. The data from the earliest centuries are of course defective and very incomplete. A card register and a bibliography were arranged, the latter especially with the purpose to determine as accurately as possible for each historical item the area to which it refers. I took much trouble to use every source of some importance, although some of them were rather inaccessible; so not only rare books such as PILGRAM's "Untersuchungen über das Wahrscheinliche der Wetterkunde", NEIKTER's "Rigidiores Hiemes", or BILLING's "Chronique d'Alsace", have been consulted in the original, but also English sources as BAKER and LOWE, which happened not to be available on the continent, have been studied in the library of the British Museum. Although the collected material may certainly not claim completeness, still, when comparing it with some recent papers, as SPEERSCHNEIDER's [1]), there appears to be every reason to consider the result of my compilation as fairly final in its outlines.

Only in a few cases, when recording the data for more remote times, I went back to the original chronicles. Controlling those innumerable witnesses by means of the original sources might sometimes have resulted in the elimination of errors, but these improvements in details were out of all proportion to the enormous labour involved, and following this course would have indefinitely postponed the completion of the investigation. Moreover the compilations of PILGRAM, PFAFF, SCHNURRER, ARAGO—DE BARRAL, and others seem on the whole reliable, and the data control and complete each

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The border of the periphery runs from the Shetlands over the East of Scotland, follows the Welsh border, runs over Brighton, E. of the Channel Islands over St. Nazaire to St. Sebastian; it contains the N. E. of Spain and the S. of France with the exception of the coastal parts; also the plain of the river Po, and passes over Fiume and Vienna to Gotland, then goes with a bend to the south-west, so that only the S. W. coasts of Sweden and Norway lie within the periphery.

For the periphery the results of this investigation hold only partially.

[1]) C. J. H. SPEERSCHNEIDER. "Om Isforholdelse i Danske Farvande 690 – 1860". *Publik. Dansk Met. Inst.* 1915. (On Ice-conditions in the Danish waters from 690 to 1860).

other. On the other hand it seemed absolutely necessary to restrict the critically examined material as much as possible to a single climatic province. In the older works, e.g. PILGRAM'S and SCHNURRER'S — on which KÖPPEN'S material is largely based — areas have been mixed up (Southern Germany, Austria, Italy, France etc.) which occasionally must yield contradictory results.

The remaining uncertainty in the data so obtained is chiefly caused by the very vague accounts of mild winters, for which there is not, as in the case of the cold ones, an incontestable criterium, e.g. ice-formation, or the blocking up of traffic; and in a lesser degree by the difficulty of determining the character of those winters which deviate but little from the normal standard or present an uncertain character. A closer investigation might only partly obviate this uncertainty. On the other hand, it appeared in the course of this investigation that the data concerning winters with an unmistakably cold or mild character, even for very remote times, are more complete and reliable than might reasonably be expected, so that they constitute a sound basis for investigation, not only because they represent the only available information preceding the thermometric series, but also for their intrinsic value.

After the historical material had been carefully collected and compared I assigned to each abnormal winter a positive or negative coefficient of temperature, ranging from + 5 to — 5[1]), the others being taken as normal and marked zero. As a starting point I took the year 760, i.e. the winter 759—760; previous to the middle of the eighth century the information seemed rather doubtful and too incomplete. From the frequency of the abnormal winters after the 14th century it would follow that, from that time, the data, at any rate those referring to the cold winters, are fairly complete, so that over the last five centuries only very few abnormal winters seem to be lacking in our list. The whole series 760– 1916 contains 1157 years.

This series was then submitted to an inquiry for a possible periodicity, without any presupposition and without starting from solar periodicities, as was done in the former investigation. Four methods were applied:

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[1]) See *Peterm. Mitt.* l.c. 175. These "temperature coefficients" have been assigned to the winters in a similar way as KÖPPEN applied the values for his "hervorragende Winter". I adopted the scale —5 to +5, however. The enumeration of the coefficients assigned to each winter and the classification of the abnormal winters according to their severity, duration and extent, must be reserved for a more elaborate paper.

1. The integration method, recently recommended by W. SCHMIDT (Vienna)[1]) and in its main lines already followed by BUYS BALLOT for his determination of the so-called "surplus value".

This method, when applied to the different groups of our material (cold and mild, cold winters only, severe winters only, etc.) ultimately did not yield more than a rather vague indication of six fluctuations between about 1340 and 1870, hence a period averaging about 90 years.

2. A geometrical method has been applied to the data by Mr. J. W. N. LE HEUX. This interesting investigation yielded as probable result a periodicity of about 85 years, perhaps one of 4 × 85 years, followed by 85-year periods.

3. A graphical method, which may be considered as a simplification of SCHUSTER's periodogram. In this way the intervals 45, 58, 72, 82, 90, 105, 225 were found as possibly periodical; 90 and 225 seeming to have the greatest probability.

4. The harmonic analysis. Notwithstanding the impurity and incompleteness of the material this method too has been tried. With the kind collaboration of Dr. J. P. VAN DER STOK the elaborate calculations were commenced at the Meteorological Observatory at De Bilt. The prospect of success was so little promising, however, that it was decided to stop the work.

Putting everything together, these attempts have not led to a decisive result. It is a remarkable fact that also for the periodicity of sun-spots even FOURIER's method, applied by TURNER, KIMURA and others, has yielded contradictory and for the greater part doubtful results. Have we to conclude that there exists no periodicity in the abnormal winters, or is the terrestrial phenomenon the result of a complex of periods which it is for the present impossible to disentangle — as is the case with the activity of the sun, for which nobody denies the existence of periodicities and the preponderance of a nearly eleven year period? That the fluctuations in terrestrial temperature really reflect, though in a small degree only, the variations in the solar activity, cannot be doubted: this point was settled by the investigations of KÖPPEN, NORDMANN, NEWCOMB, BRAAK and others[2]), and since for the winters of the Western part of Europe

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[1]) Dr. W. SCHMIDT. "Nachweis von Perioden langer Dauer". *Meteor. Zeitschr.* 1911, 9 and 1913, 8. (On the proof of the existence of long periods).

[2]) S. NEWCOMB. *Astrophys. Journal* 1901. A. WOLFER. *Meteor. Zeits.* 1902, *Astron. Mitteilungen* XCIII and following years, *Annals of the Observatory Batavia*. etc. See for a literature index JUL. v. HANN's *Handbuch der Klimatologie*, Bd. I, 3d Ed. (1908), p. 354 ff.

sometimes the influence of the North-Atlantic, in other years that of the Euro-Asiatic action-centre prevails, it is not *a priori* improbable that this correlation would be more strongly reflected in an area with a so unstable atmospheric equilibrum. An approximately 89-year period, which in my former investigation came out most clearly of all, was also found in the present investigation or was at any rate suggested by it. Now there remained one means to try the reality of such a period. If it really exists, that part of the last period comprised between the middle of the 19th century and the present day, (the interval of about 65 years for which the thermometric observations in Western Europe may be considered fairly homogeneous and trustworthy) must fit in with the curve derived from the historical data since 760.

For this test the historical as well as the modern observational material had to be brought into a mutually comparable form.

The historical records between 760 and 1916 comprise 13 periods of 89 years. For each year of the period the "temperature-coefficients" were now combined, half weight being assigned to the first seven periods, before 1383. These coefficients were however at the same time reduced to temperature variations in degrees centigrade; this proved feasible by comparing the "historic" series with the observational series Zwanenburg—Utrecht (de Bilt) from 1734 to the present day, the necessary reductions having been duly applied. Although such a transformation cannot of course give more than a rough estimate for the real temperature variations, still for applying the test before mentioned these values seemed preferable to using the "temperature-coefficients". So we obtain, assuming a 89-year period, an average of thirteen "historical" temperature curves.

Modern series of observations. The period 1852—1916 contains the 25th to 89th year of the latest 89-year period, 1828—1916. As good representatives of the Western European climatic district the stations Paris (St. Maur) and Utrecht—de Bilt were chosen [1]); for meteorological elements I took: 1. the monthly averages over the meteorological winter; 2. the three minimal temperatures observed in each winter but in different months [2]). In

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[1]) My sincere thanks are due to Prof ANGOT of Paris and Prof. VAN EVERDINGEN and Mr. GALLÉ at De Bilt for communicating unpublished observational data and for hints concerning the reduction of the ZWANENBURG series.

[2]) "Meteorological winter" always means the months December, January and February taken together. The winter is named after the year in which the latter two months fall. Often November and March are taken into account with half weight, the result is then reduced to 3 months, however.

order to be able to mix these data together into one series, the probable error of each series was determined and the amount of the deviations for each winter expressed in it, so as to eliminate the arbitrary element involved in the term "abnormality". For each station the two series thus obtained for the values $\frac{e}{d}$ and $\frac{e'}{d'}$ were finally combined and of these numbers the average was taken

TABLE I.

Curve from 13 periods of 89 years, 760—1916.

Year of the Period	Temp.-deviation	Year	Temp.-deviation	Year	Temp.-deviation	Year	Temp.-deviation
1	+ 0.2	24	+ 0.9	47	— 0.6	70	+ 1.2
2	— 1.1	25	+ 0.5	48	— 0.6	71	+ 1.0
3	— 2.0	26	— 0.1	49	+ 0.1	72	+ 0.5
4	— 1.8	27	— 0.7	50	0.0	73	+ 0.1
5	— 1.5	28	— 0.9	51	— 0.4	74	— 0.2
6	— 0.7	29	— 0.5	52	— 0.7	75	— 0.3
7	— 0.4	30	— 0.2	53	— 0.8	76	— 0.4
8	— 0.1	31	+ 0.3	54	— 0.4	77	— 0.2
9	— 0.7	32	+ 0.2	55	+ 0.2	78	+ 0.1
10	— 1.1	33	+ 0.1	56	+ 1.1	79	+ 0.8
11	— 1.1	34	— 0.2	57	+ 1.9	80	+ 0.9
12	— 0.7	35	0.0	58	+ 2.0	81	+ 0.9
13	— 0.3	36	+ 0.3	59	+ 1.1	82	+ 0.3
14	— 0.3	37	+ 0.4	60	— 0.2	83	+ 0.4
15	— 0.2	38	+ 0.3	61	— 1.3	84	+ 0.8
16	— 0.4	39	+ 0.3	62	— 1.4	85	+ 1.4
17	— 0.3	40	+ 0.5	63	— 1.4	86	+ 1.7
18	— 0.3	41	+ 0.4	64	— 1.3	87	+ 1.9
19	— 0.2	42	+ 0.5	65	— 1.2	88	+ 1.8
20	— 0.2	43	+ 0.4	66	— 0.5	89	+ 1.4
21	— 0.2	44	+ 0.6	67	+ 0.1		
22	+ 0.2	45	+ 0.3	68	+ 0.8		
23	+ 0.6	46	— 0.5	69	+ 1.1		

TABLE II.

Winter-temp. Paris—de Bilt combined, 1852—1916.

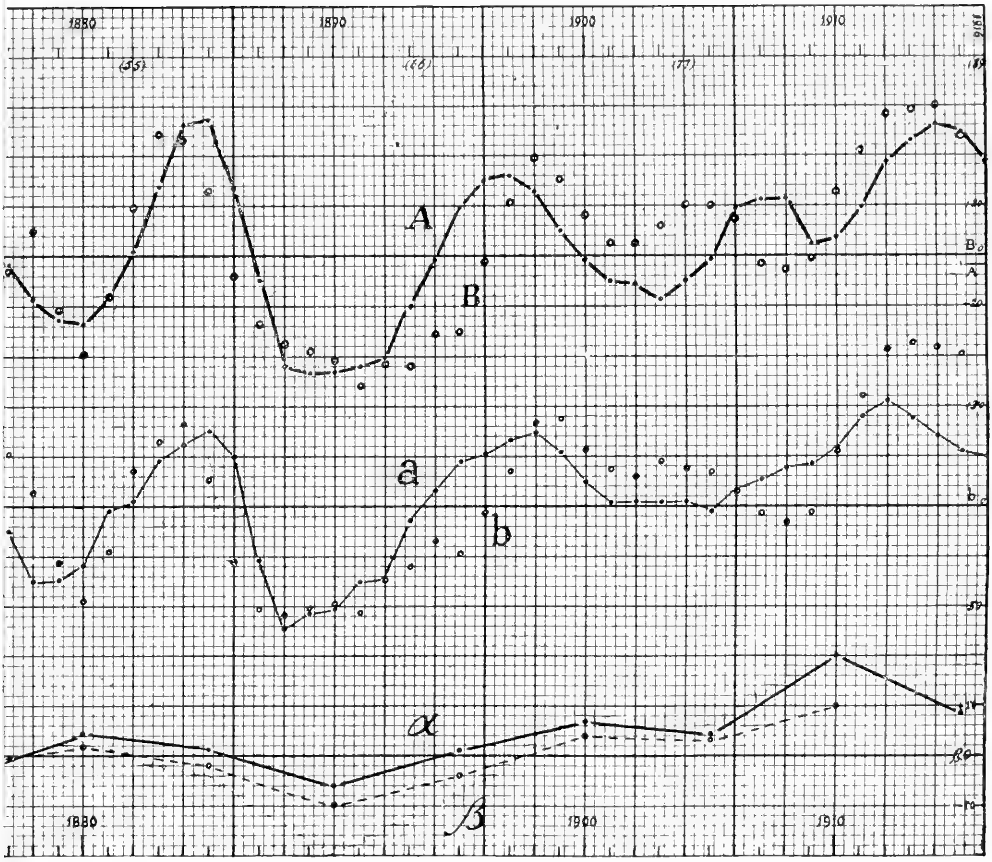
Year of the Period	T. deviation	Year	T. deviation	Year	T. deviation	Year	T. deviation
25	+ 19	42	+ 8	59	— 8	76	+ 12
26	+ 7	43	+ 19	60	— 27	77	+ 20
27	— 21	44	— 27	61	— 35	78	+ 20
28	— 33	45	— 12	62	— 37	79	+ 15
29	— 27	46	+ 10	63	— 41	80	— 3
30	— 12	47	+ 8	64	— 52	81	— 5
31	— 6	48	+ 8	65	— 43	82	— 1
32	— 8	49	— 9	66	— 44	83	+ 26
33	— 19	50	— 6	67	— 31	84	+ 42
34	— 13	51	+ 9	68	— 30	85	+ 57
35	+ 6	52	— 21	69	— 2	86	+ 59
36	+ 11	53	— 39	70	+ 21	87	+ 60
37	+ 8	54	— 18	71	+ 39	88	+(56)
38	+ 6	55	+ 19	72	+ 31	89	+(81)
39	+ 16	56	+ 48	73	+ 16		
40	+ 28	57	+ 46	74	+ 5		
41	+ 26	58	+ 25	75	+ 5		

year for year. It may be assumed that the so obtained thermometric curve is in the main fairly representative of the course of winter temperature from 1852 to 1916 in the area dealt with.

The data for the historical and the thermometric curve, both smoothed by $C = \frac{1}{9}(a + 2b + 3c + 2d + 2e)$, are found in tables I and II.

I first verified whether some neighbouring periodicity (e.g. of 88, 90, $88\frac{1}{2}$ years) could improve the similarity between the two curves; a simple graphical method sufficed for this. The result was negative; only a period of $88\ ^1/_2$ years gave *in part of the curve* a somewhat better result; there appeared nevertheless to be sufficient reason for preferring the 89-year period.

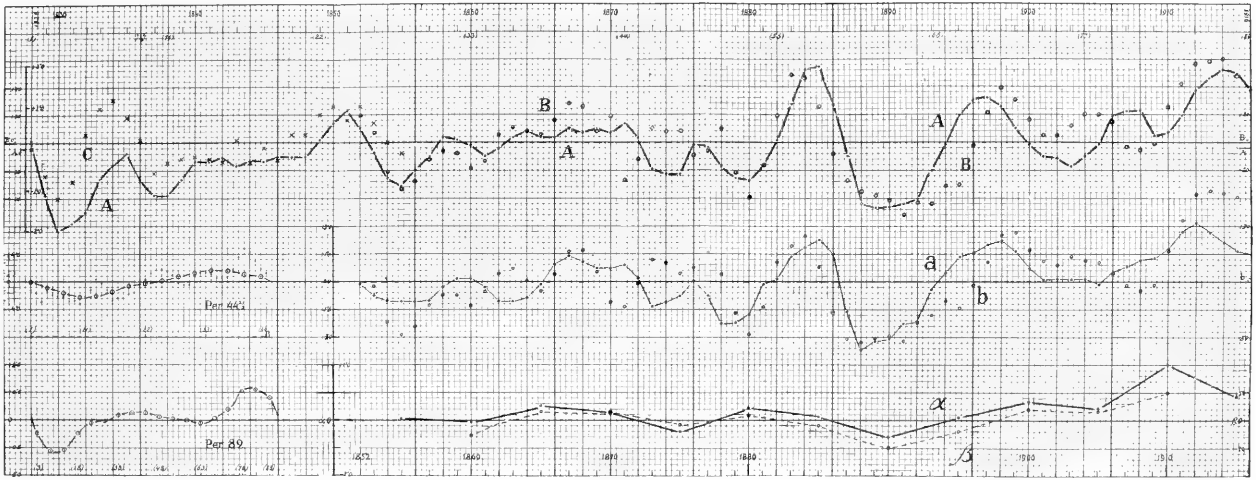
In order to meet the objection that the averaging process used



-1916 taken together.
t combined, 1852—1916.
1855.
-1827, the great deviations only.
the monthly averages only.

:ity of 89 years.
:ity of 44½ years.

C. EASTON, "Periodicity of Winter Temperatures in Western Europe, since the year 760".



A. Historical curve. thirteen periods of 89 years 760—1916 taken together.
B. Thermometrical curve: Paris (S. Maur) and De Bilt combined, 1852—1916.
C. Thermometrical curve: Paris (Observatoire) 1828—1855.
a. Historical curve: twelve periods of 89 years 760—1827, the great deviations only.
b. Thermometrical curve: Paris—De Bilt 1852—1916, the monthly averages only.

α. Historical curve A, smoothed twice.
β. Thermometrical curve B, smoothed twice.

Strongly smoothed curve of a periodicity of 89 years.
Strongly smoothed curve of a periodicity of 44$\frac{1}{2}$ years.

for the thermometric records, or the incorporation of the latest period in the historical data, might have brought about a fictitious agreement between the curves, I proceeded in the following manner. Instead of *all* the "historical" winters only the *very* cold or decidedly mild winters (coeff. $=> 3$ or -3), were used for a new 89-yearly curve, the same weights being assigned to all and the years after 1827 being omitted. These data are the best, of course, but they are scarce (one in 8.7 years). For further comparison another Paris—de Bilt curve was plotted since 1852, now based exclusively on the easily controllable deviations of the monthly means.

These latter curves have been drawn in the middle of the diagram (a and b); the original curves are indicated by A (Table I) and B (Table II), the more strongly smoothed curves by α and β (Table III).

TABLE III.

Comparison of the historical data in the 89-year period with the thermometric data. (Very much smoothed).

89-y. Period	Year of the period	Paris—de Bilt
— 0.02	33	— 0.27
+ 0.27	38	+ 0.29
+ 0.15	43	+ 0.27
— 0.20	48	— 0.18
+ 0.22	53	+ 0.19
+ 0.07	58	— 0.20
— 0.30	63	— 0.98
+ 0.04	68	— 0.41
+ 0.34	73	+ 0.41
+ 0.22	78	+ 0.33
+ 1.00	83	+ 1.00

It seemed important to know approximately the shape of the thermometric curve before 1852. I therefore marked in the diagram the deviations of the monthly means (dev. from the average 1816 — 1860) of Paris-*Observatoire* for 1828—1855, by the line C (little crosses); although this material is decidedly inferior to that used for the line B (C evidently lies too high) it still forms an interesting complement.

For the sake of brevity a, b and C have only been shown in the diagram, not in the tables.

The scale of the diagram — in which the smallest subdivision, somewhat reduced in the reproduction, corresponds to two millimetres — is the following:

For curve A, 89-y. period (full line: 1,5 centimetre for 1°0 C., average deviation per meteor. winter from the mean —0,68. (This average is negative, since the historical data concerning cold winters are much more numerous than those concerning mild winters). The zero-line for the curve A has been derived, however, for better comparison, from the period-years 1852—1916, it is — 0,56 (assumed: — 0,60) and so lies a little above the true average of the 89-year period. Since the sum of all positive and negative deviations of the B-curve (Paris—de Bilt) is 127, while that of the corresponding deviations of the A-curve over these 62 years amounts to 49, the scale-ratio 3 : 1 has been adopted.

For the curves *a* and *b* the zero-line of the b-curve (deviations from monthly means) has of course been taken as the common normal line. The scale is: 1 cm. for 1,5° C. sum of deviations in the meteor. winter. Obviously the values of the curve represent the "historical" deviations very incompletely; besides the unreduced "temp.-coefficients" were used here; 1 t.c. = about 0,5 mm.

For the curves α and β (strongly smoothed means) the scale is 2 cms. for 1°0 C. with β; 1 cm. for 0,5 unit with α. The first dot of the curve erroneously lies one scale-division too low.

The agreement between the two curves, built up from absolutely different materials, certainly does not extend to details, as a preliminary comparison of the separate historical periods had already revealed. Nevertheless this agreement is, I believe, so good as to be somewhat surprising. In both curves not only the dip between the period-years 25 and 31 is the same, also the wave crests in the following years, but during the last 40 years the full-drawn line and the dotted line show the same undulations, allowance being made for a small difference of phase. This similarity is striking where the details have been eliminated, as in the historical and thermometric curves α and β. At any rate it seems to me that the curves are essentially of the same character, so as entirely to preclude an accidental conformity, especially if it is borne in mind that this similarity is lost as soon as the 89-year period is lengthened or shortened by one year.

It is only natural that the agreement between the curves *a* and *b* is less complete than between the others; still I think this agreement is also remarkable enough to confirm the conclusion that the resulting agreement is not imaginary, and hence that the 89-year period has a *real* existence.

On the nature of this 89-year periodicity I will not say much, now that analysis has yielded nothing definite. It is evident that we have no simple curve here. It should be mentioned that Prof. TURNER, after his elaborate investigation of the fluctuations of the sun's activity [1]), obtained as chief periodic terms: (*a*) 8.3, (*b*) 10.2, (*c*) 11.4, and (*d*) 14.7 years, and that $a \times 11 = 91.3$, $b \times 9 = 91.8$, $c \times 8 = 91.2$, $d \times 6 = 88.2$, on the average about 90 or 91 years. The deviation from our 89-year period (the average length of which appears not to be very elastic) would however be large enough to disturb the agreement in the long run, if TURNER's investigations did not extend over a much shorter time than our climatological series. The phenomenon of sun-spots (and only this element of solar activity has been studied for a long time) has only been observed for three centuries, and so incompletely before 1820 that WOLFER assigns to the earlier observations an average weight of only 0.4 as compared with the later ones. The analysis of a much longer series of solar observations would perhaps have yielded periodic terms of a somewhat longer or shorter duration. It should also be noticed that the approximately 11-year period, with the largest amplitude, has by all other observers been found shorter than by TURNER (NEWCOMB, WOLFER and SCHUSTER 11 1/8, R. WOLF and HIRAYAMA 11 1/3, KIMURA 11 1/9). — Now $11\ 1/8 \times 8 = 89$.

Moreover it followed already from my former investigation — and this conclusion is confirmed by the new material — that very likely neither the length nor the amplitude of the periods that may lie hidden in the 89-year climatic periodicity, remain constant. TURNER says of the solar periods found by him: "that their coefficients do not remain constant", only the 11,4 years period being sensible at the present time.

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[1]) From the extensive literature on the periodicity of the solar activity we here only note:

R. WOLF and SPÖRER (*Mem. d. Soc. d. spettrosc. ital.*, X), R. WOLF (*Astr. Nachrichten* 2563), NEWCOMB (*Astrophys. Journal* XIII), A. WOLFER (*Astron. Mitteilungen* XCIII seq.), KIMURA (*Monthly Notices R A.S.* 73), HIRAYAMA (cit. KIMURA), SCHUSTER (*Phil. Trans.* 206, 1906), TURNER (*Monthly Not.* 73, 74), MICHELSON (*Aph. J.* XXXVIII), DOUGLASS (*Aph. J.* XLI).

The conclusion of MICHELSON's investigation with the "harmonic analyser" is interesting: "Indeed it would seem that with the exception of the 11 y.-period and possibly a very long period of the order of 100 years, the many periods found by previous investigators are illusory".

In this respect I should like to refer to a conclusion of my former investigation: (These Proceedings, Aug. 1905, p. 160 ff.) that retardation or acceleration of the sun's activity corresponds to analogous phenomena in the meteorological curve.

The deeper causes underlying both phenomena may be left undiscussed here.

It is worth noticing that I have not been able to discover in my material either BRÜCKNER's well-known period of 34.8 years, nor that of 12.4 years found by KAPTEYN[1]), which does not however prove anything against the reality of these periods.

Leaving theory aside we may now see whether an empirical statistical examination of the material leads to useful results.

Since such a 89-year period cannot but be related to the activity of the sun, which is most apparent in a sun-spot period of about 11 years, the idea suggests itself to subdivide our period into 8 minor periods of $11^1/_8$ years. If we wish to avoid an artificial classification, the varying length of the average solar period of about 11 years should not be taken into account; every interval consequently contains 11 years, the 45th period-year being divided over neighbouring intervals in order to retain integers. Hence each interval should be reckoned at $11^1/_8$ years. (This same classification might have been adopted if no relation between the terrestrial and the solar phenomenon were suggested).

Within each 11 year interval the positive or negative deviations from the average winter temperature were now determined and expressed in degrees centigrade. The result is seen in table IV, where also, in the second and third rows, the corresponding values are given for the intervals of 22 and $44^1/_2$ years.

An examination of these statistics shows that in our material there is no certain indication of a fluctuation corresponding to the eleven year solar period; neither can a longer period than 89 years be recognised.

Most clearly of all a $44^1/_2$-year period manifests itself; the diagram, in which only the full-drawn curve represents the doubly smoothed values (17 years combined; scale 1 cm. = 4°0 C.; all abnormal winters) is given below. (The two curves to the left).

It may further be stated that nothing points to a gradually increasing or decreasing frequency of severe winters in historical

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[1]) E. BRÜCKNER, "Klimaschwankungen seit 1700", Vienna and Olmütz, E. HOLTZEL 1890; J. C. KAPTEYN, "Tree-growth and meteorolog. factors", Receuil des travaux botaniques néerlandais, 1914.

TABLE IV.

Deviations of winter temperature in intervals of 11, 22, 44½ years since 760.

Number of period	89-year per.	Eleven year intervals							
		I	II	III	IV	V	VI	VII	VIII
I	760—848	—19	0	—12	—1	+1	—17	—12	+4
			—19		—13		—16		—8
				—32				—24	
II	849—937	—1	—17	—19	—9	—7	22	—9	—9
			—18		—28		—29		—18
				—46				—47	
III	938—1026	—28	0	—10	—15	—22	—25	—14	—9
			—28		—25		—47		—23
				—53				—70	
IV	1027—1115	—18	—17	—1	—23	—38	—10	—6	—5
			—35		—24		—48		—11
				—59				—59	
V	1116 1204	—23	—15	—15	—29	—14	—12	+11	+8
			—38		—44		—26		+19
				—82				—7	
VI	1205—1293	—41	—30	—13	—14	—19	+2	—21	+17
			—71		—27		—17		—4
				—98				—21	
VII	1294—1382	—5	—32	—32	—2	—13	—17	—21	+1
			—37		—34		—30		—20
				71				—50	
VIII	1383—1471	—23	4	—8	0	—26	—1	—10	—10
			—27		—8		—27		—20
				—35				—47	
IX	1472—1560	—3	—6	+11	—2	—9	+6	—1	+9
			—9		+9		—3		+8
				0				+5	
X	1561—1649	—38	—7	+2	+3	—11	—6	—13	+22
			—45		+5		—17		+9
				—40				—8	
XI	1650—1738	—27	—16	+3	+5	—5	—8	+18	0
			—43		+8		—13		+18
				—35				+5	
XII	1739—1827	—26	—12	—11	+7	—8	—20	—6	+7
			—38		—4		28		+1
				—42				—27	
XIII	1828—1916	—11	—5	—3	—4	—2	—7	—1	+32
			—16		—7		—9		+31
				—23				+22	

times. This has been remarked before (These Proceedings, 1905, p. 193) and it is confirmed by the fuller material; it is in agreement with **JUL. v. HANN**'s conclusion in his "Lehrbuch der Meteorologie" 1915, p. 115, where it is stated that there is no evidence of "fortschreitende Aenderungen der Jahrestemperatur in historischer Zeit." (no progressive change of annual temperature in historical times).

On the other hand I am of opinion that another conclusion of v. **HANN**: "das die Ergebnisse zu Vorausbestimmungen der Witterung vorläufig keine Grundlagen abgeben" (p. 647) (that the results offer no base for weather-predictions as yet) no longer strictly holds.

Let us examine the following table containing only the cold winters since 760, assembled in intervals of $22^1/_4$ years. The two rows at the bottom give respectively the averages for the six last 89-year periods and for all 13 periods (the differences are unimportant). The figures in brackets represent the very cold or hard winters (coeff. 4 or 5).

TABLE V.

Frequency of severe (resp. very hard) winters 760—1916, in 22-year periods.

Period N°.	P. year 1—22	P. year 23 - 45	P. year 45—67	P. year 68 89
I	1 (1)	0 (0)	1 (1)	0 (0)
II	1 (1)	1 (1)	2 (1)	2 (0)
III	2 (1)	1 (0)	3 (1)	1 (0)
IV	3 (1)	2 (0)	2 (2)	1 (0)
V	3 (1)	2 (1)	2 (1)	0 (0)
VI	5 (2)	1 (1)	3 (1)	1 (0)
VII	2 (1)	3 (1)	2 (0)	1 (1)
VIII	3 (0)	2 (1)	3 (2)	2 (1)
IX	1 (0)	2 (1)	2 (0)	1 (0)
X	2 (1)	2 (0)	2 (2)	0 (0)
XI	4 (1)	1 (1)	3 (1)	0 (0)
XII	1 (1)	3 (0)	4 (3)	1 (0)
XIII	2 (1)	2 (0)	2 (2)	1 (0)
VIII—XIII	2.2 (0.7)	2.0 (0.5)	2.7 (1.7)	0.8 (0.2)
I—XIII	2.3 (0.9)	1.7 (0.5)	2.4 (1.3)	0.8 (0.2)

On account of the necessarily automatic classification, the contrast between the relatively cold and the relatively warm intervals in this table certainly is not exaggerated; this is also shown by a comparison with table IV. So, for example, the cold winter 1871 as well as the severe winter 1895 undoubtedly belong to the third (cold) sub-period, although they lie just outside the automatically limited 22-year interval.

Though the true cause of the periodicity lies for the present in the dark, such a table would seem to offer some base for a weather prognosis a long time in advance, at any rate a limited one, somewhat complementary to the weather prognosis from year to year, the prospect of which has been opened by the investigation of Mr. P. H. GALLÉ, recently published in these Proceedings. Perhaps I may on this point quote Prof. J. C. KAPTEYN, who on page 7 of his "Tree-growth and meteorological factors" writes: "What I heard and what I read has long brought me to think that in the present state of science any investigation on long period regularity in the weather must be a purely empirical one. Though the empirical finding out of a regularity may hardly be claimed as a scientific achievement, it may nevertheless become in future of the greatest utility in weather forecast a long time in advance". It may still be noticed that although the nature of the relation between the climatological curve and the activity of the sun cannot yet be explained, such a relation undoubtedly exists, so that the foundation for a prognosis is in our case more solid than it would be for a regularity, discovered by purely empirical methods.

In 1905 I wrote in PETERMANN's Mitteilungen (Vol. 51, Number VIII, p. 176): "zum Beispiel scheint der Schluss auf einen Teilabschnitt mit aussergewöhnlich wenig kalten Wintern, an deren Beginn wir uns jetzt befänden, gewiss gerechtfertigt". (The conclusion seems justified that we are at present at the beginning of a period with an extraordinarily small number of cold winters). Also the diagram in the "Proceedings" of that year shows between ± 1900 and 1913, that an almost total absence of severe winters might be expected (Proc. Acad. Amst. 1904, curves I, C and IIa). Now a great positive temperature surplus has indeed occurred during the last series of winters (see our Diagram).

In the same way, the prognosis for the next 22-year interval (1917 to 1938) would point to a considerable fall of the average winter temperature, with at least two very cold winters, of which one severe.

In what follows the conclusions of our investigation are summarised. These conclusions are chiefly based on table IV, partly on table V and as to Concl. VII also on my former investigation, published in these Proceedings. It should be remembered that all this holds good only for the West European "climatic district" and partly also for its periphery.

CONCLUSIONS.[1]

I. — Within each interval of 44$^1/_2$ years, to begin with A.D. 759.5 (1872.0) the first half is colder than the second.

(The difference in temperature deviations has been found on an average 20° per 44 winters; after the year 1383 on an average 26°.

Exceptions or apparent exceptions to this rule: 2 of 26 cases since 760; none since 1200).

II. — The difference between the temperature deviations of successive intervals of 22$^1/_4$ years, to begin with 759,5 (1894,25) is alternately negative and positive.

(Of 51 cases 6 exceptions or seeming exceptions; no exceptions after the year 1450).

III. — Within each interval of 89 years, to begin with the year 759,5 (1827,5) the first half is colder than the second.

(The difference in temperature deviations has been found to be on an average 22° per 89 winters.

Exceptions to the rule: of 13 cases since 760 two, besides two doubtful ones; since 1116 one exception).

IV. — The difference between the temperature deviations of successive intervals of 44$^1/_2$ years, to begin with 759,5 (1872,0) is alternately negative and positive.

(Of 25 cases 6 (7?) exceptions to this rule, after 1383 two exceptions).

V. — Within each interval of 89 years to begin with 759,5 (1872,5) cold winters prevail in the first quarter, warm winters in the last quarter, relatively to the mean temperature in the whole period.

(Of 26 cases 4 exceptions to this rule; after the year

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[1] Cf: W. Köppen "Anzeichen einer 89 j. Periode" (Indication of an 89 y. Period) *Ann. d. Hydrographic a marit.* (*Met. 1917;* also J. P. van der Stok "Bijdr. Klimaat v. Nederland" (Climate of the Nederlands) in *Tijds. Kon. Ned. Aardrijksk. Genootschap* 1918 n⁰. 3.

1000 two (unimportant) ones), after the 15th century no exceptions. There are indications of a stronger anomaly at the middle of the period, which, however, cannot be proved.

VI. — The chance that the last quarter of a 89-year period (826,25—848,5 . . . 1894,25—1916,5) contains a smaller number of hard winters than the preceding and following 22-year intervals is 0,88. Within the last quarter of a 89-year period the chance that any winter will be severe (resp. very severe) is less than 0,4 (resp. 0,007), i. e. less than $^1/_2$ ($^1/_5$) of the general chance. In the neigbouring 22-year intervals (e. g. 1872—1894 and 1916—1938) this chance is about 3 (5) times as great.

VII. — Increased and accelerated activity of the solar surface corresponds in general to the winter cold in Western Europe setting in more forcibly and rapidly than usual; inversely a weakened and retarded activity of the sun corresponds to winters setting in later and more mildly in a later part of the period.

VIII. — It follows from our material that all the temperature averages used since 1852, until now (e. g. 1852 to 1916) are too high with respect to the true mean. As far as can be judged at present only a 89-year series might give a true average. Per meteorological winter this will lie about 0°,18 below the average which is now available from observations extending over some 65 years.

Astronomy. — "*On the Parallax of some Stellar Clusters.*" (First communication). By Mr. W. J. A. SCHOUTEN. (Communicated by Prof. J. C. KAPTEIJN).

(Communicated in the meeting of January 26, 1918).

With regard to most stellar clusters a direct measurement of the parallax or a determination of their distance from the proper motions of the stars is wholly excluded. We must therefore have recourse to other means. Prof. KAPTEYN has shown in Contrib. Mt. Wilson Observ. N°. 82 how the parallax of stellar clusters can be ascertained from the apparent magnitude, if the luminosity-curve is known and is the same everywhere in space.

If we determine the mean apparent magnitude $\overline{m}$ of all those individual stars of a group of stars, practically at equal distances from the sun, which are brighter than m_0, we shall have — supposing the luminosity-curve to have the form: $\varphi\,(M)\,dM =$
$= \frac{h}{\sqrt{3.41}}\, e^{\,-h^2(M-K)^2}\, dM$ —

$$\overline{m} = \frac{\int_{-\infty}^{m_0} m\, e^{-h^2(m+5-K+5\,log\,\pi)^2}\, dm}{\int_{-\infty}^{m_0} e^{-h^2(m+5-K+5\,log\,\pi)^2}\, dm}$$

or, simplified:

$$\overline{m} = K - 5 - 5\,log\,\pi - \frac{1}{2h}\,\frac{e^{-P^2}}{\int_{-\infty}^{P} e^{-z^2}\, dz},$$

in which $P = h\,(m_0 - K + 5 + 5\ \log\ \pi)$.

If h and K are known π can be determined at once.

It is our intention to investigate how far this method of determining distances is practicable and what results are obtained with some of the best-known stellar clusters. In order to facilitate its practical application we have used in our investigation the method in a somewhat altered form.

In the article of Prof. KAPTEYN which we quoted, a preliminary value for the parallax of the Small Magellanic Cloud was determined by way of example and the prospect of a more accurate determination of it by means of a photo, to be found in the Astronomical

Laboratory at Groningen, was held out. Prof. KAPTEYN had the kindness to charge me with this task. For this and for many a useful hint during my investigation I tender my best thanks.

In order to determine how many stars of every magnitude the stellar cluster contains we diminish the number, given in the catalogue by the normal number for the surface under observation. The latter is calculated with the aid of Publ. Gron. N°. 27, Table IV, which table we could consult, thanks to the kindness of Dr. VAN RHIJN, even before it had been published [1]).

We take as unit the distance corresponding to $\pi = 0''.1$. The relation between the absolute and the apparent magnitude is expressed then by the formula: $M = m + 5 + 5 \log \pi$. As all stars of the cluster are at the same distance, the distribution of the absolute magnitudes is the same as that of the apparent.

We now determine the numbers $\frac{A_{m+1}}{A_m}$ in which A_m represents the number of stars of the magnitude m and see from the luminosity-curve to which proportion $\frac{A_{M+1}}{A_M}$ these numbers correspond. Then we know to which M every m corresponds and we can determine π.

We employ the luminosity-curve given by KAPTEYN in Publ. Gron. N°. 11, Table 6. As we intend to express all magnitudes in the visual Harvard scale the luminosity-curve is also reduced on this scale. We shall find, if we apply a correction Potsdam-Harvard $= + 0^m.17$:

M	$\log A_M$	A_{M+1}/A_M	M	$\log A_M$	A_{M+1}/A_M
— 6.5	4.725	4.09	+ 1.5	9.507	2.56
— 5.5	5.337	5.45	+ 2.5	9.916	2.17
— 4.5	6.073	4.47	+ 3.5	0.253	1.86
— 3.5	6.723	4.17	+ 4.5	0.522	1.56
— 2.5	7.343	3.94	+ 5.5	0.714	1.46
— 1.5	7.939	3.63	+ 6.5	0.878	1.46
— 0.5	8.499	3.40	+ 7.5	1.042	
+ 0.5	9.030	3.00			

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[1]) As we made use of visual magnitudes it would have been better to have based our calculations on Table V of the work mentioned. That table was at that time not yet at our disposal. We have ascertained that this inexactness can nowhere have had any influence on the results obtained.

We assume that the luminosity-curve of **Kapteyn** shows the frequencies of the different absolute magnitudes in the stellar clusters observed. *If* the values that shall be found from the successive values for the parallax, correspond, we may conclude that our hypothesis is true.

Most calculations, wanted for our investigation, had been performed already, when **Shapley** began to publish his interesting "*Studies based on the Colors and Magnitudes in Stellar Clusters*" [1]). In these he had found i.a. remarkable results about the relations between colour and magnitude. If from his researches it were to follow that the distribution of colours in stellar clusters differs from that in the remaining part of space, it is very improbable that we should find then the same luminosity-curve. In order to examine how far this results from what **Shapley** found, we must compare the distribution of colours found by him with that among the stars of the same absolute magnitude in a well-defined part of space. The latter is, however, only imperfectly known. No great value can be put upon investigations about stars with a measured parallax because of the selection of parallax stars. One of the most important contributions towards the solution of this problem was given by **Schwarzschild** in his "*Ueber die Häufigkeit und Leuchtkraft der Sterne von verschiedenem Spektraltypus*" [2]). He made use of the component which is at right angles to the direction towards the apex of the proper motions in Boss' Preliminary General Catalogue. If we enlarge the table, occurring on page 510 of his work, by interpolation and express the numbers in percentages of

M *Type*	+ 5.0	+ 4.0	+ 3.0	+ 2.0	+ 1.0	0.0	— 1.0
B	0	1	2	3	7	12	23
A	5	7	25	32	41	37	29
F	53	48	29	19	9	4	2
G	25	21	11	8	5	5	5
K	17	23	30	33	29	31	30
M	0	0	3	5	9	11	11
Mean colourindex	+ 0.81	+ 0.84	+ 0.81	+ 0.80	+ 0.74	+ 0.75	+ 0.70

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[1]) Contrib. Mt Wilson Observatory Nos. 115, 116, 117 and 126; Proc. Nat. Acad. of Sciences, Vol. I. page 483; Vol. II, page 525; Vol. III, page 25, 267, 276 and 479.

[2]) Sitzungsberichte Akademie Berlin 1914, p. 489—510.

the number of stars of a fixed absolute magnitude, we shall find the foregoing table.

In this table have also been mentioned the mean colour-indices, which we find for every absolute magnitude, if we employ for every type colour-indices used by SHAPLEY. Although the way in which the spectra are divided varies greatly, yet the average colour for the various absolute magnitudes is nearly the same. From this we may conclude, what might also have been understood beforehand, that it is difficult to form an opinion about the division of the spectra from the mean colour-indices.

SHAPLEY's results have been compared, whenever this was possible, with the above table. In large outlines they were found to correspond. To compare them minutely would, however, serve no purpose, because:

1st SCHWARZSCHILD's results have only a preliminary value;

2nd SHAPLEY did not, in various instances, determine the colour-indices for all stars of a certain magnitude and we have no certainty that the stars he made use of, are representative;

3rd we cannot decide which stars do or do not belong to the cluster;

4th SHAPLEY's hypothetical colour-classes are not identical with the usual spectral types or need not be so;

5th our determinations of parallaxes always leave some doubt as to the absolute magnitudes which may even be a couple of magnitudes larger or smaller.

We cannot by this method ascertain with absolute certainty whether from SHAPLEY's observations must be concluded that the distribution of colours in stellar clusters differs from the one in our system of stars. By Prof. KAPTEYN this problem has, however, been solved in another way in an article not yet published. He was so kind as to communicate to me on this subject what follows:

SHAPLEY found that the absolutely bright stars in *Messier* 13 are redder than those that are less bright. Now it is a well-known fact that in our system the white stars are on an average absolutely brighter than the red ones. At first sight these facts seem to contradict each other. But the seeming contradiction disappears if we take into consideration what follows:

Limiting ourselves to a comparison of the luminosity-curves of *K* and *B* stars, we find that the mean absolute brightness of the *K* stars is indeed much inferior to that of the *B* stars, or in other words, that the luminosity-curve of the *K* stars spreads itself around an absolute brightness, which is much inferior to that of the *B* stars; but at the same time that the spread of the K stars is much larger than that of the B stars. In consequence of this circumstance there is a comparatively large number of intrinsic bright K stars and therefore it is possible that for a certain bright M the K stars outnumber the B stars. For such absolute brightness the average colour-index will be large and it may be larger yet than for a fainter absolute magnitude.

In order to work out this problem still further, Prof. KAPTEYN has deduced from Contrib. Mt. Wilson Observ. N°. 82, Publ. Gron. N°. 11 and from an article of KOHLSCHÜTTER on the luminosity-curve of the K stars, unpublished as yet, the number of stars per unit of volume near the sun, of each spectral type between the abs. magn. —0.5 and + 0.5.

If we assume the luminosity-curve to be of the form

$$\varphi(M)\, dM = \frac{h}{\sqrt{3.14}}\, e^{-h^2(M-K)^2}\, dM,$$

Prof. KAPTEYN found that the curves of each type are determined by the following values of the constants K and h.

Spectrum	K	h
Bo—B5	+ 0.5	0.442
B8—A9	+ 3.2	0.800
F	+ 6.7	0.520
G	+ 10.3	0.235
K	+ 14.2	0.191

We shall suppose as a first approximation that there are only A stars and K stars and in concurrence with SEARES, that their mean colour-indices are respectively + 0.2 and + 1.3 [1]. From the data, mentioned above, we now calculate the relative frequencies of each spectrum (in the present instance A and K) for every absolute magnitude and from these we deduct for every M the mean colour-index in the mixture of A and K stars.

These calculations have been performed by Prof. KAPTEYN and the results have been compared with SHAPLEY's results for *Messier* 13. In order to be able to do this, he assumed that the parallax of this

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[1] According to the above investigation of SCHWARZSCHILD the A stars are, if $M = 0.0$, 37% and the K stars 31% of the total number.

stellar cluster is $\pi = 0'',000159$.[1]) We can find now for every apparent magnitude the corresponding absolute magnitude as $M = m - 14.0$.

We can also calculate the mean colour-index for every absolute magnitude, supposing the luminosity-curves in *Messier* 13 to be the same as in our system. These colour-indices have been put down in the third column of the following table. The fourth column contains the colour-indices observed by SHAPLEY.

		Mean Colour-index		
m	M	Computed	Observed	O—C
12.2	— 1.8	+ 1.30	+ 1.28	— 0.02
12.9	— 1.1	+ 1.28	+ 0.86	+ 0.42
13.5	— 0.5	+ 1.12	+ 0.96	— 0.16
14.1	+ 0.1	+ 0.85	+ 0.73	— 0.12
14.7	+ 0.7	+ 0.59	+ 0.65	+ 0.06
15.3	+ 1.3	+ 0.34	+ 0.39	+ 0.05

The agreement between the third and fourth columns is sufficient.

In this way Prof. KAPTEYN has proved not only that we may not conclude from SHAPLEY'S results that the distribution in stellar clusters differs from that in our sidereal system, but also that his observations give support to the hypothesis that in stellar clusters the same frequency of absolute magnitudes exists that we have observed among the stars near the sun.

By HERTZSPRUNG a method has been indicated to determine the parallax of stellar clusters containing δ Cephei variable stars, the period of which is known.[2]) Miss LEAVITT had shown in *Periods of* 25 *variable stars in the Small Magellanic Cloud*, Harvard Circular 173, that there exists a linear relation between the logarithm of their period and their brightness.

The log. of the period increases by about 0.48 per magnitude.

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[1]) This parallax differs from the value $\pi = 0'',00040$, which we found from the catalogues of LUDENDORFF and SHAPLEY, but it is, considering its accuracy, not at variance with it.

[2]) E. HERTZSPRUNG, Ueber die räumliche Verteilung der Veränderlichen vom δ Cephei Typus, A.N. 4692, Bd. 196, 201, 1913.

HERTZSPRUNG calculated the mean period and the theoretical parallax of 13 δ Cephei variable stars, mentioned in Boss' catalogue, and therefore also their absolute magnitudes. With a period of $6^d,6$, M was found to be —2,3 (visual magn.). With these data we can find for every δ Cephei variable star the parallax, if its period and magnitude are known.

In four cases in which such variable stars in the cluster are known, we have indicated which result is found for the parallax by this method.

No great accuracy may be expected from HERTZSPRUNG's method, ingeniously devised though it may be. The scale of magnitudes on which Miss LEAVITT's researches are based, has evidently not been checked very accurately. HERTZSPRUNG's determination of the mean absolute magnitude of δ Cephei variable stars was based on the parallactic movement of only 13 stars. Therefore SHAPLEY [1]) proposed that we should confine ourselves to the variable stars in the clusters that have a short period. As it is very conspicuous that all variable stars in *Messier* 3 and *Messier* 5 have all the same period, SHAPLEY presumes that all δ Cephei stars in the stellar clusters have the same M everywhere. To a period of $0^d.54$, according to HERTZSPRUNG, corresponds, in connection with the law found by Miss LEAVITT, $M = + 1^m.5$ (photogr.). SHAPLEY begins by supposing M to lie between —0,5 and + 1.5 (photogr.). We then can determine the parallax from the mean magnitude of the variable stars.

Our results are made to agree perfectly with those of SHAPLEY, if we take for the absolute magnitudes of the variable stars:

in Messier 3: $M = + 4.2$ (photogr. magn.)
„ Messier 5: $= + 4.0$
„ Messier 13: $= + 3.2$
in the Small Magellanic Cloud: $= + 3.9$

So there is an agreement between the determinations of parallaxes from variable stars and those obtained by the aid of the luminosity-curve, if the mean magnitude of the variable stars in the clusters is fainter by a couple of magnitudes than is assumed by HERTZSPRUNG and SHAPLEY.

By way of example we shall determine in the above manner the parallax of two clusters. For the application of this method to other sidereal clusters we refer to a communication that will follow later on.

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[1]) H. SHAPLEY, A Method for the Determination of the relative Distances of globular Clusters, Proc. Nat. Acad. Vol. III, 479, July 1917.

Messier 3.

N. G. C. 5272; $\alpha_{1900} = 13^h\ 37^m\ 35^s$, $\delta_{1900} = +28°\ 52',9$, $b = +77°, l = 8°$; class: $C\,3$[1]).

H. VON ZEIPEL, Catalogue de 1571 étoiles contenues dans l'amas globulaire Messier 3 (*N. G. C.* 5272), Annales de l'Observ. de Paris. Vol. XXV, 1906.

The three photos used have been taken at Paris with the instrument of the Carte du Ciel and the scale of magnitudes has been determined by the method generally used at Paris for the Carte du Ciel, viz. by comparing the diameters of images exposed during different periods of time. The zero point was determined in accordance with the Harvard Photometric Revision.

S. I. BAILEY. Variable Stars in the Cluster Messier 3. Annals Harvard Observ. Vol. 78, Part I, 1913.

This treatise contains i. a. the magnitudes of 22 stars, used for comparing, found by the meridian-photometer and by comparison with the North Polar Sequence. The 137 variable stars have been measured on 90 plates. Of 110 the luminosity-curve and the period were determined.

Of the 22 standard magnitudes of BAILEY 20 occur also in VON ZEIPEL. With the aid of these stars we examine how far VON ZEIPEL's scale of magnitudes is right [2]). To this purpose we compare v. Z.'s magnitudes with the photometric ones in BAILEY, Table I, column 15. It will be seen then that V. Z.'s magnitudes are, all of them, $1^m.4$ too bright. If we count the stars of VON ZEIPEL's catalogue and apply a constant correction $= +1^m.4$ to the magnitudes we shall find the following numbers:

$$\frac{A_{11.9}}{A_{10.9}} = 3.37. \text{ This makes } -0.42 = 10.9 + 5 + 5 \log \pi$$

$$\log \pi = 0.74-4 \qquad \pi = 0''.0005^5$$

Also:
$$\frac{A_{12.4}}{A_{11.4}} = 3.44 \text{ makes } \pi = 0''.0004$$

$$\frac{A_{12.9}}{A_{11.9}} = 3.09 \qquad \pi = 0''.0005$$

$$\frac{A_{13.4}}{A_{12.4}} = 2.72 \qquad \pi = 0''.0006$$

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[1]) The position and the distribution in classes has been taken from Annals Harvard Observ. Vol. 60, N⁰. 8.

[2]) The correction for the magnitudes of BAILEY by SHAPLEY (Proc. Nat. Acad. Vol. III, p. 480) only refers to fainter stars than those employed by us.

$$\frac{A_{13.9}}{A_{12.9}} = 2.55 \qquad \pi = 0''.0005$$

$$\frac{A_{14.4}}{A_{13.4}} = 2.16 \qquad \pi = 0''.0007$$

From 6 determinations we find as the mean:

$$\pi = 0''.00055 \pm 0.00003 \ (p.\,e.)$$

According to SHAPLEY the average photogr. magnitude of the variable stars is 15.5 and $5 \log \pi = -16.3$, so that we get as the mean absolute magnitude for these stars $M = 15^m.5 - 11^m.3 = 4^m.2$.

The average period is $0^d.54$, so that, according to HERTZSPRUNG, the absolute magnitude $M = 0^m.0$ (visual). If, imitating HERTZSPRUNG, we assume the colour-index of the variable stars in the cluster to be $+1^m.5$, we shall find in this way for the parallax $\pi = 0''.00015$.

h and χ Persei.

h Persei, *N. G. C.* 869, $\alpha_{1900} = 2^h\ 12^m.0$, $\delta_{1900} = +56°41'$, $b = -3°$, $l = -103°$; class: *D* 2.

χ Persei, *N. G. C.* 884, $\alpha_{1900} = 2^h\ 15^m.4$, $\delta_{1900} = +56°39'$, $b = -3°$, $l = 103°$; class: *D* 2.

O. A. L. PIHL. The stellar cluster *χ* Persei micrometrically surveyed, Christiania, 1891.

This catalogue contains the places and magnitudes of 236 stars. The magnitudes have been determined visually with great exactness in accordance with VOGEL's observations with a Zöllner Photometer.

K. SCHWARZSCHILD. Beiträge zur photographischen Photometrie der Gestirne, Publ. der V. Kuffner'schen Sternwarte in Wien, Bd. V, C 1—138, 1900.

This publication contains i. a. the photographic magnitudes of 111 stars in *χ* Persei and 91 stars in *h* Persei. They have been found by taking extrafocal photos and by comparison with observations of the Pleiades.

A. VAN MAANEN. The Proper Motions of 1418 stars in and near the clusters *h* and *χ* Persei, Diss. Utrecht 1911, Recherches astronomiques de l'Observatoire d'Utrecht V.

Four plates of KOSTINSKY and two of DONNER have been measured. Of 1410 stars the diameter has been measured. In order to find standard magnitudes the *B.D.* magnitude for bright stars, corrected in accordance with Annals Harvard Observ. Vol. 54, was used; for stars with less brightness the magnitude was determined from counts of stars with the aid of Publ. Gron. N°. 18 and from estimations by NIJLAND.

B. Messow. Die beiden Sternhaufen im Perseus N. G. C. 869 und 884, Astron. Abhandl. der Hamburger Sternwarte in Bergedorf, Bd. II, N°. 2, 1913.

The places and magnitudes of 649 stars were determined by the measurement of two photographic plates taken in Potsdam by Eberhard and Ludendorff. From the diameters the magnitude was graphically determined with the aid of the standard magnitudes from Schwarzschild. They are photographic and have been expressed in the *P.D.* scale.

The catalogues of Krüger, Oertel, Vogel, Schur, Bronsky et Stebnitzky, Lindemann, Ball and Rambaut were found unfit for our purpose, either on account of incompleteness or their inexact magnitudes.

We tell off Van Maanen's catalogue and determine the number of stars of the cluster for every apparent magnitude. The interval between the magnitudes is sufficient for seven determinations of parallaxes. If we class them according to the decreasing magnitude of the stars employed, we get:

I	$\pi = 0''.004$
II	3
III	3
IV	5
V	7
VI	6
VII	8

$$\text{Mean } \pi = 0''.005 \pm 0''.0005$$

The results show a systematic course with decreasing magnitudes. The cause of this may be a mistake in the scale of magnitudes, or the fact that the catalogue is not sufficiently complete for the fainter magnitudes used.

We now perform the same calculations for Messow's catalogue. Less value is to be set upon the results we shall find then, because Messow's magnitudes are based upon Schwarzschild's photographic scale and Kapteyn's luminosity-curve applies to visual magnitudes.

We find from 7 determinations:

$$\pi = 0'',003 \pm 0'',0003.$$

In order to find whether the different results of Van Maanen and Messow can be explained from the scales of magnitudes used, we reduce Van Maanen's magnitudes to Schwarzchild's standards. For this purpose the apparent magnitudes of 197 stars in Van Maanen's catalogue and Schwarzschild's treatise are compared with each other. The identification is facilitated by using Pihl, or Oertel and Bronsky et STEBNITZKY. It appears that for small magnitudes SCHWARZSCHILD's scale is narrower than VAN MAANEN's. For the corrected magnitudes the numbers A_m are determined then.

After this we find from 4 determinations:

$$\pi = 0'',0035 \pm 0,0007.$$

This result now agrees with what we found from MESSOW's catalogue. The former result, based on MESSOW's visual magnitudes is probably more reliable.

We now perform the calculations for the catalogue of PIHL and find from 4 determinations:

$$\pi = 0'',004 \pm 0'',0005.$$

If we take into account all determinations we may deduce from them as the most likely value for the parallax:

$$\pi = 0'',004.$$

Amsterdam, December 1917.

Chemistry. — *"The Passivity of Chromium"*. (Second Communication). By Dr. A. H. W. Aten. (Communicated bij Prof. A. F. Holleman).

(Communicated in the meeting of Jan. 26, 1918)

In the first communication on this subject [1] the potentials were discussed, which different kinds of chromium present, when they are in contact with an electrolyte in a currentless condition. This second paper gives the results of determinations of the potential presented by chromium in anodic and cathodic polarisation.

1. *Anodic polarisation of electrolytic chromium in solutions of chromous-sulphate.*

The phenomena that make their appearance on anodic polarisation of electrolytic chromium, are dependent on the nature of the metal on which the chromium has been deposited. The results given below refer to chromium that has been deposited from a solution of chromic chloride on copper. The measurements took place according to the method of Le Blanc [2]. The commutator used in this was arranged so, that the electrode was polarized during a certain time, was then currentless for a time of equal duration, was then connected with the compensation apparatus, and was finally currentless again.

The potential as function of the strength of the current had in this the following course. In currentless condition the potential was — 0.55. With a very feeble current the potential rose to + 0.1, and increased only little when the current was strengthened.

Above a certain limiting value of the strength of the current the potential rapidly rose again to + 0.7 V, and increased but little on further strengthening of the current. This course is represented by line I in figure 1. It appears from the shape of the line that from *A* to *B* a slowly progressing reaction takes place at the anode, from *B* to *C* a rapidly proceeding reaction. Between *C* and *D* the limiting current for this reaction is reached. At *D* a new reaction

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[1] These Proc. Oct. 1917.

[2] Zeitschr. für physik. Chemie. 5, 469 (1890).

begins. The line I in fig. 1 is found when we raise the current strength immediately after every reading without waiting till the potential has reached a constant value. When now, after a potential of about + 0.8 V has been reached, we make the strength of the current fall rapidly, line II in fig. 1 is found. In this case the potentials are much more negative with equal strength of current than when we work with increasing strength of current. Also in currentless condition the electrode is much more negative than before the polarisation, viz. — 0.8 V as against — 0.55 V.

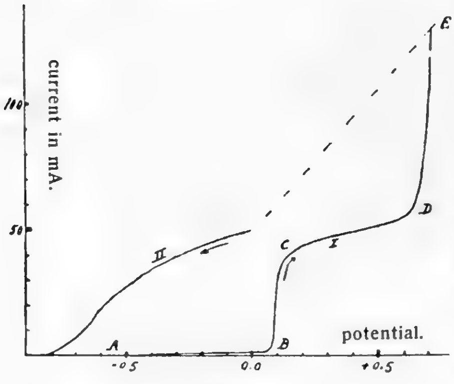


Fig. 1.

When the strength of the current is not rapidly increased, but when we wait after every increase of the strength of the current, till the potential has become constant, we obtain a line lying between the first mentioned. In fig. 2 the line for rapid increase of the strength of the current is given by I, that for rapid decrease by II, the line for slow increase of the strength of the current by III. Line III is also found when the strength of the current is made to decrease slowly. In this last mode of procedure we get from a point on line I to a point of line II, and then the potential rises slowly to a point of line III. In consequence of this the potential as function of the time presents a peculair course, which is represented in figure 3. The upmost line was found when the current was rapidly changed from 5 mA to 1 mA.

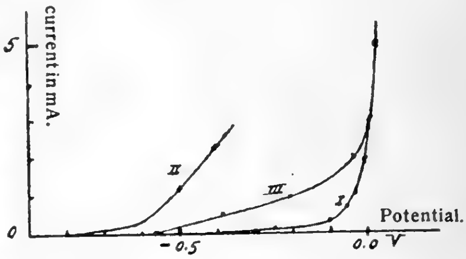


Fig. 2.

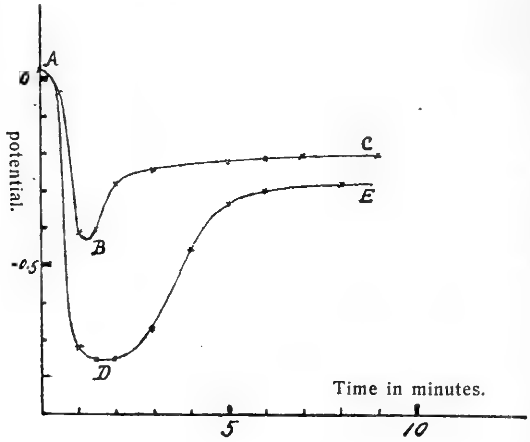


Fig. 3.

The potential was at 5 mA *A*, descended on transition to 1 mA to *B*, and then rose slowly to *C*. The lower line was found when the current was rapidly changed from 5 mA to 0.1 mA. This line exhibits the same course, but more pronounced. The course of the lines in figure 3 can be accounted for by the assumption that the anodic polarisation has two kinds of influence on the potential. In the first place the potential is much more positive than the end-potential *during* anodic polarisation; in the second place the potential is more negative than the end-potential *after* anodic polarisation. There is, therefore, a passivating action, which manifests itself during the polarisation, and there is at the same time an activating action, which shows itself after the polarisation. Both actions are the stronger as the strength of the polarising current is the greater, as will be more fully discussed later.

When the current is broken after anodic polarisation, the potential descends to about —0.8 V, then a *slow* rise to —0.55 V takes place again.

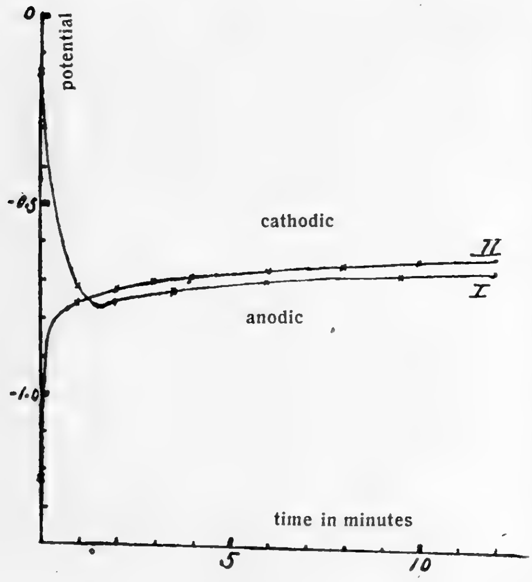


Fig. 4.

This course is shown by line I in figure 4. Here the current was brought from 5 mA to 0.

In the same figure line II gives the course of the potential after cathodic polarisation with 5 mA. Here the potential is very strongly negative at first, after one or two minutes the potential has already risen to —0.75 V, and from this point the line for cathodic polarisation coincides about with that for anodic polarisation. I.e. through anodic polarisation the potential is at first too positive, through cathodic polarisation too negative, after a few minutes, however, the condition after anodic and cathodic polarisation has become the same.

As was already observed, it follows from line I in figure 1 that two different reactions take place on anodic polarisation of chromium. One at about + 0.1 V, another at about + 0.7 V. The latter is undoubtedly the anodic formation of chromate. What reaction takes place at + 0.1 V is still to be investigated. There exist here three possibilities. 1. the chromium can go here into solution as chromous-ion (perhaps also as chromic-ion). 2. oxidation can take place in the solution from $Cr^{\cdot\cdot}$ to $Cr^{\cdot\cdot\cdot}$, 3. the copper on which the chromium has been precipitated, can go into solution. This last possibility must be considered, as the chromium does not very firmly adhere to the copper. The chromium might, therefore, present cracks, and this might be the reason that the copper went into solution.

That the reaction at + 0.1 V is not an oxidation from chromous- to chromic-ion appears from this, that pretty much the same course was found for the current potential line when as electrolyte a solution of KCl was used. Only the potential, for which a great increase of the strength of the current was found, lay somewhat lower, viz. at from — 0.1 to 0.0 V. The same thing appears from the following experiment: A naked, and a platinized electrode were anodically polarized in a solution of chromous-sulphate. No rise was noticeable at + 0.1 V. The strength of the current rose at once with a potential of —0.5 V; hence oxidation evidently takes place here from $Cr^{\cdot\cdot}$ to $Cr^{\cdot\cdot\cdot}$. The limiting current had already been reached at 2 mA, and a further rise of the strength of the current did not set in before + 1.0 V (oxygen generation). For comparison the anodic oxidation of $FeSO_4$ was examined. Here the limiting current was not reached before 90 mA. In figure 5 the current potential lines of platinum in chromous sulphate and ferrous-sulphate have been drawn.

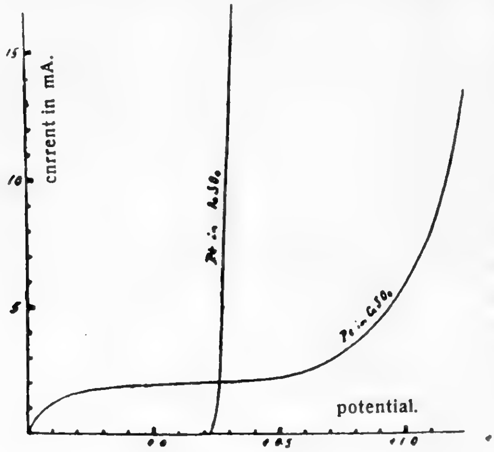


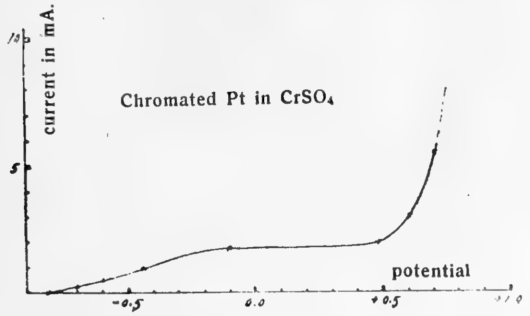
Fig. 5.

When an electrode of copper is anodically polarized in a solution of KCl, a current potential line is found, which, as regards the first portion, agrees pretty well with the line for chromium precipitated on copper. Hence it is possible that the first vertical part in the chromium line is caused by the going into solution of copper.

That the metal on which the chromium has been electrolytically precipitated, plays a part in anodic polarisation, appears from the following observations. Chromium that has been deposited on silver, presents about the same current potential line as chromium on copper.

The only difference is, that the first vertical portion lies at a more positive potential, viz. + 0.4 V.

Fig. 6.

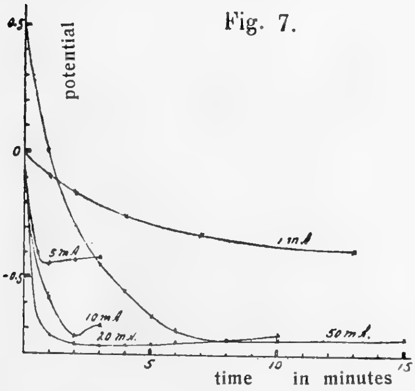


For chromium on platinum or on gold the shape of the current potential line is different. Figure 6 represents the behaviour of chromium on platinum, when the strength of the current is quickly increased. From — 0.8 V a slow increase of the strength of the current takes place till a potential of about —0.1 V has been reached. Then the line has a very flat course, till the formation of chromate begins at + 0.7 V. When now the strength of the current is slowly raised, and we wait every time till the potential has become constant, we find a line with a much flatter course, for which already at 0.1 mA the potential rises to + 0.7 V. It appears from this, that electrolytic chromium cannot continue to go into solution at a potential that lies in the neighbourhood of the potential of equilibrium. It may be, that the ascending course that the line exhibits at —0.8 V in figure 6, does not correspond with the solution of chromium, but with the solution of hydrogen present in the electrolytic chromium.

Immediately after the current has been broken, the chromium on platinum or gold is not activated, but presents a potential of + 0.7 V. This phenomenon is treated more at length under 2.

2. *The activation of chromium by anodic polarisation in solutions of chromous sulphate.*

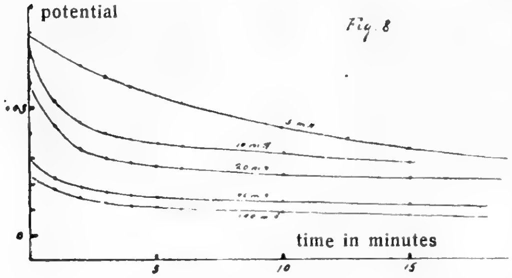
Fig. 7.



Chromium that has been deposited electrolytically on silver, presents like chromium on copper, an active potential after anodic polarisation. Immediately after anodic polarisation the potential is fairly positive. It descends, however, in a few minutes, to a pretty great negative value. In figure 7 the course of the potential has been drawn as function of the time, when the current of 1, 5, 10, 20, and 50 mA is suddenly broken off. The general course of these lines is as follows: When the current is interrupted, the chromium is still somewhat passive, the potential moves however quickly to more negative values, and finally approaches a value of about — 0.5 V.

The way in which this value of — 0.5 V is reached, is however different for different strengths of current. For feeble anodic polarisation, 1 mA, the potential falls gradually from 0.0 V. At 5 mA a faint minimum is reached, after which the line begins again to ascend slowly. For 10 mA this minimum lies lower, for 20 and 50 mA still somewhat lower (— 0.75 V). For these last two lines the rise following on the minimum, is much slower than for the others. Hence it appears from this, that the activation of chromium on silver is the stronger and the more prolonged as the strength of the current, with which the anodic polarisation takes place, is the greater.

The same phenomenon is observed for chromium that has been precipitated on gold, but the potentials are much more positive here. In figure 8 these lines are drawn, which indicate the course of the potential with the time, when the current is suddenly broken off. Here too the potential that is reached, lies at the more negative values as the polarising current has been the stronger.



Hence it appears here, that activation makes its appearance after anodic polarisation with chromium that has been deposited on copper, silver, or gold. The potentials that are reached in this, are most negative for the least noble of these metals[1]).

It was noticed in these experiments that chromium on gold or on platinum, after anodic polarisation, becomes less active when it has been cathodically polarised shortly before. Lines 1 and 2 in figure 9 give the potential after the current has been interrupted, when the electrode had not been

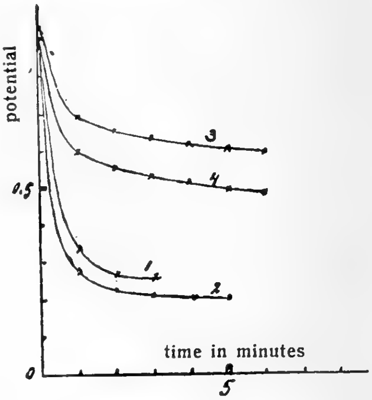


Fig. 9.

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[1]) Rathert. Zeitschr. f. physik. Chemie 86, 567 (1914) observed the same phenomenon for Cr that had been precipitated on Cu and on Pt.

cathodically polarised beforehand, lines 3 and 4 when this had been done.

This activating action of the anodic polarisation still comes to expression in a very peculiar way in this, that for chromium on gold a current potential line is found with a retrogressive part. With increasing strength of the current, the potential descends.

Such a current potential line has been drawn in figure 10. At 5 mA the potential is + 0.94; this *descends*, on increase of the strength of the current to 100 mA, to + 0.82 V. This peculiar course is easily accounted for on the ground of what precedes.

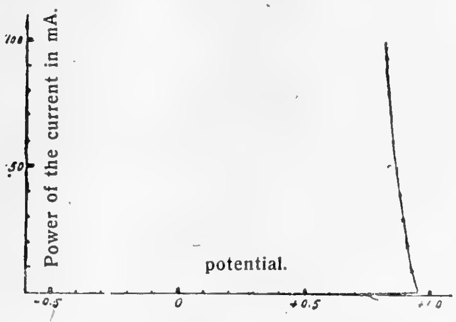

Fig. 10.

The electrode is activated every time at the breaking of the current of the commutator, and that the more strongly as the strength of the current is the greater.

In the period of time that the electrode is currentless, the potential is, therefore, more negative for a great strength of current than for a small. Now in this method of procedure the potential is not measured during the passage of the current, but shortly after (about $\frac{1}{100}$ sec.).

That the current potential line is found to be retrogressive, is therefore again a consequence of the activation *after* anodic polarisation. When these considerations are true, this retrogressive line will only be found when we work with a commutator, not when we measure the potential with a siphon *during* the polarisation. This proved actually to be the case. For chromium of Goldschmidt, which likewise gives a retrogressive current potential line in solutions of KCl, H_2SO_4 and HCl, when we work according to Le Blanc's method, a normal line was found when the potential was determined by means of a siphon.

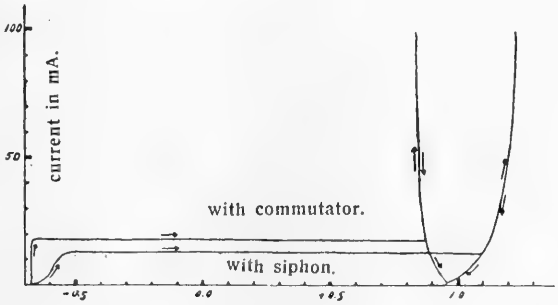

Fig. 11.

Two lines have been traced in figure 11 for measurements with commutator and with siphon in HCl. The activation by anodic polarisation appears here very clearly.

At 100 mA the potential descends in $\frac{1}{100}$ sec. from + 1.22 V to + 0.83 V, hence 0.39 V; at 5 mA it descends only 0.1 V, viz. from + 1.03 to + 0.93 V. With these potentials formation of chromate takes place. But also when the chromium goes into solution as chromous ion, the activation is very apparent after interruption of the current. Here e.g. the potential that is found according to LE BLANC, is for 10 mA about 0.1 V more negative than when measured with a siphon. In the first case the current potential line up to 18 mA has the course of a normal line, in the second case the current potential line proves that also the anodic solution in hydrochloric acid as chromous ion is a reaction that proceeds slowly.

In virtue of the difference found here between the current potential line that is determined with a commutator and one that is determined by means of a siphon, it might be imagined that the above described activation by anodic polarisation takes only place during the moments that the current is broken, hence only after, not during the polarisation. That activation takes place also *during* the anodic polarisation appears from the experiments with chromium of GOLDSCHMIDT described below.

3. *Anodic polarisation of chromium of* GOLDSCHMIDT.

The passivation and activation of chromium of GOLDSCHMIDT has been closely examined by HITTORF, especially with regard to the different factors that act in a passivating or activating way. It then appeared, as was already said in the introduction of the preceding paper, that oxidizers (HNO_3, bromine water), like anodic polarisation, make chromium passive. Cathodic polarisation on the other hand makes chromium active. In the same way the hydrogen generation, which chromium gives in active state in diluted acids (especially HCl) is able to make the active state permanent. Chromium is also made active by being placed in melted chlorides, ($Zn\ Cl_2$, KCl + NaCl). Chlorine ions have a specific activating action, hence chromium is more strongly active in hydrochloric acid than in sulphuric acid of the same concentration.

In the first place a quantitative comparison was then made between the activating action of hydrochloric acid and sulphuric acid by determination of the strength of the current required to make the metal in these solutions passive. It then appeared that the current strength required for passivation was about proportional to the concentration of the acid, as figure 12 shows. The values found for the current strength of passivation, are rather divergent; this causes the points in figure 12 to lie rather scattered.

It appears clearly from the lines found that hydrochloric acid acts more strongly activating than sulphuric acid, with the same concentration about three times as strongly. Accordingly besides the hydrogen ions, also the chlorine ions have an activating action. This follows also from this, that the strength of current of passivation in hydrochloric acid to which potassium chloride has been added, is greater than in hydrochloric acid alone. In two experiments this amounted in 0.1 n HCl + n KCl to 14.5 and 16.5 mA, in 0.1 n HCl to 5 and 7 mA. By making the concentration of the chlorine ions ten times greater, the activating action is increased two and a half times.

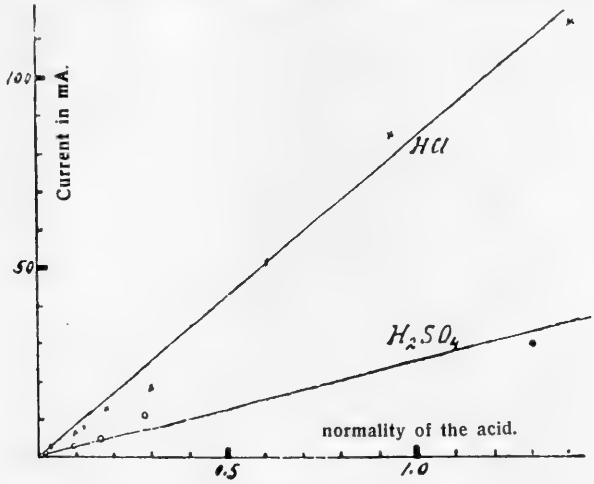

Fig. 12.

The chlorine ions, however, do not act here independently of the hydrogen ions, for in 1 n KCl without hydrochloric acid chromium already becomes passive under ordinary circumstances with the smallest strength of current.

In the second place the anodic behaviour was investigated of chromium that had been activated by placing it in melted salts. The activating action of molten NaCl + KCl may appear from the following experiment.

A piece of chromium of GOLDSCHMIDT in a saturated solution of KCl gave a potential of —0.47 V, and already became passive at 0.1 mA. ¹) After this electrode had been for half an hour in molten NaCl + KCl, the potential was —0.640. Now the electrode bore a current of 8 mA without becoming passive on slow increase of the strength of the current.

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¹) These and the following experiments were not carried out with a commutator, but the polarisation voltage was measured by means of a siphon, hence *during* the polarisation.

After the current had been broken, the potential was more active than before the polarisation viz. —0.667. In this the activating action of the anodic polarization *after* the interruption of the current manifested itself again. When after this the current was suddenly again raised to 8 mA, the electrode became passive. Hence, while on slow increase of the strength of the current the electrode resisted 8 mA, it could not bear this strength of current with rapid rise to 8 mA, notwithstanding the electrode was very active in currentless condition. This is in harmony with the phenomenon described before, that on quick increase of the current an electrode at first presents a too positive value, which afterwards becomes less positive. By quick increase of the current the potential can now become so positive, that the electrode becomes passive. If the current had been slowly made stronger, so that the potential had every time an opportunity to go back to less positive values, no passivity would have set in.

This retrogression of the potential has been graphically represented in figure 13 for chromium that had been activated in molten KCl + NaCl. The lower line gives the potential as function of the time, when the current is raised from 0.2 to 0.5 mA, the upper line when the current is raised from 1.0 to 2.0 mA. The influence of the slow increase of the strength of the current was still more apparent for a piece of chromium that had been activated in $ZnCl_2$. This resisted a strength of current of 14 mA, when the current was increased by 1 mA every three minutes. After interruption of the current this electrode was more active than before the polarisation. When after this the current was at once brought to 5 mA, the electrode became passive [1]).

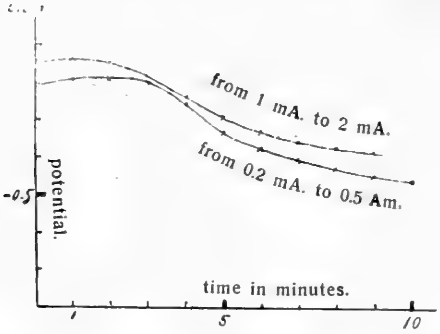

Fig. 13.

Another electrode was likewise activated in molten $ZnCl_2$. The potential was — 0.542 in saturated KCl. The electrode was anodi-

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[1]) The influence of the interruption or enfeebling of the polarizing current appears in a very peculiar way from the following experiment. A piece of chromium, which was anodically polarized with 7 mA. in KCl-solution, was brought in contact with a piece of zinc. This caused the potential to fall from — 0.538 to — 0.750. Then the current passed into the solution chiefly through the zinc, which caused the density of the current, with which the chromium was polarized, to

cally polarized, and the strength of current was slowly raised to 2 mA., at which the potential amounted to —0,405 Volt. When air was blown through along the electrode, the potential did not change more than 10 mV; the same occurred when the liquid was stirred, from which follows that the activity of the chromium was not caused here by a change in concentration of the liquid in contact with the metal. After the current had continued to pass throughout the night, the strength of the current had increased to 3.28 mA., and the potential had fallen to — 0,513. Accordingly the electrode was very active now. The solution of KCl was then siphoned off, and replaced by fresh solution. The electrode remained active, though the potential rose a little, viz. from — 0,513 to — 0,472. Here too it appears that the activity of the chromium was not the consequence of a change in the concentration of the liquid. The chromium generated small bubbles of hydrogen. Also when the metal was brought in contact with a platinum wire, it remained active. In this the hydrogen continued to develop at the chromium. When the current was broken, the potential was —0.613, and fell to — 0,642 in 15 minutes.

Now the electrode could resist 1 mA without becoming passive, and the current could be made stronger pretty quickly, in 20 minutes from 1 mA to 3.6 mA, without the activity being lost.

Accordingly the anodic current could remain interrupted for this electrode for 15 minutes, the activity being maintained.

After the anodic current had been first carried up to 3.6 mA again, at which the potential amounted to — 0,421, it was cathodically polarized with 50 mA for 15 minutes. This took place in another solution of KCl. The anode was placed in a porous vessel, so that the liberated chlorine did not get in contact with the chromium electrode. When the electrode was again brought in the original solution of KCl, the potential was strongly negative in consequence of the hydrogen charge, — 1.09 V. Then this electrode was anodically polarized with 0.5 mA. In an hour the potential rose to + 0.512 V. While therefore without previous cathodic polarisation the electrode could bear anodically 1 mA without becoming passive, it already became passive with 0,5 mA after cathodic polarisation. After interruption of the current this electrode again obtained an active potential, viz. — 0.614 V.

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become smaller. When afterwards the zinc was removed, the potential rapidly rose to — 0.35 V, after which it fell again to its original value — 0.54 in three minutes.

The decrease of the anodically polarizing current had consequently given rise here to a very considerable *rise* of the potential.

Just as **Hittorf** has found, it appeared also in these experiments that chromium is more strongly activated by molten zinc chloride than by molten KCl + NaCl.

In molten $K_2 Cr_2 O_7$, and also glowed at the air, chromium becomes very little activated, but yet it is a little more active than chromium that has not been heated. In molten KCN the metal is perceptibly, though feebly activated, it bore 1.5 mA without becoming passive.

It is remarkable that not always the electrode that has the most negative potential in currentless condition, is made passive with the greatest difficulty. Thus chromium that had been immersed in molten KCN presented a potential of — 0,44 V before polarisation, after polarisation — 0.58 V. Yet this chromium was less easy to make passive than chromium from molten $K_2 Cr_2 O_7$, which showed — 0.56 before polarisation, — 0,64 V after polarisation.

All the electrodes that had been treated with molten salts, were covered with a layer of green or black oxide. From molten $ZnCl_2$ and KCl + NaCl the metal generated hydrogen. The chromium that had been glowed at the air, exhibited blue annealing colours.

The treatment that the chromium electrode has been previously objected to, is therefore of great influence on the passifiability.

The same thing applies to the concentration of the solution in which the chromium is examined.

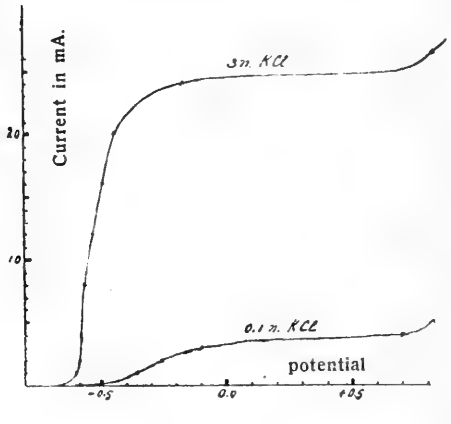


Fig. 14.

The metal is very easy to make passive in a solution of Na_2SO_4, not so easy in a solution of KCl.

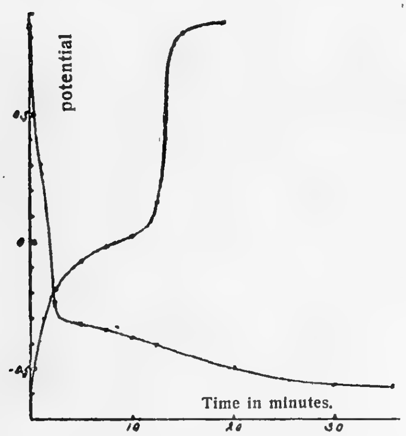
In this the strength of current of passivation is the greater as the KCl-content of the solution is the greater. Figure 14 represents the line of passivation for a same chromium electrode in a saturated solution of potassium chloride, and in a 0,1 n solution.

When an electrode that has been activated in molten $ZnCl_2$, is anodically polarized with a sufficient density of current, it becomes passive, and presents potentials which are the same as for an electrode which has lain in nitric acid beforehand for a long time, and has become passive through this. After the current has been broken off, the electrode from $ZnCl_2$ becomes again active after a short time, that from HNO_3 remains passive. Also an electrode that has been first activated by $ZnCl_2$, becomes passive through HNO_3.

In the experiment the potential rose from —0.65 to + 0.1.

After vigorous anodic polarisation the electrode had become active again and presented a potential of —0.49.

When a piece of chromium is brought into hydrochloric acid, it becomes active, and generates hydrogen. After it has been rinsed and conveyed into a saturated KCl-solution the activity continues, but is by no means so strong as the activity obtained in molten $ZnCl_2$, so that the electrode already becomes passive e.g. at 1 mA. When, however, the current is slowly made stronger, the activity continues to exist. Thus a strength of current of 12 mA could be reached for a potential of — 0.35 V in ten days. Here too it was always observed that on increase of the strength of current the potential first increased greatly, and diminished later again. The electrode continually developed hydrogen, sometimes with a crackling sound. The liquid contained a large quantity of chromhydroxide and had become alcaline in consequence of the cathodic formation of NaOH. After breaking of the current the electrode was active — 0.66 V. When after 8 minutes the current was suddenly brought to 3 mA, the electrode became passive. After interruption of the current the activity returned. In Fig. 15 the passivation with 3 mA and the activation after the current had been broken off, is represented as a function of the time.



When chromium of Goldschmidt, which had not been previously activated, is anodically polarised in a saturate solution of KCl with a current strength of 1 mA, it becomes passive. When we begin to heat, activation sets in at a certain temperature. With a strength of current of 1 mA this took place at about 60°. Then the potential fell to — 0.55 V. When the temperature is lowered during passage of the current, the electrode remains active, during which the potential rises to — 0.4 V. With higher current strength the same behaviour is found; here the temperature at which activation occurs, lies higher, for 25 mA e.g. at 75°. With lowering of the temperature the electrode remains active in this case; the potentials found here, are the more positive as the strength of the current is the greater.

If this is too great, e.g. 50 mA, the electrode becomes passive on cooling.

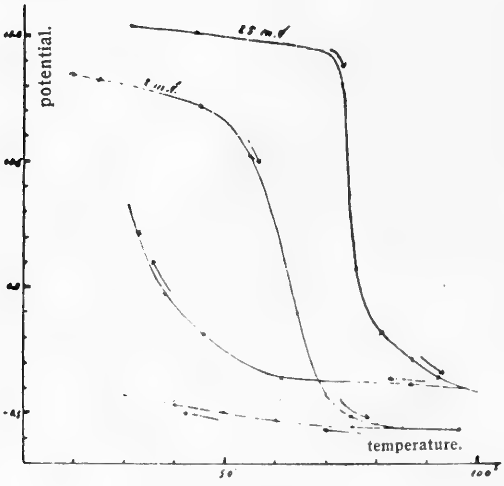


Fig. 16.

Figure 16 gives the course of the potential as function of the temperature on heating and cooling of a chromium electrode, when this is anodically polarized with 1 mA and 25 mA.

After the current had been broken, the potential was very active, at 1 mA —0.60 V, at 50 mA —0.66 V.

When the electrode has remained active after cooling during the anodic polarisation, and the current is broken, a very active potential is found. When after a short time e.g. after 20 sec. the current is closed again, the electrode cannot bear this current, but becomes passive. In harmony with this is the phenomenon that a chromium electrode which has become active through heating in a solution of KCl, on anodic polarisation, and is cooled then after the current has been broken, preserves indeed an active potential, but can resist only a very feeble anodic current without becoming passive.

Finally the influence of molecular hydrogen and of hydrogen that had been generated cathodically on the passifiability of chromium was examined. Molecular hydrogen has no influence here, as was already known from earlier researches. When a chromium electrode is brought in an atmosphere of hydrogen in a saturate KCl-solution, the chromium gets a potential of about —0.5 V, a platinum-electrode presenting —0.68 V under the same circumstances. The chromium is, therefore, not able to assume the hydrogen potential. When the chromium is cathodically polarized, so that hydrogen is vigorously generated, the metal at first shows a strongly negative potential, which is the consequence of the acquired hydrogen charge. The chromium pretty soon loses this hydrogen charge, at which the potential gradually rises to —0.5 V. Both before and after cathodic polarisation, this electrode became passive on anodic polarisation with 0.2 mA. Accordingly the cathodic polarisation had not been able to activate the electrode perceptibly. This electrode *did* become active when brought in strong hydrochloric acid. Now the potential amounted to —0.7 V in a saturate KCl. solution, and no passivation took place on anodic polarisation with 1.2 mA. Hence it appears from this that cathodic polarisation activates the chromium less strongly than treatment with hydrochloric acid. It appeared from further experiments that cathodic polarisation *diminishes* the activity.[1]) Thus an electrode which had been in a solution of KCl for two days, and had remained active in it, exhibited a potential of —0.57 V. On anodic polarisation with 0.3 mA the chromium remained active with a potential of —0.46 V. Then this electrode was cathodically polarised with 50 mA for $1^1/_2$ hours. The potential was —0.79 V after interruption of the current, and rose to —0.192 in $1^1/_4$ hours. Hence the activity had greatly diminished *after* cathodic polarisation, which also appeared from this, that the electrode became passive already on anodic polarisation with 0.01 mA in this case.

On strong cathodic polarisation other electrodes obtained a much more negative potential, down to —1.22 V. Notwithstanding such an electrode was strongly active as far as the values of the potential are concerned, it could not withstand anodic polarisation with 0.1 mA without becoming passive. After activation in strong hydrochloric acid the electrode easily resisted 0.4 mA, at which the potential was —0.55.

4. *Summary of the results.*

1. Electrolytic chromium becomes passive in chromous sulphate on anodic polarisation with a sufficiently great strength of current. After breaking of the current the potential of the electrode is more negative than before the polarisation.

2. When the polarising current is made stronger the potential at first assumes a too positive value, which slowly falls to a more negative value. When the current is made feebler, the same thing takes place in reversed order.

3. The activation to which chromium is subjected by anodic polarisation, is the stronger as the electrode is polarized more vigorously.

4. GOLDSCHMIDT Chromium, which has been activated by treatment with molten $ZnCl_2$ or KCl + NaCl, can be anodically polarized in a solution of KCl without becoming passive. The strength of current which the chromium can resist in this, is greater when the current is gradually strengthened than when the strength of the current is rapidly increased.

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[1]) This appears already from the experiment described on p. 1129.

5. Previous cathodic polarisation makes the anodic current strength which this chromium can resist without becoming passive, smaller.

6. The increase of the activity on continued anodic polarisation is caused by a change in the metal surface.

7. Chromium of GOLDSCHMIDT that has become passive through anodic polarisation in a solution of KCl, becomes active on heating of the solution, also during the passage of the current. On cooling the activity remains preserved, at least if the current is not too strong.

When the chromium cools down in the solution without becoming anodically polarized, the activity which it retains after cooling, is smaller than when the current continues to pass on cooling.

In the literature a few more examples are found of the activation through the anodic polarisation observed here. Thus FLADE [1]) states that a nickel electrode as negative pole of an element, the positive pole of which was platinum in chromic acid, gave a stronger current (hence was more negative) when it had first been strongly *anodically* polarised.

The same thing was found by RATHERT [2]). A nickel electrode which in $NiSO_4$ presented a potential of + 0,237 V, had a potential of — 0,125 V after anodic polarisation, of + 0,64 V after cathodic polarisation. He attributes this phenomenon to changes in the concentration in the liquid. That for chromium such an explanation is not valid, has already been demonstrated above.

In the following paper we shall endeavour to give an explanation of the above described phenomena.

Chemical Laboratory of the University.

Amsterdam, January 1918.

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[1]) Zeitschr. f. physik. Chemie **76**, 513, (1911).

[2]) Zeitschr. f. physik. Chemie **86**, 567, (1914).

Chemistry. — "*Ultramicroscopic investigation of very thin metal-films obtained by evaporation in high vacuum*". II. By Prof. W. Reinders and L. Hamburger. (Communicated by Prof. J. Böeseken).

(Communicated in the meeting of September 29, 1917).

Introductory.

§ 1. In a previous paper[1]) we stated the results of the ultramicroscopic investigation of the extremely thin films of rock-salt, silver, gold, and tungsten which are deposited on the glass wall of the bulb either during evaporation in high vacuum or by means of cathodic atomising.

It appeared that the bright sublimate of sodiumchloride was optically vacant and must be looked upon as a substance in the amorphous-vitreous state. This state is metastable. Heating or the access of moist air soon effects an opalescence, brought about by the arising of small crystals, the growth of which can be investigated under the ultramicroscope.

Of the metals, tungsten too gave a deposit similar to NaCl; even when so much of the metal had been sublimated, that the glass had assumed a rather darkish tint, no structure could be observed even under the most intense lighting. But gold and especially silver formed a finely-coloured and distinct heterogenous deposit, that showed itself under the microscope as a mosaic of small and variously-coloured particles. The colour changed proportionally to the thickness of the deposit and the heterogeneousness became more marked.

The silver precipitate was like the salt precipitate highly susceptible to the influence of a rise in temperature and moist air, which immediately brought about a change in colour. In order to be able to observe the precipitate in its original form it must be protected from the influence of the air, in which we succeeded by covering it, when still in the vacuum, by a thin layer of Canadabalsam. In comparing the covered (with Can. B.) and non-covered precipitates it appeared that the effect of the air causes a considerable coarsening of the precipitate and that the original, well-preserved precipitate showed a but scarcely perceptible heterogeneity. We still add in

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[1]) These Proceedinsg **19** (1916) 958.

fig. 1 and 2 (plate 1) a couple of photographic representations bearing on this statement.

It now occurred to us whether the silver, too, like NaCl and tungsten was initially precipitated in a structureless state and was caused to crystallize but afterwards and by secondary causes such as heating. In this connection experiments were made, in which the sublimate was formed extremely slowly and the glass-wall was cooled down during the whole process of the sublimation to the temperature of liquid air. Though under these circumstances the crystallisation of the precipitate was greatly impeded, yet no structureless precipitate could be obtained.

From this we see that silver passes very easily into the crystallised state. But how do the other metals behave? Under what conditions is a structureless deposit developed and when are heterogeneous particles formed? Which metals reveal, like gold and silver, when in very thin layers, definite colours; is this property chiefly decided by the thickness of the layer and the degree of dispersion of the metal, independent of the nature of the latter, or must the cause be looked for in the very nature i.e. in the selective absorption of the metal itself.

In order to find the answer to these various questions we have continued and enlarged our former investigations and shall relate the results in the following pages.

The Ultramicroscopic Investigation.

§ 2. *The apparatus.*

The tested elements, in the shape of thin filaments were wrapped on the support of an incandescent lamp where they were heated by means of an electric current and brought into a state of evaporation, after careful evacuation of the lamp. In most cases there was fused to the lamp a side-tube (*b*) with Canadabalsam, which had previously been freed from the dissolved gases and which was kept immersed in liquid air during the time of burning of the lamp. When a metal-sublimate had formed on the glass of (*c*), the Canadabalsam was melted and carefully spread over the surface.

The inner wall of the glass had been carefully cleaned — in order to obtain a greater possibility of avoiding any disturbance in the ultramicroscopic investigation arising from dust particles.

As it was known besides that traces of remaining gases have a great influence on the evaporation of a metal¹) and as it proved

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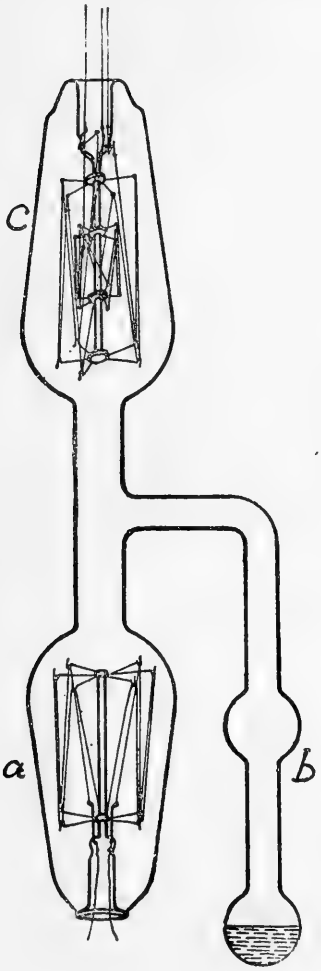
¹) I. Langmuir, Trans. Amer. Inst. of Electr. Eng. 1913, 1902.

that even slight quantities of water-vapour may affect the nature of the deposit, great care was devoted to the evacuation. By means of the affixture of a tungsten auxiliary-lamp (*a*), which was now and then burned at over-tension, we cleaned up as well as possible the gases that remained after the melting-off of the air-pump or that might once more arise in the lamp [1]).

When after the heating of the metalwire a sublimate had been deposited on the glass-wall in (*c*) and this had been covered by the Canadabalsam, then the lamp was opened and a fragment of the glass-wall was observed under the ultramicroscope. For lighting we used a Lilliput arc-lamp of E. Leitz or a Philips projection-lamp of 500 Watt, the latter being highly satisfactory on account of its tranquil and steady light. To concentrate the light we used a cardioidcondensor.



By making sure that the thickness of the glass-wall was no more than 0.6 mm. we succeeded in bringing the metal-film on the glass exactly in the focus of the condensor and could observe it with the microscope. For our objective we used Zeiss' special-objective V with glycerine-immersion.

§ 3. *The investigated elements.*

We investigated carbon and the metals *W, Mo, Pt, Fe, Ni, Au, Cu, Ag, Mg, Zn* and *Cd*. Except carbon and zinc, they were all brought into wire-mould by the tensile process. Zinc was got into that state by squeezing it during gentle heating through a narrow opening into a diamond. From lead and tin, too, wires were similarly formed; but the vapour tension of these metals under temperatures below their melting-point is so slight, that no sublimate could be obtained with them. Neither did we succeed with aluminium, in which case it is probably a thin oxide coating that constitutes the impediment. Tellurium, antimony, arsenic and bismuth, sublimate very well

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[1]) I. Langmuir, Journ. Amer. Chem. Soc. **35**, 105, 931 (1913).

indeed below their melting-point, but it was impossible to make them into wires. Hence they could not be investigated conformably to the method we made use of.

The *W, Mo, Fe, Cu, Au, Ag, Mg, Cd* and *Zn* we made use of, may be looked upon as chemically pure; this was borne out by analysis and measuring of the temperature-coefficient of the resistance (e.g. with *Au* 39.8, with *Ag* 39.2). For carbon we used the well-known carbon-filament lamp wire. Two kinds of platinum were used: one being extremely pure, a second containing something of the other *Pt* metals as impurity. The *Ni*-wire contained 99,4 °/₀ *Ni* and besides traces of *Co, Fe, Zn* and *Al.*

With base metals slight oxidizing of the wire, e.g. with the leading-in of the support into the glass cover can generally not be avoided. For this reason hydrogen was admitted into the balloon, after joining the lamp to the airpump, and the wire in this atmosphere was brought to a slight incandescence and then it was evacuated once more in the usual manner. With some very low-melting metals special lamp-shapes were applied, in order to prevent a too great heating of the metal to be tested during the fusing into the balloon.

§ 4. *Silver.*

Concerning this metal many observations have been made during the first investigation. It struck us, that though the Canadabalsam sufficiently arrests the direct change of colour in the deposit of metal that is observed with access of air, yet it cannot eventually prevent it. It may be that it is not altogether indifferent itself, or that it does not altogether arrest the diffusion of the air-gases, at any rate it has some effect in the long run, and therefore it was desirable, in order to determine the nature of the unaffected deposit of metal to control the observation with a Canadabalsam-coating or with some other protector. Our choice fell on the salt-films formed by sublimation, concerning which, one of us [1]) had observed during previous investigations, that it does not perceptibly effect a tungsten-film that is already deposited. Having observed that the sublimated salt forms a perfectly homogeneous, optically insoluble layer of glass, we might expect it not to interfere by a structure of its own, with the observations of the metal. Among the various salts calcium-fluoride [2]) was found to be preferable, on account of its insolubility in water. In order to fix the layer of salt, we wound, in lamp *c* (fig. 1) across a second set of supporting-hooks W-wire with separate

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[1]) L. Hamburger Chem. Weekblad **13** (1916) 535.

[2]) L. Hamburger en D. Lely. Ned. Octrooi-aanvrage no. 6502.

feeding-wires, which was covered with CaF_2. By bringing this wire into a state of incandescence the salt could be sublimated. As the layer of salt does not sufficiently protect the underlying metal from the subsequent effect of the immersion-liquid (glycerine), it was in its turn covered as well with a layer of Canadabalsam (in the vacuum).

The precipitate we tested had been brought on the glass under a temperature of liquid air. The part that had been covered with CaF_2 as well as with Canadabalsam, remained perfectly unaltered with access of air and under the microscope it revealed a weak mosaic. In spite of all the precautions taken, the out-crystallisation could not be hindered.

Where no second covering with Canadabalsam had been applied, the access of the immersion liquid (glycerine) caused a change of colour in the deposit (blue → brownish red) and the ultramicroscopic image was much richer in ultramicrons and the ultramicrons themselves became stronger. Hence CaF_2 by itself did not sufficiently protect.

Influence of Temperature.

With our first investigations it had already been observed that short heating at a comparatively low temperature, say 260° already causes obvious alterations in the deposit of silver, revealing themselves both in change of colour and in a coarsening of the deposit.

These alterations become distinctly apparent, on locally heating the precipitate formed under room temperature, by momentarily pressing a very hot glass rod against the outside of the glasswall. In the homogeneous blue field the heated place then stands out as a red spot, in a red field as a yellow one. This change sets in, not only in the unopened evacuated lamp, but also when the silver is in contact with the air or is covered with a layer of Canadabalsam.

If the glass-shard on which such a spot arises, is moved across the field of vision of the microscope, it appears, that whereas the non-heated part consists of a mosaic of but very weak ultramicrons, the heated part presents an exceedingly luminous field of vision. The primary colour of the fields observed in the microscope is always complementary to that which is observed with the naked eye. Though the distance of the separate particles has not perceptibly altered, yet the particles themselves are much more luminous and it also seems as if the layer of the luminous particles has become thicker.

Hence the separate ultramicrons have increased in size and probably in number too, at the expence of a quantity of silver, which at first was invisible.

Consequently the silver deposit consists of two parts: 1st a number of more or less perceptible ultramicrons and 2nd an optically insoluble part, which like the layer with NaCl, CaF_2 and W can be looked upon as vitreous-amorphous. Owing to the heating the second part merges into the first.

We see that the absorption-colour of the precipitate, as observed with the naked eye, changes during its growth in the direction yellow-red-blue, whereas during heating it is modified in an opposite direction. Hence it appears that this absorption-colour is chiefly determined by the vitreous amorphous part and not by the ultramicrons.

The lowest temperature, in which these changes set in was now more closely investigated.

Knudsen [1]) states, that as regards the appearance of the silver sublimate no difference could be observed whether the temperature of the glass-wall against which it is deposited, is 575° or several hundred degrees lower.

But we already saw, that heating to 260° caused an appreciably difference in the layer formed with 20°.

Heating to 130° had a similar effect. The blue tint changed into green-yellow.

With a rise of temperature from 20° to 80° and a short duration of heating we could not observe any change. It is probable, though, that in time a change will set in, so that a sharp demarcation-line above which the deposit is unstable and below which it is stable cannot be drawn.

We tried to investigate whether the sublimates formed under a very low temperature differ at all from those arising under room temperature. With former processes concerning this matter the result attained had been but a negative one; with both temperatures the same colour-scale was gone through. It could be expected that eventual variations would be but very slight ones. In order to be able to ascertain this, we chose a cylindrical glass-balloon with a straight, axial filament, being immersed midway in liquid air during the time of burning. Hence the lower part of the glass had a temperature of about —180°, the upper part was at room temperature. As, owing to the great heat-conductivity of silver, a strong refrigeration takes place with the leading-in and suspensionwires, the filament must be fairly long in order to glow equally along a sufficient distance. In our case the length of the double-filament was 20 and the diameter of the glass-cylinder 5 cm. A very distinct difference in the colour

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[1]) Ann. d. Physik. (4) **50** (1916), 472.

of the sublimate could not be ascertained. With the precipitate formed with —185° the transition colourless-yellow-blue was somewhat more rapid than with the precipitate that arose with an ordinary temperature. On the whole the more reddish tints were absent. Allowing for the experimental difficulties to provide for the filament a perfectly uniform division of temperature, we must not set too much store by these variations.

§ 5. *Gold.*

The first colour appearing with very slow sublimation in vacuum is pink; as the deposit becomes thicker, the colour becomes red, violet, blue, then green, and finally the reflectioncolour of metallic-gold is discerned.

With access of air the pink tint gradually disappears and the green becomes somewhat deeper tinted. But those changes are not so strong as those observed with silver. Canadabalsam applied to it in vacuum altogether checks it.

Ultramicroscopically the difference between the parts covered- and non-covered with Canadabalsam is not nearly so great as with silver. The very thin, pink-coloured layer is optically insoluble, the somewhat thicker deposits reveal a Tindalleffect and the blue and the green coloured ones show a more or less distinct mosaïc of variously coloured ultramicrons. (cf. fig. 3. Plate I).

On the whole the canadabalsam keeps the ultramicrons less luminous, especially if the sublimation takes place very slowly and if during that time the glass-wall is kept at a very low temperature. Frequently it is only the Tindall-effect with occasional strong particles, which according to the image of a controllamp without gold, are chiefly dust particles.

Local heating — by keeping a hot glass-rod against the back of the glass — effects, as with silver, a marked change. The green-blue precipitate becomes red, with still intenser heating the centre becomes yellow with a green reflex and red towards the edges, violet becomes red and with intenser heating yellow. Generally the colour flows back and the lightabsorption becomes slighter. Ultramicroscopically a very strong mosaic arose in the heated part. When pushing the object under the microscope from the non-heated part to a heated place, we see the mosaic appear more and more distinct and strong.

In places where originally the precipitate was optically non-soluble, the ultramicron-mosaic was brought to development by means of the heating.

Hence we observe with the gold as well as with the silver a conglomeration due to the heating, a contraction of an originally homegeneous film, in which very small microns probably already existed, into larger particles.

If we strongly heat a blue-green gold-precipitate (with a thin flame against the back of the glass), then the hottest spot will turn red with a gold reflection-colour, the colour roundabout being very light to a somewhat darker yellow ; round this yellow colour it turns blue again. Ultramicroscopically it appears that in this strongly-heated place not much more conglomeration has set in. There is still a similar mosaic as in the less intensely-heated places. In this spot the gold is strongly fixed to the glass and as in this spot the glass has been heated so as almost to get into a soft state, it seems likely that the gold is diffused in it, partly solved. But this diffusion cannot be very intense, for with aqua regia everything already gets dissolved and only colourless glass remains.

§ 6. *Platinum.*

With evaporation it forms a more or less dark, greyish-black precipitate, no selective absorption being visible. As the immersion-liquid can loosen the precipitate from the wall, it was once more covered in vacuum with Canadabalsam.

Ultramicroscopically nothing is to be seen, except some very dispersed, highly luminous large particles and a fairly large number of weaker separate particles which may originate in the platinum, but as appears from the experience with gold and silver they cannot possibly be the bulk of the sublimated *Pt.* Only exceptionally do we notice in between a weak Tindalleffect, which is distinctly visible in some places only, where owing to the shorter distance between filament and wall a thicker precipitate had arisen. *Hence the Pt precipitate is optically insoluble.*

If we strongly heat the back of the glass with a thin flame, so that the glass becomes soft, then the greyish precipitate on the heated spot becomes yellowish-brown. (The heating was effected in the air whereas the *Pt* was covered by a layer of CaF_2). Around it was a broad, annular zone, which was of a considerably lighter grey than the surrounding field of non-heated platinum.

Under the ultramicroscope it appeared once more that the heating had effected the formation of ultramicrons. [1])

Starting from the strongest-heated part to the non-heated part we could observe very satisfactorily the transition: very strong network,

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[1]) Heating glass + CaF_2 alone, causes no change in the CaF_2.

weaker network, Tindalleffect, extremely weak or no Tindalleffect at all.

The network is not always uniform in the strongly-heated parts. There are sometimes more and less luminous sections, while occasionally the ultramicrons no longer coalesce, but are broken up into distinctly separate larger particles. This partly depends on the local thickness of the Pt-layer, partly also on the fact that the glass-wall has been heated to a state of softening.

§ 7. *Tungsten.*

The precipitate formed at room temperature is altogether optically insoluble and uncommonly proof against a change in temperature. If the vacuum is well-maintained, heating up to 380° does not effect a microscopically perceptible change and ultramicroscopically at the utmost a weak Tindall-effect is observed. Distinct conglomeration only sets in when the glass is heated up to its melting-point by means of a thin flame. If instead of the easily melting lead-glass, we used a balloon of ordinary high-fusible glass, we observed that in this case as well, an appreciable lightning of the ultramicroscopic appearence only set in, when the glass had been heated to softening. So the cause of the conglomeration must not be looked for in the greater mobility of the tungsten particles, but in a shifting of the subsoil. Hence the ultramicroscopic figure was quite different from that with *Au* and *Ag*. Instead of a regular mosaic we see a conglomeration with fibrous structure, that somewhat reminds us of a wrinkled-up film.

§ 8. *Molybdenum.*

As with *W* and *Pt* it forms a colourless precipitate from greyish to black. Ultramicroscopically we see a great many but slightly luminous separate particles, which have a tendency to unite into small wreaths of 10 or 20 or into longer rows. Especially when the precipitate is not protected by Canadabalsam these groupings become very marked; but when the precipitate, by means of careful sublimation is formed in a high vacuum, being then covered with CaF_2 and Canadabalsam, or Canadabalsam only, they occur as well. A connected network between the separate particles is not to be seen.

A similar image became visible, when during the entire time of burning the lamp was immersed in liquid air.

If a lamp in which a very dark precipitate had formed, was heated to 380°—400° when still in vacuum, being then covered with Canadabalsam, a very strong Tindall-effect became apparent in the field between the separate particles, but no visible network as yet.

By local heating up to the melting-temperature of the glass a strongly luminous fibrous conglomeration was formed, but no network. The somewhat less intensely-heated parts, where the glass had not been deformed, again presented a field full of separate particles, among which a strong Tindall-effect was noticeable, (cf. fig. Plate I).

Compared with tungsten and platinum, molybdenum shows a rather marked tendency to coalesce into separate particles or even accumulations of particles. Apparently this is even the case at a temperature of liquid air. But it is highly probable, that under these circumstances a homogeneous sublimate arises first, and the separate particles are only formed when heated to room temperature, with or without the co-operation of Canadabalsam.

As appears from the entering of a Tindall-effect during intense heating, the separate particles do not represent all the molybdenum. A predominant part is once more optically insoluble, and it seems that even under intense heating it can only convert itself into very small ultra-microns and can either not at all or but very partially coalesce with the cores already existing.

§ 9. *Nickel.*

This again forms a non-coloured, black sublimate. On account of the risk of oxidation, in this case, too, the precipitate was covered with Canadabalsam when still in vacuum. Apparently this causes no change.

Ultramicroscopically a rather black and consequently an already fairly thick precipitate, formed at room temperature, displayed, a field pretty full of extremely fine, weak separate particles, contracted here and there to somewhat larger conglomerates, around which the smaller particles are absent. Another lamp which had been immersed in liquid air during the time of burning gave a similar image. No connected mosaic such as with *Au* and *Ag* exists.

With metals such as nickel, iron, molybdenum great care had to be devoted to the exhausting of gases. They were carefully "burned out on the pump". [1])

§ 10. *Iron.*

A precipitate formed by very slow sublimation (total duration 7

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[1]) Molybdenum, evaporated in an atmosphere of nitrogen under low-pressure forms a spongy sublimate. I. LANGMUIR, J. Amer. Chem. Soc. 38, 2277 (1916).

to 8 hours) at room temperature was partially covered with Canadabalsam when still in the vacuum.

Notwithstanding the use of a tungsten auxiliary-lamp the vacuum was found to be but fairly good at the expiration of the sublimation. With access of air the tint of the greyish-black precipitate shows no modification.

In the part protected with Canadabalsam hardly anything is to be seen, no connected network and but few separate particles. The latter are often in a circular position, inside we find larger particles, which seem to have grown at the expense of the others. By the side of these we also notice bifurcated larger particles, typical initial forms of the crystalline state.

Some other lamps, in which a precipitate had been similarly formed, revealed a similar image: various separate particles, but no mosaic and even no Tindalleffect.

Finally we heated in a last lamp, and after the formation of the sublimate and when still in the vacuum, a single spot by means of a thin flame to such an extent, that the glass began to bend inwards (450°). The result was that the tint of the precipitate became much lighter. When the lamp was opened after refrigeration the heated places of the precipitate showed under the microscope a very fine and weak, but quite perfect network of ultramicrons, whereas the non-heated spots only showed many separate particles. The colour of the deflected light of the ultramicrons was, when using HUYGENS' ocular, pale-grey metallic.

Hence with iron as well it appears that the principal part of the metal-precipitate is present in an optically insoluble form, which by means of heating is brought to segregation in ultramicrons.

§ 11. *Carbon.*

Carbon, as well, gave a black, uncoloured precipitate, optically insoluble.

§ 12. *Copper.*

Here arise again beautifully-coloured precipitates, as we observed with Au and Ag; they show great similarity especially with those of Au. The thinnest layers are yellowish red, then follows red and blue, at last blueish-green and finally the reflection-colour of metallic copper sets in.

The ultramicroscopic image too, bears a close resemblance to that of gold.

There are many separate particles and in between a weak but perfectly connected mosaic. Here again local heating turns the green field into a red one and a copperreflection-colour sets in.

Another precipitate, formed by very slow sublimation (total 38 hours) and examined without the protection of canadabalsam, only revealed the separate particles and no interlying mosaic. The separate particles can but be a small part of the metal; hence the principal part is optically insoluble.

§ 13. *Cadmium.*

This shows the phenomenon which R. WOOD[1]) has already pointed out in a recent investigation, viz. that the vapour may be in an extremely supersaturated state and does not condense on the glass wall. This appears very strikingly from the following experiments: A cadmium filamentlamp stands an electric current of 0.30 Ampère without fusing off and without blackening. Then the current was decreased to 0.26 Ampère and the wall of the bulb was locally and momentarily brought into contact with wadding soaked into liquid air. In this place a precipitate soon arises and it is only in this place that the vapour-molecules further separate, so that after some interval a fairly thick metallic reflecting layer, sharp-edged with regard to the surrounding glass-wall, has formed. Finally the filament is sundered.

We conclude, that at first cadmiumvapour, developed in the bulb, was in equilibrium with the electrically heated filament, but greatly supersaturated as regards the cold wall. The vapourmolecules striking against the glass-wall were completely reflected. By local strong refrigeration the oversaturation is neutralised. The vapour-tension decreases and the equilibrium between the vapour and the heated wire is broken. The metal now rapidly sublimates towards the cold wall, until at last the filament becomes so thin, that it breaks.

Not always was the over-saturation so strong. Sometimes a condensate was formed spontaneously, even at room-temperature, but it was very irregular as regards its thickness and in some places it was altogether absent.

With transmitted light the precipitate is blue, while somewhat thick precipitates very soon reveal metallic reflections. A suchlike precipitate with metallic reflection showed in the ultramicroscope an exceedingly strong mosaic of particles with great wealth of colour. The apparent size of these particles does not show much difference with that of silver, deposited at room-temperature.

Another lamp that had been immersed in liquid air during the

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[1]) Phil. Mag. (6) 32 (1916), 364.

W. REINDERS and L. HAMBURGER: "Ultramicroscopic investigation of very thin metalfilms obtained by evaporation in high vacuum". II.

Fig. 1.
Ultramicroscopic image of *silver* under canadabalsam. v = 450 ×.

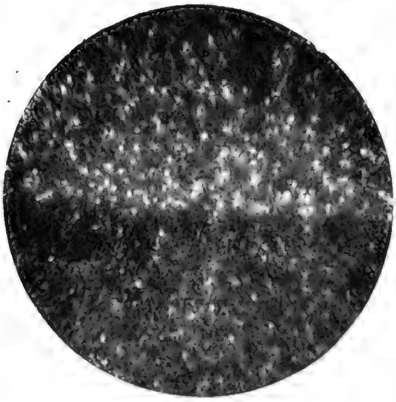

Fig. 2.
Silver; lower part protected by canadabalsam from the influence of the air, the upper part not protected. v = 800 ×.

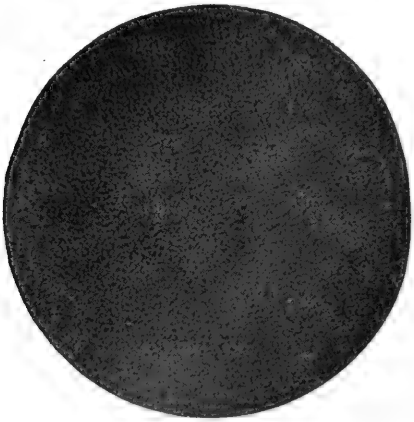

Fig. 3.
Gold. v = 450 ×.

Fig. 4.
Molybdenum (heated in vacuo).
v = 800 ×.

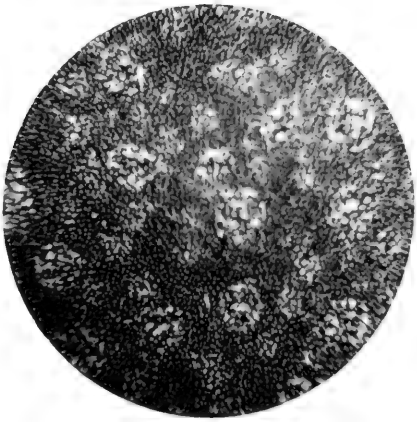

Fig. 5.
Cadmium. v = 450 ×.

sublimation produced a less thick, beautiful, blue precipitate, which was covered with Canadabalsam when in vacuum. It revealed typical conglomerates of circularly-placed particles, in the neighbourhood of which the field was exhausted, being elsewhere set with fairly large ultramicrons (cf. fig. 5 Plate I).

Cd. vapour condenses more readily on copper and on lead than on glass.

§ 14. *Magnesium.*

This too, shows the symtoms of supersaturation, which were observed with cadmium. Therefore the lamp was kept immersed in liquid air during the entire time of burning. The precipitate is highly metallic-reflecting and blue with transmitted light. Under the microscope we observe in a mosaic of weaker, for the greater part yellow-coloured particles, a number of larger separately-situated particles, which are much more luminous.

§ 15. *Zinc.*

This metal shows, like Cd and Mg, at roomtemperature symptoms of supersaturation, hence the precipitate was obtained once more at a temperature of liquid air. It was blue with transmitted light. The microscopic field was entirely occupied with extremely fine nearly invisible particles, among which were regular circles of small conglomerates. In this respect it bears a close resemblance to cadmium.

Measurements of the Electric Conductivity.

As the observations of the colour and the conclusions drawn from them concerning alterations in the thin layers of metal are accompanied by the error of subjectiveness and the drawback that these alterations cannot be expressed in figures, we tried to obtain more positive facts by measurements of the electric conductivity. These measurements have this advantage that they can be executed in the vacuum and hence enable us to become acquainted with the possible influence of the layer CaF_2 and Canadabalsam. This wish, to obtain a closer control about a possible influence of the protecting layers was the direct motive for the subsequent conductivity measurements. Our method of measuring was the same as the one Messrs. S. WEBER and E. OOSTERHUIS followed with their investigation about the connection between the electric resistance and the thickness of the layers of metal [1]. In the execution of the

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[1] These Proceedings XIX (1916), p. 597.

same we were assisted by Mr. Oosterhuis, to whom we herewith tender our thanks.

To the inside of the cylindrical glass-balloon in which later on the filament was to glow, two or more silverelectrodes were fixed. This was effected by first silvering the whole wall and then removing part of the silver with nitric acid. The wall was then carefully cleansed, the support with the evaporation-filament was inserted, the balloon was evacuated and the filament was brought to a state of incandescence.

Gold.

By evaporation in vacuo we obtained a sublimate the resistance of which was 119 Ohm. This layer proved not to be quite constant. The resistance decreased slowly in vacuo, more rapidly with access of air and by contact with glycerine and water as the following numbers will indicate:

	in vacuo					air admitted					
time	0	0,5	24	48	h.	0	3	6	24	48	h.
resistance	119	112	99	95	Ohm.	99	83	80	74,5	72,4	Ohm.

	glycerine admitted				water added		
time	0	24	72	h.	0	24	h.
resistance	71,8	66,4	64,7	Ohm.	60,6	58,0	Ohm.

By covering with CaF_2 and Canadabalsam the layer of gold becomes very constant against the influence of the air, as the following experiment will be seen to bear out.

A steel-blue gold precipitate, formed while the whole lamp was immersed in liquid air, at roomtemperature obtained a resistance which was first 250, after $1\frac{1}{2}$ hr. 211, after 24 hrs. 194 Ohm.

After covering with CaF_2 $W = 181$ Ohm.

After covering with Canadabalsam 187, two days later 189 Ω.

After entrance of air 185, 186,5 and finally 187 Ohm. By all these operations the resistance does not materially alter.

When a precipitate has been formed at a temperature of liquid air and is then brought at room-temperature, a marked non-reversible decrease of the resistance sets in. With the lamp as described above it decreased from 2000 to 250 Ohm. With another lamp which had been kept immersed in liquid air for 24 hrs and in this situation had a constant resistance of 40.000 Ohm, it decreased by heating to room-temperature to 6400, to increase once more, when in liquid air to 6800. After heating to room-temperature the resistance was very unstable, first 5800, later on 12.000 Ohm and the next day ∞.

Silver.

The precipitates of silver are even less stable than those of gold.

Example: In a lamp, immersed in liquid air, the sublimation was continued until the colour of the precipitate was blue-violet and the resistance 1000 Ohm. It was not constant, however, but fell slowly; after 1 hour 835, after 7 hrs 768, after 14 hrs 758 Ohm. Heated to roomtemperature it first fell to 161 Ohm, but then rose again and after two days it had become ∞. [1]) Once more we now sublimated at roomtemperature till the resistance was 14.800 and did not appreciably alter in a period of two hours. Covered with a sublimate of CaF_2 it fell to about 11.000 Ohm, to rise again after one day to 15.880. By spreading with Canadabalsam it became 13.520, after two hours 20.620, then rose owing to special circumstances [2]) to 120.000 and after 24 hours to 159.000. The vacuum was now interrupted and air was admitted into the lamp. The resistance rose somewhat, to 165.000, then it rose slowly, so that after 3 hours it was 185.000 and after five days 335.000 Ohm.

The conclusion to be drawn from this measurement is firstly that the layers of silver are very unstable and secondly that by applying a protection of CaF_2, and of Canadabalsam they are not materially influenced, whereas these layers make a very good protection against the influence of the air, at least in the beginning.

Platinum.

Thin layers of this metal are much more stable than those of Au and Ag.

A precipitate formed at a temperature of liquid air had a resistance of 872.5 Ohm, 2 hrs later 875.7 Ohm. Continually keeping it at about — 185°, CaF_2 was now sublimated over the metal; resistance 876.6. Hence it remains constant.

By heating at roomtemperature the resistance first decreased to 673 and then to a limit value of ± 384 Ohm. Then after thawing we brought from a side-tube that had all the time been kept at — 183°, Canadabalsam on the precipitate. The resistance remained 386 Ohm and after 24 hrs it still had the same value. With breaking of the vacuum and with the entrance of air the resistance suddenly decreased to 360, and then continued constant.

This lowering may be attributed to the fact, that part of the

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[1]) The colour of the silver-sublimate appeared to have become much lighter.

[2]) Probably because in the same room Tesla-discharges were effected; cf. among others of J. Clay. (These Proceedings 14 (1911) 126).

metal had not been covered with Canadabalsam and that in that place the moist air may have influenced.

Tungsten.

This metal gives the most constant layers. A precipitate formed at the temperature of liquid air and kept at it, had a resistance of 5330 Ohm, which decreased after some hours to 5310, but then remained constant for 10 hrs.

Brought to roomtemperature, the resistance fell to 300 Ohm, again in liquid air 3618; at roomtemperature 2662, after 24 hrs 2627. The resistance-changes caused by modifications of temperature, are here too non-reversible, but the deviations are not nearly so great as with the former metals.

After applying a layer CaF_2, 2110 Ohm (constant).

After covering with Canadabalsam 2282.

After breaking the vacuum and entrance of air 2297, 24 hrs later 3100, 120 hrs later 12.000 Ohm.

Practically the metal is altogether protected from the influence of the air by the two protecting layers CaF_2 and Canadabalsam, at least in the initial stage. In the long run the resistance rises again, probably owing to oxidation. [1])

On the whole the resistance-measurements show, that a layer CaF_2 and Canadabalsam do not alter the underlying metal, and that they give a very good protection against the atmospheric influence. With our ultra-microscopic observations we must have seen the unaltered form of the metal-layer that had been sublimated at roomtemperature.

The precipitates formed at a temperature of liquid air, when heated at roomtemperature, indicate a non-reversible change of resistance, which is considerable, especially with Ag and Au.

Though many causes for this change may be supposed, it does not seem unlikely that it must be attributed to a change in the finer structure of the precipitate, so that we observe in the ultra-microscope at room-temperature a different formation than the one formed originally at about — 180°.

Summary and Discussion of the Results Obtained.

A. Structure of the precipitates.

On comparing the various elements we investigated, we are struck

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[1]) We may remark in passing, that also in the case when the wall of the bulb is immersed in liquid air, the tungsten being deposited on the silver-electrodes during the evaporation of the tungstenwire, forms with the silver a bronze-brown product, an indication for the formation of an alloy.

by the fact that the high-melting elements W, C, Mo, Pt, Ni, Fe, when condensing on the glass-wall in vacuo [1]) produce a sublimate that is either altogether optically non-soluble, or for a considerable part optically non-soluble, and consisting for another part of distinctly separate particles. The lower-melting elements with a greater vapour-tension Au, Ag, Cu, Mg, show on the other hand a far greater tendency to coarser condensation and form a connected network of ultra-microns.

Of the first group the highest-melting elements W, C and Pt and also iron produce an optically insoluble precipitate. Of the larger particles, which could then still be observed in the visual field, it is uncertain whether they were metal particles and highly probable that they were dust particles or impurities of the glass wall. The other metals of this group — Mo and Ni — produced distinctly separate particles.

Of the metals of the second group Ag, Au and Cu offered a more or less distinct network and in between also larger separate particles. Mg, Zn and Cd showed on the glass wall at room temperature local condensation and growth on account of the supersaturation of the vapour, caused by reflection against the glass wall. The condensate made to adhere to the glass-wall at a temperature of liquid air, offered, when examined at room-temperature, a coarse heterogeneous appearance in which we could ascertain an arrangement of the particles (which had probably set in during the heating at room-temperature).

So we can conclude: *in proportion as the temperature of incandescence required for the slow sublimation, is higher, does the sublimate formed assume a finer structure.*

The temperature of the glass-wall, on which the precipitate settles, has a great influence on the nature of the same. Owing to a very low temperature the deposits of Ag, Au and Cu become entirely or at any rate for the greater part not optically soluble, whereas Zn, Mg and Cd are obtained as uniform deposits, which ultra-microscopically reveal the mosaic-structure.

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[1]) When not working with a sufficient vacuum and when the wall on which the metal condenses is not brought to a sufficiently low temperature, the layers will generally not be formed in a homogeneous state. With the numerous investigations relating to constants of thin layers of metal, the latter do not correspond to the condition, in which those who examined them, imagined them to be. Constants of homogeneous layers of metal, the thickness of which is slight with regard to the wave-length of the light, have in our opinion not yet been satisfactorily determined.

By means of heating, an already formed and optically insoluble precipitate obtains distinct heterogeousness and mosaic-structure. This could be observed particularly well with Ag, Au, Pt, Fe and Wo. The conduct of molybdenum is somewhat divergent, because here the particles show an inclination towards arrangement into larger conglomerates, circles or straight lines, making the impression of inchoating crystallization.

With the lower melting metals — Cd, Zn, Mg — as well higher condensation temperature results in a coarsening of structure. If we compare the condensates, which KOHLSCHÜTTER and EHLERS [1]) obtained with Zn, Cd and As, with those originating during our experiments, it will appear that the latter are ever so much finer. With the experiments of KOHLSCHÜTTER and EHLERS the metal was one-sidedly heated in an evacuated tube of hard fusible glass or of quartz in an electric furnace and the condensate that was deposited in a colder part of the tube, was examined. It stands to reason that the temperature of the glasswall in this colder part is far above room temperature, while with our experiments the glasswall was refrigerated in liquid air. K. and E. obtained distinctly separate particles, from 0.01 to 0.05 mm. diameter, whereas with our experiments the dimensions were ten and even more times smaller. Otherwise they too determined with these metals the circles of larger particles which had grown at the expense of the neighbouring smaller ones.

Mindful of the condensation-temperature we can enlarge the above mentioned conclusion and formulate it as follows: *The structure of the deposit formed is coarser in the same proportion as with the vapour tension of this metal, at the temperature of the wall, against which a metal vapour condenses, is greater.*

The succession in which this coarsening is observed is as follows: 1. the precipitate is optically insoluble; 2. the precipitate is for the greater part optically insoluble but also shows larger separate particles; 3. instead of the homogeneous, optically insoluble part a connected network of more or less strong ultramicrons sets in; 4. the ultramicrons of the network become larger and more separate; 5. the smaller particles are distinctly separate and occasionally show conglomeration, inchoating crystal-structure or the growth of some larger ones at the expense of surrounding smaller ones.

Let us now test our experiments to the observations of KNUDSEN [2])

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[1]) Z.f. Elektrochemie **18** (1913) 373.

[2]) Ann. der Physik. (4) **50** (1916) 472.

and LANGMUIR [1]) on the condensation and reflection of metal vapours. KNUDSEN assumes that reflection of metal vapour against the glass wall is only possible when above a certain critical point and that below this temperature the colliding molecules are not reflected, but remain fixed to the place where they strike the glass wall.

Our observations concerning the influence of heating on already existing deposits point out, that this view cannot be the right one. Far below the critical point, accepted by KNUDSEN the particles in a very thin layer show very marked mobility, enabling them to agglomerate into optically perceptible microns (experiments with silver). Hence a difference must be made between reflection against the glass wall and mobility of the particles in the very thin layer of metal. The latter continues to exist at a much lower temperature than the former.

LANGMUIR (l.c. 2253) observes as well that with irreversible sublimation, during which process the glass wall had such a low temperature that no perceptible evaporation is possible, a sintering- or crystallisation-process could take place.

LANGMUIR (l.c. 2250) looks upon the collision of metalvapour-molecules against solids such as glass as altogether non-elastic and on this ground supposes pure reflection of metalvapour against the glasswall to be exceptional. But the experiments bearing on the supersaturation of Cd, Mg and Zn vapour indicate that such a reflection is quite possible and may even give rise to a considerable supersaturation of the metalvapour.

B. Colour of the metal-deposits.

W, Mo, C, Fe, Fi, Pt produce colourless deposits; the other metals produce a coloured film, which especially with Ag, Au and Cu can boast a great wealth of colour.

This colouring capacity does not directly coincide with the structure of the metal. On the one hand Au, Ag and Cu show both in their deposits with mosaic-structure, being formed at room temperature and in the structureless deposits being formed at a temperature of liquid air, the same colour and the same sequence of colours, on the other hand no colouring sets in with Pt, W, Fe, when the structureless deposit of this local heating is brought to a mosaic structure.

It follows that the capacity of displaying colour is an individual

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[1]) Journ. Amer. Chem. Soc. **38** (1916) 2221.

property of the metal and is determined by the selective absorption of the atoms.

We should like to acknowledge the valuble assistance rendered by Mr. R. D'HOLLANDER in helping us to prepare many of the elaborate experiments.

Delft, Anorg. and Physic.-chemical Laboratory
of the Technical University.
Eindhoven, Lab. of Philips, "Metal Filament Lamps" Works Ltd.

Physics. — "*On the measurement of very low temperatures.* **XXVII.** *Vapour-pressures of hydrogen in the neighbourhood of the boiling point and between the boiling point and the critical temperature*". (Continued). By P. G. Cath and H. Kamerlingh Onnes. Communication N°. 152*a* from the Physical Laboratory at Leiden.

(Communicated in the meeting of September 29, 1917).

§ 4. *Results.* The following table gives a summary of the results obtained. The temperatures have been reduced to the absolute scale using the corrections which are deduced from the Leiden determination of the compressibility of helium [1]). Our thanks are due to

TABLE I.

Vapour-pressure of hydrogen
between its boiling point and critical temperature.

Nr.	θ	T	p (in cms)	p (in atm.)	ΔT (calc.-obs.)
I	−248°.50 K.	24°.59 K.	219.92	2.8937	+0.05
II	247.48	25.61	267.99	3.5262	−0.04
III	246.65	26.44	319.80	4.2080	+0.02
IV	245.87	27.22	370.65	4.8770	+0.02
V	245.80	27.29	376.30	4.9524	+0.03
VI	245.68	27.41	384.32	5.0566	+0.02
VII	244.70	28.39	475.46	6.2561	+0.25
VIII	243.22	29.87	601.52	7.9148	+0.17
IX	241.73	31.36	748.42	9.8476	+0.05
	241.07	32.02	822.6	10.825	+0.03
	240.49	32.60	893.1	11.752	+0.01
	−240.16	32.93	936.4	12.322	0.00

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[1]) Leiden Comm. N°. 102*b*. These Proceedings. X, p. 589.

Mr. J. M. Burgers for the care bestowed on the regulation of the temperatures.

The pressures [1] are given in international cms mercury of 0° C. — the reduction of the mercury columns which were read on the brass scales being made with 0.000163 as the coefficient of expansion — and in international atmospheres respectively, the international atmosphere at Leiden being taken equal to 75.9488 cms mercury at 0° C. The table also contains the results in the neighbourhood of the critical point as found in Comm. N°. 151*c*.

As regards the independence of the condensation-pressure from the quantity of liquid condensed the following data may suffice:

TABLE II.

T	p (in atm.)	quantity
31°.36 K.	9.8476	little liquid
31.35	9.8431	much liquid
27.22	4.8770	little liquid
27.22	4.8759	much liquid

These data refer to two determinations and the differences observed in both cases do not betray any systematic deviation in the direction as would have to be expected, if the hydrogen were impure.

In the representation of the observations by a formula we were able to avail ourselves of Professor Verschaffelt's kind assistance, who succeeded in obtaining a satisfactory agreement with the observations by means of the relation

$$T\ \log p = -56.605 + 3.8015\,T - 0\ 10458\,T^2 + 0.003321\,T^3 - 0.0000510\,2T^4.$$

(p being expressed in international atmospheres [2]).

The deviations reduced to degrees are larger, however, (especially in VII and VIII) than we had a right to expect in view of the arrangements of the measurements. As it concerns deviations not only from the formula, but from a smooth curve, we must assume in the meantime that in VII and VIII some unexplained experimental error lies at the bottom of the disagreement.

§ 5. *Determination of the boiling point of hydrogen.* In order to connect our determinations to those of Comm. N°. 137*d* we have made

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[1] Leiden Comm. N°. 44. These Proceedings. I, p. 213.

[2] According to this formula the critical pressure (at $T_k = 33°.18\ K$) is found to be 12.75 int. atm. In Comm. N°. 151*c* 12.80 atm. was found.

two series of determinations A and B using the same vapour-pressure apparatus and the same cryostat as were used by **Kamerlingh Onnes** and **Keesom**, B falling in the immediate neighbourhood of the boiling point. The results were as follows:

TABLE III.

Vapour-pressure of hydrogen in the neighbourhood of the boiling point.

Nr.	θ	T	p (in cms)	p (in atm.)	ΔT (calc.-obs.)	Kamerlingh Onnes and Keesom
A_1	— 252°.47	20°.62 K.	81.094	1.0670	— 0.01	
B_1	252.52	20.57	80.336	1.0571	+ 0.01	
B_2	252.59	20.50	78.532	1.0333	0.00	
B_3	252.69	20.40	76.224	1.0030	0.00	
		20.39	76.000	1.0000	0.00	20.35
B_4	252.88	20.21	72.876	0.9589	+ 0.04	
A_2	254.06	19.03	50.227	0.6609	+ 0.02	19.01
A_3	256.15	16.94	23.251	0.3059	0.00	16.93
A_4	257.73	15.36	11.402	0.1498	0.00	15.35
A_5	— 258.89	14.20	6.180	0.08132	+ 0.01	14.2

The two series A and B were made with two different helium-thermometers: the satisfactory agreement between the two may be taken as a guarantee of the accuracy obtained.

The fifth column shows the deviations from the formula given in § 4 reduced to degrees; the sixth column gives the measurements by **Kamerlingh Onnes** and **Keesom** of Comm. N°. 137*d* corrected in a manner which will be explained presently.

From the data of Table III it follows by interpolation in the table that the boiling point is $T = 20^\circ.39$ K. In Comm. N°. 137*d* 20°.33 K. was found. To explain this difference we may utilize the reading of the thermometer Pt'_I which in these measurements was placed in the bath of liquid hydrogen together with the vapour-pressure tube. In the investigation by **Kamerlingh Onnes** and **Keesom**, who determined the temperatures by means of Pt'_I one of the readings gave

$$p_{co\ddot{e}x} = 78.91 \text{ cm.} \quad W_{Pt I'} = 1.928\ \Omega,$$

whereas from our measurements we find by interpolation

$$p_{co\ddot{e}x} = 78.91 \text{ cm.} \quad W_{Pt I'} = 1.9304\ \Omega$$

as 0.00115 Ω corresponds to 0.01 of a degree, it follows that the difference KAMERLINGH ONNES and KEESOM — CATH and KAMERLINGH ONNES amounts to only .02 of a degree [1]). In this we approach the probable error of the measurements.

In estimating the difference of the boiling point given in Comm. N°. 137*d* with the value now obtained by us, we have to take into account the following circumstance: in Comm. N°. 137*d* the calibration of Pt'_I is carried out with a hydrogen-thermometer described in Comm. N°. 141*a*, whereas our measurements are based on the helium-thermometer. Reducing this calibration to the readings on the helium-thermometer which was in the same bath during the calibration of the resistance in Comm. N°. 141*a*, the temperatures in the hydrogen-range (comp. Table I in Comm. N°. 141*a*) have to be raised by .02 of a degree. The value of 20.33 K found in 137*d* has thus to be replaced by 20.35 K. All the values given by KAMERLINGH ONNES and KEESOM, which are contained in column 6 of Table III have been corrected in the same manner.

The difference of 0.04 of a degree which thus remains near the boiling point is larger than might have been expected in view of the accuracy of the separate observations. With the exception of

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[1]) In Comm. N°. 137*d* § 4, note 2, p. 42. KAMERLINGH ONNES and KEESOM, compare their measurements on the vapour pressure of hydrogen with those of TRAVERS and JACQUEROD. To this end KAMERLINGH ONNES and KEESOM recalculate the results which these observers obtained with the aid of a hydrogen thermometer [the most prominent is the boiling point of hydrogen, 20°22 from the absolute zero on their hydrogen thermometer (20°.41 from the absolute zero on their heliumthermometer)] and make use to this end of the pressure coefficient of hydrogen 0,0036628 for the international hydrogen thermometer of 1000 mm. pressure at 0° C, and of the correction to the KELVIN-scale according to § 2 of Comm. N°. 137*d* for the pressure of 953 mm. at 0° C. An error having been found by KAMERLINGH ONNES and KEESOM in their recalculation we give here the table as it ought to be according to them; introducing at the same time in the second column of it the correction which we applied to the observations of K. O. and K. in table III above.

p internat. cm of mercury	*T* KELVIN-scale K. O. and K.	*T* KELVIN-scale (*T* and *I*)
76	20°.35 K.	20°.40 K.
35	17.98	17.97
10	15.09	15.14

(Note added in the translation).

this difference at the boiling point the deviations which remain between our investigation and that of 137*d* are none of them more than 0.01 of a degree and 0.02 of a degree. It seems that the deviation at the boiling point consists of two parts, viz. the deviation of 0.02 of a degree in the determination of the pressure of hydrogen with the same reading of the platinum thermometer Pt_I (or others which give the same result) and another deviation also of 0.02 of a degree in the calibration of the platinum thermometer. It is therefore quite possible that accidentally in the case of the boiling point an unfavourable concurrence of circumstances has produced a deviation which is larger than the probable error. In our series of measurements a deviation of 0.04 of a degree from the smooth curve appears to occur at B_4; this reading must therefore be looked upon as suspect and as possibly containing an experimental error.

Taking into account the observation made at the end of § 4 it seems as if in the meantime the observations VII and VIII of Table I, the one by KAMERLINGH ONNES and KEESOM at 789,1 (the value obtained for 760 by intrapolation is mainly based on this observation) and B_4 in Table III have to be rejected. A renewed set of measurements over the whole range remains desirable in order to arrive at the complete accuracy which the determination of the temperatures allows us to reach at present.

Physics. — *"Isothermals of mon-atomic substances and their binary mixtures. XIX. Vapour-pressures of neon between the boiling point and the critical point".* By P. G. CATH and H. KAMERLINGH ONNES. Communication N°. 152*b* from the Physical Laboratory at Leiden.

(Communicated in the meeting of September 29, 1917).

Using the same apparatus and the same method as described in Comm. N°. 152*a* (previous communication) we have determined vapour-pressures for neon between the boiling point and critical point. The pressures below 20 atmospheres were measured with the open standard-gauge of the laboratory[1]; for pressures higher than

TABLE. Vapour-pressures of neon

Number	$\theta = T - 273.09$	T	$p_{co\ddot{e}x.}$ in atm.	$p_{co\ddot{e}x.}$ calculated	O—C in degrees
	−248°.67 C.	24°.42 K.	0.4256	0.4330	+ 0.06
	248.51	24.58	0.4276	0.4476	+ 0.15
	247.49	25.60	0.5942	0.6445	+ 0.25
	246.66	26.43	0.7963	0.8350	+ 0.16
	245.88	27.21	1.0094	1.052	+ 0.14
	245.68	27.41	1.0740	1.114	+ 0.17
I	243.69	29.40	1.888	1.895	+ 0.01
II	241.77	31.32	2.980	2.984	+ 0.01
III	240.25	32.84	4.173	4.136	— 0.04
IV	236.82	36.27	7.970	7.909	— 0.05
V	233.60	39.49	13.213	13.300	+ 0.04
VI	231.71	41.38	17.428	17.474	+ 0.01
	229.26	43.83	24.305	24.25	— 0.02
	−228.66	44.43	26.049	26.09	+ 0.01

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[1] Leiden Comm. N°. 44. These Proceedings I, p. 213.

20 atmospheres we used the closed hydrogen-manometer M 60 [1]).

We are much indebted to Mr. J. M. BURGERS for the care bestowed on the regulation of the temperatures.

The data in the table which contains the results of these measurements (I—VI) have been reduced to the absolute scale according to the corrections for the helium-thermometer, as deduced from the Leiden determinations of the compressibility of helium [2]). The table also contains the values given by KAMERLINGH ONNES and CROMMELIN [3]) and those which are due to KAMERLINGH ONNES, CROMMELIN and CATH. [4]) Our measurements form the immediate continuation of the latter research.

The observations may be represented by a formula with three constants of the form

$$T \log p = A + BT + CT^2,$$

$$\text{where}\quad A = -\,65.061$$
$$B = \quad 2.8191$$
$$C = \quad 0.01118,$$

for which we are indebted to Professor J. E. VERSCHAFFELT [5]).

The agreement with the observations may be judged from the 5th and 6th column [6]).

At some of the temperatures readings were taken with larger and smaller quantities of liquid in order to test the influence on the vapour-pressure of possible impurities of the neon used. It appears from these measurements that the purity of the neon was not as high as we should have wished, although it had repeatedly been

$T = 43°.83$ K.	
$p_{coëx.}$ in atm.	quantity of condensed gas
24.240	12 c. c.
300	226 „
355	393 „
409	550 „

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[1] Leiden Comm. N⁰. 97*a* (§ 3). These Proceedings IX, p. 754.

[2] Leiden Comm. N⁰. 102*b*. These Proceedings X, p. 589.

[3] Leiden Comm. N⁰. 147*d*. These Proceedings XVIII (1) p. 515.

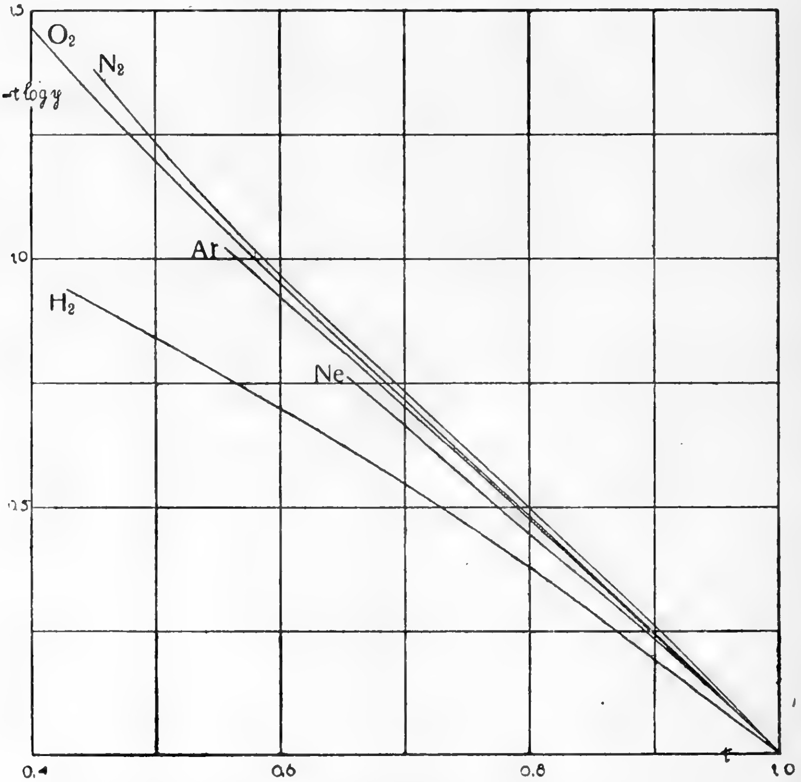
[4] Leiden Comm. N⁰. 151*b*. These Proceedings XIX (2) p. 1058.

[5] According to this formula the critical pressure (at $T = 44.74$ K.) would be $p_k = 27.11$ int. atm., as against 26.86 found in Comm. N⁰. 151*b*.

[6] The formula was chosen so as to give a good correspondence with the observations beyond 29°.40. At the lower temperatures the correspondence is not so satisfactory.

distilled from charcoal cooled in liquid air. The equilibrium-pressures which were obtained at $T = 43°.83$ K are given in the above table. It will be seen that pressures show a not inconsiderable systematic rise as the quantity of condensed gas increases.

As the nature of the impurity is still uncertain it is impossible in the meantime to apply corrections to the pressures observed, on the basis of the formulae given by Keesom [1]) for the change of the vapour-pressure by small admixtures.



In the accompanying diagram are given the reduced vapour-pressure curves for oxygen, nitrogen, argon, neon, and hydrogen, all according to the Leiden-observations. It is very striking that the curve for hydrogen — which at these temperatures behaves as a mon-atomic substance — deviates proportionately so much more strongly from that of neon than the latter from that of argon, although the critical temperature of neon is relatively low. This raises the question whether an influence of the "quanta" may play a part in these deviations. For this influence would be much smaller for neon owing to its higher molecular weight than for hydrogen.

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[1]) Leiden Comm. N°. 79. These Proceedings IV, p. 659.

Physics. — "*On the measurement of very low temperatures. XXVIII. Comparison of the platinum and the gold resistance thermometers with the heliumthermometer*". By P. G. CATH, H. KAMERLINGH ONNES and J. M. BURGERS. Communication N°. 152*c* from the Physical Laboratory at Leiden.

(Communicated in the meeting of Sept. 29, 1917).

§ 1. *Introduction.*

The measurements of the galvanic resistance of gold and platinum communicated in Comm. Nrs. 99*a*, 99*b*, 141*a* extend over all the temperature-ranges which could be covered by means of cryostats filled with liquefied gases. The temperatures from 55° K. to 27° K. as falling outside those ranges could not be investigated. Still it is exactly in this region that the temperature change of the galvanic resistance of some pure metals shows peculiarities which render a further investigation very desirable. Even from the early investigations by H. KAMERLINGH ONNES and J. CLAY [1]) it appeared that the strong change of the galvanic resistance which shows itself at hydrogen-temperatures must have its beginning in the range in question. NERNST [2]) has noticed that the difference between two platinum-thermometers expressed in their resistance at 0° C. can be approximately represented by a linear function of either of the resistances. [3])

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[1]) H. KAMERLINGH ONNES and J. CLAY, Leiden Comm. N°. 107*c*, p. 25.

[2]) W. NERNST, Sitz. Ber. Berl. Akad. 1911, p. 314.

[3]) This follows immediately from MATTHIESEN's rule. The applicability of this rule in the region of low temperatures was shown by FLEMING down to — 190° C. As regards its extension to still lower temperatures, KAMERLINGH ONNES and CLAY found that, although at the temperatures to be reached with liquid hydrogen the additive resistance is no longer exactly equal to that at less low temperatures, still the formula $r_{xT} = r_{iT} + px$ by which they represent the difference between the resistance r_{iT} of a wire of the pure or ideal material at T and the resistance r_{xT} of a wire of the same dimensions of a material containing an admixture x (p large and constant), in a *first* approximation gives a representation of the influence which small impurities have on the change of resistance with temperature (Leiden Comm. N°. 99*c*, p. 20, July 1907). Putting $r_{xT}/r_{xo} = w$ and comparing two wires I and II, we get $w_{II} - w_I = \lambda' \, (1 - w_I)$, that is NERNST's formula with $\lambda' = \dfrac{p_{II}x_{II} - p_I x_I}{1 - p_I x_I}$. Comp. note on page 1169 further on.

A relation of the second degree as used by HENNING[1]) gives a better correspondence. HOLST[2]), who tested it on an extensive experimental material, found the correspondence satisfactory above about 80° K. But at the temperatures of liquid hydrogen it fails.[3]) In order to learn the nature of the difference in question measurements between 55° K. and 27° K. are thus of great importance, while at the same time they may open up new points of view concerning the dependence of the galvanic resistance on the temperature. By the construction of the hydrogen vapour cryostat an investigation in this range of temperatures had become a possibility, and we have therefore undertaken a series of measurements for the purpose.

We have extended our results by the addition of a number of measurements outside the range in question, by which we are in a position to communicate data concerning the resistance of gold and platinum running without any intermission through the temperature-range from 15° K. to 273° K.

§ 2. *Apparatus.* For the arrangement of our measurements between 27° K. and 55° K. we refer to Comm. N°. 151*a* fig. 1, which shows the gas-thermometer and resistances which were used mounted in the vapour cryostat. The measurements outside this range were made in a cryostat which did not differ much in its arrangement from forms which were described on previous occasions.

The gold and platinum resistances were wound on porcelain tubes with a double screw-thread baked in. The platinum wires were obtained from HERAEUS as extremely pure. The gold wires were drawn in the workshops of the PHILIPS's Incandescent lamp factories through new diamonds through the kind intermediary of Dr. G. HOLST. Both to him and Dr. C. HOITSEMA, who provided us with pure minting gold for these wires, we extend our sincere thanks. The diameters of the gold and platinum wires were $^1/_{10}$ and $^1/_{20}$ of a mm. Before they were used the wires were brought into a thermally stationary condition by being alternately immersed in liquid air and glowed, a few times in succession. One of the platinum wires for which this operation had been omitted showed a considerable change in its resistance at 0° C. after the completion of the measurements; the results of the measurements with this wire were

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[1]) F. HENNING, Ann. d. Phys. **40** (1913), p. 635.

[2]) G. HOLST, Leiden Comm. N°. 148*a*, p. 8—10. These Proc. XVIII (1), p. 833—836.

[3]) W. H. KEESOM and H. KAMERLINGH ONNES, Leiden Comm. N°. 143, p. 15. These Proc. XVII (2), p. 904.

hereby rendered doubtful: for this reason they have been omitted. The temperatures were determined on the heliumthermometer which is described in Comm. N°. 151*a*. Its freezing point pressure was that of the international helium thermometer viz. 1000 mm.

§ 3. *Measurements and results.*

In general the resistances above 1 ohm were measured according to KOHLRAUSCH's method of the overlapping shunts; those below 1 ohm with a compensation-apparatus free from thermo-effects according to DIESSELHORST, by the successive compensation of the potential on a standard ohm and on the resistance which are joined in series in one circuit. The temperatures are reduced to the absolute scale by means of the corrections calculated from the helium isothermals (Comm. N°. 102*a*,*b* [1]) and using 0.0036614 as the pressure-coefficient [2]). The accuracy of the measurements may be put at .01 of a degree for the determinations above —216° C and below

TABLE I. Temperature-change of the resistance of the gold wires *Au*—11 and *Au*—12.

θ	T	W_{11}	$w_{11}=\left(\frac{W}{W_0}\right)_{11}$	θ	T	W_{12}	$w_{12}=\left(\frac{W}{W_0}\right)_{12}$
0°.00 C.	273°.09 K.	2.8845	1.00000	0°.00 C.	273°.09 K.	12.161	1.00000
– 61.87	211.22	2.1797	0.75565	– 61.87	211.22	9.1890	0.75561
84.97	188.12	1.9166	0.66443	84.97	188.12	8.0822	0.66460
102.22	170.87	1.7200	0.59628	102.22	170.87	7.2556	0.59663
130.28	142.81	1.3992	0.48507	130.28	142.81	5.9062	0.48567
145.86	127.23	1.2194	0.42273	145.86	127 23	5.1500	0.42349
164.37	108.72	1.0028	0.34764	164.37	108.72	4.2388	0.34856
183.95	89.14	0.76900	0.26660	183.95	89.14	3.2565	0.26778
205.31	67.78	0.50756	0.17596	205.31	67.78	2.1566	0.17734
208.18	64.91	0.47204	0.16365	208.18	64.91	2.0075	0.16508
216.26	56.83	0.37226	0.12906	216.26	56.83	1.5853	0.13036
222.78	50.31	0.29220	0.10130	222.78	50.31	1.24664	0.10251
236.80	36.29	0.13462	0.04667	236.79	36.30	0.58166	0.047830
240.25	32.84	0.10204	0.03538	240.26	32.83	0.44276	0.036409
243.68	29.41	0.073627	0.02553	243.67	29.42	0.32277	0.026541
245.80	27.29	0.058824	0.02039	245.80	27.29	0.25856	0.021262
252.57	20.52	0.024361	0.00845	252.57	20.52	0.11073	0.0091054
—255.01	18.08	0.017124	0.00594	—255.01	18.08	0.079576	0.0065435

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[1]) They are therefore expressed on what was called the international KELVIN scale in Leiden Comm. Suppl. N°. 34*a*.

[2]) Comp. Leiden Comm. N°. 141*a*, p. 5. These Proc. XVII (1) p. 503.

TABLE II. Temperature-change of the resistance of the platinum wires *Pt*—21 and *Pt*—26.

θ	T	W_{21}	$w_{21} = \left(\frac{W}{W_0}\right)_{21}$	θ	T	W_{26}	$w_{26} = \left(\frac{W}{W_0}\right)_{26}$
0°.00C.	273°.09K.	57.849	1.00000	0°.00C.	273°.09K.	20.430	1.00000
—23.01	250.08	52.579	0.90890	—23.01	250.08	18.564	0.90867
43.02	230.07	47.961	0.82907	43.02	230.07	16.924	0.82839
61.30	211.79	43.702	0.75545	61.30	211.79	15.415	0.75453
103.02	170.07	33.908	0.58615	103.02	170.07	11.945	0.58468
112.72	160.37	31.601	0.54627	112.72	160.37	11.128	0.54469
120.83	152.26	29.663	0.51277	120.83	152.26	10.442	0.51111
130.37	142.72	27.370	0.47313	130.37	142.72	9.629	0.47132
143.65	129.44	24.159	0.41762	143.65	129.44	8.493	0.41570
183.95	89.14	14.234	0.24605	183.95	89.14	4.9799	0.24375
187.11	85.98	13.449	0.23249	187.11	85.98	4.7018	0.23014
195.88	77.21	11.266	0.19475	195.88	77.21	3.9294	0.19233
205.31	67.78	8.9430	0.15459	205.31	67.78	3.1074	0.15210
208.18	64.91	8.2499	0.14261	208.18	64.91	2.8625	0.14011
212.05	61.04	7.3278	0.12667	212.05	61.04	2.5373	0.12419
216.26	56.83	6.3398	0.10959	216.26	56.83	2.1869	0.10704
222.78	50.31	4.8753	0.084278				
228.71	44.38	3.6739	0.063510				
229.26	43.83	3.5726	0.061757				
229.31	43.78	3.5615	0.061566				
230.00	43.09	3.4302	0.059297				
231.70	41.39	3.1247	0.054016				
233.61	39.48	2.7955	0.048324				
236.80	36.29	2.2881	0.039554				
236.84	36.25	2.2807	0.039425	236.81	36.28	0.75768	0.037087
240.26	32.83	1.8059	0.031219	240.26	32.83	0.58722	0.028743
241.77	31.32	1.6152	0.027921	241.78	31.31	0.52036	0.025470
243.21	29.88	1.4575	0.025195	243.21	29.88	0.46372	0.022698
243.67	29.42	1.4030	0.024253				
244.69	28.40	1.3030	0.022524				
245.79	27.30	1.1917	0.020618	245.79	27.30	0.37198	0.018208
252.47	20.62	0.7251	0.012535	252.51	20.58	0.2078	0.010172
252.57	20.52	0.7182	0.012415	252.57	20.52	0.2064	0.010103
252.59	20.50	0.7180	0.012412	252.57	20.52	0.2063	0.010098
254.05	19.04	0.6499	0.011235	252.59	20.50	0.2062	0.010093
255.01	18.08	0.6108	0.010559	252.68	20.31	0.2046	0.010015
256.15	16.94	0.5713	0.0098758	252.84	20.25	0.2021	0.0098924
257.73	15.36	0.5253	0.0090808	255.01	18.08	0.1706	0.0083504
—258.89	14.20	0.4974	0.0085984	—258.91	14.18	0.1340	0.0065589

—252° C. For the measurements between those limits owing to the lesser constancy of the temperature in the vapour cryostat the accuracy must not be estimated higher than .02 of a degree.

Tables I and II contain the results of the measurements of the two gold and the two platinum wires. The first and second columns give the temperatures at which the measurements were made, both corrected to the KELVIN scale. The third column contains the resistance in ohms, and the fourth its ratio to the resistance at 0° C.

§ 4. *Discussion.*

The above data which include both for gold and for platinum values of $w = \frac{W}{W_0}$ for different wires, measured at equal temperatures, may be used to investigate the laws governing the change of the differences of resistance of two wires with temperature and in particular to find over which domain and with which accuracy NERNST's and HENNING's relations hold for platinum and for gold.

A. Platinum.

Table III gives the difference $\Delta w = w_{21} - w_{26}$ between the two platinum wires *Pt*—21 (this wire is also called $Pt_{1915\,I}$ and afterwards Pt_{XXI}) and *Pt*—26 (this wire is also called $Pt_{1915\,VI}$ and afterwards Pt_{XXVI}) together with the value of $(1 - w_{26})$.

In fig. 1. Δw is represented as a function of $(1 - w_{26})$ (curve *A*). Attention may be drawn to the sudden bend which the curve shows at low temperatures. It appears from the figure that HENNING's

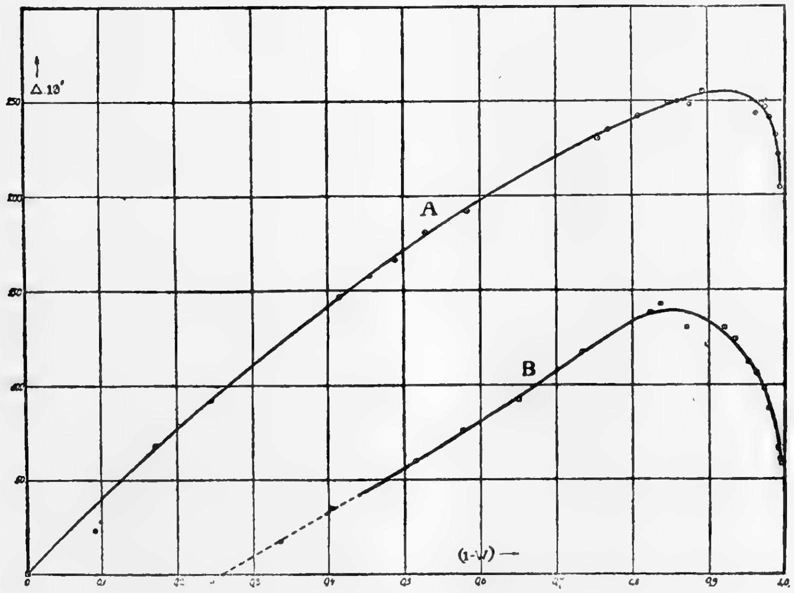

Fig. 1.

formula 1) — which expresses the difference as a quadratic function of $(1 - w_{26})$ — can be used down to

$$1 - w_{26} = ca.\ 0.92$$
$$T = ca.\ \ 50^\circ\ K.$$

TABLE III. Temperature-change of the resistance of platinum wires.

θ	T	$1-w_{26}$	$\Delta = w_{21} - w_{26}$
0°.00C.	273°.09K.	0.00000	0.00000
— 23.01	250.08	0.09133	0.00023
43.02	230.07	0.17161	0.00068
61.30	211.79	0.24547	0.00092
103.02	170.07	0.41532	0.00147
112.72	160.37	0.45531	0.00158
120.83	152.26	0.48889	0.00166
130.37	142.72	0.52868	0.00181
143.65	129.44	0.58430	0.00192
183.95	89.14	0.75625	0.00230
187.11	85.98	0.76986	0.00235
195.88	77.21	0.80767	0.00242
205.31	67.78	0.84790	0.00249
208.18	64.91	0.85989	0.00250
212.05	61.04	0.87581	0.00248
216.26	56.83	0.89296	0.00255
236.81	36.28	0.96291	0.00243
240.26	32.83	0.97126	0.00248
241.78	31.31	0.97453	0.00247
243.21	29.88	0.97730	0.00250
245.79	27.30	0.98179	0.00241
252.57	20.52	0.98990	0.00232
255.01	18.08	0.99265	0.00221
—258.91	14.18	0.99344	0.00204

---

1) F. HENNING, Ann. d. Phys. 40 (1913), p. 635 seqq.

$$\Delta = M\,(w-1) + N\,(w-1)^2 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

According to **Nernst** the curve would be represented by a straight line [1]), which apparently gives a very insufficient approximation.

A more detailed examination gives rise to the following remarks.

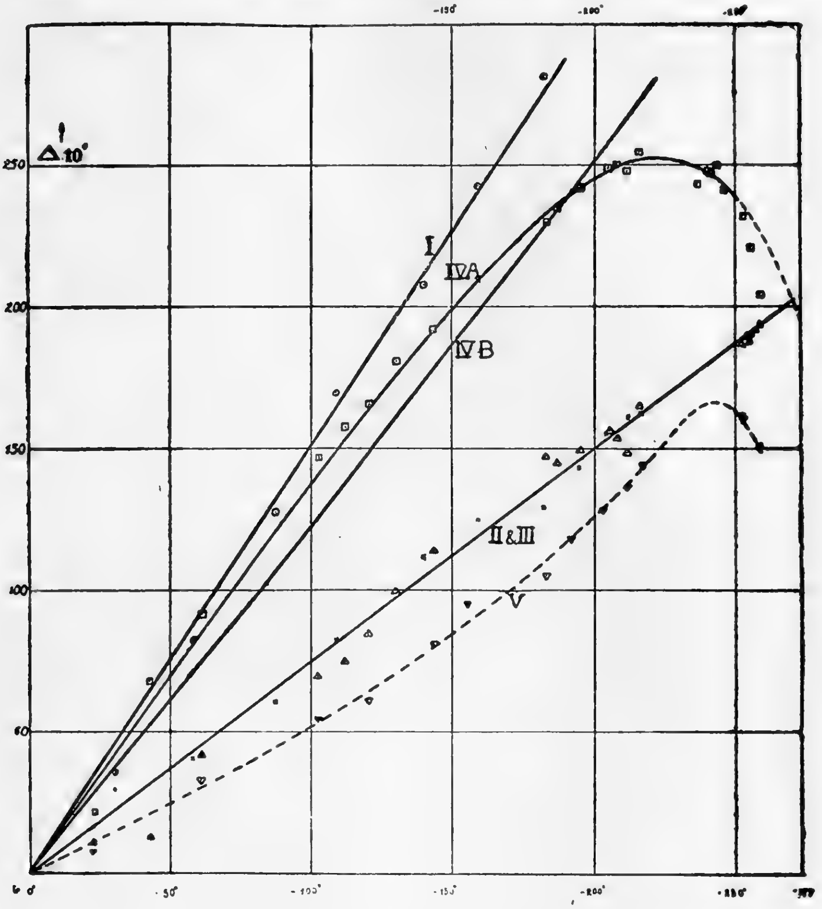

Fig. 2.

⊙ : $\Delta_{I} = (w)_{Pt-I'} - (w)_{Pt32-PTR}$

× : $\Delta_{II} = (w)_{Pt-I'} - (w)_{Pt-III}$

△ : $\Delta_{III} = (w)_{Pt-I'} - (w)_{21}$

⊡ : $\Delta_{IV} = (w)_{21} - (w)_{26}$

▽ : $\Delta_{V} = (w)_{28} - (w)_{21}$.

1). In fig. 2 the differences Δ between the following wires are plotted as a function of the temperature

(*a*) , ... $\Delta_{I} = (w)_{PtI'} - (w)_{Pt32(PTR)}$

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[1]) W. **Nernst**, Sitz. Ber. Berl. Akad. 1911, p. 314.

$$w_{II} = \frac{w_I - \lambda}{1-\lambda}\text{; therefore } w_I - w_{II} = \frac{\lambda\,(1-w_I)}{1-\lambda} = \lambda' \,.\, (1-w_I) \quad . \quad . \quad (2)$$

putting $\lambda' = \dfrac{\lambda}{1-\lambda}$ Comp. note 3 on p. 1163.

cf. J. CLAY, Jahrb. Rad. u. Elektr. XII, p. 263, 1915. (drawn on half as small a scale compared to the others).

(*b*) . . . . $\Delta_{II} = (w)_{Pt\,I'} - (w)_{Pt\,III}$

H. KAMERLINGH ONNES and J. CLAY Leiden Comm. N°. 99*b*; cf. J. CLAY l. c.

c) . . . . $\Delta_{III} = (w)_{Pt-I'} - (w)_{21}$

obtained by computing from the tables for Pt'_{I} [1]) the values of w corresponding to the temperatures at which *Pt*—21 was measured.

(*d*) . . . . $\Delta_{IV} = (w)_{21} - (w)_{28}$

(from table III).

(*e*) . . . . $\Delta_{V} = (w)_{28} - (w)_{21}$

(The platinum wire *Pt*—28 was measured at the same time as *Pt*—21 and others at the temperatures of liquid methyl chloride, ethylene, oxygen and hydrogen, but not in the range between 20° K. and 56° K.)

Some of these curves show striking points of resemblance, others equally striking differences. Three of them, viz. Δ_{I}, Δ_{II}, Δ_{III} may be represented with a sufficient approximation by a linear function of θ:

$$\Delta_{I} = -3{\cdot}03\,.\,\theta\,.\,10^{-5} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

(down to —180° C. i. e. as far as the measurements extend)

$$\Delta_{II} = -0{\cdot}748\,.\,\theta\,.\,10^{-5} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

(down to —216° C. again as far as the measurements reach)

$$\Delta_{III} = -0{\cdot}748\,.\,\theta\,.\,10^{-5} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

(down to the hydrogen region) [2])

This last result is very noteworthy: Δ_{III} does not show any sign of the bend which occurs in Δ_{IV} and Δ_{V}. It follows that this line is better represented as a linear function of θ than as a linear function of w (NERNST's formula). Compare for this: table IV which gives besides the observed value of Δ_{III} in column *A*:

$$\Delta_{A} = -0{\cdot}748\,.\,\theta\,.\,10^{-5}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and in column *B*:

$$\Delta_{B} = 189\,.\,10^{-5}\,.\,(1 - w_{Pt\,I'}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

It is also interesting to note that Δ_{II} and Δ_{III} coïncide completely (down to —216° C); the resistance of *Pt III* (Comm. N°. 99b, 1907) and that of *Pt* 21 (1915/16) thus display a similar course in this region.

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[1]) H. KAMERLINGH ONNES and G. HOLST, Leiden Comm. N°. 141*a*; these Proceedings XVII (1) p. 501. G. HOLST, Leiden Comm. N°. 148*a*; these Proceedings XVIII (1) p. 829.

[2]) In fig. 2 these differences are represented by the straight lines I, II and III.

TABLE IV. Temperature-change of the resistance of platinum wires.

$$\Delta_{III} = (w)_{Ptl'} - (w)_{21}$$

θ	$(w)_{Ptl'}$	Δ_{III}	A	B
0° C.	1·	0	0	0
— 23·01	0·90901	11.10^{-5}	17.10^{-5}	17.10^{-5}
43·02	0·82920	13. „	32. „	32. „
61·30	0·75587	42. „	46. „	46. „
103·02	0·58685	70. „	77. „	78. „
112·72	0·54702	75. „	85. „	86. „
120·83	0·51362	85. „	91. „	92. „
130·37	0·47413	100. „	98. „	100. „
143·65	0·41876	114. „	107. „	110. „
183·95	0·24752	147. „	138. „	142. „
187·11	0·23393	145. „	140. „	145. „
195·88	0·19624	149. „	147. „	152. „
205·31	0·15615	156. „	154. „	160. „
208·18	0·14415	154. „	156. „	162. „
212·05	0·12815	148. „	159. „	165. „
216·26	0·11124	165. „	162. „	168. „
252·57	0·01429	187. „	189. „	186^5. „
254·05	0·01314	190. „	190. „	187. „
255·01	0·01248	192. „	191. „	187. „
256·15	0·01178	190. „	192. „	187. „
257·73	0·01100	192. „	193. „	187. „
—258·89	0·01054	194. „	194. „	187. „

The curves Δ_{IV} and Δ_{V} both show a bend at the temperatures below about —216° C. In the region above —200° C. Δ_{IV} has its *concave* side turned downwards, Δ_{V} on the other hand its *convex* side. This difference is of some interest in connection with the formula proposed by CLAY.

$$\Delta = C\,(\alpha\theta + \beta\theta^2) \quad . \ . \ . \ . \ . \ . \ . \ . \ . \quad (7)$$

where

$$\alpha = 0\;0039164$$

$$\beta = -\,0{\cdot}009427\,.\,10^{-4}$$ ¹)

The formula is quadratic, but it contains only one parameter (C):

TABLE V. Temperature-change of the resistance of platinum wires.

θ	Δ_{IV}	A	B
0° C.	0	0	0
– 23·01	23.10^{-5}	31.10^{-5}	28.10^{-5}
43·02	68 "	59 "	53 "
61·30	92 "	85 "	74 "
103·02	147 "	140 "	126 "
112·72	158 "	153 "	139 "
120·83	166 "	163 "	149 "
130·37	181 "	176 "	161 "
143·65	192 "	191 "	178 "
183·95	230 "	233 "	230 "
187·11	235 "	236 "	234 "
195·88	242 "	242 "	244 "
205·31	249 "	248 "	257 "
208·18	250 "	250 "	262 "
212·05	248 "	251 "	267 "
216·26	255 "	252 "	273 "
236·81	243 "	250 "	
240·26	248 "	249 "	
241·78	247 "	247 "	
243·21	250 "	245 "	
245·79	241 "	244 "	
252·57	232 "	237 "	
255·01	221 "	233 "	
–258·91	204 "	227 "	

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¹) J. Clay, l. c., p. 261.

it gives a curve which has its *concave* side towards the θ-axis. It cannot serve, therefore, as a formula of reduction for *Pt*—21 and *Pt*—26 (comp. table V, column *B* on the previous page).

Δ_{IV} may be represented from 0° to —240° C. by the formula

$$10^5 \,.\, \Delta_{IV} = -1{\cdot}400 \,.\, \theta - 0{\cdot}35 \,.\, 10^{\frac{\theta}{100}} \quad . \quad . \quad . \quad . \quad (8)$$

(comp. table V, column *A* on the previous page)[1])

II. H. KAMERLINGH ONNES and G. HOLST found some time ago that the curve which represents the deviations from the linear formula

$$\theta = -243 + 243 \frac{W}{W_0} = -243\,(1 - w) \quad . \quad . \quad . \quad . \quad (9)$$

shows a pretty considerable bend in the region of the lowest temperatures which may be reached with liquid oxygen[2]). This bend is closely connected with the point of inflexion discussed in Comm. N°. 95.

As the measurements could not be extended beyond —216° C. at that time, it was impossible to study the deviation at lower temperatures. We are now in a position to supplement this investigation by giving in fig. 3 the curve showing the deviations from the linear formula for the two platinum thermometers *Pt*—21 and *Pt*—26[3]). It appears that below —220° C. the deviation becomes very considerable and the steepness of the curve shows clearly that the above linear relation cannot be used in this region for the calculation of temperatures.

III. Comparison of the wires in the temperature-region of liquid hydrogen.

The wires *Pt*—*B* (comp. Comm. N°. 119 p. 19), Pt_I and *Pt*—21 were compared with *Pt*—26 in the range from 14.2 to 20.5 K. For this purpose the following quadratic interpolation-formula was established for *Pt*—26:

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[1]) Table V contains besides the values of Δ_{IV} as given by the measurements (comp. table III) the values calculated from the exponential formula (8) (column *A*) and those calculated from CLAY's formula (7) with $C = -0.00305^8$. This value of C is computed from the value of Δ_{IV} at $\theta = -183°.95$ (in accordance with Jahrb. Rad. u. Elektr XII p. 261. 1915).

The differences between Δobs and $\Delta calc\ (B)$ rise to 0.00021, corresponding to a temperature difference of about 0.05 of a degree.

In fig. 2 the functions corresponding to the formulae (8) and (7) are represented by the curves IV *A* and IV *B* respectively.

[2]) Leiden Comm. N°. 141a, p. 9. These Proc. XVII (1) p. 506.

[3]) The curve represents

$$\Delta = -243\,(1 - w) - \theta$$

as a function of θ.

$$10^5\, w = 1059.5 - 86{\cdot}70\, T' + 4.111\, T'^2 \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The differences between the observed and the calculated values

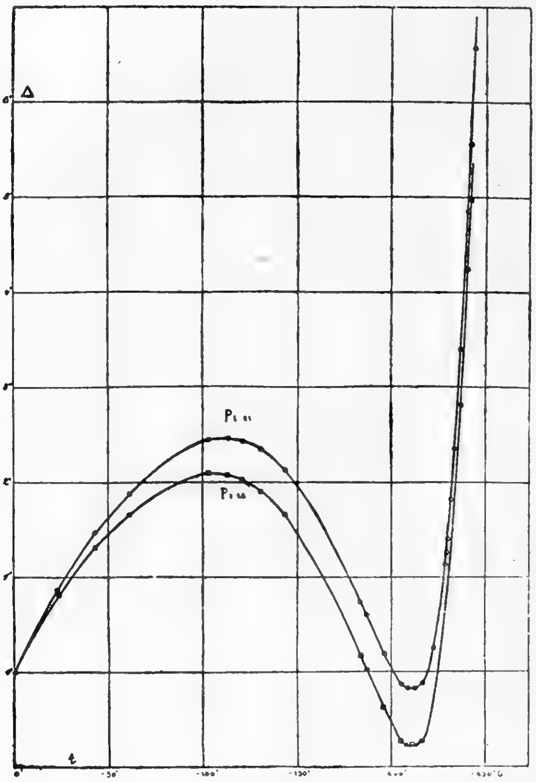

Fig. 3.

of w do not rise beyond $1.7 \,.\, 10^{-5}$, which is less than $2\,{}^0/_{00}$ of the value of the resistance (comp. table VI; in column O the value of w is given according to the measurements, in column C the value derived from (10)).

By means of this formula and of a curve of deviations w_{26} was calculated for the temperatures at which measurements had been made for the other wires. The results of the comparison will be found in table VII.

B. Gold.

Table VIII gives the difference $\Delta w = w_{12} - w_{11}$ between the two gold wires Au—11 (this wire is also called Au_A 1915 and afterwards Au_{XI}) and Au—12 (this wire is also called Au_B 1915 and afterwards Au_{XII}) together with the value of $(1 - w_{12})$. The corresponding curve representing Δw as a function of $(1 - w_{11})$ will be found as B in fig. 1.

The curve shows a bend at about 210°.

TABLE VI. Temperature-change of the resistance of the platinum wire *Pt*—26.

T	*O*	*C*	Diff.
20°58 K	1017·2.10^{-5}	1016·4.10^{-5}	+ 0·8.10^{-5}
20·51	1009·8 „	1010·6 „	— 0·8 „
20·31	1001·5? „	994·4 „	? *)
20·25	989·2 „	989·6 „	— 0·4 „
[18·45]	861·0 „	859·3 „	+ 1·7 „
18·08	835·0 „	835·8 „	— 0·8 „
[16.37]	742·5 „	741·9 „	+ 0·6 „
15·15	690·2 „	689·6 „	+ 0·6 „
14·18	655.9 „	656·7 „	— 0.8 „

*) When w_{obs} *(O)* is plotted as a function of *T* this point is found to fall outside the curve passing through the remaining points. This is presumably due to some error in the value of w_{obs}.

TABLE VII. Temperature-change of the resistance of platinum wires.

Pt—B, Comm. No. 119, p. 19.

T	*Pt—B*	*Pt*—26	Diff.
20°2 *K*	0·0171	0·0098^5	0·0072^5
14°2	0·0135	0·0066	0·0069

Pt—I', Comm. No. 141a.

T	*Pt—I'*	*Pt*—26	Diff.
20°43 K.	0·01421	0·01004	417.10^{-5}
19·45	1343	929	414. „
18·04	1244	833	411. „
17·01	1182	774	408. „
16·04	1130	728	402. „
14·70	1073	673	400. „

Pt — 21. (Comp. table II).			
T	*Pt* — 21	*Pt* — 26	Diff.
20°·52 K.	0·01242	0·01010	232.10^{-5}
19·04	1124	901	223. „
18·08	1056	835	221. „
16·94	988	771	217. „
15·36	908	699	209. „
14·20	860	659	201. „

TABLE VIII. Temperature-change of the resistance of gold wires.

θ	T	$(1 - w_{11})$	$\Delta = w_{12} - w_{11}$
0°C.	273°·09 K.	0	0
− 61·87	211·22	0·24435	− 0·00004
84·97	188·12	0·33557	+ 0·00017
102·22	170·87	0·40372	0·00035
130·28	142·81	0·51493	0·00060
145·86	127·23	0·57727	0·00076
164·37	108·72	0·65236	0·00092
183·95	89·14	0·73340	0·00118
205·31	67·78	0·82404	0·00138
208·18	64·91	0·83635	0·00143
216·26	56·83	0·87094	0·00130
222·78	50·31	0·89870	0·00121
228·71	44·38	0·92311	0·00130
231·96	41·13	0·93579	0·00124
236·79	36·30	0·95329	0·00112
240·26	32·83	0·96465	0·00106
243·67	29·42	0·97444	0·00098
245·80	27·29	0·97961	0·00087
252·57	20·52	0·99155	0·00066
255·01	18·08	0·99406	0·00060
−258·35	14·74	0·99621	0·00058

Chemistry. — *"The Allotropy of Cadmium"*. VI. By Prof. ERNST COHEN.

(Communicated in the meeting of December 29, 1917).

1. Some time ago FREDERICK H. GETMAN published a paper [1] on the allotropy of cadmium in which he describes some experiments bearing upon my investigations on this subject. However, in GETMAN's assumptions as well as in the conclusions he deduces from his careful experimental work some mistakes have crept in. In the present paper we intend to prove that if these mistakes are removed, GETMAN's results become identical with those of COHEN and HELDERMAN. As a result GETMAN's work affords a very welcome control as well as a confirmation of the results formerly got by ourselves.

2. Having found in our dilatometric experiments that there exist three (at least) different modifications of cadmium to which the names Cd_α, Cd_β and Cd_γ, respectively, were given, we stated that the transition temperature of the transformation

$$Cd_\alpha \rightleftarrows Cd_\beta$$

is to be found in the vicinity of 60°. We were unable until now to fix this temperature more closely with the dilatometer in consequence of the presence of Cd_γ.

3. GETMAN prepared in the same way as we did a number of HULETT-cells, which were constructed according to the scheme

$$\text{Cd} \;\Big|\; \begin{matrix}\text{Solution of } CdSO_4 . {}^8/_3 H_2O \\ {}^1/_2 \text{ saturated at } 20^\circ\end{matrix} \;\Big|\; \begin{matrix}\text{Cd-amalgam } x\ {}^0/_0 \\ \text{by weight.}\end{matrix}$$

Whereas x was generally 12.5 in our cells, GETMAN has chosen $x = 8$. The negative electrode consisted of a platinum spiral covered with an electrolytic deposit of cadmium. If these cells are maintained for some time at 20° they finally give a constant e. m. f. In perfect agreement with our statement GETMAN found that three different types of cells were formed in this way:

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[1] Journal Americ. Chem. Soc. **39**, 1806 (1917).

10 percent of the cells reached the constant value of 50 millivolt at 25° (COHEN and HELDERMAN'S γ-cells);

70 percent of the cells reached the constant value of 48 millivolt at 25° (COHEN and HELDERMAN'S β-cells);

20 percent of the cells reached the constant value of 47 millivolt at 25° (COHEN and HELDERMAN'S α-cells).

4. In determining the e. m. f. of these cells between 0° and 40° as a function of temperature, GETMAN found for the
α-cells:

$$E_\alpha = 0.04742 - 0.000200\ (t-25) \quad . \quad . \quad . \quad . \quad (A)$$

This is the same equation as has been given by COHEN and HELDERMAN [1]).
For the β-cells he found:

$$E_\beta = 0.04862 - 0.000201\ (t-25) \quad . \quad . \quad . \quad . \quad (B)$$

These cells have not been investigated in this direction by COHEN and HELDERMAN.

Finally GETMAN deduces for the γ-cells:

$$E_\gamma = 0.05047 - 0.0002437\ (t-25) \quad . \quad . \quad . \quad . \quad (C)$$

which is the same formula as that found by HULETT.

5. Concerning the behaviour of the cells above 40° GETMAN says: "On raising the temperature of the cells above 40°, the temperature coefficients were found to undergo a reversal of sign. The values of the e. m. f. given by the different cells when maintained for a short time at a temperature above 40° were found to be quite divergent. On prolonged standing at this temperature, however, the divergence in e. m. f. was found to disappear and final equilibrium values agreeing within the limits of experimental error were obtained. It was found that stabilization between 40° and 80° could be brought about more quickly by maintaining the cells at a temperature of 95° for several days. From the data obtained with five cells which had been stabilized in this manner, an interpolation formula [2])

$$E = 0.04280 + 0.000170\ (t-25) \quad . \quad . \quad . \quad . \quad (D)$$

was derived by the method of least squares. On applying this formula to the data obtained with other cells, satisfactory agreement between the observed and calculated values of e.m.f. was secured".

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[1]) These proc. **17**, 1050 (1915).

[2]) Journal Amer. Chem. Soc. **39**, note on page 1811 (1917).

6. After having rightly pointed out that the discontinuity in the temperature coefficient of e.m.f. mentioned above cannot be ascribed to any abrupt change in the behaviour of the half saturated solution of cadmium sulphate, GETMAN continues as follows [1]: "*That the discontinuity cannot be ascribed to any change in the amalgam has been proven by the investigations of* BIJL [2], *in which the e.m.f. of a cell formed by connecting an 8 %₀ cadmium amalgam with a standard electrode was found to decrease regularly as the temperature was raised from* 25° *to* 75°."

Consequently he ascribes this discontinuity to the transformation of Cd-α into Cd-β in the vicinity of 40°, calculating the transition temperature t by equating the formulas (A) and (D):

$$0.04742 - 0.000200\,(t-25) = 0.04280 + 0.000170\,(t-25),$$

from which he finds:

$$t = 37^\circ.49,$$

that is to say, whereas COHEN and HELDERMAN found that Cd-α changes into another modification (Cd-β) in the vicinity of 60°, GETMAN considers 37°.49 to be the transition temperature of these modifications.

7. In the first place I should like to call the reader's attention to the mistake in that part of GETMAN's statement, which is given above (§ 6) in italics. The contrary has been proven quantitatively by BIJL [2]. This is evident from that part of BIJL's diagram which describes the behaviour of the 8 %₀ amalgam used by GETMAN in his cells. (Fig. 1.)

This amalgam contains 13.4 atomic percents of cadmium and the diagram shows that the amalgam passes in the vicinity of 40° from the *heterogeneous* into the *homogeneous liquid* phase. Consequently, cells with a constant negative electrode which contain below 40° the heterogeneous 8 %₀ amalgam will show at this temperature an abrupt change of their temperature coefficient [3].

These facts have been overlooked by GETMAN; the temperature of 37°.49 found by him consequently does not correspond to the transition temperature of Cd-α into Cd-β.

8. In order to prove this, it may be borne in mind that GETMAN investigated some β-cells between 0° and 35° in which Cd-β had been

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[1] The italics are mine (COHEN).

[2] Zeitschr. f. physik. Chemie **41**, 641 (1902).

[3] ERNST COHEN and H. R KRUYT, Zeitschr. f. physik. Chemie **72**, 84 (1910).

generated by maintaining at 20° some cadmium which had been electrolytically deposited on a platinum spiral.

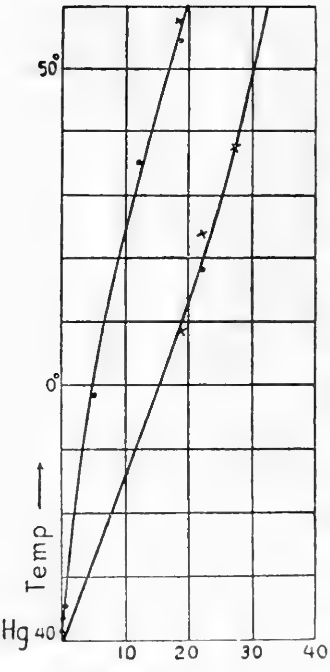

Fig. 1.

According to **Getman**'s measurements for these β-cells our formula (B) holds good between 0° and 35°. On the other hand, **Getman** investigated similar cells above 40°, but the Cd-β which they contained had been formed by heating α- (or γ-) cells at a temperature of 95° for several days, the cadmium thus being in contact with a solution of cadmium sulphate.

According to **Getman**'s measurements above 40° our formula (D) holds good for these cells.

Whereas the elements contained the *heterogeneous* amalgam as long as they were at temperatures below 40°, they contained the *homogeneous liquid* phase above this temperature. The temperature at which the e. m. f. of these two types of cells become identical, corresponds to the temperature at which the heterogeneous phase of the 8 °/₀ amalgam changes into the *homogeneous liquid* state.

If the electrical measurements of **Getman** are accurate, the point of intersection S_2 (Fig. 2) of the curves B and D should to be found at the temperature indicated by **Bijl**'s diagram, viz. 40°.

From **Getman**'s equations:

$$0.04862 - 0.000200\,(t - 25) = 0.04280 + 0.000170\,(t - 25)$$

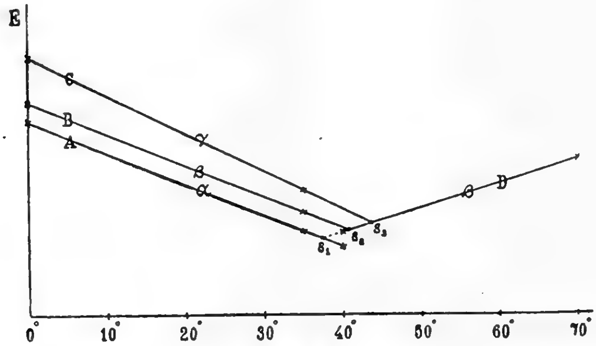

Fig. 2.

we find in perfect agreement with BIJL's results:

$$t = 40°.7.$$

9. We have now to look at the meaning of the temperature of 37°.49 found by GETMAN, which he considers to be the transition temperature of the transformation of Cd_α into Cd_β. At 37°.49 the curves *A* and *D* intersect, viz. this is the temperature at which the cells

Cd-α	Solution of $CdSO_4 \cdot {}^8/_3\, H_2O$ ${}^1/_2$ sat. at 20°.	Cd-amalgam 8 $^0/_0$ by weight (*heterogeneous*)

and

Cd-β	Solution of $CdSO_4 \cdot {}^8/_3\, H_2O$ ${}^1/_2$ sat. at 20°.	Cd-amalgam 8 $^0/_0$ by weight *homogeneous liquid*

have the same e. m. f.

From our diagram (Fig. 2) we see that this temperature must be lower than that (40°.7) at which the *heterogeneous* amalgam changes into the *homogeneous liquid* state.

As a matter of fact it was found by GETMAN 3° lower. Evidently it does not correspond to the transition temperature of the transformation of Cd_α into Cd_β. The same may be said with regard to the point of intersection S_3 of the curves *C* and *D*. According to our diagram (Fig. 2) this point is to be found at a temperature above 40°.7. From GETMAN's measurements we find 43°.5.

10. GETMAN considering 37°.49 as the transition temperature of Cd_α into Cd_β next points out that our dilatometric experiments are open to objection. He says: "It must be borne in mind, however, that in each of these dilatometric measurements never less than 300 g. of cadmium were used, *and it is a well established fact that when so large a mass of a metal undergoes molecular transformation the true transition temperature may be far removed from that at which the transformation actually takes place.*"

Here again a mistake has crept in. Cet. par. the accuracy of dilatometric measurements as carried out by ourselves increases with the quantity of material used, as the change of the level in the capillary tube at constant temperature increases with this quantity. Moreover, the fact that the direction in which the oil moves shows a reversal of sign [1]) if the temperature increases only 0°.5, proves that any retardation of the transformation has not taken place. It must be borne in mind that our metal had always been treated

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[1]) These Proceedings **17**, 54 (1914).

with the solution of an electrolyte in order to exclude retardation.

Neither my own experience in this field of research nor that of others mentioned in the literature agrees with GETMAN's statement, which has been given above in italics.

11. We shall consider now another part of GETMAN's paper. He also repeated the experiments carried out by COHEN and HELDERMAN in order to prove in another way the reality of existence of Cd_{β}.

About this part of our investigations we have written [1]: In order to ascertain if the e. m. f. of the β-cells has a real significance, experiments may be carried out on the following lines: At temperatures above the transition point of the transformation Cd-$\alpha \rightleftarrows$ Cd-β (which we found to lie in the vicinity of 60° by dilatometric measurements) the e. m. f. of α-cells must be higher than that of β-cells. After cooling the cells below the transition point mentioned, the contrary will be the case.

Our experiments were carried out in the following way: We constructed a large number of HULETT-cells; one of these, the e. m. f. of which had been originally 0.050 volt at 25°.0, attained a constant e. m. f. of 0.047 volt (at 25°.0) after 4 weeks at 47°.5.

We combined this cell (N°. 7) with another (N°. 22), the e. m. f. of which was 0.048 Volt at 25°.0. The two cells *AB* (N°. 7) and *CD* (N°. 22) were connected by a siphon which contained the *same* solution of cadmium sulphate as was present in the cells. (Fig. 3).

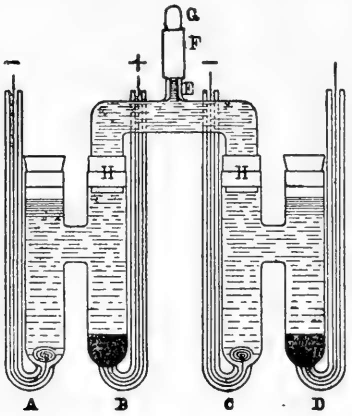

Fig. 3.

The lateral tube *E* of the siphon was closed by a rubber tube *F* in which was put a glass rod *G*. The entire apparatus was brought into a thermostat which could be kept at will at 25°.0 or 64°.5. We measured the e. m. f. between the cadmium which had been electrolytically deposited on the platinum spirals *A* and *C* against the *common* amalgam electrode *B* (12.5 °/₀ by weight).

It is absolutely necessary to use a *common* electrode as the cadmium amalgam of 12.5 °/₀ by weight does not form a

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[1] These Proceedings **17**, 638 (1914); Zeitschr. f. physik. Chemie **89**, **493** (1915); Transactions of the Faraday Society **10**, 216 (1915).

heterogeneous system at 64°.5; its e.m.f. is then a function of its composition. The use of *two* amalgam electrodes *B* and *D* might give rise to serious mistakes, if there were only small differences in their composition. The *absolute* e.m.f. of our amalgam electrode against cadmium in *A* and *C* does not play any role in our measurements.

In this way we found:

	E.m.f. at 25°.0	E.m.f. at 64°.5	E.m.f. at 25°.0
Cell 7	0.04741	0.04029	0.04741
Cell 22	0.04815	0.03979	0.04806

A second experiment with two cells (N°. 4 and 8) newly constructed, gave the following results:

	E.m.f. at 25°.0	E.m.f. at 64°.5	E.m.f. at 25°.0
Cell 8	0.04757	0.04737	0.04776
Cell 4	0.04839	0.04633	0.04789

Getman, on repeating these experiments [1]) got the same results, viz. there takes place a reversal of poles, if the cells are measured at the temperatures indicated above. This is shown by the following measurements of Getman.

	E.m.f. at 25°.0	E.m.f. at 64°.5	E.m.f. at 25°.0
Cell *B* 1	0.04729	0.04734	0.04772
Cell *A*—1	0.04823	0.04652	0.04822

With reference to these results Getman says: "It will be observed that at 64°.5, the e. m. f. of cells 7, 8 and *B*-1 was greater than that of cells 22, 4 and *A*-1, whereas at 25°, the e. m. f. of the latter group of cells was greater than that of the former. From this inversion of poles Cohen concluded that an actual transformation of Cd_α into Cd_β occurs in the neighbourhood of 60°".

Again a mistake has crept into Getman's statement. Evidently we are not allowed to conclude from this inversion of poles that a transition point exists in the vicinity of 60°, as the e.m.f. of the cells has not been measured at a sufficient number of different temperatures between 25° and 64°.5. We only concluded from our experiments that the value of the e. m. f. of our β-cells (48 millivolt at 25°.0) has a real significance and has to be ascribed to the existence of Cd-β in connection with our pycnometric and dilatometric measurements.

12. With regard to the sentence "from this inversion of poles

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[1]) The fact that Getman used cells which contained an 8$^0/_0$ amalgam, whereas the amalgam of our cells contained 12.5$^0/_0$ of cadmium does not matter *here*, as both theses amalgams are heterogeneous at 25°.0 and *homogeneous liquid* at 64 ; 5.

COHEN concluded that an actual transformation of Cd_α into Cd_β occurs in the neighbourhood of 60°," mentioned above, GETMAN writes: "The author's data for cells *A*-1 and *B*-1 appear to confirm this conclusion, but when the e. m. f. of these cells was determined at close intervals of temperature from 0° to 70° no evidence of a transition temperature in the vicinity of 60° was obtained. On the other hand, distinct discontinuities in the e. m. f.-temperature curves were obtained at 39° with *A*-1 and at 36°.8 with *B*—1. Since the agreement between COHEN's cells and the author's at 25° and 64°.5 is satisfactory, it is highly probable that equally close agreement would have been obtained at other temperatures and that had COHEN measured his cells over a wider range of temperatures and plotted the values of e. m. f. against the temperatures he would have obtained no evidence of a transition point at 60°."

There is a mistake in this conclusion, as we shall now demonstrate.

COHEN and HELDERMAN's experiments as well as those of GETMAN, which have been described in § 10, have been carried out on the assumption (the correctness of which has been proven by the measurements) that Cd_α may be heated above its transition temperature without transformation into Cd_β, nay, what is more, it would have been impossible to carry out the measurements if there had not taken place a strongly marked retardation. Consequently it is obvious that GETMAN could not find a transition point using his *B*-1 cell (which contained Cd_α) in the manner he described.

On the other hand also, his *A*-1 cell which contained Cd_β at all temperatures between 0° and 70°, could not show a discontinuity at 60°.

Moreover, we see from our diagram (Fig. 2) that the discontinuity of the e. m. f. of cell *B*-1 which contained Cd_α must occur at a lower temperature than that of cell *A*-1 where Cd_β is present. As a matter of fact GETMAN got this result. With the *B*-1 cell this temperature is 36°.8, with the *A*-1 cell it was found to be 39°.[1]) The discontinuity itself has nothing to do with the transformation of Cd_α into Cd_β but again is to be ascribed to the change of the *heterogeneous* phase of the amalgam into the *homogeneous* liquid one.

13. Finally I want to point out a mistake which has been made

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[1]) That these temperatures have not been found to be 37°.49 and 40°.7 respectively, may probably be ascribed to the fact, that only one single cell has been used in this case for the determination. On the other hand a small difference in the composition of the positive poles of the cells may have been the reason of this deviation.

where GETMAN proposes the question whether there exist more than two modifications of cadmium between 0° and 100°.

COHEN and HELDERMAN deduced from the equations (*A*) and (*C*), found by them (as well as by GETMAN) for their α- and γ-cells respectively, that the *metastable* transition point $Cd_\gamma \rightarrow Cd_\alpha$ lies at 94°.8. Although we know that the transition point of $Cd_\alpha \rightleftarrows Cd_\beta$ lies between 0° and 100°, we are not allowed to conclude that there exist more than two *stable* modifications within this range of temperatures. We found that the γ-modification is always *metastable* within this range and GETMAN, who used this modification in his γ-cells, evidently agrees with this opinion.

Consequently his words: "the results of the present investigation, however, seem to render the existence of more than two allotropic modifications of cadmium between 0° and 100° extremely doubtful", lose their significance. Three different forms of cadmium may possibly exist between 0° and 100°, but one of these (Cd_γ) is always *metastable* within this range of temperatures.

SUMMARY.

1. It has been demonstrated that in the assumptions made by GETMAN in his paper on the allotropy of cadmium some mistakes have crept in. Consequently the conclusions which he has drawn from his careful experimental work ought to be revised.

2. If these mistakes are removed GETMAN's results become identical with those of COHEN and HELDERMAN. In this way GETMAN's work has been a very welcome control as well as a confirmation of the results formerly got by ourselves.

Utrecht, Dec. 1917. VAN 'T HOFF-*Laboratory*.

Chemistry. — "*On the Hydrindene*—1. 2. *diols*". (Preliminary note). By Prof. J. BÖESEKEN and Mr. CHR. VAN LOON.

(Communicated in the meeting of February 1918).

By treating 1-hydroxy- 2-hydrindamine with nitrous acid A. SPILKER [1]) prepared hydrindene glycol; it was readily soluble in water and, after crystallising from benzene, melted "at about 120°".

FR. HEUSLER and H. SCHIEFFER [2]) oxidised coaltar-fractions, richly containing indene, with permanganate of potassium and obtained a substance, which crystallised from benzene in needles, melting at 98°—99°; they took this compound for identical with SPILKER's glycol and ascribed the diverging melting point to an error.

R. WEISZGERBER [3]) converted indene-chlorohydrin, which he had prepared by the addition of hypochlorous acid to indene, by boiling with potassium acetate and glacial acetic acid into a diacetate; this could not be obtained in a pure state, and, on saponification by means of a hot solution of potassium hydroxide, yielded hydrindene glycol, which crystallised from water in needles, melting sharply at 158°. Direct treatment of indene-chlorohydrin with a hot solution of potassium hydroxide produced a smeared matter, from which hydrindene glycol with the same m. p. could be extracted. The much lower melting points of the above-mentioned samples, WEISZGERBER ascribed to their preparation from impure indene, containing coumarone, having used pure indene (from sodium indene [4]) himself.

Obviously it has been overlooked that cis-trans isomerism will occur here; *viz.* the hydroxyl groups can lie on one side, or, on either side of the plane of the five-membered ring: (See diagrams on next page.)

It has been our intention to prepare these isomers (both of which should be resolvable into optical antipodes) and, moreover, to determine the configuration of each by means of the boric acid-method, wich has been elaborated by one of us [5]).

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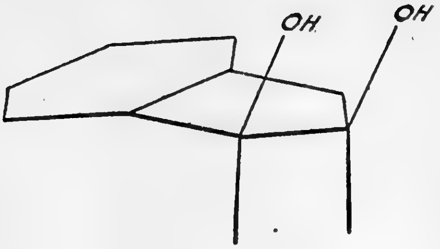
[1]) A. SPILKER, Ber. d. D. chem. Ges. **26**, 1538—45 (1893).

[2]) FR. HEUSLER and H. SCHIEFFER, Ibid **32**, 28 - 34 (1899).

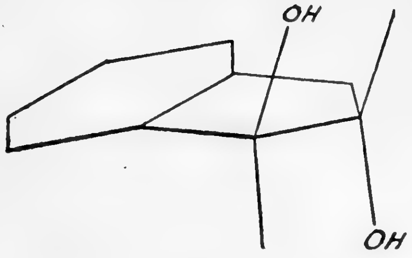
[3]) R. WEISZGERBER, Ibid **44**, 1436—48 (1911).

[4]) R. WEISZGERBER, Ibid **42**, 569—72 (1909).

[5]) J. BÖESEKEN and collaborators, Recueil **30**, 392; **31**, 80 and 86 These Proceedings, **21**, 157; **26**, 3.



cis



trans

Cis-hydrindene-1. 2. *diol.*

First of all we repeated the oxidation of a dilute emulsion of indene in water with potassium permanganate, conformable to HEUSLER and SCHIEFFER. We too obtained in a very poor yield (less than 13 °/₀ of the theoretical one) hydrindene glycol, which however, after crystallising once from benzene, melted at 105°—106° after sintering strongly from 95°. By executing the oxidation in a solution in aqueous acetone or methylalcohol, and at —40°, the yield could but little be increased, *e.g.* to 18,5 °/₀ (m. p. 103,5°—104,5°). Recrystallising again and again from benzene leads to felt-forming needles, which sinter slightly at 105,5° and melt at 107,5°—108° [1]); the substance could be sublimated in a vacuum, by which the m. p. was not altered.

On the strength of a preliminary determination of the influence of this diol, and of the one to be described, on the conductivity of boric acid, we assign — provisionally — the cis-configuration to the glycol with the m. p. 107,5°—108° [2]).

Trans-hydrindene-1. 2. *diol.*

WEISZGERBER's diol having the more divergent properties, we surmised it to be the other stereomer. We prepared it in a more usual way: By boiling indene dibromide [3]) with a solution of potassium acetate in acetic acid a fraction is obtained, boiling at 169°—174°/13 mm., (the yield being 52—66 °/₀ of the theoretically possible one), from which we could isolate by fractional distillation a pure *hydrindene*-1. 2. *diol-diacetate*:
b. p. 169°—169,5°/13 mm., $d^{20,4}_{4}$ 1,1771 n^{20}_{D} 1,5170, M_D found 60,15, M_D calc. 59,74.

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[1]) All melting points were determined by means of thermometers according to ANSCHÜTZ, and were corrected when necessary.

[2]) BÖESEKEN has already mentioned this: these Proc. **26**, 6.

[3]) A. SPILKER and DOMBROWSKY, Ber. d. D. chem. Ges. **42**, 572—3 (1909).

On saponifying the fraction boiling at 169°—174°/19 mm. with a hot solution of potassium hydroxide the diol is formed, which had already been obtained by WEISZGERBER; we got a better yield however (58 °/₀ instead of 30 °/₀ of the theoretically possible one, calculated from the diacetate). After sublimating in a vacuum and repeatedly crystallising from toluene the trans-diol forms snow-white highly lustrous leaflets, m.p. 159,8°—160°.

Though our analyses and our determinations of the molecular weight in phenol [1]) pointed out, that the obtained substances were the sought-for hydrindene diols, the possibility still existed that one of the two might be an aequimolecular compound of cis- with trans-hydrindene diol. In fact H. LEROUX [2]) isolated such compounds of the naphtane-2.3.diols and of the naphtanetriene-2.3.diols: these compounds preserved constant melting points throughout recrystallisations from various solvents; in aqueous solution however — as appears from the depression of the freezing point — they were dissociated into their components. The isolation of the cis- and of the trans-isomer out of the (cis + trans)-compound could only be brought about by acetylation, from which a mixture of cis- and trans-diacetates results, which could be separated by crystallising.

If the occurrence of an inversion of cis into trans, or the reserve, be excluded, it will be clear that never, in converting an aequimolecular (cis + trans)-compound into derivatives, the cis- or the trans-derivative can be obtained in a yield exceeding 50 °/₀ of the theoretical one. So, having prepared from our hydrindene diols with the melting point 107,5°—108° and 159,8°—160°, the *cis-* and *trans-hydrindene*-1.2.*diol-dibenzoates* (melting respectively at 109,5°—110,5° and at 76,5°—77,5°), and the corresponding *di-phenylurethanes* (melting with strong decomposition at 205° and at 206,5°), in yields larger than 50 °/₀ of the theoretically possible amounts, we have proved the unity of each of our diols.

On saponification by means of alcali the trans-derivatives directly gave back the trans-diol; the cis-derivatives however, generally gave rise to preparations, having indeed the appearance of the diol with the m.p. 107,5°—108°, but melting already at 100,5°—101,5°, even after recrystallising and sublimating. This new diol m.p. 100,5°—101,5° is a *polymorphic modification* of the *cis-diol*, as appears from

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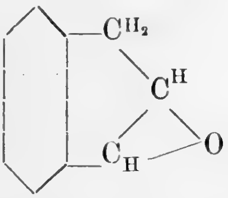
[1]) The data, including those of the other substances to be described, will be given later in a full communication.

[2]) H. LEROUX, Ann. de Chim. et de Phys. (8) **21**, 536—47 (1910).

the following observations: A mixture of the two forms at 100°—105° scarcely exhibits any change, but melts at 107,5°—108°, just like the form with the highest melting point: samples that have been fused before, melt either at 100,5°—101,5°, or at 107,5°—108°; a few times we even saw the metastable form liquefy at 100,5°—101,5°, solidify completely on slowly heating higher, to melt anew at 106,5°—108,5°.

Indene-oxyd



On treating indene in a solution in moist ether with mercuric oxide and iodine, and after shaking the resulting solution of indene iodohydrin with powdered potassium hydroxide, we obtained the oxide as a deep brown liquid. At our first attempt this liquid decomposed spontaneously, a second time during the distillation under diminished pressure. We ascribed this decomposition to the impurity of our solution of the iodohydrin and in consequence we treated indene bromohydrin, which can readily be got in a pure state according to the prescription of W. J. Pope and J. Read [1]), in ether with powdered potassium hydroxide. Actually we now obtained, by distillation under reduced pressure in a nearly theoretical yield, *indene oxide* as a colourless liquid, poorly soluble in water, with a naphtalene-like odour, b.p. 113°/19,5 mm., $d^{23,6}_{4}$ 1,1258, $n^{23,1}_{D}$ 1,5627, M_D found 38,05 M_D calc. 37,40. The oxide crystallises spontaneously in big rhombic plates, melting at 31°—31,5°. In a few days the crystals begin to become soft, which is likely to be due to a polymerisation. Indene oxide may become an important intermediate product for the preparation of 1.-or 2.-mono- and 1.2.-di-derivatives of hydrindene.

Rearrangement into 2.-keto-hydrindene (β-hydrindone).

Heusler and Schieffer (l.c.), by boiling with dilute sulphuric acid, converted their glycol into β-hydrindone, m.p. 58°. Weiszgerber observed the same reaction of his substance, and wrongly interpreted this as a proof of identity.

By treating both our diols and the oxide in the same way, we got each time β-hydrindone, which in all three cases melted at

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[1]) W. J. Pope and J. Read, Journ. Chem. Soc., Trans. **101**, 758 (1912).

57°—58° (the m.p. of a mixture was also 57°—58°); the identity of the resulting samples of β-hydrindone moreover appeared from the concurring m.p. of the three oximes. We did not get any indication of the possible formation of α-hydrindone.

The isomerisation of indene oxide to β-hydrindone also takes place when water is absent, *viz.* in a dry ethereal solution by means of some anhydrous zinc chloride; therefore we are not inclined to accept the intermediate formation of one of the glycols during the conversion of the oxide to β-hydrindone.

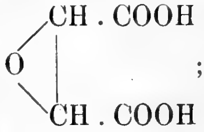
β-Hydrindone has a very pleasant odour, resembling that of the methylester of anthranilic acid; by its scent we could observe the ketone as a by-product from many reactions, mostly in acid but also in alcaline solutions.

Hydration of indene oxide.

When it is attempted to add the elements of water to the oxide, nearly always the odour of β-hydrindone distinctly appears and in fact we could in several cases isolate the ketone by steamdistillation. Besides, a poorly soluble condensation product is always formed, which we consider to be anhydro-bis-β-hydrindone, for this compound has been obtained by Heusler and Schieffer from β-hydrindone by the action of diluted acid or of a solution of sodium hydroxide as well.

However, the hydration may be conducted in such a way (*viz.* in a very diluted aqueous solution) as to limit the rearrangement into β-hydrindone; besides, not only cis-, but also an important quantity of trans-hydrindene diol is always formed, in which latter case obviously a change of the configuration must have occurred.

This result places in a very dubious light the determination of the configuration of the cis- and trans-cyclohexane-1.2.diols [1]), naphtane-2.3.diols [2]), naphtatriene-2.3.diols [3]) and cyclopentane-1.2.diols [4]), since it was based on the assumption that the cis-configuration belongs to the glycol, which proceeds from the addition of water to the oxide.

(We perceive that W. Lossen, Niehrenheim and Dueck [5]) think they have obtained a mixture of racemic acid and of mesotartaric acid from ethylene-oxide-α-β-dicarboxylic acid  ; still it is pos-

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[1]) L. Brunel-Ann de Chim. et de Phys. (8) **6**. 280 (1905).

[2]) H. Leroux-ibid. (8) **21**, 496 and 500 (1910).

[3]) H. Leroux-ibid. (8) **21**, 511 (1910).

[4]) M. Godchot and Taboury-Bull. Soc. Chim. (4) **13**, 535—42 (1913).

[5]) W. Lossen, c.s.-Leb. Ann. **348**, 305 (1906)

sible that their oxido-acid was a mixture of the cis- and the trans-compound).

The change of the configuration, established by us, joins the inversions so often observed when one attacks the valencies of an atom, which is bearer of stereoisomerism. However, it differs thus far from the known cases, that — after definitive confirmation of the determination of the configuration by means of boric acid — it will be certain which glycol has been formed with change of the configuration.

From the mixture of the two hydrindene diols, resulting from the hydration of the oxide, it is easy to extract the one, having the highest m.p., as it is much less soluble. By recrystallisation from toluene or water we obtained the trans-diol, m.p. 159,5°—160,5° (a mixture with the diol from the diacetate showed no depression). The mother-liquors furnished the more soluble glycol, accompanied however by a small quantity of the isomer; these mixtures begin to melt from 92°—93° (this seems to be the eutectic temperature). Only after many fruitless exertions, when the cis-dibenzoate turned out to be less soluble than the trans-dibenzoate, we succeeded along that course in isolating the cis-dibenzoate, m.p. 108,5°—111° from the mixture and in converting it by saponification into the pure cis-diol, m.p. 107°—108,5° after sintering at 106° (no depression after mixing with the diol, obtained by potassium permanganate, was observed).

During the investigation, which for the present has been communicated in part, the connection with the *Walden Inversion i.a.* has ever been kept in view. After conclusion of the work one of us hopes to lay down the results in a dissertation.

Lab. for Organic Chemistry of the
Technical University

Delft, February 19, 1918.

Mathematics. — *"On linear inner limiting sets"* [1]. By Prof. L. E. J. Brouwer.

(Communicated in the meeting of April 27, 1917).

We consider an inner limiting set I, determined inside the unit interval as the intersection (greatest common divisor) of the sets of (non-overlapping) intervals $i_1, i_2, \ldots$, each point of $i_{\nu+1}$ being also a point of i_ν. Then the complementary set C of I with regard to the closed unit interval is the union (common measure) of the closed sets $a_1, a_2, \ldots$, each $a_{\nu+1}$ containing a_ν. *We shall suppose that I as well as C is uncountable in each sub-interval of the unit interval*; then we may assume that each a_ν contains as its nucleus a perfect set p_ν. The difference of a_ν and p_ν will be indicated by v_ν, the complementary set of p_ν, considered as a set of intervals, by u_ν, and the inner limiting set determined as the intersection of $u_1, u_2, \ldots$ by U. Then the points of each u_ν lie everywhere dense, and each u_ν is a set of order-type η of intervals, whose length does not exceed a certain value ε_ν having the limit zero for indefinitely increasing ν.

Let us assume that we dispose of such a set j_ν of order-type η of intervals each being an element of one of the sets $u_\nu, u_{\nu+1}, u_{\nu+2}, \ldots$, that j_ν contains no point of v_ν, but does contain all points of U not belonging to v_ν. We shall indicate a method leading from j_ν to such a set $j_{\nu+1}$ of order-type η of intervals each lying inside an interval of j_ν, and being an element of one of the sets $u_{\nu+1}, u_{\nu+2}, u_{\nu+3}, \ldots$, that $j_{\nu+1}$ contains no point of $v_{\nu+1}$, but does contain all points of U not belonging to $v_{\nu+1}$, each interval of j_ν containing a subset of $j_{\nu+1}$ of order-type η.

Let AB be an arbitrary element of j_ν being at the same time an element of u_μ $(\mu \geq \nu)$, let F be the subset of $u_{\mu+1}$ lying inside AB,

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[1] To the last footnote of my former communication on inner limiting sets (these *Proceedings* XVIII, p. 49) must be added that the changed form in which Schoenflies has referred to my reasoning (applying it to a special case only, and deducing the general theorem from this special case) is irrelevant. The error is contained in the sentence (Entwickelung der Mengenlehre I, p. 359, line 5—8 from the top): "Ist nämlich P irgend eine abzählbare Menge, die nicht dicht in bezug auf eine perfekte Menge ist, und geht man durch Hinzufügung sämtlicher Grenzpunkte zu einer abgeschlossenen Menge Q über, so kann diese keinen perfekten Bestandteil enthalten, ist also ebenfalls abzählbar".

and w the set of intervals which is left from F after destroying all points of $v_{\nu+1}$ contained in F. Let PQ be an arbitrary element of w, s_ρ the set of intervals determined as the intersection of u_ρ and PQ, t_ρ the set of intervals which is left from s_ρ after destroying its first and its last element, *in so far those elements exist,* γ the set of intervals determined as the union of $t_1, t_2, t_3, \ldots$, and φ the set of intervals which is generated by constructing in each element of w a set of intervals in the same way as γ has been constructed in PQ. Then the required set of intervals $j_{\nu+1}$ is generated by constructing in each element of j_ν a set of intervals in the same way as φ has been constructed in AB.

If we understand by u_0 as well as by j_0 the unit interval itself, then we arrive from j_0 at j_1 by the same process which has led us from j_ν to $j_{\nu+1}$.

The inner limiting set determined as the intersection of $j_1, j_2, \ldots$ contains all points of U belonging to none of the sets v_ν, so a fortiori all points of U belonging to none of the sets a_ν, so also all points of the unit interval belonging to none of the sets a_ν. As, on the other hand, this inner limiting set can neither contain a point of a v_ν, nor (as a subset of U) a point of a p_ν, it finally cannot contain a point of a a_ν either. *So it is identical to the complementary set of C, i.e. to I.*

If we construct a ternal scale on the unit interval, and if (designing by ϱ_ν an arbitrary finite series of digits 0, 1 or 2, among which ν digits 1 occur) we understand by $d_{\nu+1}$ the set of the intervals l_{ρ_ν} whose end-points have the coordinates $\cdot\varrho_\nu 1$ and $\cdot\varrho_\nu 2$, then we can first represent the set of intervals j_1, biuniformly and with invariant relations of order, on the set of intervals d_1; thereupon we can in each interval of j_1 represent the subset of j_2 contained in it, biuniformly and with invariant relations of order, on the subset of d_2 contained in the corresponding interval of d_1; and so on. In this way we determine a continuous one-one transformation of the unit interval in itself by which I passes into the set τ_2 of the points expressible in the ternal scale by means of a sequence of digits containing an infinite number of digits 1, whilst C passes into the set τ_1 of the points expressible in the ternal scale by means of a sequence of digits containing only a finite number of digits 1. Thus, indicating the *geometric types*[1]) of τ_1 and τ_2 by $\overline{\mu}$ and $\overline{\nu}$ respectively, we have proved the following

THEOREM 1. *Each inner limiting set contained in a linear interval,*

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[1]) Comp. these *Proceedings* XV, p. 1262.

and, as well as its complementary set, uncountable in each sub-interval, possesses the geometric type $\overline{\nu}$, *and its complementary set possesses the geometric type* $\overline{\mu}$.

Let H and K be two arbitrary points of τ_1, we can choose ν in such a way that neither H nor K is an endpoint of an interval of d_ν. Let us indicate the set of points which is the complementary set of d_ν, by e_ν, and the set whose elements are the intervals of d_ν, and the points of e_ν, by r_ν. Then we can construct a one-one transformation of d_ν and e_ν each in itself, by which the relations of order between the elements of r_ν remain invariant, and H passes into K. This transformation can be extended to a continuous one-one transformation of the unit interval in itself, for which the subsets of τ_1 and τ_2 contained in corresponding intervals of d_ν, correspond to each other. We thus have generated a continuous one-one transformation of the unit interval in itself, by which τ_1 passes into itself, and the point H chosen arbitrarily in τ_1, passes into the point K chosen likewise arbitrarily in τ_1, *so that* τ_1 *is a homogeneous set of points.*

Let H and K be two arbitrary points of τ_2 contained in the intervals h_ν and k_ν of d_ν respectively, we can construct a one-one transformation of the set of intervals d_1 in itself leaving invariant the relations of order, by which h_1 passes into k_1; this transformation can be extended to a one-one transformation of the set of intervals d_2 in itself leaving invariant the relations of order, by which h_2 passes into k_2; continuing indefinitely in this way, we generate a continuous one-one transformation of the unit interval in itself, by which each d_ν, so also τ_2 passes into itself, and the point H chosen arbitrarily in τ_2, passes into the point K chosen likewise arbitrarily in τ_2, *so that* τ_2 *too is a homogeneous set of points*, and we have proved the following

THEOREM 2. *Each inner limiting set contained in a linear interval, and, as well as its complementary set, uncountable in each sub-interval, is homogeneous, and its complementary set is likewise homogeneous.*

Physics. — *"On the Course of the Values of a and b for Hydrogen at Different Temperatures and Volumes".* II. By Dr. J. J. VAN LAAR. (Communicated by Prof. H. A. LORENTZ).

(Communicated in the meeting of Jan 26, 1918).

IX. Influence of the field of force according to Boltzmann.

When we substitute the value

$$dN = 4\pi\, r^2\, dr \times e^{-\theta P_r} \times \tau \times n$$

in the general Virial formula

$$pv = RT - \tfrac{1}{3}\,\Sigma\, r f(r) = RT - \tfrac{1}{6}\,N \int dN \;\; r\,\frac{dP_r}{dr}$$

for dN, the number of molecules in the spherical shell between r and $r+dr$ round the molecule under consideration, then pv becomes:

$$pv = RT - \tfrac{2}{3}\,\pi\, Nn \int_{s}^{r_a} \tau r^3\, \frac{dP_r}{dr}\, e^{-\theta P_r}\, dr.$$

In this P_r represents, therefore, the function of force in the points at a distance r from the centre of the considered molecule (thought spherical), so that $f(r) = dP_r : dr$. The integration extends from $r = s$ (the distance of the centres of two molecules — thought incompressible at the collision, so that s represents the diameter of the molecule) as far as the field of force extends outside the molecule $(r = r_a)$.

The factor $e^{-\theta P_r}$ is the well-known BOLTZMANN *temperature* "distribution factor" under the influence of the field of force. The parameter θ is $= N : RT$, in which N represents the total number of molecules in the volume v, so that $N = nv$, when n represents the number of molecules in the unit of volume.

The other factor τ is the *volume* distribution factor. For $r = s$ (at collision) $\tau_s =$ will be $= \dfrac{b}{b_g}\,\dfrac{v}{v-b}$ (see further below), which will approach to 1 for $v = \infty$, as this fictitious quantity b then becomes $= b_g$. For $r = \infty$ τ is $= 1$ for all values of v. But already at a short distance from the considered molecule τ can be put $= 1$.

Let us at first only consider the exceedingly rarefied gas state — i. e. the planetary condition of matter — then $\tau = 1$ everywhere, so that we may write: [1])

$$pv = RT - \tfrac{2}{3}\pi\, Nn\, s^3 \int\limits_{s-\delta}^{s} \frac{dP_r}{dr} e^{-\theta P_r}\, dr - \tfrac{2}{3}\pi\, Nn\, r_1{}^3 \int\limits_{s}^{r_a} \frac{dP_r}{dr} e^{-\theta P_r}\, dr, \quad (a)$$

in which we assume that at the *collision* the distance of the molecules remains $= s$ (no appreciable compression), and that also the *attraction* extends only to a small distance from s, so that a mean value $r_1{}^3$ can be brought as a constant factor before the integration sign.

In this r_a is in any case to be supposed *greater* than s. For if this were not so, and if the attraction only worked at an exceedingly short distance $s + \delta$ just before the collision, as is still sometimes supposed, the virial of attraction would, for smaller volumes, be subjected to the *same* volume distribution factor τ_s as the virial of collision — with the consequence that the equation of state would not contain *two* distinct constants a and b, but only one, and would assume the form $p(v-\beta) = RT$.

Critical phenomena — which can be explained theoretically justly by the different behaviour of the two separate virial parts, in this way that the factor τ will have no or hardly any influence on the virial of attraction for smaller volumes (as the middle value τ_1 can then always be assumed to be near 1), whereas it will exert a great influence on the virial of collision (for $v = v_k$, where $v = 3$ or 2 times b, τ_s will already have a value between 1,5 and 2) — these phenomena would *entirely fail* to appear. For then there is no distinction possible at all between the two constants a and b, on account of which the equation of state can be brought in the well-known form $(p + {}^{a}/_{v^2})(v-b) = RT$ (see also further below).

It is, therefore, almost *completely excluded* that the attraction should not take place until at immediate (or almost immediate) contact of the molecules. For *very small* volumes (smaller than the critical volume), where the centres of the molecules possess e.g. a mean smaller distance (l) than r_a, it may be expected that also the virial of attraction will be somewhat influenced by the factor τ, so that a becomes dependent on v. But then we are already past the critical point — where $l = s\sqrt[3]{4} = 1,6\, s$. ($r_a$ will appear to be not much greater than about 1,6 s). Hence the quantity a would

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[1]) Cf. also my Article in the Arch. Teyler (2) T VII, Troisième partie, 1901; chiefly chapter X, p. 28—34.

then be able to increase with decreasing v, but on the other hand the value of a can also decrease in consequence of absorption of the lines of force, when v becomes smaller.

X. The Virial of Attraction.

Let us first determine the value of the virial of attraction.

For $r = s$ P_r will evidently be $= -M$, the (negative) maximum value of the force function. For $f(r)$ is taken so in the virial formula that attraction becomes *positive*. If e.g. $P_r = -c : r^q$, then $f(r)$ becomes $= dP_r : dr = qc : r^{q+1}$, hence properly positive. Further $P_r = 0$ for $r = r_a$, where the attraction stops or becomes imperceptible, so that the second integral yields:

$$\left(-\frac{e^{-\theta P_r}}{\theta}\right)_{-M}^{0} = \frac{e^{\theta M} - 1}{\theta} = \frac{RT}{N}\left(e^{\frac{MN}{RT}} - 1\right)$$

And as $N \times \frac{2}{3}\pi s^3$ is evidently $= N \times 4m = (b_g)_\infty$ ($m =$ volume molecule; the index g refers to infinitely large volume; the index ∞ to infinitely high temperature), we may write $(b_g)_\infty \times \left(\frac{r_1}{s}\right)^3 = \nu\,(b_g)_\infty$ for $N \times \frac{2}{3}\pi r_1^3$, and the value of the virial of attraction, as $n : N = 1 : v$, becomes:

$$V_a = -\frac{\nu\,(b_g)_\infty}{v}\,RT\left(e^{\frac{MN}{RT}} - 1\right).$$

Let us now put

$$\nu\,(b_g)_\infty\,RT\left(e^{\frac{MN}{RT}} - 1\right) = a\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\alpha)$$

then for $T = \infty$:

$$a_\infty = \nu\,(b_g)_\infty\,RT\left(\frac{MN}{RT} + \tfrac{1}{2}\left(\frac{MN}{RT}\right)^2 + \ldots\right) = \nu\,(b_g)_\infty \,.\, MN. \quad . \quad . \quad (\beta)$$

Hence, when we put $MN = \alpha$, we get:

$$a = a_\infty \times \frac{e^{\alpha/RT} - 1}{\alpha/RT} = a_\infty \times f(a)\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$V_a = -\frac{a}{v} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The quantity α is therefore determined by

$$\alpha = MN = \frac{a_\infty}{\nu\,(b_g)_\infty} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

We shall presently consider the temperature function $f(a)$ in the quantity a somewhat more fully.

It may only be remarked already here, that when we generally put the integral of work:

$$\int_s^{r_a} \frac{d\,(NP_r)}{dr} e^{-\theta P_r} dr = RT\,(e^{\alpha/RT} - 1) = \Sigma \;, \quad . \; . \; . \; . \quad (\gamma)$$

$f(a)$ may be represented by

$$f(a) = \frac{\Sigma}{a} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (4)$$

It is easy to see that (4) will hold generally, whatever be the form of the factor of distribution, provided this be only such a function of θP_r, that it becomes $= 1$ for $P_r = 0$. a_∞ will then always be $= v\,(b_g)_\infty\, a$.

In the special case that the factor should be *constant* $= 1$, Σ becomes simply $= (NP_r)_s^{r_a} = 0 - (N \times - M) = MN = a$. $f(a)$ then also becomes $= 1$, so that the quantity a becomes independent of the temperature.

XI. The Virial of Collision.

Here P_r will assume the value $+\infty$ for $r = s - \sigma$ — at least when the molecule is supposed t obe incompressible — whereas P_r for $r = s$ will evidently again have the value $- M$. This makes the first integral indicated in the expression (a) for pv:

$$\left(- \frac{e^{-\theta P_r}}{\theta}\right)_\infty^{-M} = - \frac{e^{\theta M}}{\theta} = - \frac{RT}{N}\, e^{\alpha/RT},$$

and we find for the virial of repulsion ($n : N$ is again v):

$$V_b = \frac{(b_g)_\infty}{v}\, RT\, e^{\alpha/RT}.$$

Let us then put:

$$(b_g)_\infty \times e^{\alpha/RT} = b_g, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (\delta)$$

then b_g becomes:

$$b_g = (b_g)_\infty \times e^{\alpha/RT} = (b_g)_\infty \times f(b)\,, \quad . \; . \; . \; . \; . \quad (5)$$

and

$$V_b = RT \frac{b_g}{v} \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (6)$$

The temperature function $f(b)$ can evidently generally be represented by the equation

$$f(b)=1+\frac{\Sigma}{RT} \qquad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (7)$$

We now get instead of (a), in relation with (2) and (3):

$$pv=RT+RT\frac{b_g}{v}-\frac{a}{v}=RT+\frac{RT\,b_g-a}{v}=RT+\frac{B}{v}, \quad . \; . \quad (8)$$

in which b_g and a are given by (5) and (1) as functions of the temperature. B represents the so-called "second" virial coefficient, when — as we have supposed — v is $=\infty$ (very great). Otherwise B is still a function of v (through the volume distribution factor τ), and instead of $B:v$ can be written $(B:v)+(C:v^2)+$ etc., in which B, C, etc. are still only functions of the temperature.

When we neglect the possible influence of v on the quantity a — hence when we do not take the volume too small — only the influence of the factor τ_s at the collision remains. When we write for this (see also § 9):

$$\tau_s=\frac{b}{b_g}\,\frac{v}{v-b}, \qquad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (9)$$

then b_g in (8) becomes $b_g\times\tau_s$, and we may write:

$$pv=RT\left(1+\frac{b}{v-b}\right)-\frac{a}{v}=RT\frac{v}{v-b}-\frac{a}{v},$$

i.e.

$$p+\frac{a}{v^2}=\frac{RT}{v-b}, \qquad . \; . \; . \; . \; . \; . \; . \; . \quad (10)$$

through which VAN DER WAALS' well-known separation of the two constants a and b has been brought about. We repeat once more that this was, therefore, only possible by this, that only for b_g a volume distribution factor with $v:v-b$ appears, and *not* for a, after which the factor RT in the virial of collision can be united with the principal term RT.

We should, however, never forget that the quantity b thus introduced for $v-b$ is an entirely *fictitious* b, and is in no *direct* relation with the real quantity $b_g=(b_g)_\infty\times f(b)$. For b follows namely from (9):

$$b=b_g\times\frac{\tau_s}{1+\tau_s\dfrac{b_g}{v}}, \qquad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (9a)$$

so that b can only be expressed in b_g, when τ_s is known in *another, independent* way as function of v, which has not yet succeeded as yet. For $v=\infty$, however, τ_s is always $=1$, and b becomes b_g, while for $v=b$, where $\tau_s=\infty$, b is $=v=v_0$. Hence the fictitious quantity b will lie between b_g and v_0, i.e. between $(b_g)_\infty \times f(b) = N \times 4\,m \times f(b)$ for $v=\infty$, and about $N \times \frac{6}{\pi} m$ (with cubic distribution) for $v=v_0$. Only at high temperature $f(b)=1$ may be put, and the upper limit becomes $=N \times 4\,m$. ($m=$ volume of one molecule). [1])

That τ_s becomes infinitely great for $v=v_0$, is clear when we consider that then the molecules (cubically distributed) get to lie close against each other, so that every time a *finite* number of molecule centres will lie inside an *infinitely thin* spherical shell, which causes the number of molecules per volume unity not to have the ordinary middle value n at those places, but to be $=n \times \tau_s = \infty$. Then the middle value n is formed by the alternate values 0 (between two centres) and ∞ (at the place of the centres).

XII. Another Derivation of the Virial Collision.

When objections should be raised against the way in which the distribution factor $e^{-\theta P_r}$ is introduced also for the virial of collision, in view of the circumstance that it may be expected that the density of the molecules will not be modified any further after passage through the sphere of attraction, so that the assumption of $P_r=+\infty$ for the collision (which would render the said factor $=0$) seems unjustifiable (P_r can, indeed, never become ∞, because just so much work is performed by the quasi-elastic forces till the normal (relative) velocity of the colliding molecule is exhausted) — we can also arrive at the result of (5) and (6) in the following way, which is not open to the same objection.

In the foregoing paper we found for the virial of collision the following expression:

$$V_b = \frac{RT'(b_g)_\infty}{v}\left[1-\alpha\sqrt{T'}+\tfrac{2}{3}(\alpha\sqrt{T'})^2-\tfrac{2}{5}(\alpha\sqrt{T'})^3\right],$$

in which α represents a coefficient which is in connection with the atomic forces within the molecule, and determines the degree of

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[1]) We draw attention here to this, that whereas from $v=\infty$ to $v=v_0$ the *real* quantity $b_g=(b_g)_\infty \times f(b) \times \tau_s$ *increases* from b_g to ∞ (in consequence of τ_s), the *fictitious* quantity b in $v-b$ will *decrease* from b_g to $b_0=$ about $6/\pi \times Nm$.

compression. When we put in this $a=0$ (perfectly hard, incompressible spheres), we obtain:

$$V_b = \frac{RT'\,(b_g)_\infty}{v},$$

which in this case we should also have obtained immediately by supposing ϱ constant under the integral sign (see the first Paper). For then the integral had become (the indices 0 and b refer to the interval of collision; in Communication I ϱ_a was written instead of ϱ_b):

$$2\,\varepsilon n\,(b_g)_\infty \int_{\rho_b}^{\rho_0} (\varrho - \varrho_0)\,d\varrho = -\,\varepsilon n\,(b_g)_\infty (\varrho_0 - \varrho_b)^2,$$

and as $\varepsilon\,(r_0 - r_b)^2$ is $= \varepsilon\,(\varrho_0 - \varrho_b)^2 = RT' : N$, V_b becomes as above.

We wrote RT' and not RT. This is owing to this that the mean relative normal velocity u_0, with which a molecule strikes against another, is *increased* by a certain amount on account of the traversing of the sphere of attraction, and this in such a way that

$$\tfrac{1}{2}\,\mu u'^2_n - \tfrac{1}{2}\,\mu u^2_n = \int \frac{dP_r}{dr}\, e^{-\theta P_r}\, dr = \Sigma : N,$$

in which Σ is given by (γ) in § 9. But $\tfrac{1}{2}\,\mu u^2_n$ being $\doteq RT : N$, also

$$RT' - RT = \Sigma, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

so that we get:

$$\frac{RT'}{RT} = 1 + \frac{\Sigma}{RT} = e^{\alpha/RT},$$

hence

$$V_b = RT'\,\frac{(b_g)_\infty}{v} \times e^{\alpha/RT},$$

through which $f(b)$ becomes $= e^{\alpha/RT}$, in agreement with (5).

XIII. Objections to the Function $e^{-\theta P_r}$. Another Factor of Distribution.

For $f(a)$ and $f(b)$ we find therefore resp. the expressions

$$\left.\begin{aligned} f(a) &= (e^{\alpha/RT} - 1) : \alpha/RT = 1 + \tfrac{1}{2}\,\frac{a}{RT} + \tfrac{1}{6}\left(\frac{a}{RT}\right)^2 + \ldots \\ f(b) &= e^{\alpha/RT} \qquad\qquad\quad\; = 1 + \frac{a}{RT} + \tfrac{1}{2}\left(\frac{a}{RT}\right)^2 + \ldots \end{aligned}\right| \quad . \quad (12)$$

And these two functions of the temperature are *not* the same, as the experimental results (see the foregoing communication) have taught. The difference is such that even a very considerable error in these results could not have given rise to such a difference. For at the critical temperature $1:RT=8{,}2306$, so that with $\alpha=0{,}06426$ (see foregoing Paper, 2nd table) the value of $f(a)$ becomes $=1{,}3180$, while at the BOYLEpoint, where $1:RT$ is $=2{,}545$, the value of this function is $=1{,}0864$. That of the temperature function $f(b)$ becomes, however, in the two cases resp. 1,6971 and 1,1777, so that $b_g:a$ would become $=(b_g)_\infty : a_\infty \times (1{,}697:1{,}318)=\underline{1{,}288}\ (b_g)_\infty : a_\infty$ in the first case, on the other hand $=\text{id} \times 1{,}178:1{,}086=\underline{1{,}084}\ (b_g)_\infty : a_\infty$ in the second case.

Experimentally *the same* value (viz. 2,545) was found for the two relations. With BOLTZMANN's distribution factor they would *not* be the same, but be in the ratio of 1,288 : 1,084, i. e. $b_g:a$ would have to be $\underline{1{,}19}$ times greater than at the BOYLEpoint, which would require an error of almost 20 °/₀ in one of the two observations. And this is very unlikely, indeed — unless the ratio of $b_g:b_k$, for which we assumed 1,044 (see foregoing Communication, § 2), should have to be about 1,24. But since the value of the fictitious b in $v-b$ at the critical point will certainly not be 20 °/₀ smaller than the limiting value for great volume, this supposition is not particularly probable either [1]).

The same thing applies to a still greater degree with respect to the $f(a)$ and $f(b)$, calculated by REINGANUM and KEESOM [2]), in which they started from the same distribution factor, but where r in the virial of attraction was taken not almost constant $=r$, (see § 9), but varying between s and ∞. In order to render the integration possible, a definite form, viz. $-c:r_q$, was then used for P_r $(q>3)$. Then $\frac{dP_r}{dr}=\frac{qc}{r^{q+1}}$, hence $r^3\frac{dP_r}{dr}=\frac{qc}{r^{q-2}}$. If q were $=$ or <3, the integral would become infinitely great (for $q=3$ logarithmically infinite). For $q=$ or >4 this difficulty disappears.

From the general formula (42) on p. 32 of Suppl. 24 we can derive

$$a=\frac{q}{q-3}\,(b_g)_\infty\,\alpha\left[1+\frac{q-3}{2q-3}\,\frac{\alpha}{RT}+\tfrac{1}{2}\,\frac{q-3}{3q-3}\left(\frac{\alpha}{RT}\right)^2+\tfrac{1}{6}\,\frac{q-3}{4q-3}\left(\frac{\alpha}{RT}\right)^3+\cdot\ \cdot\right]$$

for the quantity a [after subtraction of

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[1]) For this we should have to assume that the value of τ_s in (9^a) for a volume corresponding with v_k were much greater at lower temperature than at higher temperature, where the ratio 1,044 is determined. Possibly also the value of α on the critical isotherm is *slighter* for large volume than for smaller volumes, because the factor τ_1 could then play a part also in the sphere of attraction. (Cf. also the conclusion of § 9, and the footnote in § 11).

[2]) Suppl. Comm. Leiden No. 24, 25 en 26.

$$b_g = (b_g)_\infty \left(1 + \frac{\alpha}{RT} + \tfrac{1}{2}\left(\frac{\alpha}{RT}\right)^2 + ..\right),$$

and multiplication by RT, as KEESOM's B represents $= b_g - (\alpha : RT)$], when $N \times {}^2/_3\, \pi s^3 = N \times 4m = (b_g)_\infty$ is put (KEESOM writes n, where we have put N), and for $\frac{R}{k} v$ ($=$ our $N \times M$) α is written (hv is namely $= \frac{v}{kT} = \frac{R}{k} v : RT$).

For $q = \infty$ this becomes $a = (b_g)_\infty\, \alpha\left(1 + \frac{1}{2}\frac{\alpha}{RT} + \frac{1}{6}\left(\frac{\alpha}{RT}\right)^2 + \ldots\right)$, in agreement with what we found in § 10, as the sphere of attraction becomes infinitely thin for $q = \infty$, so that $r_a = r_1 = s$, hence $\nu = 1$.

When with KEESOM we assume $q = 4$, the functions of the temperature become therefore :[1])

$$\left.\begin{aligned} f(a) &= 1 + \tfrac{1}{5}\frac{\alpha}{RT} + \tfrac{1}{18}\left(\frac{\alpha}{RT}\right)^2 + \ldots \\ f(b) &= 1 + \frac{\alpha}{RT} + \tfrac{1}{2}\left(\frac{\alpha}{RT}\right)^2 + \ldots \end{aligned}\right\} , \quad . \quad . \quad . \quad . \qquad (12a)$$

hence $f(a)$ still less pronounced than ours with the coefficients $\frac{1}{2}$, $\frac{1}{6}$, etc., so that the difference between the two functions of the temperature $f(b)$ and $f(a)$ would become still greater than ours, and the slow decrease of attraction over a greater region, according to the law $q = 4$, would therefore lead to still *more unfavourable* results with respect to the experimentably found *equality* of the two functions of the temperature (at least for T_k and T_B) than our assumptions.

The only factor of distribution that would yield *equal* expressions for the two functions of the temperature, is

$$(1 + \theta P_r)^{-2}$$

instead of $e^{-\theta P_r}$. For then the integral of work Σ becomes:

$$\Sigma = \int_s^{r_a} \frac{d\,(NP_r)}{dr}(1 + \theta P_r)^{-2}\, dr = \left(-\frac{N}{\theta}\frac{1}{1 + \theta P_r}\right)_{-M}^{0} =$$

$$= RT\left(\frac{1}{1 - \theta M} - 1\right) = \frac{\alpha}{1 - {}^{\alpha}/_{RT}},$$

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[1]) With $q = 4$ the factor would become $4\,(b_g)_\infty\, \alpha$, hence $\nu = 4$, which corresponds with a mean value $r_1 = 1{,}6\, s$ (see § 9).

as $\theta = N : RT$ and $MN = \alpha$. For $a_\infty = v(b_g)_\infty \times \Sigma_\infty$ (cf § 10) at $T = \infty$ $(\theta = 0)$ we find therefore again $v(b_g)_\infty \times \alpha$, and further for

$f(a) = \Sigma : \alpha$, and $f(b) = 1 + \frac{\Sigma}{RT}$ (See (4) and (7)):[1]

$$f(a) = f(b) = \frac{1}{1 - {}^{\alpha}/_{RT}} = 1 + \left(\frac{\alpha}{RT}\right)^{2} + \cdots \quad . \quad . \quad \textbf{(12}b\textbf{)}$$

This function of the temperature $f(a)$ is, therefore, the *strongest* of all. It duly gives $f(b) = f(a)$. Then follows ours, viz. (12), derived from BOLTZMANN's function of distribution $e^{-\theta P_r}$, on the assumption of rapidly decreasing attraction, only between s and r_a. It is *weaker* and gives $f(b) > f(a)$. At last comes that of REINGANUM and KEESOM, likewise derived from $e^{-\theta P_r}$, but with attraction from $r = s$ to $r = \infty$, and $q = 4$. This it the *weakest* of all, and gives a still greater difference between $f(b)$ and $f(a)$, which pleads against it.

It is the question whether the proposed distribution factor is theoretically justified. But it has the great disadvantage that the denominator already becomes *infinite* for $RT = \alpha$, and would then become *negative* for smaller values of T, which is of course impossible. The agreement with the values of a calculated experimentally from the *found* values of B (namely by dividing B by $(T : T_B) - 1$, see the foregoing communication) is almost the same as for the function $(e^{\alpha/RT} - 1) : \alpha/RT$, which we considered valid not only for a, but also for b.

In the subjoined table a has been calculated from $a = a_\infty : (1 - \alpha/RT)$. The values of a_∞ and α have this time been determined from the values of a, found for $-252°$ and $20°$ C. For $-252°$ C. a is namely $= -475 . 10^{-6} : -0{,}808 = 588 . 10^{-6}$, and for $20°$ C. a was $= 380 . 10^{-6}$, so that we find $\underline{a_\infty = 370{,}0 . 10^{-6}}$ and $\underline{\alpha = 0{,}02797}$.

It is seen that the agreement is pretty satisfactory; only the values between 20° C. and the critical temperature are again all too low.

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[1] It is again noteworthy that $f(b)$ can also be obtained by carrying out the integration $\int_{s-\delta}^{s} \frac{dP_r}{dr} (1 + \theta P_r)^{-2} \, dr$ between the limits ∞ and $-M$ for P_r. We then get namely $\left(-\frac{1}{\theta} \frac{1}{1 + \theta P_r}\right)_\infty^{-M} = -\frac{RT}{N} \cdot \frac{1}{1 - \theta M}$, so that again (see also § 11) $f(b)$ becomes $= (1 - \theta M)^{-1} = (1 - {}^{\alpha}/_{RT})^{-1}$.

t	T	$1-{}^{\alpha}/_{RT}$	$a\,.\,10^6$ calc.	$a\,.\,10^6$ found.
$-257^\circ{,}10$	15,99	0,5220	709	740
$-255{,}32$	17,77	0,5699	649	(599)
$-252{,}47$	20,62	0,6293	588	588
$T_k = -239{,}91$	33,18	0,7698	481	486
$-204{,}62$	68,47	0,8884	417	472
$-103{,}54$	169,55	0,9549	388	414
$+\ 20$	293,09	0,9739	380	380
$+100$	373,09	0.9795	378	376

All the functions of the temperature considered approach ∞ at $T=0$ or in the neighbourhood of $T=0$, and become, therefore, very great for low temperature. I. e. in the equation (8), viz.

$$pv = RT - \frac{a - RT\,b_g}{v},$$

p would become negative already for comparatively large values of v at very low temperatures. This is, however, practically no objection, for it only means that the *boundary line* where the saturated vapour cannot exist any longer and condenses to liquid, is shifted more to the right (i. e. to the side of the still greater volumes). The negative values of p then fall within the boundary line in the metastable region as before.

That a becomes very great, might also be interpreted in this way. At very low temperature, where the molecules with exceedingly small velocity pass through the sphere of attraction, the accumulation round a molecule will be very great; these will at last all fall together, which would again mean condensation to liquid.

And as for the increase of b_g to infinite large, this would entail that the fictitious quantity b in $v-b$ would approach v more quickly than otherwise would have been the case. For in (9^a), viz. $b = \tau_s b_g :$ $: (1 + (\tau_s b_g : v)$, the fact that b_g becomes great in consequence of the low temperature, has now the *same* effect as otherwise the becoming great of τ_s in consequence of the small volume. I. e. that for a volume v, where else (at high temperatures) the fictitious quantity b would still be near $(b_g)_\infty$, and far from v, this will *now* (viz. at low temperatures) already have drawn much nearer to v. This is again no practical objection, though the probability to such a behaviour may not be great.

The theory of the factor of distribution $e^{-\theta P_r}$ (or $(1 + \theta P_r)^{-2}$, etc. etc.) rests in my opinion on nothing but a misunderstanding, and the theory of the virial of attraction and collision should be built up *on entirely new grounds.*

XIV. More Accurate Theory of the Virial of Attraction and Collision.

The method of calculation followed up to now (REINGANUM, VAN LAAR, KEESOM) might in a certain sense be called the *"static"* method. In this it is assumed that the molecules are distributed according to a certain law round the considered molecule, in which their motion, resp. velocity and direction, is entirely eliminated (disregarding the mean final velocity at the collision). In the place of this BOLTZMANN's factor of distribution $e^{-\theta P_r}$ is then substituted, which is to set everything right again. But in my opinion BOLTZMANN's considerations are no longer valid for separate *micro-complexes,* as molecules in collision, and immediately before impact, or passing each other at a small distance in the sphere of attraction.

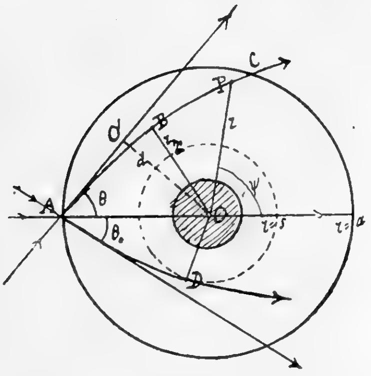
It is easily seen that the effect of the attraction will be *this,* that the at first rectilinear path (at least for large volume) will be *inclined* more or less towards the molecule under the influence of the attraction in the sphere of attraction, and that therefore molecules which would otherwise remain further from the molecule under consideration, will now get into spheres where the attraction is *greater.* And the smaller the velocity with which the molecules will pass, the stronger this enlarging influence will be. If the temperature is exceedingly low, *all* the molecules that pass the border of the sphere of attraction $(r = r_a)$ with their centres, will collide with the molecule under consideration, through which for all the maximum value M is obtained for $-P_r$, and a *maximum* value will therefore be found for a — but not an *exponentially infinitely large one,* as with BOLTZMANN's factor of distribution.

The same thing holds for b. Molecules that would not collide under other circumstances, will now collide under the influence of the attraction that causes them to deflect, and the number of colliding molecules will therefore be increased, in more or less degree as the temperature will be higher or lower. And it is easy to see that here too the value of b_g will approach to a *maximum* for $T = 0$ (as the radius of the sphere of attraction r_a remains finite), and *not* to infinite. Also the absurdness of the infinitely great value for P_r at the end of the collision is quite obviated now. Of course the distribution remains *finite,* and *during* the collision there comes no change in this at all. (On the assumption of $e^{-\theta P_r}$ the density would decrease during the interval of collision, thought infinitely small, from $n \times e^{\theta M}$ to $n \times 0$, which is nonsense, because when a molecule has once come to collision, the number of them, as has been said, does not change again during the impact).

Thus we are naturally led to a new method of calculation, which — in opposition to the usual one, the *static* one — might be called the *dynamic* one. What happens to the molecules that pass each other and that collide, will then have to be considered separately — though much can be simplified also with this mode of viewing these things, and much can be brought under a comprehensive point of view.

And thus we have again returned from Boltzmann to Maxwell with the consideration of the separate paths or groups of paths.

XV. The fundamental Path Equations.



As may be seen from the subjoined figure, the curved path now gets nearer to O (in the point B, with the minimum distance r_m) than the straight path ($u = \infty$, i. e. $T = \infty$) in the point O' with the minimum distance $OO' = d = a \sin\theta$. The angle, at which the path of the centre of the moving molecule under consideration (O is the stationary centre) comes within the sphere of attraction at A with respect to the joining line AO, is namely indicated by θ. All these angles lie on the circumference of a cone with A as vertex. As the path is still undisturbed at A, the frequency of the angle θ is as usual $= \sin\theta\, d\theta$. Later on we shall have to integrate for all possible angles θ. When the centres of the molecules lie on the circumference of the sphere $r = s$, they collide. The limiting angle θ_0 is that for which the path just touches the circle $r = s$ (in D). All the paths that enter under a smaller angle with respect to AO, give rise to *collision.*

It is self-evident that this limiting angle θ_0 in the limiting case $T = \infty$ (rectilinear paths) is given by $\sin\theta = s : a$, as AD then becomes the straight tangent. (a is the radius of the sphere of attraction; outside a there is no attraction any more).

If now the attractive force is in the path $= -f(r)$ (hence always directed towards the centre O), then the equation

$$y\frac{d^2x}{dt^2} - x\frac{d^2y}{dt^2} = 0, \text{ or } \frac{d}{dt}\left(y\frac{dx}{dt} - x\frac{dy}{dt}\right) = 0, \text{ or } y\frac{dx}{dt} - x\frac{dy}{dt} = \text{const.}$$

follows from $\mu\frac{d^2x}{dt^2} = -f(r)\cos\psi$ and $\mu\frac{d^2y}{dt^2} = -f(r)\sin\psi$ with $x = r\cos\psi$, $y = r\sin\psi$ (r is the radius vector, ψ the amplitude).

But from $\frac{dx}{dt} = \cos\psi\frac{dr}{dt} - r\sin\psi\frac{d\psi}{dt}$, $\frac{dy}{dt} = \sin\psi\frac{dr}{dt} + r\cos\psi\frac{d\varphi}{dt}$ follows immediately $y\frac{dx}{dt} - x\frac{dy}{dt} = -r^2\frac{d\psi}{dt}$ (which is also immediately seen), hence

$$r^2\frac{d\psi}{dt} = -c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which c is a constant (which is still to be determined more closely). This is the well-known law of sectors.

The square of velocity $u^2 = \left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2$, expressed in pole coordinates, evidently becomes $= \left(\frac{dr}{dt}\right)^2 + r^2\left(\frac{d\psi}{dt}\right)^2$. But also we have:

$$\frac{d^2x}{dt^2}\frac{dx}{dt} + \frac{d^2y}{dt^2}\frac{dy}{dt} = -\frac{f(r)}{\mu}\left[\cos\psi\frac{dx}{dt} + \sin\psi\frac{dy}{dt}\right],$$

i. e. because the expression between [ ] in the second member, in virtue of $r^2 = x^2 + y^2$, is also $= \frac{dr}{dt}$:

$$\tfrac{1}{2}\frac{d}{dt}\left[\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2\right] = -\frac{f(r)}{\mu}\frac{dr}{dt},$$

hence

$$\tfrac{1}{2}\,d\,(u^2) = -\frac{f(r)}{\mu}\,dr = -\frac{1}{\mu}\frac{dP_r}{dr}\,dr,$$

when P_r represents the function of force, and μ the mass of a molecule. Integrated between the limits A and P, this gives therefore (in A P_r is $=0$, and $u = u_0$):

$$\tfrac{1}{2}\,\mu\,(u^2 - u_0^2) = 0 - P_r,$$

or

$$u^2 = u_0^2 - 2\frac{P_r}{\mu}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the well known law of the vis viva. When now we substitute the above found value in pole coordinates for u^2, we get:

$$\left(\frac{dr}{dt}\right)^2+\frac{c^2}{r^2}=u_0{}^2-2\frac{P_r}{\mu}, \quad \ldots \ldots \ldots \quad (3)$$

as $r^2\left(\frac{d\psi}{dt}\right)^2$ is $=c^2:r^2$ according to (1). This equation (3) is of great importance for our calculations. For the equation of path the following equation follows from this in connection with $\frac{d\psi}{dt}=-\frac{c}{r^2}$:

$$\frac{d\psi}{dr}=\pm\frac{c}{r^2\sqrt{u_0{}^2-2\frac{P_r}{\mu}-\frac{c^2}{r^2}}} \quad \ldots \ldots \quad (4)$$

The upper sign evidently holds for $r=a$ to $r=r_m$ (A to B), the lower sign for all the points beyond B.[1]

We must now first determine the constant c. This can take place in two different ways. The simplest way is to examine the state in the point A, where the path coincides with the tangent AO'. If the considered point P lies in the immediate neighbourhood of A, then evidently $\sin(\psi-\theta)$ is $=a\sin\theta:r$ in $\triangle\ OAP$. Differentiation with respect to r gives $\cos(\psi-\theta)\frac{d\psi}{dr}=-\frac{a\sin\theta}{r^2}$, hence because ψ is then $=180°$ and $r=a$:

$$\left(\frac{d\psi}{dr}\right)_A=\frac{tg\ \theta}{a}.$$

But from (4) follows, because $P_r=0$ in A, and the positive sign holds:

$$\left(\frac{d\psi}{dr}\right)_A=\frac{c}{a^2\sqrt{u_0{}^2-\frac{c^2}{a^2}}}.$$

From the two expressions follows immediately:

$$c=u_0\times a\sin\theta.$$

This causes (3) to become:

$$\left(\frac{dr}{dt}\right)^2=u_0{}^2\left(1-\frac{a^2\sin^2\theta}{r^2}\right)-2\frac{P_r}{\mu}, \quad \ldots \ldots \quad (3a)$$

hence we find for the point B, where the radius vector r gets the minimum values r_m, and where therefore $dr:dt=0$:

---

[1] We must remark that by differentiation of (3) follows $\frac{d^2r}{dt^2}-\frac{c^2}{r^3}=-\frac{f(r)}{\mu}$.

$$\frac{a^2}{r_m^2}\sin^2\theta = 1 + \frac{-P_{r_m}}{{}^1\!/_2\,\mu u_0^2} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (5)$$

Therefore we find for the limiting angle θ_0, as r_m becomes $= s$ in D, and $-P_{r_m} = M$:

$$\frac{a^2}{s^2}\sin^2\theta_0 = 1 + \frac{M}{{}^1\!/_2\,\mu u_0^2} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (6)$$

XVI. Calculation of the Virial of Attraction.

Now the calculation of the virial of attraction can be carried out in the following way.

As the sum of the radii at the entrance of a molecule with radius ${}^1\!/_2 s$ inside the sphere of attraction a is exactly $= a$ (the centre of the entering molecule is then, namely, exactly on the *circumference* of that sphere), the number of entrances per second will be given by the well-known relation

$$N = \pi n a^2 \times u_0 = \omega u_0.$$

The number of entrances for the direction θ is therefore $N_\theta = = \omega u_0 \times \sin\theta d\theta$, so that the number of molecules that will be found on the element ds of the portion of the path AB, is given by

$$\omega u_0 \sin\theta\, d\theta \times \frac{ds}{u} = \omega u_0 \sin\theta\, d\theta \times dt,$$

when $u = \frac{ds}{dt}$ is the velocity in the element of path in question. But dt is given by (3^a), through which we get for the number in question:

$$\omega u_0 \sin\theta\, d\theta \times \frac{dr}{-u_0\sqrt{1 - \frac{a^2}{r^2}\sin^2\theta - \frac{2P_r}{\mu u_0^2}}}.$$

As, namely, $dr : dt$ is negative on AB, the negative sign has been taken in the extraction of the root. Let us write:

$$-\frac{2P_r}{\mu u_0^2} = \frac{-P_r}{{}^1\!/_2\,\mu u_0^2} = \frac{-P_r}{M} \times \frac{M}{{}^1\!/_2\,\mu u_0^2} = F(r) \times \varphi,$$

in which φ is therefore in connection with the *temperature* through ${}^1\!/_2\,\mu u_0^2$. (M again represents the maximum value of $-P_r$ for $r = s$).

Hence we have for the total *virial of attraction* of the considered molecule:

$$\tfrac{1}{3}N \times M\omega \left[\int_{\theta_0}^{90^\circ}\int_{r_m}^{a} \frac{r(-F'(r))\,dr \times \sin\theta\, d\theta}{\sqrt{1-\frac{a^2}{r^2}\sin^2\theta+\varphi . F(r)}} + \int_{0}^{\theta_0}\int_{s}^{a} \frac{(r(-F'(r))\,dr \times \sin\theta\, d\theta}{\sqrt{\quad id. \quad}} \right], \quad (7)$$

in which the first (double) integration refers to all the entering molecules that do *not* strike against the molecule in question, and where, therefore, a *minimum value* of r is passed through in B (which minimum in the limiting case $\theta=\theta_0$ will get exactly on the circumference of the sphere $r=s$), whereas the second integration refers to all the *colliding* molecules. For the attractive force $\frac{dP_r}{dr}=-\frac{d(-P_r)}{dr}$ we have written $-MF(r)$ according to the assumption $-P_r : M=F(r)$. In consequence of the negative sign of the root, the limits of integration are reversed with respect to r.

Besides we still have multiplied by 2 in the above expression, since evidently the second portion of the path from B to C, or from the collision back to the circumference of the sphere of attraction, will yield exactly the same integral value. Everything then takes namely place in the reversed order, the limits of the integrals remaining quite the same. Moreover the summation is extended over *all* the molecules N, which with the virial factor $\frac{1}{3}$ yields therefore in all still a pre-factor $\frac{1}{3} \times \frac{1}{2} N \times 2 = \frac{1}{3} N$. (The total number of molecules N has been divided by 2, because else all the pairs of molecules would have been counted double).

We now have to choose a suitable expression for $F(r)$. The accurate law referring to the attraction being unknown, it will not make much difference for the determination of the dependence of the temperature (lying in the quantity φ), which interpolation function is used, provided $F(r)$ become $=0$ for $r=a$, and $=1$ for $r=s$. The more so when — as will probably be always the case — a and s do not differ *much*. We can, therefore, choose a function for which the above integrations become *possible*. This cannot be completely reached, as we shall at once see, but through the assumption

$$F(r)=\frac{-P_r}{M}=\frac{\frac{1}{r^2}-\frac{1}{a^2}}{\frac{1}{s^2}-\frac{1}{a^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

we can get a long way. In consequence of this we get:

$$-F'(r)=\frac{2}{r^3} : \left(\frac{1}{s^2}-\frac{1}{a^2}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (8a)$$

and this is indeed not $=0$ for $r=a$, but no essential objection in principle can be raised against this. It may, namely, be assumed that the molecules will *suddenly* experience an attractive force (proportional to $1:r^3$) at their entrance inside the sphere of attraction. If only $-P_r$ itself becomes $=0$ for $r=a$, which is the case on our above assumption.

Fontanivent, Autumn 1917.

(*To be continued*).

KONINKLIJKE AKADEMIE VAN WETENSCHAPPEN
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CONTENTS.

Physiology. — *"On the refractory stage of the frog's gastrocnemius"*. By Miss L. Kaiser. (Communicated by Prof. G. van Rijnberk).

(Communicated in the meeting of Jan. 26, 1918)

The values given for the length of the refractory stage — the short time following upon an ultra maximum stimulus, during which a new stimulation has no effect — are widely different.

As a rule the time of the refractory stage is considered equal to that of the latent period for which Tigerstedt[1] by a great number of experiments found an average of 0.005″. Tigerstedt likewise found that this value was affected by changes relating to the temperature, the kind of stimulus, its strength, the tension or to the mass to be moved. According to Waller[2] fatigue lengthens the latent period.

Helmholtz[3], determining the length of the refractory stage itself arrived at $\frac{1}{600}''$ (that is about 0.0016″), Kronecker and Stanley Hall[4] at $\frac{1}{143}''$ (about 0.007″) and sometimes $\frac{1}{300}''$ (about 0.0032″), and Samjloff[5] at 0.002″.

As regards the effect of various circumstances on the length of the refractory stage I could find no data. Schenck[6] saw that fatigue and Ishihara[7] that refrigeration were unfavourable to the summation, which is of course stronger in proportion as the second stimulus reaches the muscle at a moment when it is able to show distinctly that it is stimulated.

To determine whether the length of the refractory stage is influenced by various circumstances and if so, in what direction and to what extent, the following experiments were made.

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[1] Archiv. f. Physiologie 1885 Supplement.

[2] Luciani. Physiologie des Menschen III.

[3] Verhandl. der Kön. Preuss. Akademie 1854.

[4] Archiv. f. Physiologie 1879 Supplement.

[5] Pfl. Archiv. Bd. 143.

[6] Pfl. Archiv. Bd. 96.

[7] Pfl. Archiv. Bd. 111.

Fatigue.

The gastrocnemius of a frog which had been poisoned with curarine was prepared in the usual way, and fastened to a lever with a counter-weight of 5 grammes; the whole weight acting on the muscle amounted to 10 grammes. The electrodes were fastened to the femur and to the tendon. The curves were written on the rooty paper of a fall-rotatorium as indicated by SCHENCK. For the untired muscle the minimum distance was found at which two stimuli still gave a summation of the contractions, and then after the fatigue caused by a number of induction-shocks (100 in 1′), the stimuli were applied at the same distance from each other. If the refractory stage was lengthened by fatigue of the muscle, which might with some ground be expected, then it would appear in a simple and conclusive way from the fact that a stimulus-interval only just causing summation before the fatigue, would give no summation if the muscle was fatigued, whilst by lengthening the interval so much that summation only just set in, the new length of the refractory stage might be arrived at. I succeeded, however, but seldom in finding quickly the stimulus-interval which differed so little from the refractory stage, that an increase of the latter must be the cause of the entire disappearance of the summation. In most cases I had to content myself with investigating the changes which underwent the degree of summation i.e. the difference between the contraction-heights caused by one and by two stimulations, expressed in the contraction-height after one stimulation.

As an example of one experiment first number 4.

At an interval of 0.002″ summation sets in amounting to 0.1; after fatigue by means of 400 break shocks the summation is reduced to 0. Lengthening of the stimulation-interval to 0.005″ scarcely causes the summation to return: it becomes 0.043; at a stimulation-distance of 0.015″ the degree of summation has become 0·12.

In experiment 64 fatigue caused by a great many break and make shocks changes a degree of summation of 0.066 successively to 0.069, 0.08, 0.07, 0.07, 0.109, 0.14, 0.13, 0.08, and 0.09. The stimulation-interval is always 0.0053″.

The following is experiment N°. 20.

After 25 break and make-shocks the degree of summation, which was at first 0.14 has become 0. Lengthening of the stimulation-interval from 0.0025″ to 0.004″ raises it again to 0.04. After 125 shocks the summation has disappeared again, whilst an increase of the stimulus-interval to 0.0063″ still gives a summation of 0.12.

30 experiments gave the following results.

Number of the experiment	Temperature	Stimuli applied	Stimulation interval	Contraction height after one stimulation	Degree of summation
1	?	0	0.005″	15.3 mm.	0.11
		—	„	8.5	0.179
2	10°	0	0.0075″	20.5	0.073
		400 O.S.	„	24.5	0
3	14°	0	0.0033″	14	0.14
		300 O.S.	„	7	0.214
		450 „	„	2	0.5
4	10°	0	0.002″	20	0.1
		400 O.S.	„	22.5	0
		„	0.005″	23	0.043
		„	0.015″	25	0.12
5	11°	0	0.002″	24.5	0.06
		400 O.S.	„	22	0.02
6	13°	0	0.0034″	27	0.08
		300 O.S.	„	29	0.04
		900 „	„	19.5	0.13
		900 + $^1/_3$ h. rest	„	21.5	0.07
		1300 O.S.	„	11.5	0.39
		1700 „	„	6	1
7	13°	0	„	25	0.16
		300 O.S.	„	29	0.031
		600 „	„	22.5	0.02
		900 „	„	15.3	0.11
9	13°	0	0.004″	23	0.19
		250 O.S.	„	11	0.09
		450 „	0.008″	5	0
10	?	0	0.0068″	29	0.21
		300 O.S.	„	25	0.08
		600 „	„	10	0.2
		700 „	„	7.5	0.06
		800 „	„	4.5	0.11

Number of the experiment	Temperature	Stimuli applied	Stimulation interval	Contraction height after one stimulation	Degree of summation
11	22°	0	0.0036″	14.5 mm.	0.1
		50 O.S.	„	10.6	0.03
		125 „	„	6.75	0.03
		175 „	„	4	0.25
12	22°	0	0.005″	60	0.06
		400 O.S.	„	18	0.02
13	22°	0	0.0045″	63	0.05
		250 O.S.	„	15.5	0.18
14	21°	0	0.0054″	59.5	0.06
		25 O.S.	„	40	0.32
		65 „	„	29	0.005
		215 „	„	13	0.077
15	22°	0	0.0042″	12.6	0.19
		100 O.- and C.S.	„	8	0.125
		200 „	„	5 5	0.09
16	22°	0	0.0036″	36	0.028
		50 O.- and C.S.	„	31.5	0.03
		300 „	„	17	0.09
17	21°	0	0.0039″	46.3	0.028
		100 O.- and C.S.	„	47.5 ?	0.055 ?
		200 „	„	42	0.07
		300 „	„	35	0.09
		400 „	„	25.5	0.04
		600 „	„	7	0.28
18	21°	0	0.003″	40.5	0.11
		75 O.- and C.S.	„	40.5	0.03
		125 „	„	30	0.01
		125 „	0.005″	23	0.22
		175 „	0.003″	7	0.07

Number of the experiment	Temperature	Stimuli applied	Stimulation interval	Contraction height after one stimulation	Degree of summation
19	22°	0	0.003″	42.5 mm.	0.07
		10 O.- and C.S.	„	41	0.1
		20 „	„	42	0.06
		30 „	„	42	0.08
		40 „	„	41.6	0.08
		80 „	„	39.5	0.05
		130 „	„	39	0.05
		130 „	„	36	0.05
		155 „	„	35.5	0.04
		175 „	„	33	0.03
		225 „	„	30.5	0.08
		225 „	„	27.5	0.07
		255 „	„	20	0.3
		255 „	„	24	0.04
20	22°	0	0.0025″	54.5	0.14
		15 O.- and C.S.	„	56.5	0.02
		30 „	„	55	0.037
		45 „	„	55.25	0.03
		75 „	„	49.5	0
		75 „	„	54 ?	0 ?
		85 „	„	54	0.04
		95 „	„	53	0
		95 „	0.004″	53	0.04
		105 „	„	51	0.06
		115 „	„	49	0.02
		125 „	„	49	0
		125 „	0.0063″	46	0.12
		225 „	„	43.5	0.1
		325 „	„	35.5	0.07

Number of the experiment	Temperature	Stimuli applied	Stimulation-interval	Contraction-height after one stimulation	Degree of summation
21	22°	0	0.004″	65 mm.	0.16
		10 O.- and C.S.	"	67.5	0.07
		20 "	"	67	0.08
		40 "	"	70	0.05
		60 "	"	65	0
		60 "	0.005″	65	0.04
		80 "	"	62	—0.005
22	23°	0	0.003″	45.5	0.08
		15 O.- and C.S.	"	46	0
		15 "	0.0046″	46	0.11
		30 "	"	53	0.086
		50 "	"	42.7	0.14
		80 "	"	41.7	0.11
		130 "	"	39.5	0.11
		130 "	0.003″	41	0.01
		205 "	"	34	0
23	23°	0	0.003″	52	0.11
		20 O.- and C.S.	"	45.5	0
		20 "	0.0056″	38	0.19
		45 "	"	37	0.14
		70 "	"	34.5	0.1
		95 "	"	30	0.13
		115 "	"	28	0.14
		165 "	"	25	0.02
		205 "	"	16.5	0
24	25°	0	0.003″	49	0.08
		10 O.- and C.S.	"	49	0.06
		30 "	"	48	0.07
		50 "	"	47	0.09
		70 "	"	41	0.03
		90 "	"	22.5	0.07
		110 "	"	16.5	0.09
		130 "	"	7	0

Number of the experiment	Temperature	Stimuli applied	Stimulation-interval	Contraction-height after one stimulation	Degree of summation
25	25°	0	0.0033″	—	—
		10 O.- and C.S.	„	73 mm.	0.04
		30 „	„	69	0.05
		45 „	„	67.7	0.03
		60 „	„	69	0.0007
		75 „	„	59.5	0.03
		90 „	„	57	0.09
		120 „	„	47.5	0.17
		170 „	„	38	0.21
		220 „	„	30.5	0.25
		220 „	„	19	0
		230 „	„	11	0.18
		240 „	„	7.5	0.13
		250 „	„	3.5	0
		250 „	0.0066″	3.5	0.43
26	25°	0	0.0033″	—	—
		10 O.- and C.S.	„	66	0.023
		20 „	„	70	0.007
		30 „	„	69	0
		45 „	„	56.5	0.035
		60 „	„	47	0.032
		70 „	„	38	0
27	26°	0	0.0036″	58	0.10
		10 O.- and C.S.	„	56 ?	0.044 ?
		20 „	„	37	0.04
		30 „	„	26.5	0.113
		40 „	„	13	0.115
32	?	0	0.0036″	38.5	0.3
		10 O.- and C.S.	„	43	0.18
		20 „	„	43	0.16

Number of the experiment	Temperature	Stimuli applied	Stimulation-interval	Contraction-height after one stimulation	Degree of summation
32	?	40 O.- and C.S.	0.0036″	43.5 mm.	0.11
		140 „	„	41	0.09
		260 „	„	35	0.09
		340 „	„	30	0.09
		340 „	„	29.5	0.02
		385 „	„	27	0.09
		435 „	„	24.5	0.05
		465 „	„	23	0.011
64	12°	0	0.0053″	38	0.066
		250 O.- and C.S.	„	44.5	0.069
		500 „	„	43	0.08
		800 „	„	35.5	0.07
		800 „	„	28	0.07
		820 „	„	25.25	0.109
		870 „	„	25	0.13
		1020 „	„	25	0.08
		1220 „	„	22	0.09
65	9°	0	0.0053″	44	0.023
		200 O.- and C.S.	„	59	0.07
		400 „	„	50	0.09
		600 „	„	39	0.13
66	11°	0	0.0058″	44.5	0.13
		200 O.- and C.S.	„	59.5	0.075
		400 „	„	53	0.04
		600 „	„	45.5	—
		800 „	„	32	0.22

From the above values it appears, that the stimulation interval which, before fatigue sets in, only just produces distinct summation, varied from 0.002″ to 0.0075″ and mostly lay between 0.003″ and 0.005″. The degree of summation was then about 0.1. After the first ten, fifteen or twenty contractions this value had considerably decreased in most cases (experiments 20, 21, 22, 23, 26, 27, 28, 32 and 66) viz. to about half its original height, and in some cases (experiments 22, 23, 26) even to 0. Apart from numerous irregularities the degree of summation is likewise lowered by further stimulations but more slowly than at first; after a continued stimulation it became 0, for instance, in experiments 2, 4, 9, 20, 21, 24, 25. In 7 cases it became evident that an increased stimulation interval may cause summation again. The interval was lengthened

in N°.	4	from	0.002"	to	0.015"
,, ,,	9	,,	0.004"	,,	0.008"
,, ,,	20	,,	0.0025"	,,	0.0063"
,, ,,	21	,,	0.004"	,,	0.005"
,, ,,	23	,,	0.003"	,,	0.0056"
,, ,,	25	,,	0.0033"	,,	0.0066"
,, ,,	26	,,	0.0033"	,,	0.0054".

On the other hand there are likewise a rather great number of experiments (Nos. 1, 3, 13, 14, 17, 19, 64, 65) in which, mostly only at first, the degree of summation was heightened by stimulation. These changes were, however, as a rule much smaller than those mentioned above, while attempts to retain summation with smaller stimulation-intervals did not succeed.

It has not come to light, to what cause this instability in the consequences of the fatigue must be attributed. Those curve-series in which the decrease of the contraction-height, in other words the effect of the fatigue is most regular, (experiments 19, 20, 21, 23) likewise present a rather regular decrease of the degree of summation. Irregularities in the change of the contraction-heights mostly coincide with irregularities in the degree of summation. The decrease in the summation is most evident in those experiments in which fatigue considerably lengthened the *duration* of the contraction whilst its *height* almost remained the same (experiments 2, 4, 5, 6, 7, 8, 10); all these were carried out in March at a rather low temperature.

Hence the effect which fatigue may have on the duration of the refractory-stage, is a lengthening one.

Temperature.

In the following experiments the muscle was placed in a glass vessel, which in experiments 29 to 33 was filled with 0.7 °/₀ NaCl-solution, while in experiments 34, 60, 61, 62, 63 it acted as a moist chamber. The muscle was fastened underneath to the short arm of the lever and made equilibrium with a weight of about 5 grammes.

For the rest the installation was the same as in the other experiments.

The following is experiment 62.

At 10° the degree of summation was 0.04, the stimulation-interval being 0.0047″. After refrigeration to 5° no summation set in at this

Number of the experiment	Temperature	Stimulation-interval	Contraction-height after one stimulation	Degree of Summation
29	20°	0.0027″	78 mm.	0.21 ?
	10°	„	54,7	0.021
	10°	0.0075″	50	0.07
	5°	„	49.5	0.037
	5°	0.013″	49.5	0.05
30	18°	0.0045″	57	0.14 ?
	13°	„	34.5	0
	8°	0.0117″	34	0.007
31	20°	0 0054″	39	0.18
	17°	„	27	0.18
33	21°	0.0018″	20	0.5
	10°	0.0063″	9,5	0.3
	10°	0.0018″	4.5	0.1
34	20°	0.0018″	27	0.43
	10°	„	20	0
60	20°	0.0022″	45	—
	20°	0.0042″	42	0.15
	10°	„	38	—
	10°	0.0063″	38	—
	10°	0.0095″	38	0.13
62	10°	0.0047″	49	0.04
	5°	„	46	0
	5°	0.0115″	46	0.05
	2°	0.0115″	53	0
	2°	0.021″	53	0.037

interval, nor at an interval of 0.0063", whilst lengthening to 0.0115" made the degree of summation 0.05. The muscle having been cooled down to 2° the summation was 0 at a stimulation-interval of 0.0115" and 0.0157", whilst it became 0.037 at an interval of 0.021".

The other experiments gave similar results (See page 1223).

Refrigeration therefore lengthens the refractory stage.

The temperature from which we started being an arbitrary one the effect of heating was also investigated.

From the following values

Number of the experiment	Temperature	Stimulation-interval	Contraction-height after one stimulation	Degree of summation
60	9°	0.005"	39 m.m.	0.05
	20°	"	47.5	0.5
	20°	0.0022"	47.5	0.03
61	9°	0.0053"	35.25	0.12
	26°	0.001"	77	0.117
63	8°	0.0078"	42	0.098
	22°	0.00105"	60	0.25
67	12°	0.0042"	42.75	0.05
	30°	0.0021"	83	0.096

it appeared that heating shortens the refractory stage.

Carbonic acid.

In the moist chamber, used in the foregoing experiments carbonic acid was led; for the rest the installation was the same. Though carbonic acid gas will act upon the muscle but very slowly from outside, it may yet be assumed that within not too long a time it will have a marked influence on the condition of the muscle, since it prevents the carbonic acid formed by function in the muscle, from leaving it.

The following is experiment 59.

At a stimulation-interval of 0.0042" the muscle originally gives a summation of 0.06. After having been surrounded by carbonic acid gas for 15', it shows no summation at this interval, whilst summation becomes 0.06 again, if the stimulus-interval is lengthened to 0.0068".

Remaining in the carbonic acid the muscle gives at intervals of 10′ a summation of 0.046, 0.036, 0.035 and 0. Five minutes after, at a stimulus-interval of 0.0089″ the degree of summation is 0.009. After 65′, at an interval of 0.0115″, the summation amounts to 0.019; ten minutes later it is at this distance 0, for an interval of 0.0136″ 0.015, for one of 0.0168″ 0.03. 40 minutes after, after 115′ the degree of summation becomes 0 also for this distance.

The values of the other experiments are the following:

Number of the experiment	Temperature	Exposure to CO_2	Stimululation interval	Contraction-height after one stimulus	Degree of summation
43	13°	0	0.002″	28 m.m.	0.035
		10′	„	27.5	0.035
		20′	„	—	—
44	?	0	0.002″	29	0.55
		?	„	—	—
45	14°	0	0.002″	56.5	0.009
		10′	„	35.25	0.02
		10′	0.003″	35.25	0.02
		20′	0.002″	37	0
		20′	0.003″	32.5	0
		30′	0.002″	27.5	—
		30′	0.003″	20.5	0.1
46	14°	5′	0.0035″	62	0.024
		15	„	60	0.05
		25	„	57	0.026
47	17°	0	0.0033″	37.5	0.04
		10′	0.0066″	15	0.1
		10′	0.011″	15	0.17
48	11°	0	0.0055″	18	0.11
		5′	„	13	0.115
		10′	„	14.5	0
		15′	0.008″	13	0.08

Number of the experiment	Temperature	Exposure to CO_2	Stimulation-interval	Contraction-height after one stimulation	Degree of summation
49	?	0	0.003″	50 m.m.	0.04
		10′	„	45	0.04
		20′	„	40	0.025
50	?	0	0.0022″	41.5	0.08
		0	„	45	0.08
		10′	„	50.5	0.05
		20′	„	53	0.038
		30′	„	53	0.075
		30′	„	56	0.027
		40′	„	54	0.027
		50′	„	50.5	0.03
		60′	„	52	0.03
51	12°	0	0.0018″	37	0
		0	0.0054″	33	0.035
		5′	„	49.5	0.01
		25′	„	48.5	0.01
		35′	„	46	0.022
		50′	„	45	0
52	16°	0	0.002″	60	0.033
		15′	„	64.5	0.05
		25′	„	54.5	0.12
		35′	„	54	0.13
53	13°	0	0.0063″	46.5	0.15
		30′	„	43.5	0.103
		45′	0.0063″	46.5	0
		45′	0.0084″	46.5	0.02
		45′	0.0157″	46.5	0.054
54	13°	0	0.0048″	54	0.074
		20′	„	55.5	0.117

Number of the experiment	Temperature	Exposure to CO_2	Stimulation-interval	Contraction-height after one stimulation	Degree of summation
54	13°	30′	0.0048″	56 mm.	0.107
		50′	"	57.5	0.05
		60′	"	55	0.046
55	14°	0	0.00525″	34	0.03
		15′	"	23.5	0.04
		20′	"	19	0
		30′	"	—	0
56	6°	0	0.0054″	43	0
		?	"	23	0
		?	0.0072″	23	0
		?	0.0054″	20.5	0
57	12°	0	0.005″	40	0.04
		10′	"	35	0.03
		20′	"	33	0.03
		30′	"	32	0.03
		40′	"	32	0
		50′	—	—	?
58	8°	0	0.0066″	20.5	0.07
		10′	0.011″	26	0.019
		20′	0.014″	15	0.033
		30′	"	13.5	0.07
		40′	"	9	0.1
		40′	0.0088″	9	0
59	10°—13°	0	0.0042″	42	0.06
		15′	"	34	0
		15′	0.0068″	34	0.06
		25′	"	33	0.046
		35′	"	27.5	0.036
		45′	"	28	0.035

Number of the experiment	Temperature	Exposure to CO_2	Stimulation-interval	Contraction-height after one stimulation	Degree of summation
59	10°—13°	55′	0.0068″	27.5 mm.	0
		60′	0.0089″	25.5	0.009
		65′	0.0115″	26	0.019
		75′	„	33	0
		75′	0.0136″	33	0.015
		75′	0.0168″	33	0.03
		90′	„	35	0.013
		100′	„	35.5	0.056
		115′	„	31.5	0

From this table it appears that if the muscle was surrounded for some time by carbonic acid, the degree of summation grew considerably less, and even fell in about half the cases to about 0 (experiments 45, 48, 51, 53, 55, 57, 58 and 59). Lengthening of the stimulation-interval often caused the summation to return as in experiments 48, 53, 58, 59. In some cases the degree of summation increased after carbonic acid had been led in for some time, viz. in experiments 46, 47, 52, 54 and 55; this was, however, mostly followed by a decrease. Also in this series of experiments a decrease in the summation, with an eventual necessity to lengthen the stimulation-interval, most clearly manifested itself, when the duration of the contraction grew much longer and its height did not decrease much.

From the foregoing experiments it appears that the refractory stage of the frog's gastrocnemius is lengthened by being cooled down, by carbonic acid and by fatigue, especially in those cases in which under these influences the contraction is protracted. In connection with this, the lengthening of the refractory stage most regularly sets in at refrigeration, whilst fatigue gives the most unstable results. This lengthening takes place in various degrees; mostly the stimulation-interval was at last from two to five times its original length, so that it may be assumed that the refractory stage was likewise changed to about that extent.

As regards the average length of the refractory stage, the values of the intervals used are of course somewhat greater, because the limit where summation only just takes place had mostly not been exactly arrived at. The average of the stimulation-intervals was for

9°	0.006″
10°	0.0047″
11°	0.0039″
12°	0.0047″
13°	0.0034″
14°	0.0033″
18°	0.0045″
20°	0.003″
21°	0.0029″
22°	0.0036″
23°	0.003″
25°	0.0032″
26°	0.0036″

in which, though somewhat irregular because of the relatively small number of experiments, it is evident that this, and consequently also the refractory stage, is greater at a low temperature. After refrigeration or heating to a certain temperature, the values found become respectively greater and smaller than those above.

The average of the values is somewhat less than 0.004″, so that the average value of the refractory stage of the frog's gastrocnemius must also be somewhat less than 0.004″, which is 0.001″ less than the value found by Tigerstedt for the latent period.

The figure represents the contraction curves of a muscle after it had been cooled down from 14° to 4°. At 14° the stimulation-interval still showing summation was found to be 0.003″. At 4° stimulation with the same interval produces no summation; the contraction-curve caused by two stimulations (II) coincides with the contraction-curve after one stimulation (I). The interval was lengthened to 0.0075″, then to 0.017″ without summation following (curves II′ and II″). Lengthening to 0.028 gives curve II‴, in which the summation is plainly visible.

Chemistry. — *"The double acid from normal- and allo-cinnamic acids"*. By A. W. K. de Jong. (Communicated by Prof. P. van Romburgh).

(Communicated in the meeting of January 26, 1918).

This double acid can be prepared by well known methods, for instance by allowing a solution, saturated at ordinary temperature with respect to both acids, to crystallize slowly, or by dissolving in such a solution equal quantities of the two acids by heat, when on cooling the double acid separates.

It can be readily prepared from normal cinnamic acid as follows: A dilute aqueous solution of sodium cinnamate (10 grm. of cinnamic acid per litre) is exposed to light in open tanks (at Buitenzorg about 3 months suffice for tanks of about 12 litres capacity and 500 cm^2 surface, with illumination in the fore-noon only). The solution is then evaporated to dryness; the acids are liberated by concentrated hydrochloric acid and are separated from the sodium chloride by hot motor spirit. The petrol solution often gives on cooling crystals of β-cinnamic acid, which on standing pass into the α-variety. After cooling, the solution is exposed to the air in a dish. Generally crystallisation begins soon and well formed, often rhomb-shaped crystals of the double acid separate. On further evaporation of the solution either crystals of allo cinnamic acid are formed, or this acid separates as an oil.

Since there exist two modifications of the normal acid, and three are known of the allo-acid, it would be quite possible that several different double acids should exist.

Now it is not difficult to prepare a solution, saturated with respect to both acids, which is at the same time in equilibrium with one of the three different forms of allo-cinnamic acid. For this purpose a warm concentrated solution of the two acids is prepared and should then be boiled for $^1/_4$ hour. The flask is plugged with cotton wool, to prevent infection, and the contents are cooled, which causes separation of the double acid, together with melted allo-acid (the temperature here is 25—30°). This solution is in equilibrium with allo-acid of melting point 42°. If the solution is inoculated with allo-cinnamic acid melting at 58° or 68°, it soon crystallizes and solutions are obtained which are in equilibrium with these two modifications. If these solutions are allowed to evaporate in the air, the double acid separates together with the modification of the allo-cinnamic acid employed for inoculation.

The petrol solution, mentioned above, can also furnish solutions answering the purpose, by inoculation with the desired modifications.

In this way it is easy to make the double acid crystallise in the presence of large crystals of allo-cinnamic acid of melting point 58° or 68°.

By boiling the warm concentrated petrol solution of the two acids for $^1/_4$ hour, it is possible to obtain by cooling the double acid which separates in the presence of melted acid of m.p. 42°.

The double acids obtained in this manner were analysed according to the method given in Rec. Trav. chim. 31, 259, when it was found, that all consisted of equal quantities of the acids, as is seen from the following table. A preliminary experiment with 0.0545 grm. allo- and 0.0510 grm. normal cinnamic acid showed that the 20 c.c. of benzene used dissolved 2.25 c.c. N/10 allo-cinnamic acid as aniline salt.

Double acid	Quantity taken grm.	Titration		Recovered dissolved cinnamic acid grm.	Normal cinnamic acid found	
		15 c.c.	20 c.c.		grm.	%
with allo-, m.p. 42°	0.1040	3.35	4.5	0.0185	0.0520	50
„ „ „ 58°	0.1005	3.1	4.1	0.0245	0.0511	52
„ „ „ 68°	0.1015	3.05	4.1	0.0215	0.0496	49

The various double acids show the same melting intervals; they begin to sinter at about 56° and are fully melted at about 90° ¹).

In order to ascertain, whether these varieties of double acids formed in the presence of crystals of allo-cinnamic acid of m.p. 58° and 68°, and in a solution, in equilibrium with the acid of m.p. 42°, were identical, an aqueous solution was prepared saturated at room-temperature with respect to both acids, and in equilibrium with the acid of m. p. 42° (in this case also, when working at

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¹) Possibly the triclinic allo-cinnamic acid of Erlenmeyer Jun., with m.p. 80°, is the double acid of normal and allo-cinnamic acids. Ber. **39**, 1571 (1906); **40**, 662, 663 (1907). Crystallographic measurements were not made on account of the absence of the necessary instruments.

25—30°, the allo-acid was obtained in the melted condition in the presence of the crystals of the double acid).

To portions of this solution crystals of the double acid were added, which, in order to prevent infection by traces of allo-cinnamic acid, had been washed a few times with a little alcohol and then dried between filter paper. If the double acids were different, those which had been formed in the presence of acid of m.p. 58° and 68°, in a solution in equilibrium with the acid of m.p. 42°, should have brought about crystallisation of allo-cinnamic acid melting at 58° or 68° respectively and of the corresponding double acids. The experiment showed, however, that this is not the case; the double acids had no effect whatever on the solution, which proves them to be identical.

Similar experiments were carried out with a petrol solution prepared by boiling for ¼ hour a solution from which the double acid and allo-cinnamic acid of m.p. 68° had crystallised, with an adequate quantity of allo-cinnamic acid and the double acid. Next day the solution had not crystallised, but it did so at once on shaking. After standing for a few hours with occasional shaking the experiment with the double acids was carried out, but here also no effect at all was observed. The solution itself was found to crystallise at once on inoculation with allo-cinnamic acid of m.p. 58° and 68°, and to a very large extent, much more than the aqueous solution. It need hardly be pointed out that in these experiments it is necessary to guard against infection.

After the stir created by BIILMANN's well known paper in which he first described the ready inter-conversion of the allo-cinnamic acids and attributed it to trimorphism, various papers have appeared, both from supporters and opponents of his views.[1] The opponents wish to regard the acids as chemical isomerides; thus STOBBE and SCHÖNBURG[2]) write as the result of their elaborate investigation "dasz die Allo- und Isozimmtsäuren drei chemisch verschiedene Individuen sind."

This conclusion cannot however be correct, since my experiments prove the identity of the double acids of normal and of allo-cinnamic acids, formed in the presence of the allo-acids of m.p. 58° and 68° and in a solution in equilibrium with the acid of m.p. 42°. For if the allo-cinnamic acids were chemically distinct there ought also to be three different double acids with normal cinnamic acid.

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1) For literature references see Ann. **402**, 187 (1913).

2) Ann. **402**, 258 (1913).

But if we accept BIILMANN's view, the occurrence of one and the same double acid and of three forms of allo-cinnamic acid is intelligible, since it would represent the most stable molecular arrangement, and other arrangements would be very metastable or incapable of existence.

I hope shortly to publish some experiments designed to clear up this contradiction.

Buitenzorg, October 11, 1917.

Physiology. — *"On the Respiratory Movements of the Frog"*. By Prof. V. Willem. (Communicated by Prof. G. van Rijnberk).

(Communicated in the meeting of March 23, 1918).

When preparing the practical exercises given to students in biology at the Physiological Laboratory of Amsterdam, it appeared to me that the numerous essays which the literature on the respiratory movements of the frog offers, leave still several points unexplained, and that the technical experiments executed by us furnished a more exact insight into some particularities.

I have namely registered better curves of the pressure in the mouth-cavity than those which were hitherto obtained. The manometer was brought into this cavity through the ductus Eustachii, after piercing the tympanum; sometimes the experiment was applied to frogs whose tongues had previously been removed. In other experiments the tube was brought into the bucco-pharyngeal cavity along the oesophagus.

The pressure in the lungs was registered according to the method of Graham Brown. At the same time the movements of the throat were registered five times enlarged. During these experiments the frog was lying on its back, and the lever was either erected as for the suspension-curves of the heart, according to Engelmann, or the animal was sitting in the normal squatting posture, and the registration was performed by means of a special apparatus which I shall describe later on.

As an example I communicate here a diagram demonstrating the relations between the movements of the bottom of the mouth and the modifications of the pressure in the bucco-pharyngeal cavity. By means of a number of such like graphical representations I have composed the following diagram of the synchronism in the phenomena of the natural respiratory ventilation; in a more circumstantial essay I shall explain the experimental data from which this scheme has been composed.

1. During the space of time (1—2) the pressure in the lungs (P) descends first quickly, afterwards more slowly; at the same time the pressure in the mouth-cavity, which was about 0 (atmospheric pressure) (B) rises to the same level (about 1 centimeter water).

This proves that the moment of the opening of the glottis that brings the lungs into connection with the bucco-pharyngeal cavity, must be placed on the ordinate 1. The period 1—2 is consequently the duration of the expiration of the lungs.

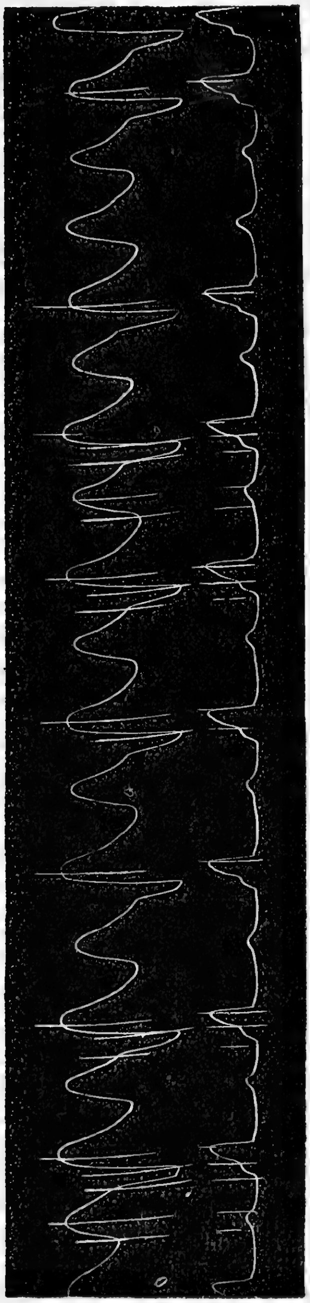
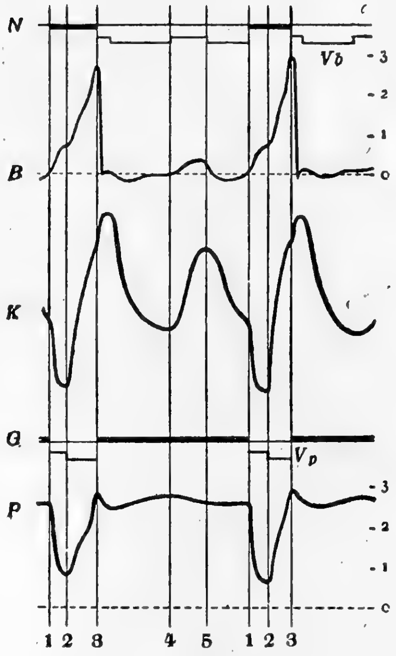


Diagram 1. At the top. Movements of the mouth-bottom.
At the Bottom. Pressure in the bucco-pharyngeal cavity.



N.	Opening and closure of the nostrils.
Vb.	Bucco-pharyngeal ventilation.
B.	Pressure in the bucco-pharyngeal cavity (at the rightside, scale in cm. water).
K.	Registration of the **movements** of the throat.
G.	Opening and closure of the glottis.
Vp.	Pulmonary ventilation.
P.	Pressure in the lungs (scale in cm. water).

This rising of the pressure in the mouth-cavity presses the bottom of the mouth down (*K* 1—2); this displacement is consequently only passive.

2. From the moment 2 the bottom of the mouth rises; at the same time (from 2 to 3) the bucco-pharyngeal pressure (*B*), which had remained for an instant stationary, rises to about 3 centimeters of water. The pressure in the lungs presents an equal rising: it appears consequently that the glottis is still open. The period 2—3 corresponds consequently to the inspiration of the lungs.

The comparison of the three curves indicates that this phenomenon is to be attributed to an active contraction of the muscles of the mouth-bottom. For the rest the pressure-curve has a diphasical course, which is not shown by the curve of the displacement of the fore-end of the hyoid: this difference indicates that the compression of the air is the consequence of a more complicated movement than that of the registered point. For the rising of the mouth-bottom takes place in two stages and the hyoid moves during its displacements up and down.

3. In the position 3 the bucco-pharyngeal pressure (*B*) suddenly descends to 0; consequently the opening of the nostrils takes place there.

Sometimes we perceive in this position an inflexion; the origin of this inflexion-point can be explained in the following manner: the air in the mouth which was more and more compressed during the former period, offered to the contraction of the muscle an increasing opposition, which at the opening of the cavity suddenly ceases; thereupon the upward movement continues with a greater velocity. This inflexion, as can easily be understood, occurs expecially in such cases in which the contraction of the muscles is not very strong.

This last part of the upward movement of the mouth-cavity (beyond ordinate 3) is accompanied by a slight rising of the pressure in the bucco-pharyngeal cavity, which follows immediately after the descent of the lever (*K*); this is in this case a very short expiration-phase in the bucco-pharyngeal ventilation (*Vb*).

From this moment 3 the lung-curve proves to be independent of the pressure in the mouth-cavity: consequently the glottis has closed itself at the very moment that the nostrils opened themselves.

4. The following part of the diagram *K*, between 3 and 1 reproduces the course of the bucco-pharyngeal ventilation. It consists here of three parts: a first descending part of much longer duration than the rising 2—3; an upward movement, the top of which is

lower than the former; and at last a second descending part that ends in an expiration of the lungs. We know for the rest that the number of such like fluctuations is very variable, and for simplicity's sake only one has been represented.

These fluctuations of the mouth-bottom are accompanied by modifications of the pressure in the mouth which is alternately above and below the zero-point, and amounts at the utmost to a few millimeters of water. Consequently these movements are caused by active movement of the muscles, and are the factors of the bucco-pharyngeal ventilation.

At the same time the graphical representation of the pressure in the lungs shows little fluctuations presenting an opposite direction to that of the diagram *K*. Since GRAHAM BROWN it is generally admitted that they are brought about by the contraction resp. expansion of the lungs on account of the displacement of the larynx.

In a subsequent essay I intend to communicate the variations of the typical movement-complex.

Physics. — *"On the Energy of the Gravitation Field in* EINSTEIN'S *Theory."* By Dr. GUNNAR NORDSTRÖM. (Communicated by Prof. H. A. LORENTZ.)

(Communicated in the meeting of January 26, 1918.)

In a preceding paper we considered some general theorems derived from EINSTEIN'S gravitation theory, and especially a field with spherical symmetry.[1] Referring to this paper, which in the following will be denoted by a Roman I, the energy of the gravitation field will now be calculated, according to EINSTEIN'S conception viz. characterized by the quantity t_4^4 [2]) from formula (10) I. In order to obtain a result that holds for an electric field too, I shall first calculate the gravitation field of an electric centre.

§ 1. *The field of an electric centre.*

The gravitation field of an electric body can be calculated by the aid of the variation principle in the form (1) I or (1*a*) I, if only we keep in mind that the electro-magnetic field gives an additive contribution to HAMILTON'S function $\mathfrak{M}$.

We put

$$\mathfrak{M} = \mathfrak{M}^{(e)} + \mathfrak{M}^{(m)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $\mathfrak{M}^{(e)}$ refers to the electro-magnetic field, $\mathfrak{M}^{(m)}$ to the matter (in a limited sense). For $\mathfrak{M}^{(e)}$ we have [3])

$$\mathfrak{M}^{(e)} = \frac{1}{8\pi} \sqrt{-g} \sum_{\alpha\beta\mu\nu} g^{\alpha\mu} g^{\beta\nu} \left(\frac{\partial \varphi_\beta}{\partial x_\alpha} - \frac{\partial \varphi_\alpha}{\partial x_\beta}\right)\left(\frac{\partial \varphi_\nu}{\partial x_\mu} - \frac{\partial \varphi_\mu}{\partial x_\nu}\right) + 2 \sum_\sigma \varphi_\sigma \mathfrak{w}^\sigma, \quad (2)$$

where φ indicates the components of the 4 dimensional potential, $\mathfrak{w}_\sigma$ the components of the 4 dimensional electric current.

When the field is stationary and all electric charges at rest, we have

$$\varphi_1 = \varphi_2 = \varphi_3 = 0, \qquad \mathfrak{w}^1 = \mathfrak{w}^2 = \mathfrak{w}^3 = 0.$$

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[1] G. NORDSTRÖM, "On the mass of a material system according to the gravitation theory of EINSTEIN. These Proceedings, XX, 1917, p. 1076.

[2] It will be known that a different conception of the gravitation theory has been enunciated by H. A. LORENTZ.

[3] J. TRESLING, These Proceedings XIX, p. 892.
A. D. FOKKER, These Proceedings XIX, p. 968.

We put

$$\varphi_4 = \varphi, \qquad \mathfrak{w}^4 = \varrho.$$

φ indicates then the electro-static potential, ϱ the density of the electricity. Further we assume, that the field possesses spherical symmetry and choose the time coordinate so that $g_{r4} = 0$. We then have $\sqrt{-g} = u\,w\,p^2$, and for $\mathfrak{M}^{(e)}$ we find the following expression, the validity of which is seen most simply for a point on one of the axes of coordinates, but which must be generally valid as $\mathfrak{M}^{(e)}$ depends on r only and not on the direction from the centre,

$$\mathfrak{M}^{(e)} = -\frac{p^2}{4\pi\,uw}\left(\frac{d\varphi}{dr}\right)^2 + 2\,\varphi\varrho \quad . \quad . \quad . \quad . \quad . \quad (3)$$

For the integral of $\mathfrak{M}^{(e)}$ over a 4 dimensional extension of a fitly chosen form we obtain

$$\iiiint \mathfrak{M}^{(e)}\,dx_1\,dx_2\,dx_3\,dx_4 = 4\pi\,(t_2 - t_1)\int_{r_1}^{r_2} \mathfrak{M}^{(e)}\,r^2\,dr, \quad . \quad . \quad . \quad (4)$$

where

$$4\pi\int_{r_1}^{r_2} \mathfrak{M}^{(e)}\,r^2\,dr = \int_{r_1}^{r_2} \left\{ \frac{-r^2 p^2}{uw}\left(\frac{d\varphi}{dr}\right)^2 + 8\pi\,\varphi\,\varrho\,r^2 \right\} dr. \quad . \quad (4a)$$

The laws for the electric field we found by variation of φ, while u, w, p, ϱ were kept constant. As the expressions for $\mathfrak{G}^*$ and $\mathfrak{M}^{(m)}$ do not contain φ, we obtain by this variation

$$2\frac{d}{dr}\left(\frac{r^2p^2}{uw}\frac{d\varphi}{dr}\right) + 8\pi\,\varrho\,r^2 = 0$$

The integration gives

$$-\frac{r^2p^2}{uw}\frac{d\varphi}{dr} = e\,(r), \qquad \frac{d\varphi}{dr} = -\frac{uw}{p^2}\frac{e\,(r)}{r^2} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where $e(r)$ denotes the total charge in a sphere with radius r. Outside the body $e(r)$ is constant and equal to the total charge of the body.

As in the infinite w has the value c (see I p. 1079), we easily see that e and ϱ are its charge and density in *electro-magnetic units*, φ on the contrary the potential in *electro-static units*.

Now we shall calculate the gravitation field. This calculation can be made with the aid of formulae (38) I. Then we must first calculate the stress-energy-tensor for the electro-magnetic field by replacing $\mathfrak{M}$ in formula (2) I by $\mathfrak{M}^{(e)}$ and by introducing the expression (2).

We shall however shorten the calculation by application of the variation principle in the form (28) I. In these variations we must keep φ and ϱ constant and vary u, w and $v = rp$. On the right-hand side of (28) I we have of course to introduce $\mathfrak{M} = \mathfrak{M}^{(e)} = \mathfrak{M}^{(m)}$, so that this form is split up into two parts which we shall consider separately. First we consider the part containing $\mathfrak{M}^{(e)}$. We then obtain, attending to (4a) and (5),

$$4\pi\,\delta\int_{r_1}^{r_2}\mathfrak{M}^{(e)}\,r^2\,dr = \int_{r_1}^{r_2}\left\{-\left(\frac{d\varphi}{dr}\right)^2\delta\left(\frac{v^2}{uw}\right)\right\}dr = -\int_{r_1}^{r_2}\frac{u^2w^2e^2}{v^4}\,\delta\left(\frac{v^2}{uw}\right)dr,$$

$$\delta\int_{r_1}^{r_2}\mathfrak{M}^{(e)}\,r^2\,dr = \int_{r_1}^{r_2}\frac{uw\,e^2}{4\,\pi v^2}\left(\frac{\delta u}{u} - 2\frac{\delta v}{v} + \frac{\delta w}{w}\right)dr. \quad . \quad . \quad . \quad (6)$$

As to the part containing $\mathfrak{M}^{(m)}$ we still have our former formula (36) I, if only we keep in mind that, $\mathfrak{M}$ being replaced by $\mathfrak{M}^{(m)}$, $\mathfrak{T}$ gets the significance of a stress-energy-tensor for the matter, if the electric field is considered as *not* belonging to the matter. In this § $\mathfrak{T}$ will keep this altered significance.

Considering the equation (31) I and (6) together with (36) I, in which $\mathfrak{M}^{(m)}$ has been introduced instead of $\mathfrak{M}$, we obtain for the variation formula:

$$\left.\begin{aligned} 2\delta\int_{r_1}^{r_2}\left\{\frac{w\,v'^2 + 2v\,v'\,w'}{u} + uw\right\}dr = \varkappa\int_{r_1}^{r_2}\left\{\left(\frac{uw\,e^2}{4\pi v^2} + 2r^2\,\mathfrak{T}_r^r\right)\frac{\delta u}{u} + \right. \\ \left. + 2\left(-\frac{uw\,e^2}{4\pi\,v^2} + 2\,r^2\,\mathfrak{T}_p^p\right)\frac{\delta v}{v} + \left(\frac{uw\,e^2}{4\pi\,v^2} + 2r^2\,\mathfrak{T}_4^4\right)\frac{\delta w}{w}\right\}dr. \end{aligned}\right\} \quad . \quad (7)$$

Executing the variations we find the following set of equations, which take the place of the set (38) I

$$\left.\begin{aligned} &-\frac{wv'^2 + 2\,vv'w'}{u^2} + w = \frac{\varkappa}{u}\left(\frac{uw\,e^2}{8\pi\,v^2} + r^2\,\mathfrak{T}_r^r\right), \\ &-\frac{wv'' + v'w' + vw''}{u} + (vw' + wv')\frac{u'}{u^2} = \frac{\varkappa}{v}\left(-\frac{uw\,e^2}{8\pi\,v^2} + r^2\,\mathfrak{T}_p^p\right). \\ &-\frac{2v\,v'' + v'^2}{u} + u + 2v\,v'\frac{u'}{u^2} = \frac{\varkappa}{w}\left(\frac{uw\,e^2}{8\pi\,v^2} + r^2\,\mathfrak{T}_4^4\right). \end{aligned}\right\} . \quad (8)$$

These equations determine the gravitation field, when the tensor $\mathfrak{T}$ for the matter and the distribution of the electricity are given[1]).

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[1]) Comparing this set of equations with (38) I we see from the right-hand sides of (8), how the components of the stress-energy-tensor for the electro-magnetic

We shall calculate the field *outside the electric body* where we have

$$\mathfrak{T}^r_r = \mathfrak{T}^p_p = \mathfrak{T}^4_4 = 0,$$

$$e = \text{constant}.$$

In order to execute the calculation of the field we must fix the system of coordinates and we do this by putting the condition

$$p = 1 \quad \text{viz.} \quad v = r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Introducing this in (8), the last equation divided by $-u$ gives

$$\frac{1}{u^2} - 2r\frac{u'}{u^3} = 1 - \frac{\varkappa}{8\pi}\frac{e^2}{r^2},$$

$$\frac{d}{dr}\left(\frac{r}{u^2}\right) = 1 - \frac{\varkappa}{8\pi}\frac{e^2}{r^2}.$$

By integration we obtain

$$\frac{r}{u^2} = r - \alpha + \frac{\varkappa}{8\pi}\frac{e^2}{r},$$

where α is an integration constant. To simplify the formulae we put

$$\frac{\varkappa e^2}{8\pi} = \varepsilon^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and find

$$\frac{1}{u^2} = 1 - \frac{\alpha}{r} + \frac{\varepsilon^2}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

This formula expresses u as a function of r. An expression for w gives us the first formula (8). If in that formula we introduce the expression found for $\frac{1}{u^2}$ and reverse the sign, we obtain

$$(w + 2r\,w')\left(1 - \frac{\alpha}{r} + \frac{\varepsilon^2}{r^2}\right) = w - w\frac{\varepsilon^2}{r^2}.$$

A simple calculation gives

$$2\frac{w'}{w} = \frac{\dfrac{\alpha}{r^2} - 2\dfrac{\varepsilon^2}{r^3}}{1 - \dfrac{\alpha}{r} + \dfrac{\varepsilon^2}{r^2}}.$$

On the right-hand side the numerator is just the derivative of the

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field can be expressed by e, u, w, p, and the coordinates. We find that in the electro-magnetic field $+\frac{uw}{8\pi p^2}\frac{e^2}{r^4}$ corresponds to $\mathfrak{T}^r_r$ and $\mathfrak{T}^4_4$, $-\frac{uw}{8\pi p^2}\frac{e^2}{r^4}$ to $\mathfrak{T}^p_p$. For the diagonal sum of the components we find identically zero.

denominator, so that integrating, and choosing the integration constant in such a way that in the infinite w^2 has the value c^2, we find

$$w^2 = c^2 \left(1 - \frac{a}{r} + \frac{\varepsilon^2}{r^2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

So we have calculated the gravitation field. Comparing (11) and (12) we obtain

$$u\,w = c \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and further we find (comp. I note on p. 1087)

$$\sqrt{-g} = c \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

Because of (10) ε determines the electric charge of the body. The constant a determines the mass of the body. Formula (50) I gives namely

$$m = \frac{4\pi\, a}{\varkappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

The same expression for m has been deduced in I p. 1087 for the case $c = 1$. Now we see that this expression also holds when the unit of time has been chosen in another way.

We obtained the relation (15) by application of formula (50) I, where we assumed, that the matter has a finite extension. If the electro-magnetic field is reckoned to the matter as is done in I, the matter has, strictly speaking, an infinite extension. This has however no influence on the validity of formula (50) I in the case in question. The quantities of the electric field approach namely sufficiently to zero, when the distance from the centre increases.

We may ask on what conditions $g_{44} = w^2$ can become zero and negative. According to (12) u^2 becomes then infinite resp. negative. The expression (12) shows that outside the body

$$w^2 = 0 \quad \text{for} \quad r = \frac{a}{2} \pm \sqrt{\frac{a^2}{4} - \varepsilon^2}.$$

For smaller values of r w^2 is negative.

Only when

$$\frac{a^2}{4} > \varepsilon^2$$

w^2 becomes therefore zero and negative for real values of r. If on the contrary $\varepsilon^2 > \frac{a^2}{4}$, then w^2 and u^2 are everywhere finite and positive. Then we may very well assume the mass and the charge to be concentrated in a mathematical point.

§ 2. *The energy of the gravitation field.*

In this § we shall calculate the distribution of the energy in a gravitation field with spherical symmetry viz. we shall calculate the quantity $\mathfrak{t}_4{}^4$ in such a field. The body which excites the field may also be electric. Our calculation be based upon the formulae (17) I and (13) I, which give

$$\varkappa \mathfrak{t}^4{}_4 = -\tfrac{1}{2}\varkappa \sum_\alpha \mathfrak{T}^\alpha_\alpha + \tfrac{1}{2}\sum_\tau \frac{\partial \mathfrak{A}_\tau}{\partial x_\tau} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (16)$$

We suppose $\sum_\alpha \mathfrak{T}^\alpha_\alpha$ to be given as a function of r. When we can calculate the vector $\mathfrak{A}$ we find from this formula also $\mathfrak{t}_4{}^4$. We shall deduce an expression for $\mathfrak{A}_1$ which holds at a point of the X_1 axis ($x_2 = x_3 = 0$). According to (5) I we have:

$$\mathfrak{A}_1 = \tfrac{1}{2}\sqrt{-g}\sum_{\mu\nu\sigma}(g^{\mu 1}\, g^{\nu\sigma} - g^{\sigma 1}\, g^{\mu\nu})\left(\frac{\partial g_{\mu\sigma}}{\partial x_\nu} + \frac{\partial g_{\nu\sigma}}{\partial x_\mu} - \frac{\partial g_{\mu\nu}}{\partial x_\sigma}\right).$$

As in I § 3 we shall choose the time-coordinate, so that $g_{r4} = 0$. At a point of the X_1 we have because of (33) I and (25) I:

$$\left.\begin{aligned} & g^{11} = -\frac{1}{u^2}, \quad g^{22} = g^{33} = -\frac{1}{p^2}, \quad g^{12} = g^{13} = g^{23} = 0, \\ & \frac{\partial g_{12}}{\partial x_2} = \frac{\partial g_{13}}{\partial x_3} = -\frac{1}{r}(u^2 - p^2), \quad \frac{\partial g_{22}}{\partial x_1} = \frac{\partial g_{33}}{\partial x_1} = -2\,p\,p'. \end{aligned}\right\} \quad . \qquad (17)$$

As to the calculation of $\mathfrak{A}_1$ we first remark that the terms of the summation for which $\sigma = \mu$ give a contribution zero. In order that a term may give a contribution different from zero, either σ or μ has to be equal to 1, while the other two of the three indices σ, μ, ν must be equal.

$\mu = 1$, $\sigma = \nu \neq 1$ gives for $\mathfrak{A}_1$ the contribution:

$$\tfrac{1}{2}\sqrt{-g}\cdot g^{11}\left(g^{22}\frac{\partial g_{22}}{\partial x_1} + g^{33}\frac{\partial g_{33}}{\partial x_1} + g^{44}\frac{\partial g_{44}}{\partial x_1}\right),$$

$\sigma = 1$, $\mu = \nu \neq 1$ gives the contribution:

$$-\tfrac{1}{2}\sqrt{-g}\;g^{11}\left\{g^{22}\left(2\frac{\partial g_{12}}{\partial x_2} - \frac{\partial g_{22}}{\partial x_1}\right) + g^{33}\left(2\frac{\partial g_{13}}{\partial x_3} - \frac{\partial g_{33}}{\partial x_1}\right) - g^{44}\frac{\partial g_{44}}{\partial x_1}\right\}.$$

The two contributions together give $\mathfrak{A}_1$. Introducing the expressions (17) we obtain for $\mathfrak{A}_1 = \mathfrak{A}_r$

$$\mathfrak{A}_r = -4\frac{w\,p\,p'}{u} + \frac{2}{r}\,u\,w\left(1 - \frac{p^2}{u^2}\right) - 2\frac{p^2 w'}{u}. \quad . \quad . \qquad (18)$$

For a point on the X_1 axis, the right-hand side is equal to $\mathfrak{A}_1$. If we consider a point not on the X_1 axis, the same expression holds for the radial component $\mathfrak{A}_r$.

Formula (18) holds for each system of coordinates in which the spherical symmetry is taken into consideration, if only the time-coordinate is chosen so that $g_{r_4} = 0$. If we specialize the system of coordinates by putting $p = 1$, then the expressions (11) and (12) are valid outside the body and for u and w we obtain

$$\mathfrak{A}_r = \frac{2c}{r}\left(\frac{a}{r} - \frac{\varepsilon^2}{r^2}\right) - c\left(\frac{a}{r^2} - 2\frac{\varepsilon^2}{r^3}\right),$$

$$\mathfrak{A}_r = \frac{ca}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Because of the spherical symmetry we have, of course, for the components of $\mathfrak{A}$ in the directions of the axes of coordinates in space

$$\mathfrak{A}_\tau = \frac{x_\tau}{r}\frac{ca}{r^2}, \qquad \tau = 1, 2, 3. \quad . \quad . \quad . \quad . \quad . \quad (19a)$$

Making up the divergency we obtain

$$\sum_\tau \frac{\partial \mathfrak{A}_\tau}{\partial x_\tau} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

and formula (16) gives

$$\mathfrak{t}_4^4 = -\tfrac{1}{2} \sum_\alpha \mathfrak{T}_\alpha^\alpha .$$

Outside the body the right-hand side is zero and therefore also

$$\mathfrak{t}_4^4 = 0 \qquad (r > R) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

If the system of coordinates is chosen so that $p = 1$ [1]*, the gravitation field has everywhere outside the body the density of energy zero.* This is also true when the body is electrically charged, because for the electro-magnetic field the sum of the diagonal components of the stress-energy-tensor is equal to zero (see the note on p. 1087).

Now we may ask whether the other stress-energy-components t_μ^ν for the gravitation field are also equal to zero. This is suggested by formula (17) I, which gives

$$\sum_\alpha \mathfrak{t}_\alpha^\alpha = 0.$$

That really all $\mathfrak{t}_\mu^\nu$ are zero in systems of coordinates for which $p = 1$ is easily proved with the aid of formula (52) of EINSTEIN, Grundlage. The details will be left here aside.[2]

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[1] Then we have also $\sqrt{-g} = c$ as has been proved in § 1

[2] For a non-electric centre E. SCHRÖDINGER has meanwhile proved this too: E. SCHRÖDINGER, Die Energiekomponenten des Gravitationsfeldes. Phys. Zeitschr. **19**, 1918 p. **4**. (Remark in the proof.)

If a system of coordinates is chosen for which $p \neq 1$, both $\mathfrak{t}_\mu^\nu$ and the other $\mathfrak{t}_\mu^\nu$ are different from zero. **Einstein** deduced approximated values for the $\mathfrak{t}_\mu^\nu$ in a system of coordinates in which the velocity of the light is independent of the direction of propagation[1], and these expressions show that the $\mathfrak{t}_\mu^\nu$ are not zero. In this case $\mathfrak{t}_\mu^\nu$ can also be calculated by means of formulae (16) and (18). For that system of coordinates in which the velocity of light is independent of the direction of propagation, we have according to **Droste**[2] for u, p, w outside the body

$$u = p = \left(1 + \frac{a}{4r}\right)^2, \qquad w^2 = 1 - \frac{a}{r\left(1 + \frac{a}{4r}\right)} \quad . \quad . \quad (22)$$

These expressions can be introduced into (18). The further calculation will not be given here.

The property of the components $\mathfrak{t}_\mu^\nu$ that they can be made to vanish by a transformation of coordinates when the field possesses spherical symmetry, is evidently connected with the circumstance, that not for every transformation of coordinates these quantities behave like tensor-components. It is remarkable, that the change of the system of coordinates, necessary to make the components $\mathfrak{t}_\mu^\nu$ vanish is undetectably small for really existing gravitation fields.

This circumstance renders the conception of the quantities $\mathfrak{t}_\mu^\nu$ as stress-energy-components somewhat less sympathic, and supports the conception of the gravitation energy enunciated by **Lorentz**. Whatever may be however our opinion on this subject we have no ground to banish the quantities $\mathfrak{t}_\mu^\nu$ from **Einstein**'s gravitation theory. As is evident from our considerations in I § 2, several general theorems may be derived in a simple way with the aid of these quantities.

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[1] A. **Einstein**, Näherungsweise Integration der Feldgleichungen der Gravitation, Berl. Ber. 1916, p. 688.

[2] **Droste**, Het zwaartekrachtsveld, p. 20, equation (31).

Mathematics. — *"On Elementary Surfaces of the Third Order."* (Fourth communication.) By Dr. B. P. HAALMEIJER. (Communicated by Prof. L. E. J. BROUWER.)

(Communicated in the meeting of February 23, 1918).

It has been proved that F^3 cannot exist if no plane section consists of three lines[1]). We now proceed to show that F^3 cannot exist if that surface does not contain 3, 7, 15, 27, or an infinite number of lines.

This note is divided into two parts, the first contains a few theorems we shall want later on, in the second we show that except those mentioned, no other number of lines is possible on F^3.

§ 1. *Theorem* 1. *If a plane section of F^3 consists of three lines forming a triangle, then through none of the angles can pass a further line of F^3.*

Let the plane be denoted by α, the lines by a_1, a_2, a_3 and their points of intersection by A_1, A_2, and A_3 (A_1 is the point where a_2 and a_3 meet etc.). We choose on a_1 an arbitrary point B_1 (not coinciding with A_2 or A_3). B_1 is limiting point, both of sequences of points of F^3 situated above α and below α. For suppose B_1 were limiting point only of sequences situated below α. Then choose a plane β through B_1, not containing the line a_1 and not passing through A_1. Let b be the line of intersection of α and β and let b intersect a_2 at B_2 and a_3 at B_3. Let a parallel line in plane β converge towards b from above. The point B_1 on the limiting line is supposed not to be limiting point of points of F^3 situated on the converging lines, hence B_1 must count double on b as point of intersection with the curve in β. Besides b carries the points B_2 and B_3 : a contradiction.

From the above follows that in every segment of one of the lines a_1, a_2, and a_3, containing no angle of the triangle, the sectors of F^3 meet from different sides of α. In plane α four branches meet at A_1 : $A_3 A_1$ and CA_1 on a_3 and $A_2 A_1$ and BA_1 on a_2 (fig. 1). From the above result, in connection with the assumption that F^3 is a

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[1]) Again *line* will be used for *straight line.*

twodimensional continuum, we conclude that each branch is connected with the neighbouring ones alternately above and below α.

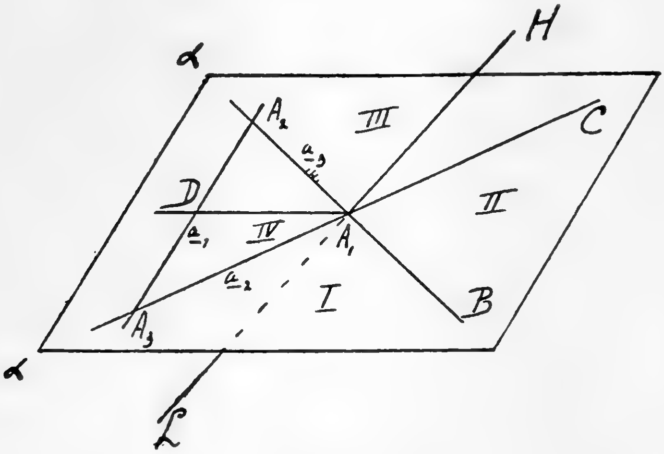

Fig. 1.

Let $A_3 A_1$ be connected with BA_1 above α by I, BA_1 with CA_1 below α by II, CA_1 with $A_2 A_1$ above α by III and lastly $A_2 A_1$ with $A_3 A_1$ below α by IV.

Now suppose a third line LH of F^3 passed through A_1. Of course LH cannot be situated in α. Let the semiline A_1H depart on I and A_1L on II. We consider the plane β through A_1H and A_1D (fig. 1). In β the curve consists of LH and an oval passing through D. This oval also passes through A_1 because in β a branch must depart from A_1 on IV. Now let β turn slightly round LH, then the same conclusions hold for these new planes and this means that in an infinite number of planes through LH ovals would pass through A_1. This however contradicts the results obtained in the third communication (p. 744 at the top). The only way to escape immediate contradiction is to assume the ovals to be degenerated in all planes β. Then however F^3 would contain an infinite number of lines, a possibility we do not consider while looking for the possible finite numbers.

Theorem 2. *If F^3 contains a finite number of lines, this number is equal to* $3 + n \,.\, 4$ (*n being a positive integer*). *This division can be made in such a way that the lines of which the first group of* 3 *consists, lie in one plane and every new group of* 4 *intersects one of these* 3. *Besides every group of* 4 *can be divided into two groups of* 2, *each lying* (*with one of the first* 3) *in one plane.*

Before proving this we shall consider some auxiliary questions.

Let a be a line of F^3. Throughout this note we assume that F^3 contains no infinite number of lines. In the third communication we proved that to every point A of a corresponds a point B of that line with the same tangent plane. Suppose A and B move in opposite directions when the tangent plane turns round a. Then there are two meeting points P and Q. We showed that F^3 cannot exist if in no tangent plane through a the restcurve degenerates. Now it may be possible that this degeneration just takes place in the tangent plane of a meeting point, for instance P. The restcurve would then consist of two different lines through P (both different from a [1]), or two coinciding lines through P.

We begin by showing that this last case is impossible. Let α be the tangent plane at P. The section in α consists of a and another line b, counting double. Now let tangent planes through a converge towards α, then the two corresponding points on a converge from different sides towards P and the ovals in the tangent planes converge towards b. From this follows that in each of the two semilines in which P divides b, the sectors of F^3 meet from the same side of α, but from different sides for different semilines.

If on the other hand we start from line b, then it appears that P is on b also meeting point of corresponding points, and line a takes the part just now played by b. Hence in each of the semilines of a sectors meet from the same side of α, but from different sides for different semilines. These results, combined with the assumption that F^3 is a twodimensional continuum, cannot be fitted in with any connection between the four branches meeting at P in α.

The possibility might be put forward that b counts double in every plane. Then however in all these planes the restcurve would be a line, and F^3 would contain an infinite number of lines.

We shall now consider the case that the restcurve in α consists of two different lines through P. This means that the curve in α consists of three different lines a, b and c through P. Let the tangent planes $\alpha_1, \alpha_2 \ldots\ldots$ through a converge towards α. Then the points $P_1, P_2 \ldots.$ on a converge from the one, and the corresponding points $P'_1, P'_2 \ldots.$ from the other side, towards P.

The ovals in the tangent planes must, in the long run, intersect the line at infinity at two points, and are then divided by that line

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[1] The possibility that the restcurve degenerates into a and a second line different from a we did not consider in the third communication, because we then assumed that no second line of F^3 intersects a. Here however this case must be considered. It then appears fairly easily that, anyway for a surface of the third order with a finite number of lines, this case cannot occur.

in two branches. Now a priori, two possibilities exist, that P_n and P_n' end up by being situated on different branches, which then face each other with their convex sides, or that they lie on the same branch. We proceed to show that the last case is excluded. The position in α_n is denoted by the dotted line in fig. 2. These curves

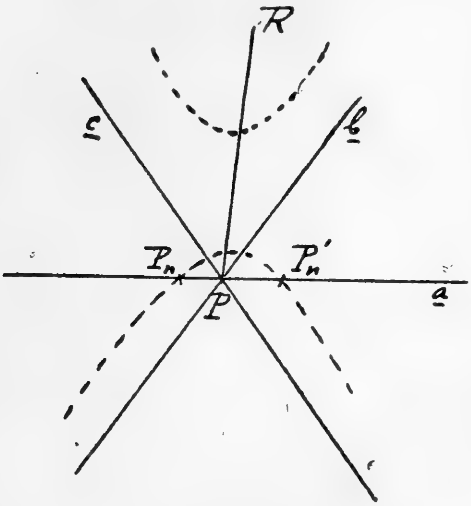

Fig. 2.

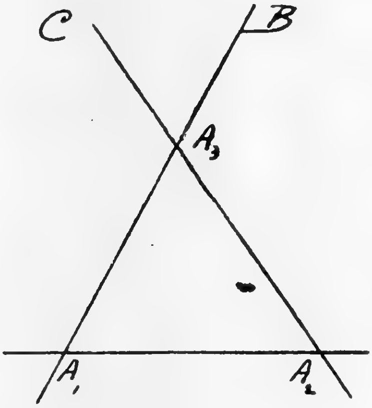

Fig. 3.

have for limiting set the lines b and c in α. The lower branch has for limiting set the lower semilines of b and c, hence the upper branch must take the upper semilines for its portion. Now if we choose through line PR a plane, for instance $\perp \alpha$, then in this plane, P must be a cusp, where the branches meet from the same side of the tangent and this is excluded for a curve of the third order.

We are now in a position to prove theorem 2. Let the section in a plane α consist of three lines forming a triangle $A_1 A_2 A_3$ (fig. 3). Above it appeared that in the neighbourhood of A_3 the connection is, for instance, as follows: $A_3 B$ is connected with $A_3 A_2$ and $A_3 A_1$ with $A_3 C$ both above α, and $A_3 B$ with $A_3 C$ and $A_3 A_2$ with $A_3 A_1$ below α. Hence if a plane through $A_1 A_2$ converges, in the neighbourhood of A_3 from above, towards α, then the oval is divided by the line at infinity into two branches having respectively for limiting sets the semilines $A_3 B + A_3 A_2$ and the semilines $A_3 C + A_3 A_1$. If on the other hand, the plane converges in the neighbourhood of A_3 from below towards α, these limiting sets become $A_3 B + A_3 C$ and $A_3 A_2 + A_3 A_1$.

From this we conclude: If a plane turning round a line a of F^3 passes through a position in which the restcurve degenerates in such a way, that the three lines do not pass through one point, and if the branches through corresponding points of a first face each other with their convex sides, then after passing through the degeneration the branches will face each other with their concave sides (or vice versa).

Now let F^3 contain 3 lines in one plane: a_1, a_2 and a_3. If there is a fourth line b_1, it cannot lie in that plane, but it does intersect one of the first 3 lines, for instance a_1.

In the plane through a_1 and b_1 the curve consists of three lines, hence another line b_2 intersects a_1. In the third communication we concluded that F^3 cannot exist without 3 lines situated in one plane, as otherwise the convexity and concavity of the branches through corresponding points of a_1 lead to a contradiction. Now the above results show, that the arrangement of this matter, which was made possible by a first degeneration of the oval in a plane through a_1, is upset again by a second degeneration, and a third is necessary to put things straight once more.

This concludes the proof of theorem 2 (we remind the reader of the fact that degeneration into 3 lines through one point is only possible, when the branches in the converging planes face each other with their convex sides.

§ 2. If F^3 contains more than 3 lines their number is at least 7, of which at least 6 intersect the same line (according to theorem 2). Now on this line can be situated, at the utmost, two meeting points of corresponding points, and it follows, that in at least one plane the curve consists of 3 lines forming a triangle. Let α be this plane, a, b and c the lines, and A, B and C their points of intersection (A is situated on b and c etc.). If F^3 contains 7 lines then one of the lines in α, for instance a, is intersected by 4 other lines, not situated in α. We proceed to show, that if the number of lines on F^3 is greater than 7, it is at least 15.

If F^3 contains more than 7 lines, then at least two lines in α are each intersected by at least 4 lines, not situated in α, or one of them is intersected by at least 8 lines not situated in α. Let us consider the first case: a and b are each intersected by at least 4 lines of F^3, not situated in α, in other words: through each of the lines a and b pass at least 2 planes different from α, in which the oval is degenerated.

Let a_1 be a line of F^3 not situated in α, but intersecting a at a point A_1. According to theorem 1, A_1 cannot coincide with either B or C. Let β be a plane through b in which the oval is degenerated. Line a_1 intersects this plane at a point not situated in α. This point must lie on one of the two lines in which the oval in β is degenerated. This line b_1 intersects b at a point B_1 different from A and C. We shall consider the plane through a_1 and b_1. The curve in this plane contains a_1 and b_1, hence it consists of three lines. None of these lines can be situated in α because $A_1 B_1$ does not coincide with one of the lines in α. Now the plane through a_1 and b_1 has a point of intersection with line c and it follows, that c is intersected by one line, hence at least 4 lines of F^3 not situated in α. Hence each of the 3 lines in α is intersected by at least **4** others and the total number is at least 15.

We now come to the second case: the line a is intersected by at least 8 lines of F^3 not situated in α, that is: in at least 4 planes through a, different from α, the oval degenerates. From these lines, which intersect a, we choose 3: a_1, a_2 and a_3 not intersecting each other. These three lines determine a scroll of the second degree. This scroll intersects at least one of the two lines b and c at two different points, because α cannot be tangent plane both at B and C. For instance, let the scroll have a point B_1 (different from C) in common with b. This means that through B_1 passes a line which intersects a_1, a_2 and a_3. But then this line has 4 points in common with F^3, hence is entirely situated on that surface. Thus b is intersected by one line, hence by at least 4 lines of F^3, not situated in α. The reasoning used for the first case now shows that c is also intersected by at least 4 lines, not situated in α, and this brings up the total to at least 19.

We now proceed to show, that if the number of lines exceeds 15, it is at least 27. If there are more than 15, the above results show that one line of α (for instance a) is intersected by at least 8, and each of the other two by at least 4 lines of F^3 not situated in α.

Let b_1 be a line not situated in α, but intersecting b, and let b_1' be the third line in the plane through b and b_1. Line b_1 intersects 4 lines, which do not intersect each other, and which all intersect a. We consider the planes through b_1 and each of these 4 lines. These planes have in common with α four different lines through the point of intersection of b and b_1, hence these planes intersect c at 4 different points. Through each of these 4 points passes a line of F^3 not situated in α. In the same way b_1' intersects 4 lines, which do not intersect each other and each of which intersects a, and once more we obtain 4 points of c through which pass lines of F^3. Now none of these last 4 points can coincide with one of the first, because in that case a line of F^3 would pass through that point and through the point of intersection of b_1 and b'_1, and this is impossible according to theorem 1.

Hence c is intersected by at least 8 lines not situated in α, and the same can be proved for b. This brings the total up to at least 27.

It remains to be proved that the number of lines is infinite, if greater than 27.

Theorem: 4 *lines of* F^3, *not intersecting each other and having one common intersecting line, have at least two intersecting lines in common.*

Let the lines be a_1, a_2, a_3, a_4 and let a be a first line, intersecting these four. Of course a belongs to F^3 also. The third lines in the planes through a and a_1, a_2, a_3, a_4 we denote respectively by a'_1, a'_2, a'_3, a'_4.

The oval must degenerate in a fifth plane α through a. Let the lines is this plane be b and c. The foregoing results show, that b and c are each intersected by at least 8 lines of F^3, not situated in α [1]. Let us consider these 16 lines: $p_1, p_2, \ldots p_{16}$. Each of these intersects 4 crossing lines of the set: a_1, a_2, a_3, a_4, a'_1, a'_2, a'_3, a'_4.

It is impossible that 2 lines p for instance p_n and p_m, intersect the same 4 lines a_n, for in that case these 4 lines would have 3 different intersecting lines in common (p_n, p_m and a), hence an infinite number, and F^3 would contain an infinite number of lines.

Now from the lines $a_1 \ldots a_4$, $a'_1 \ldots a'_4$ we can choose in 16 different ways, 4 lines which cross each other (they are situated by twos in the same plane), and considering there are also 16 lines p, each group of 4 lines a_n which cross each other, have a common intersecting line, different from a. Hence this holds for the group a_1, a_2, a_3, a_4 which we set out to prove.

Now that this theorem and theorem 1 of § 1 have been proved, refer the reader for the further proof that F^3 contains an infinite number of lines if that number exceeds 27, to the demonstration given by JUEL (Math. Ann. 76, p. 561 and 562).

This finishes the examination of the numbers of lines, without which F^3 cannot exist.

We conclude with a more or less independent result, which happened to present itself:

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[1] The possibility might be put forward, that the point of intersection of b and c is situated on a, for which case the foregoing results have not been established. This objection can be met as follows: We choose three lines a, a_n and a'_n, forming a triangle. The former results then show, that a_n and a'_n are each intersected by 8 lines, which do not intersect a. These lines can then be used instead of the lines p_1, $p_2 \ldots p_{16}$, introduced in the text.

To make it possible for F^3 *to contain* 7 (*and not more*) *lines, it is necessary that on the common line of intersection* (which certainly exists), *corresponding points move in opposite directions.*

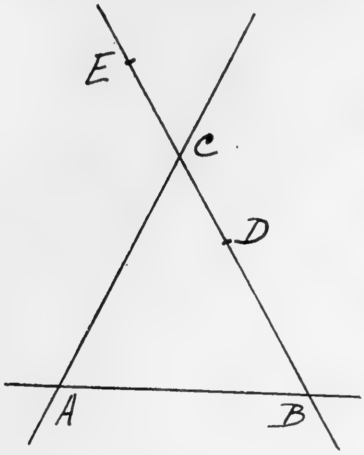

Fig. 4.

Let the curve in a plane α consist of the lines a, b, and c, forming a triangle ABC (fig. 4). Suppose a is intersected by 4 lines of F^3, not situated in α, and let corresponding points on a move in the same direction. We denote by e and d two lines intersecting a at E and D and situated in the same plane. Because corresponding points on a move in the same direction, BC and DE are couples of points which separate each other.

Now let the point P at which e and d intersect be situated *above* α (the case that P lies in the plane at infinity shall be dealt with presently). We turn plane α round AB in such a way, that the half containing the triangle moves upwards. The lines a and b are then replaced by an oval, which at first is divided into two branches by the line at infinity.

The points of intersection of this revolving plane and the lines e and d, are at first situated on different branches of the oval. However, in the point P they meet again, hence before the plane reaches this position, or just when passing through P, the points mentioned above must move on to the same branch again. This however can only be effected, either when one of the branches retires towards the line at infinity (and this is excluded, as one of the linesegments EP or DP must be intersected), or via a degeneration. Hence this case cannot occur unless the lines b and c are also intersected by lines of F^3, not situated in α.

It is not necessary to give another demonstration for the case that P is situated in the plane at infinity, or the case that C and E (fig. 4) are separated by the point at infinity of the line, as the whole argument is proof against projective transformation.

By means of a reasoning, showing strong analogy with the above, the following proposition can still be proved: *The case of* 3 (and not more) *lines on* F^3 *cannot occur, if on each of these* 3 *lines corresponding points move in opposite directions.*

Physics. — *"On the Theory of the Brownian Movement"*. By Prof. J. D. van der Waals Jr. (Communicated by Prof. J. D. van der Waals.)

(Communicated in the meeting of February 23, 1918).

Pretty numerous are the methods by which it has been tried to calculate the mean path to be expected of a "Brownian" particle suspended in a liquid or gas. Most of the methods of calculation start from the supposition that such a particle will experience a friction in its movement. This means that it is assumed that from the force acting on such a particle, a term may be separated opposite to and proportional to its velocity, and that the remaining part of the force will be independent of the velocity. Both forces are a consequence of the collisions of the surrounding molecules of the substance in which the particle is suspended. The methods of calculation based on this supposition arrive at a result which Einstein was the first to communicate, namely that:

$$\overline{\Delta^2} = \frac{RT}{N} \frac{1}{3\pi\zeta a} t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1a)$$

In this $R =$ the absolute gas constant
$N =$ the number of molecules per gramme-molecule
ζ = the coefficient of viscosity of the medium
a = the radius of the particle
t = the time
$\Delta =$ the deviation in the time t in a definite direction, e.g. in the direction of the X-axis. I shall call Δ briefly the deviation in what follows. I shall further speak of the force, when I mean the X-component of the force. The dash over Δ^2 denotes that the mean value has been taken for *all* the suspended particles (a great number).

In the derivation of this formula it has been assumed that the force of friction may be represented by the formula given for it by Stokes:

$$\mathfrak{K} = 6\,\pi\,\zeta\,a\,\dot{x} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Let us examine the way in which e.g. Langevin arrives at the formula for Δ^2. He starts from:

$$m\,\ddot{x} = -6\,\pi\,\zeta\,a\,\dot{x} + X \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

in which X will be the irregular force that does not depend on the velocity. By multiplication by x we get:

$$\frac{m}{2}\frac{d^2x^2}{dt^2} - m\left(\frac{dx}{dt}\right)^2 = -3\pi\zeta a\frac{dx^2}{dt} + Xx.$$

We then take the mean values, in which Xx falls out; we also put $\frac{d\overline{x^2}}{dt} = z$ and $m\overline{\left(\frac{dx}{dt}\right)^2} = \frac{RT}{N}$, which yields the differential equation:

$$\frac{m}{2}\frac{dz}{dt} + 3\pi\zeta a z = \frac{RT}{N},$$

through the integration of which Langevin arrives at equation (1). Now it is clear that this contains an inconsistency. When equation (3) is multiplied by $\dot{x}$, and this is then averaged over all the particles, we get:

$$\overline{m\dot{x}\ddot{x}} = -6\pi\zeta a\overline{\dot{x}^2}.$$

The lefthand member is $\frac{m}{2}\frac{d\overline{\dot{x}^2}}{dt}$, which quantity is therefore smaller than zero. Hence $\overline{\dot{x}^2}$ cannot remain constant, but it must exponentially descend to zero. This means that the Brownian movement would not always continue, but that the particles would soon be reduced to rest in consequence of the viscosity. Yet Langevin puts $m\overline{\dot{x}^2} = \frac{RT}{N}$. And it is only owing to this inconsistency that he finds the value of (1a) for Δ^2.

Not every derivation of (1a) which rests on the supposition of a friction against the thermal movement [1]) of the Brownian particles is open to the above-mentioned objection to the same extent. By the method of Einstein and Hopf, which, originally developed for another problem, was adapted for the calculation of $\overline{\Delta^2}$ by Mrs. de Haas-

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[1]) I shall understand by *thermal* or *true* velocity of a particle the velocity that a particle has at a definite moment, for which therefore holds $m\dot{x}^2 = \frac{RT}{N}$. I shall put over against it the *measurable* velocity. This will be defined by $\frac{\Delta}{t}$, in which Δ is the deviation reached in a measurable time t. t will be of the order of magnitude of 1 sec. Of course the mean value of the measurable velocity will be independent of the moment at which the measurement begins. It will, however, be dependent on the length of the interval t, as the mean value of Δ increases only proportional to $\sqrt{t}$. Compare further Remark IV at the end of this paper.

Lorentz in her Thesis for the Doctorate, equation (3) is first integrated over a small time τ, so small that the velocity *changes not much* during τ. We then get:

$$m(\dot{x}_2 - \dot{x}_1) = -6\pi\zeta a(x_2 - x_1) + \int_0^\tau X\,dt .$$

And now it is not assumed that the equation $\overline{X\dot{x}} = 0$ holds for any moment, but that $\dot{x}_1 \int_0^\tau X dt$ is zero on an average. As $\dot{x}$ does not differ much from $\dot{x}_1$ during τ, the difference between the two suppositions is not great. But whereas $\overline{X\dot{x}} = 0$ excludes the performance of work by the force X — at least of a work the *mean value* of which differs from 0 — this is not the case with the supposition $\dot{x}_1 \int_0^\tau X dt = 0$. Here the possibility exists that X on an average performs positive work of such an amount that it compensates the loss of kinetic energy by the viscosity.

Yet it seems to me that when has once it been seen that the splitting up of the force into a "force of viscosity" and "irregular forces" is untenable, also the splitting up of the force into two terms, as it has been applied by Mrs. de Haas, becomes problematical. At bottom also this splitting up is after all based on the idea of a friction against the thermal movement of the particles.

Miss Snethlage and I [1]) have therefore thought we had to take another course to arrive at an equation for Δ^2. We started for this purpose from the postulate that:

$$\overline{\mathfrak{K}\dot{x}} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

When this equation is differentiated with respect to t, it yields:

$$\overline{\frac{d\mathfrak{K}}{dt}\dot{x}} + \frac{1}{m}\overline{\mathfrak{K}^2} = 0 . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B)$$

This is satisfied in the simplest way by putting:

$$\frac{d\mathfrak{K}}{dt} = -p^2\dot{x} + q \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (C)$$ [2])

in which q will be a function of t that repeatedly changes its sign

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[1]) These Proc. Vol. XVIII, p. 1322.

[2]) See for the justification of this equation the exposition in Remark I at the end of this paper.

and the value of which is *statistically independent* of $\dot{x}$. By the aid of kinetic considerations we derived from this formula a formula for $\overline{\Delta^2}$ which had the form:

$$\overline{\Delta^2} = \frac{C}{a^2} t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1b)$$

C represents a constant the value of which we can leave undetermined for the present. When we compare this value with EINSTEIN's value (equation (1a)), we see that they may both be represented by:

$$\Delta^2 = b\,t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

with $b =$ constant, i. e. that the mean deviation is proportional to $\sqrt{t}$. This is nothing but the well-known result of the calculus of probability for the sum of a great number of terms when the mathematical expectation for that sum is zero. I shall assume in the future that Δ^2 will certainly be represented by an equation of the form (1)[1]. Then it only remains the question to calculate b. The difference between (1a) and (1b) is that this quantity is in inverse ratio to a according to (1a), and to a^2 according to (1b). The experiments carried out by Miss SNETHLAGE[2], have demonstrated that equation (1b) can certainly not be accurate, which is the more remarkable, because also other kinetic derivations of Δ^2 had yielded equations of this form[3].

Thus we were confronted by the difficulty that experiment pronounced in favour of equation (1a), whereas the derivation of (1b), though doubt as to its validity is not excluded, seemed nevertheless much less assailable to me than the introduction of a force of viscosity against the thermal movement, which was the foundation on which (1a) was based. I have had an opportunity to discuss the derivation of the two formulae with several physicists as LORENTZ, EINSTEIN, EHRENFEST, ORNSTEIN, and ZERNIKE, and it is partly owing to their remarks that I think that I am now able to give a method of calculation of $\overline{\Delta^2}$, which without starting from the supposition of a friction against the thermal velocity, leads to the accurate result, (1a), at least as far as the dependence of a is concerned.

For this I shall start from the simple formula:

$$\ddot{x} = w(t)$$

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[1] A further proof of this supposition is found in Remark II at the end of this paper.

[2] A. SNETHLAGE. Moleculair-kinetische verschijnselen in gassen, inzonderheid de Brownsche Beweging. Academisch Proefschrift. Amsterdam 1917. B. Experimenteel gedeelte.

[3] VON SMOLUCHOWSKI. Ann. d. Phys. **21** p. 769. Ann. 1906.
A. SNETHLAGE. l. c. Hoofdstuk II.

which, integrated, yields:

$$x = x_0 + \dot{x}_0 t + \int_0^t w(\theta) \,.\, (t-\theta)\, d\theta \quad . \quad . \quad . \quad . \quad (4)$$ [1]

The path travelled over in the time t is $x - x_0 = \Delta$. When this equation is multiplied by $w(t)$, and when the mean values for all the particles are taken, then $\overline{\dot{x}_0 w(t)}$ becomes $= 0$, hence:

$$\overline{\Delta \;\; w(t)} = \overline{w(t) \int_0^t w(\theta) \,.\, (t-\theta)\, d\theta} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The lefthand side is negative, it is a constant which I shall represent by Q, i.e. it is independent of the value of t, provided this is not taken too small. The proof of this I shall give in remark III at the end of this paper. When we divide this equation by t, it appears that also

$$\frac{\Delta}{t} \,.\, w(t) < 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5a)$$

in virtue of this we may put:

$$w(t) = -r^2 \frac{\Delta}{t} + s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$ [2]

in which

$$\overline{s\Delta} = 0 \,.\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

When this equation is multiplied by Δ, and averaged, we get:

$$\overline{\Delta \,.\, w(t)} = -r^2 \frac{\overline{\Delta^2}}{t} = -Q \quad \text{or} \quad r^2 = \frac{Q}{b} \quad . \quad . \quad . \quad . \quad (8)$$

Equation (6) may be expressed in words as follows: The force that acts on a particle at a given moment, may be split up into a *force of viscosity against the measurable movement* and a term that is independent of the *measurable* velocity. The splitting up of the force into a force of viscosity and an irregular force is, therefore, permitted *provided we assume that the measurable velocity, not the thermal velocity, is damped by such a force.* If we could compute r^2 by a kinetic way, we might arrive at a complete derivation of EINSTEIN's formula for $\overline{\Delta^2}$. The calculation of r^2 will, however, be no doubt attended with great difficulties. It seems, however, not very hazardous to me, to assume the value $6\pi\zeta a$ from STOKES's formula for it; possibly the value corrected by CUNNINGHAM. This

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[1] Compare Remark II at the end of this paper.

[2] Compare Remark I at the end of this paper.

seems arbitrary at first sight, but it seems to me that it will not be thought so arbitrary, when it is fully realized what the friction given by STOKES's formula, really is.

Let us imagine a particle suspended in a gas which is made to fall under the influence of gravitation, in order to determine its radius in the usual way. Here too there is a measurable movement (here the motion of falling), and a thermal movement. Now we get a correct formula for the measurable motion by assuming that a force of viscosity $-6\pi\zeta a\dot{z}$ opposes *this* movement. And as we demonstrated that also in the Brownian movement a force: a constant × the measurable velocity, opposes the measurable movement, it is very natural to assign the value $-6\pi\zeta a$ to this constant. This becomes still more plausible when we think of equation (C)

$$\frac{d\mathfrak{K}}{dt} = -p^2\dot{x} + q.$$

It contains that if there is a velocity $\dot{x}$, this will try to bring about a force in opposite sense; hence to cause the product force × velocity to become negative. It is counteracted in this by the fact that the force tries to develop a velocity in its own sense; hence to make the product in question positive. If $\dot{x}$ is the thermal velocity, the two tendencies counterbalance each other: the product remains zero on an average. The thermal velocity varies its sign repeatedly. If, however, there is a velocity in the same sense for a longer time, $\frac{d\mathfrak{K}}{dt}$ continues to keep the same sign all this time, and a force opposite to that velocity will be developed. And this will take place independent whether this velocity of longer duration is owing to gravity, or whether it represents a measurable velocity of the Brownian movement.

When we assign this value to r^2, (7) yields:

$$\overline{\Delta^2} = Q\frac{1}{6\pi\zeta a}t$$

Q is defined by equation (5). It would, however, not be possible to derive its value from it without introducing further, perhaps pretty uncertain, hypotheses. Fortunately, however, it is possible to find a value for $\overline{\Delta^2}$ in another way, namely by multiplying (6) by Δ, bearing in mind that $\frac{d\Delta}{dt} = \dot{x}$ and $\frac{1}{m}w(t) = \frac{d^2\Delta}{dt^2}$. We then find:

$$\Delta\frac{d^2\Delta}{dt^2} = \tfrac{1}{2}\frac{d^2\Delta^2}{dt^2} - \left(\frac{d\Delta}{dt}\right)^2 = -\frac{1}{m}.\,6\pi\zeta a\frac{\Delta^2}{t} + s.\Delta \quad . \quad . \quad (9)$$

When on account of $\overline{\Delta^2} = bt$, we put $\frac{d^2\overline{\Delta^2}}{dt^2} = 0$, and further $\overline{\left(\frac{d\Delta}{dt}\right)^2} = \overline{\dot{x}^2} = \frac{RT}{mN}$, we find when again we take the mean value for all particles:

$$\overline{\Delta^2} = \frac{RT}{N}\frac{1}{6\pi\zeta a}t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1c)$$

This result has half the value of that of equation ($1a$). The good result that Miss SNETHLAGE found when calculating N from her observations, shows that ($1a$) is to be preferred to ($1c$). Though accordingly it does not lead to the accurate result, I hope that the above derivation will contribute to give us a clearer insight in the theory of the Brownian movement.

When we do not want to start from the supposition $\overline{\Delta^2} = bt$, equation (9) can also be solved, when first mean values have been taken in a way that strongly reminds of LANGEVIN's. When we put $\overline{\Delta^2} = \xi$ and $\overline{\left(\frac{d\Delta}{dt}\right)^2} = \frac{RT}{mN}$ we find for ξ the differential relation [1]:

$$\tfrac{1}{2}\frac{d^2\xi}{dt^2} + r^2\frac{\xi}{t} = \frac{RT}{Nm} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

When we first take this equation without the second member, and when we substitute $\xi = \eta\sqrt{t}$, we get:

$$\frac{d^2\eta}{dt^2} + \frac{1}{t}\frac{d\eta}{dt} + \left(\frac{2r^2}{t} - \frac{1}{4t^2}\right)\eta = 0\,.$$

When we put besides $t = \frac{\tau^2}{8r^2}$, we find:

$$\frac{d^2\eta}{d\tau^2} + \frac{1}{\tau}\frac{d\eta}{d\tau} + \left(1 - \frac{1}{\tau^2}\right)\eta = 0\,.$$

This is Bessel's differential equation for $n = 1$, the solution of which runs in the current notation:

$$\eta = A\,I_1(\tau) + B\,Y_1(\tau)\,.$$

Hence:

$$\overline{\Delta^2} = \sqrt{t}\,[A\,I_1(2r\sqrt{2t}) + B\,Y_1(2r\sqrt{2t})]$$

would be the solution of the equation without the second member.

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[1] The solution of this equation following here I owe to a kind communication from Prof. W. KAPTEYN of Utrecht to whom I gladly express my gratitude here.

For the equation with the second member we must add to it the term:

$$\overline{\Delta^2} = \frac{RT}{N} \frac{1}{6\pi\zeta a} t.$$

When we again write $\frac{6\pi\zeta a}{m}$ for r^2, it appears that for t of the order 1 sec. the argument of I_1 and Y_1 becomes of the order 10^4. For such large arguments the terms with the Bessel's functions may be neglected, so that equation (1*c*) is left.

Remark 1. The equation (*C*) on p. 1256 $\frac{d\mathfrak{K}}{dt} = -p^2 \dot{x} + q$ has been derived by Miss SNETHLAGE and me from equation (*A*):

$$\overline{\mathfrak{K}\,\dot{x}} = 0$$

and used for the calculation of $\overline{\Delta^2}$. ORNSTEIN and ZERNIKE have advanced objections to this equation and the use we have made of it. Erroneously as it seems to me. But the fact that they make objections to it shows that the validity of this and simular equations requires further elucidation. In itself equation (*C*) is of course not inaccurate, but nothing can be derived from it. It only gets its meaning from the significance that is assigned to p^2 and q.

When we multiply (*C*) by $m\dot{x}$, we get[1]:

$$m\,\dot{x}\,\frac{d\mathfrak{K}}{dt} = -m\,p^2\,\dot{x}^2 + m\,q\,\dot{x}\,.$$

When we then average over all the particles, and when equation (*B*) is taken into account, we find:

$$-\overline{\mathfrak{K}^2} = -m\,p^2\,\overline{\dot{x}^2} + m\overline{q\,\dot{x}}\,.$$

Up to now we have left p^2 and q entirely undetermined. When we now, however, choose

$$p^2 = \frac{\overline{\mathfrak{K}^2}}{m\overline{\dot{x}^2}} \quad \ldots\ldots\ldots\ldots \quad (D)$$

it appears that:

$$\overline{q\,\dot{x}} = 0 \quad \ldots\ldots\ldots\ldots \quad (E)$$

$p^2 =$ constant for a swarm of suspended particles that are in a stationary state. Hence the equation (*C*) simply means this that for a definite particle I separate the $\frac{d\mathfrak{K}}{dt}$ into two terms. I choose as

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[1] I owe this derivation to an oral communication by Mr. ZERNIKE.

first term — $p^2\dot{x}$ with $p^2 = \frac{\overline{\mathfrak{K}^2}}{m\overline{\dot{x}^2}}$. I call the function of t that is left, q; it has the property that $\overline{q\dot{x}} = 0$, provided I average over all the particles. When I write $\frac{d\mathfrak{K}}{dt} = m\frac{d^2\dot{x}}{dt^2}$, ($C$) becomes a differential equation, which can be integrated. And afterwards mean values can be taken over the different particles. What objections Ornstein and Zernike have to this way of procedure, is not clear. It is entirely incomprehensible why they assert that (C) would not be a differential equation. Their statement that (C) would not hold at any moment, is clearer. But it is not valid. (A), (B), (C), (D), and (E) hold of course at any moment, because the different moments are equivalent for a stationary movement. Strictly speaking Ornstein and Zernike do not mean that these equations would not always be valid, but they assert this about other equations which are obtained when all the averages are not taken over all the particles, but over a v-group. They understand by this a group comprising all the particles that possessed the same velocity $\dot{x}_0$ at the moment t_0. When we now average over this group, equation (B) does not even hold at the moment t_0, whereas (A) and (E) do hold at the moment t_0, but not at another moment[1]). When we write the equations with such group means:

$$\overline{\mathfrak{K}\dot{x}}^{v} = 0 \qquad (A\ bis)\ etc.$$

Ornstein and Zernike are undoubtedly right in their assertion that (A *bis*) .... (E *bis*) are not always valid. But it is equally certain that they are wrong when they assert that the validity of these equations has been assumed by Miss Snethlage or me, or has been used in our reductions. We have always concerned ourselves with averages over all the particles, never with such averages of a v-group.

When, therefore, accessory misunderstandings are left out of account, it seems to me that Ornstein and Zernike's objection might be defined like this: that they think that we ought to have made use of averages over a v-group, and that we wrongly failed to do

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[1]) The authors calculate this more at length. Qualitatively this is, however, easy to see. For such a group $\overline{\dot{x}^2}$ is not constant. At t_0 we have, namely, $\overline{\dot{x}^2} = \dot{x}^2_0$ and after not too short a time $\overline{\dot{x}^2} = \frac{RT}{Nm}$.

so. In connection with this I remind of the fact that equation (C) with the value $p^2 = \frac{\overline{\mathfrak{K}^2}}{m\overline{\dot{x}^2}}$ is valid for each particle separately, and is not dependent on any mean value. It may be integrated without reservation, after which, if it is thought desirable, equation (E) can be taken into account. When $\overline{\Delta^2}$ is to be calculated, the general mean is always taken. How and why an average over a v-group could be introduced for the calculation of this quantity, is not clear to me; nor has it been demonstrated by ORNSTEIN and ZERNIKE [1]).

Accordingly it seems to me that ORNSTEIN and ZERNIKE have not succeeded in pointing out an error in our derivation. Nevertheless I do not doubt but it must exist. I think I have pointed out the error above in equation (6). Formely we had always thought that Δ was the sum of a number of terms that were statistically independent of each other. Equation (6) shows that the increments setting in after a moment t are dependent on the deviation already reached at that moment. And it is obvious that we shall not find the accurate amount for the mean value of the deviations, when we leave this correlation out of account.

It might now be imagined that this remark entailed that also the result $\overline{\Delta^2} = bt$ should be considered as doubtful. The thesis of the calculus of probability cited for it is namely only valid when the different terms of the sum are independent of each other, which is not the case here. It has been demonstrated on p. 1260 and 1261 that yet there is no reason to doubt the validity of this formula, and that it can even be derived from equation (6).

Above I have derived equation (C) from (B). I have done so because such a derivation is also valid for other analogous cases, e.g. for equation (6) on p. 1258. When we only wish to derive equation (C), we can do so also in another way, as has been done by Miss SNETHLAGE and me [2]). This derivation even brings us farther than the above given one. It justifies us in the statement that the

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[1]) The formulae derived by the writers for averages over a v-group might possibly be of value for another question, namely this: how do the particles that at first have the same velocity, spread over the different velocities?

[2]) Messrs. ORNSTEIN and ZERNIKE state l.c. that we give formula (C) without a proof, after which they furnish a proof, which, however, does not depart from ours except in this detail that we average *immediately* over all the particles, and they in stages first over the particles of a v-group, and then over the different v-groups. The result is, of course, identical.

quantity q is statistically quite independent of $\dot{x}$. This statement involves $\overline{q\dot{x}}=0$, but it comprises more. From it follows e.g. $\overline{q^2}^{\dot{x}} =$ independent of the value of $\dot{x}$ for which the $\dot{x}$-group[1]) has been taken, which cannot be derived from the simple fact that $\overline{q\dot{x}}=0$.

The derivation given here justifies at the same time the derivation of the above given equations (6) and (7) from (5*a*). These equations give, however, occasion to the following remarks. In the first place the constancy of r^2 is to be demonstrated. It follows directly from (8). We might also have started from (5), viz. $\Delta . w(t) < 0$ without dividing by t first.

Then we might have put:

$$w(t) = -r'^2 \Delta + s' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6a)$$

Multiplication by Δ and subsequent averaging would have yielded:

$$\overline{\Delta . w(t)} = -r'^2 \overline{\Delta^2} = -Q$$

so that r'^2 had not become constant. For this reason I have preferred to divide (5) first by t, and to put:

$$w(t) = -r^2 \frac{\Delta}{t} + s$$

with $r^2 =$ constant. Besides the idea of "measurable velocity" can now be applied to $\frac{\Delta}{t}$, which may lead to the application of STOKES's formula.

Another remark in connection with formula (6) is the following. When we consider the deviations Δ, Δ', Δ'' etc., obtained in the times t, t', t'', etc., which have been chosen so that they all finish at the moment t_2, hence begin at different moments t_1, t'_1, t''_1 etc., the quantities $w(t_2)$ are of course the same: the accelerations of the particles at the moment t_2. They are, however, divided into two terms in different ways. When we put again $r^2 = \frac{Q}{b}$ we may write:

$$w(t_2) = -r^2 \frac{\Delta}{t} + s \text{ with } \overline{s\,\Delta} = 0$$

but also:

$$w(t_2) = -r^2 \frac{\Delta'}{t} + s' \text{ with } \overline{s'\,\Delta'} = 0$$

or

$$w(t_2) = -r^2 \frac{\Delta''}{t} + s'' \text{ with } \overline{s''\,\Delta''} = 0 \text{ etc.}$$

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[1]) By a $\dot{x}$-group we simply mean the group of the particles for which $\dot{x}$ has a definite value. Accordingly it is something different from a "v-group of ORNSTEIN and ZERNIKE", which contains the particles which *at the moment* t_0 *had a definite value* v, but which have very different velocities at the moment at which we consider the group.

The same particles which form together a group with the same Δ, will not all have the same Δ'. This circumstance does not detract from the validity of (6) and (7), nor from the use made of it.

Remark II. At a cursory view equation (4):

$$\Delta = \dot{x}_0\, t + \int_0^t w(\theta)\,(t-\theta)\,d\theta$$

looks rather strange. It seems to be quite in contradiction with $\Delta^2 = bt$. For when we take the square and when we average, a term $\dot{x}_0^2 t^2$ appears which is by no means small compared with the other terms. When we, however, choose the group particles that have a definite velocity $\dot{x}_0$ at the moment $t = 0$, we may write for this group that:

$$\overline{\int_0^t w(\theta)\,d\theta} = -\,\dot{x}_0$$

For at the moment t they will be distributed over all the velocity groups, and they will have a mean velocity zero. At least this will be so when t is not taken *too* small. When t is of the order of 1 sec., this is amply sufficient. Hence the terms:

$$\left\{ \dot{x}_0^2 + \left(\int_0^t w(\theta)\,d\theta \right)^2 + 2\,.\,\dot{x}_0\,.\int_0^t w(\theta)\,d\theta \right\} t^2$$

will neutralize each other in the expression for $\overline{\Delta^2}$. A direct discussion of the way in which the remaining terms depend on t will be very difficult. But in this way it is at least seen that the striking appearance of terms with t^2 is only apparent. The form of (4) is, accordingly, more or less misleading, which, however, is no objection to the use made of it above. That (4) is really compatible with $\overline{\Delta^2} = bt$, follows from the reduction via equations (5) to (10).

Remark III. We have assumed on p. 1258 that:

$$\overline{w\,\Delta} < 0$$

This can be demonstrated in different ways. In the first place we might start from the relation $\overline{\Delta^2} = bt$, proved on p. 1257 which after a double differentiation, yields:

$$\overline{\mathfrak{K}\,\Delta} = -\,m\,\overline{\left(\frac{d\Delta}{dt}\right)^2}$$

When we again put $\mathfrak{K} = -6\pi\zeta a \frac{\Delta}{t} + s$ with $\overline{s\Delta} = 0$, we find at once:

$$6\pi\zeta a \frac{\overline{\Delta^2}}{t} = -m\overline{\left(\frac{d\Delta}{dt}\right)^2} = \frac{RT}{N}$$

We can, however, also follow the course indicated in the text p. 1258, and show, or make it at least highly plausible, that:

$$w(t_2)\int_{t_1}^{t_2} w(\theta)(t-\theta)\,d\theta < 0 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5a)$$

For this purpose we remember that the movement is reversible. I shall suppose the reversed movement (in which of course *all* the velocities, both those of the particles and those of the molecules of the medium must be reversed) to take place between the times t_3 and t_4. Of course then $t_4 - t_3 = t_2 - t_1$. We have further $w(t_4) = w(t_1)$ and $w(t_3) = w(t_2)$, for the forces that in the direct movement occur at the beginning of the interval, in the reversed movement occur at the end and vice versa. Finally the path $\Delta' = -\Delta$ is travelled over in the reversed movement. For the reversed movement the expression analogous to (5*a*) becomes:

$$\overline{w(t_4)\,.\,\Delta'} = \overline{w(t_4)\int_{t_3}^{t_4} w(\theta)\,.\,(t_4-\theta)\,d\theta} < 0 \quad . \quad . \quad . \quad . \qquad (5b)$$

This becomes for the direct movement:

$$\overline{-w(t_1)\,\Delta} = \overline{w(t_1)\int_{t_1}^{t_2} w(\theta)(\theta-t_1)\,d\theta} < 0, \quad . \quad . \quad . \qquad (5c)$$

for what for the reversed movement is expressed by $t_4-\theta$, for the direct movement becomes $\theta-t_1$.

If therefore we have proved the validity of (5*c*), we have proved (5*b*) for the reversed movement. On account of the equivalence of the direct and the reversed movement, we may also consider (5*a*) proved for the direct movement. This appeal to the reversed movement is not necessary. The validity of (5*a*) might have been demonstrated in the same way as has been done with (5*c*). The representation and the mode of expression seemed somewhat simpler when I started from (5*c*).

Superficially considered the sign of this expression would be expected exactly the contrary. When we namely choose a group of particles which all have the same $w(t_1)$ on t_1, and when we take the $w(\theta)$ for these particles at a later moment, and then the mean of $w(\theta)$ over the group, which quantity I shall represent by $\overline{w(\theta)}^{w(t_1)}$ we shall find that the particles have assumed all kinds of values of w, so that $\overline{w(\theta)}^{w(t_1)} = 0$. At least this will be so when $(\theta - t_1)$ is not very small; only for very small values of $(\theta - t_1)$ there exists correlation between the values of $w(\theta)$ and $w(t_1)$, and then the product $w(\theta)\, w(t_1)$ will be positive on an average.

This would, indeed, be accurate, and would lead to the opposite sign for (5c), when $\overline{w(\theta)}^{w(t_1)}$ approached to zero aperiodically. This is, however, not the case. In order to see this we observe that in virtue of the mutual independence of distribution in configuration and in velocity we may write in a notation that is easy to understand:

$$\overline{\dot{x}_{t_1}}^{w(t_1)} = 0$$

When we take:

$$\int_{t_1}^{t} w(\theta)\, d\theta = \dot{x} - \dot{x}_{t_1}$$

and when we again average for the $w(t_1)$ group, in which it may be assumed that with sufficiently long $t - t_1$ the initial $w(t_1)$ has no longer any influence on the final velocity, so that $\overline{\dot{x}(t)}^{w(t_1)} = 0$, we find:

$$\overline{\int_{t_1}^{t} w(\theta)\, d\theta}^{w(t_1)} = 0 \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (11)$$

When $w(t_1)$ is thought positive, $\overline{w(\theta)}^{w(t_1)}$ will also be < 0 for very small values of θ. The fact that the integral is zero means therefore that the positive interval is succeeded by a negative interval, before the value of $\overline{w(\theta)}^{w(t_1)}$ falls to zero¹). When under the integral sign

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¹) This may also be expressed by stating that $\overline{w(\theta)\, w(\theta+\delta)}$ is positive for very small values of δ; for somewhat larger values it is negative, descending to zero for large values. This change of sign of the product has been overlooked by ORNSTEIN (Zittingsverslag Dec. 1917, p. 1008, § 2). In consequence of this he arrives at the remarkable conclusion that the assumption $\frac{d}{dt}\overline{u^2} = 0$ is not justified. For according to his computation it follows from this that $\overline{u^2}$ is not constant, but the sum of a linear and a periodic function of t!

we multiply by $\theta - t_1$, this factor becomes greater for the negative interval than for the positive, so that:

$$\int_{t_1}^{t} w(\theta)\,(\theta - t_1)\,d\theta < 0 \qquad \text{when} \qquad w(t_1) > 0.$$

In the same way appears of course:

$$\int_{t_1}^{t} w(\theta)\,(\theta - t_1)\,d\theta > 0 \qquad \text{when} \qquad w(t_1) < 0.$$

It is true that the course of $\overline{w(\theta)}^{w(t_1)}$ can be more intricate than I have assumed here. Instead of one there may take place more reversals of sign. Not improbably $\overline{w(\theta)}^{w(t_1)}$ is represented by a damped periodic function, or at least it has a course closely resembling it. But in any case equation (11) must hold for not *too* small values of t, and in this the values of $\overline{w(\theta)}^{w(t_1)}$ which agree in sign with $w(t_1)$, will undoubtedly have smaller abscissa than those that differ in sign from it, which warrants the validity of (5*c*).

Remark IV. I pointed out on p. 1260 that the obtained result for $\overline{\Delta^2}$ probably amounts only to half or about half the true value. It is natural to try to bring a correction in this by the assumption of another value for the measurable velocity. We defined the quantity $\frac{\Delta}{t}$ as "measurable velocity". But it is the question whether this is really the quantity that is to be multiplied by $6\pi\zeta a$ in order to find STOKES's force of friction. We might, of course, define $\frac{\Delta}{t}$ as the time-average of the measurable velocity of a particle that travels over a path Δ in the time t. And when a force of viscosity opposes this displacement, it is the question whether this force may be taken proportional to the time average. It is to be expected that the measurable movement is not uniform. When we divide the interval t into sub-intervals, it is to be expected that the displacements obtained in the first of these sub-intervals will have less influence on the force of viscosity that prevails at the end of the interval t than the displacements obtained in the later sub-intervals. And this force of friction at the end of the interval was the quantity that we had in view when executing our computation.

When we further bear in mind that $\Delta_m = \sqrt{bt}$ and $v_m = \sqrt{\frac{b}{t}}$, when Δ_m represents the mean displacement and v_m the mean value of the time average of the measurable velocity, we see that v_m decreases with increase of t. It is therefore natural to suppose that a measurable velocity can be introduced that decreases with the time, thus being smaller in the later sub-intervals than in the first. If in virtue of this the force of friction at the end of t should also be put smaller than we did, namely at half the value[1], we should find for $\overline{\Delta^2}$ exactly the value of EINSTEIN's formula.

A simple calculation, however, teaches that the desired correction is not to be obtained on the ground of these considerations. For this we point out that the chance that a particle gets a deviation Δ in a time t, is represented by:

$$C e^{-\frac{\Delta^2}{m^2}} d\Delta \text{ in which } m^2 = \tfrac{2}{3}\,\overline{\Delta^2}.$$

When we now choose the group of particles that all have the same Δ, and when we divide t into two sub-intervals t_1 and t_2, the different particles of the Δ-group will travel over different paths Δ_1 in the time t_1, and over different paths Δ_2 in the time t_2, in which of course $\Delta_1 + \Delta_2 = \Delta =$ constant for the group.

When for this group we now examine the middle value of Δ_1, a simple calculation teaches:

$$\overline{\Delta_1}^{\Delta} = \Delta \frac{t_1}{t}$$

so that also:

$$\overline{\Delta_2}^{\Delta} = \Delta \frac{t_2}{t}$$

In this it has been assumed that the values of Δ_1 and Δ_2 are statistically independent of each other, when not a Δ-group, but the collection of all the particles is considered. It is known that this may be assumed as long as t_1 and t_2 are not too small, i.e. t_1 and t_2 must be sufficiently large to allow us to neglect the influence of the initial velocity and of the initial force.

It appears from this that we may divide the velocity of the particles during the interval t into two terms: a *uniform* velocity $\frac{\Delta}{t}$ and an irregular term, which, independent of the value of Δ for

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[1] We should exactly obtain this factor $^1/_2$ when we did not derive v_m by dividing Δ_m by t, but by differentiating Δ_m with respect to t.

the considered particle is equally probably positive as negative. Hence it is very probable that we may also divide the force of friction into a term $-6\pi\zeta a\frac{\Delta}{t}$, and an irregular term k. As $\overline{k.\Delta}=0$, we may insert k in s, so that we may write:

$$w(t)=-6\pi\zeta a\frac{\Delta}{t}+s \qquad \text{with } \overline{s\Delta}=0$$

as we did on p. 1258.

Remark V. In conclusion I want still to make a remark in connection with a derivation given by **Ornstein**[1]) of the formula $\overline{\mathfrak{K}x}=0$. In this he starts from equation (3), which he writes:

$$\frac{du}{dt}=-\beta u+F \quad \ldots\ldots\ldots \quad (3a)$$

and he proves that when F is a function of t, which is prescribed without taking u into account, and which further fulfils certain conditions[2]), the solution of the differential equation (3a) yields such a value for u that $\overline{Fu}=\overline{\beta u^2}$, so that $\overline{u\frac{du}{dt}}=0$.

This result is in perfect agreement with the thesis pronounced by Miss **Snethlage** and me that $\overline{\mathfrak{K}u}=0$, and in conflict with the thesis from which **Einstein** and **Hopf**, **Langevin** and others start, viz. that $\overline{Fu}=0$.

Remarkable is the conclusion drawn by **Ornstein** out of this. It runs, namely, that there is no objection to accepting equation (3a) with $\overline{Fu}=0$. It is astonishing that **Ornstein** has not noticed this contradiction. In reality he nowhere introduces the supposition $\overline{Fu}=0$ into his calculation. He simply integrates equation (3a), and then demonstrates that $\overline{Fu}$ is *not* zero, but equal to $\overline{\beta u^2}$.

It follows from $\overline{Fu}=\overline{\beta u^2}$ that we may represent F by:

$$F=\beta u+F' \text{ in which } \overline{F'u}=0\,,$$

so that $\mathfrak{K}=-\beta u+F=-\beta u+\beta u+F'=F'$ with $\overline{F'u}=0$.

In so far this derivation teaches nothing new. Yet it is interesting

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[1]) L. S **Ornstein**, Zittingsverslag Dec. 1917, p. 1011

[2]) F is a continuous function, which, however, repeatedly changes its sign, and which has another value for every particle. $\overline{F^2}$ taken over all the particles, and also the mean squares of the first and higher time-derivatives of F are constant in the time. Also the mean value of F^2 for a single particle taken over a sufficiently long time is constant in the time and constant for the different particles.

because it is very well adapted to help us to form a true conception about the forces that appear in the Brownian movement. Let us consider a sphere of finite dimensions immersed in a viscous liquid, and fastened to a cord. By means of this cord the sphere can be moved in the x direction both in positive and in negative sense. Let us now assume that a force $F(t)$ is applied, the value of which is fixed without our taking the velocities acquired by the sphere into account. We may e.g. imagine that the value of F for different moments is determined by some lottery or other, and that F further satisfies the conditions mentioned on the preceding page.

The equation of motion of the sphere will then be:

$$m\frac{du}{dt} = -\beta u + F .$$

ORNSTEIN integrates this equation and shows that $\overline{Fu} = \beta\overline{u^2}$.

Though the value of F has been fixed independently of u, yet F and u are statistically not independent of each other. And this is owing to this that the velocity u is not independent of the force F, which has given rise to it.

When we now again return to the Brownian movement, the force F will no longer be exerted by pulling a cord, but by collisions of molecules. But for the rest everything remains the same. We may, indeed, assume a friction, also counteracting the momentary velocity. And there may be reason to do so, just as was the case for the larger sphere attached to a cord. But then the motive force F does not satisfy the condition $\overline{Fu} = 0$, as is generally assumed, but we may put:

$$F = +\beta u + F' \quad \text{with} \quad \overline{F' u} = 0,$$

so that the force of friction $-\beta u$ may not be introduced, without the introduction of another force $+\beta u$ that again neutralizes it. The only remaining force F' is then independent of u.

Physiology. *"On the Electric Nebulae of Antipyretica."* By Prof. H. Zwaardemaker and Dr. H. Zeehuisen.

(Communicated in the meeting of February 23, 1918).

Two years ago[1]) one of us called attention to the excess of electrical charge, positive or negative, appearing when a solution of odorous substances in water is sprayed. Other publications on the same subject have appeared since[2]). However, a superficial preliminary investigation showed at the very outset not only that odorous substances possess this remarkable property, but also that a number of other physiologically active, pure, chemical substances produce the same, though less appreciable electrical effect[3]).

Nebular electricity is generated by odorous substances, saponins (odorous as well as inodorous), glycosides (most often slightly odorous), alkaloids (odorous as well as inodorous), antipyretica (odorous as well as inodorous), and a few more groups of other physiologically active substances that will be reported afterwards. In order to evolve the electrical phenomenon a substance, added to water, and sprayed with it, will have to satisfy some conditions, which seem to be:

1st. it should be soluble in water;

2nd. it should lower the surface-tension;

3rd. it should volatilise, when spread over a large evaporation area.

No electrical charge is evoked in any concentration by anorganic salts and acids, glycerin, sugars (provided they be pure), dextrin, lecithin, albumin, etc.

In the present communication we propose to discuss more in detail the electrical phenomenon of antipyretica, which we shall classify into three groups:

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[1]) These Proceedings, Vol. 19 p. 44.

[2]) Ibid. Vol. 19 p 334, 351. Archives Neerlandaises de Physiologie t. 1. p. 347. E L. Backman. These Proc. Vol. 19 p. 943, Pflüger's Archiv Bd. 168 p. 351; C. Huyer, De olfactologie van aniline en homologen. Diss. Utrecht 1917.

[3]) These Proc, Vol. 19 p. 340, H. Zeehuisen, 3e Physiologendag, Dec. 1917. (Proc. not out yet. Vide Ned. Tijdschr. v. Geneesk. 1918).

a. antipyretic alkaloids;
b. salicylic acid and its derivatives;
c. the other anodynes and the medicines that lower the temperature.

Group a. The antipyretic alkaloids are on the whole sparingly soluble, at the most up to $\frac{1}{1000}$ *n*. The surface tension is distinctly lowered by chinin, chinidin, cinchonin; it was not demonstrable in cinchonidin, chinamin, conchinamin. Volatility is undoubtedly a property of cinchonin (according to sublimation test) and of chinamin (in consequence of the loss of electrifying power subsequent to the suction of air). The saturated aqueous solution, sprayed under an overpressure of two atmospheres, distinctly electrifies a screen arranged in the way of the nebula at a distance of about 25 cm. The sign of this charge is positive (as in the case of most odorous substances) and the air round it and at some distance from it is charged negatively. Though clearly noticeable, the phenomenon falls behind that of the odorous substances.

Group b. Salicylic acid and its derivatives represent a group of chiefly crystalline substances, whose solubility differs much. Amongst them there are two that are completely insoluble, viz. salol and benzylsalicylate, and on that account have no or hardly any electrifying power. Of the remaining only salicylas chinini is soluble to less than $\frac{1}{1000}$ *n*, the others at least to 2.5 millinormal; some of the salts of salicylic acid are even extremely soluble in water. The lowering of the surface tension is on the whole slight, even absent in the case of the salts (after the trickling method); only with the liquid substances of this group and with aspirin is the lowering very distinct. Volatility is noticeable with salicylic acid, for it sublimes (consequently also with the salts of salicylic acid for they are dissociated in the dilute solutions), and also decidedly with the liquid representatives of the group (judging from the boiling point).

The intensity of the electrical phenomenon they evoke, varies very much. With the insoluble ones the charge, as has been said, is 0 or very near it; with the salicylic acid and the benzoic acid salts it is moderate, at the least 4 scale-divisions of our electroscope; with the others it is stronger, often even maximal, as with the typical odorous substances (camphor, alcohol, etc.).

The salicylic acid salts owe their electrifying power to the anion. The attending cation even lessens the charge of the nebula, probably because it is detrimental to the surface activity of the liquid.

As may be expected the consequence is that salicylic acid and salicylic acid salts determine the electrifying power of the nebula in a similar way, there being only a quantitative difference in such a sense that the salt produces a lower charge than the acid.

An intercomparison of the various concentrations of the acid, which is best expressed in a graphical way, shows that the optimum electrifying power of salicylic acid is located at about 1 millinormal; beyond it or below it the charge of the nebula is much weaker. Such an optimum is not only lowered, but also shifted by the coincidence of a cation, so that for salicylas natricus e.g. under the same conditions it is located in the 6 per-mille-concentration.

We have made an attempt to compare the univalent cations with regard to their capacity of shifting the optimum and of depressing the charge. When taking a 1 millinormal solution of the salicylic acid, the deflections of the electroscope will decrease proportionally through the addition of cations. The following table gives the factors by which the deflection of the electroscope, caused by the salicylic acid alone, must be divided in order to find the deflection that appears when adding a certain salt to the salicylic acid:

Factor by which the deflection of the electroscope is reduced, when spraying a mixture of 0,001 n. salicylic acid and the chloride of the subjoined alkalimetals, instead of spraying 0,001 n. salicylic acid alone.

Conc. of the added salt. solution.	Li.	Na.	K.	Rb.	Cs.	NH^4.
0.01 n	∞	∞	∞			
0.005 n	9.6	10.1	∞	∞		
0.002 n	2.57	2.86	2.69	4.00		∞
0.001 n	1.68	2.27	1.95	2.36	2.90	4.3
0.0005 n	1.35	1.40	1.49	1.67	1.77	2.20
0.00025 n	1.05	1.14	1.30	1.40	1.42	1.50
0.000.125 n		1.07		1.20		

It will be seen that the charge of the nebula decreases gradually from left to right. The factor reducing the deflection increases from Lithium to Ammonium.

$$Li. < Na. < K. < Rb. < Cs. < NH_4.$$

which calls to mind the known lyotrope series. Our nebula is also a colloidal system, consisting of the phase liquid (dispers) and gas (dispergent). On the surface of the droplets there is an electric potential, on the *bigger* droplets by which the charge on the screen is measured, it is negative. Of the other salicylic acid salts we had still at our disposal strontium- and bismuth-salt. They also possess a moderate electrifying power, just as sodiumbenzoate.

The esters of salicylic acid at our disposal: aspirin, novaspirin, diaspirin, the methylsalicylate and the spirosal, all yielded powerful charges.

Group c. The third group comprises three series: α the chinolin-derivatives, β the pyrazols, γ the paramidophenols. There is only one insoluble substance amongst them, all the rest are soluble and moreover crystalline.

For citrophen the lowering of the surface-tension could not be demonstrated. We also found that with most of the substances the surface-activity was inconsiderable. Only with euphorin, acetopyrin and kairin the lowering may be called significant. Slight volatility is generally noticeable if the evaporation area be large.

As regards the intensity of the electrical phenomenon it may be observed that the completely insoluble representative of group *c* does not yield a charge, which is in keeping with its insolubility; that the phenomenon occurs with moderate intensity in the case of pyramidon, phenacetin, and citrophen; that it is somewhat stronger with kairin, but extremely distinct with antipyrin, salipyrin, acetopyrin, tussol, antifebrin, and euphorin.

From the foregoing we may therefore conclude that all antipyretica, except those that are completely insoluble, yield the electrical phenomenon.

As has been set forth in previous publications, the electrical charge of a nebula is carried exclusively in the droplets it holds. Being sprayed under an overpressure of two atmospheres the fluid emerges from the nozzle of the sprayer in the shape of little drops. Of these drops the smaller ones are blown about and scattered in all directions. For odorous substances they are charged negatively. The bigger drops, which are charged positively, rush ahead in straight, divergent lines. When caught up at a proper distance, (for the antipyretica at 25 cm. from the sprayer) on an insulated screen, the latter will receive a positive charge that is pretty strong, though it is never so strong with antipyretica as with odorous substances (in aqueous solution).

In order to obtain normal results it is necessary to earth the sprayer. The amount of positive or negative charge present where the screen has been placed, may be largely affected by admixing cane-sugar or sodium chloride to the solution to be sprayed.

By the admixture of cane-sugar the charge on the screen is always raised, with the antipyretica extinction never occurs. The admixture of sugar is indifferent in the case of solutions of mesotan, tussol, and antifebrin. It goes without saying that in the like experiments due care should be taken that the sugar be perfectly pure, since an old sugar-solution is of itself electrifying.

The substances evolved under the circumstances can be removed from it only by filtration through animal charcoal (not so easily through extraction with absolute alcohol and subsequent cautious evaporation of the alcohol from the purified sugar left after filtration).

The effect of an admixture of sodium chloride is quite different. The concentration in which it takes place is of the greatest influence, as appears from the following table:

Factor of decreasing the charge of salicylic acid and salicylas natricus through various NaCl-concentrations.

NaClsol. (conc.)	Salicylas Na. 0.00125n.	Salicylic acid 0.00125n
0.01 n.	∞	∞
0.005 n.	∞	10.1
0.0025 n.	4.2	4.6
0.00125 n.	2.1	2.4
0.000625 n.	1.8	1.8
0.0003125 n.	1.6	1.6

Seeing that even traces of sodium chloride modify the charge, it is essential to make certain beforehand that suitable water be used for solution. Distilled water cannot be recommended, since after standing for some days in a well rinsed bottle, it obtains of itself electrifying power (analogon of the old sugar solution). By shaking it for a considerable time with pure animal charcoal and by subsequently filtering it, distilled water can also be liberated from the electrifying admixture, which is volatile, for suction of air removes it in part at least.

The tapwater of the Utrecht Water Company being nearly free from mineral salts we used it quite fresh (old tapwater also possesses electrifying power).

In cities with salt-rich tapwater the complication just described will have to be taken into account.

Physiologically the behaviour of the solutions is of interest when sodium chloride is admixed till a concentration of the latter of 0,9 °/₀ is reached.

It follows then that of

Group a. 4 obtain a higher electrifying power, 2 a lower, viz. chinamin and conchinamin.

Group b some are strengthened, others are weakened; salicylates, antipyrin and liquid derivatives are strengthened.

Group c. Antipyrins and chinolins are strengthened, urethans are weakened, and phenetidins remain unaltered.

No explanation of the sodium chloride effect has as yet been provided. Our experience with the salicylic acid salts points to a cation effect. To account for the fact we might also have recourse to the change of the surface tension in the droplets. Certain it is that the size of the droplets very much depends on the nature of the cation. The droplets of sprayed ammonium chlorid solution are much larger than those of sprayed sodium chloride solution (established ultramicroscopically).

For the rest in splitting the generally very complicate molecules all sorts of additions of electrical power may turn up.

Physics. — *"On the diffraction phenomenon caused by a great number of irregularly distributed apertures or opaque particles".* By Dr. W. J. DE HAAS. Communicated by Prof. H. A. LORENTZ. (Communication from the physical laboratory of TEYLER's Institute).

(Communicated in the meeting of December 29, 1917 [1]).

1. When before the objective of a telescope focussed on a distant lightpoint, we keep a screen, containing a great number of irregularly distributed equal circular apertures, we observe a well-known diffraction phenomenon. It will be known that the same phenomenon is observed when the investigation is made with a transparent screen, covered with opaque corpuscles which correspond to the above mentioned apertures, e. g. a glass plate strewn with grains of lycopodium. The diffraction image consists of a light central spot and round this the rings which would be found in the case of one single aperture; we now see however fluctuations in the intensity which we can best describe as a fibrous radial structure. VON LAUE [2]) investigated this phenomenon, and came to the conclusion that it could not be explained by classic optics. In this communication the considerations will be discussed which made me doubt the rightness of this conclusion. First I want to stipulate however, that I have not any objection to VON LAUE's theoretical considerations on the diffraction phenomenon by monochromatic light [3]).

2. My different investigations will only shortly be discussed. I confined myself to visual observation. The diffraction phenomena were studied in two different ways. First I worked with turned copper rings over which thin paper impregnated with indian ink had been stretched, when still wet.

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[1]) The essential contents were read in the meeting of Sept. 29, 1917.

[2]) M. v. LAUE, Die Beugungserscheinungen an vielen unregelmässig verteilten Teilchen, Sitzungsber. Akad. Berlin 1914, p. 1144; Mathematische Betrachtungen an vielen unregelmässig verstreuten Teilchen, Mitteilungen. Phys. Gesellsch. Zürich, 1916, p. 90; Ein Versagen der klassischen Optik, Berichte deutsch. physik. Ges. **18**, p. 19, 1917,

[3]) See H. A. LORENTZ, Zittingsverslag Dec. 1917, p. 1120.

When dried, this formed a sufficiently opaque tightly stretched screen. With a fine needle small apertures were made in it. In order to vary the magnitude of these apertures the needle was fixed in a round holder. This holder fitted into a cylindrical tube in which it could be slided up and down and be fixed in a definite position. This enabled us to make all apertures equally wide and also to alter their width (by protruding a longer or a shorter part of the needle from the cylinder).

In the second method I worked with glass-plates covered with lycopodium powder or with glass-plates on which small mercury spheres had been precipitated by evaporation. The mercury can easily be precipitated in spheres of $\frac{1}{200}$ mm diameter or less. They are however not all of the same magnitude. Three such plates were in my possession.

The first method offers many advantages. The phenomenon can be continuously followed with the eye while the number of apertures is increased, while in the case that the refracted light has the same direction as the incident beam, the latter does not blind as in the other methods. Visually the details of the phenomenon could be observed much quicker, more accurately and more easily than photographically, while the occurrence of colour-phenomena did not remain unobserved.

The source of light was an arc-lamp at a distance of about 5 m from the objective of the telescope. The lamp had been screened off as perfectly as possible, while the apertures in a screen of lead that could be rotated formed the light-points. In order to vary the experiments there were apertures of 0.5, 0.4, 0.3, 0.25, 0.15 mm diameter.

Beginning with a small number of apertures in the screen of black paper and passing gradually to a greater number, we see how the diffraction image of a few apertures, which agrees with the classic theory, changes into the phenomenon of the fibrous structure. This change is very striking.

Working with natural light we see with two holes a small number of diffraction lines, the middle one of which is uncoloured and passes through the point 0, where the image of the considered light-point is formed. This point will be called the centre. On both sides of the middle and most intense line a small number of lines is observed, which are alternately light and dark, and which show the spectral colours. Let the system of lines round the centre be denoted by A, the uniformly illuminated field on both sides by B.

We must remind that according to § 3 all this is superposed on the diffraction image (spot and ring) of one single aperture. With three apertures which form an arbitrary triangle, we observe three systems of lines A, respectively perpendicular to the sides of this triangle. They cross each other in the neighbourhood of the centre O.

With four or more apertures a similar phenomenon is observed and so on.

We get the impression that always line-systems, each perpendicular to a line connecting two apertures (of which systems the number always increases with the number of apertures) cross each other near the centre. This would be the reason why near the centre no pronounced lines are observed and instead of these a sunflower-like structure. But at a greater distance from the centre the line-systems must diverge. This suggested to me the conception that *these* are the fibres of the phenomenon of LAUE. Working with 50 apertures, the diffraction image has already quite the same aspect as for a glass-plate covered with lycopodium; the only difference is, that in the first case the fibrous structure is coarser than in the second. With a small number of apertures even when they are distributed in an "accidental" way, it is however possible that among all lines of connexion some directions are more represented than others.

3. This conception may be elucidated by a simple mathematical consideration.

Before the objective of a telescope focussed on a light-source at an infinite distance a screen has been placed over which a great number n of equal circular apertures is distributed. The screen is placed perpendicular to the axis of the telescope. Let F be the principal focus of the objective, and let us consider the distribution of the light in the focal plane V passing through F. According to a well-known theorem of the diffraction theory the intensity at a point P of the plane can be represented by the product of two factors. The first of these is the intensity that would be due to one single aperture, while the second is the intensity i that would be observed if instead of the given apertures we had at their centres n equal apertures so small, that they might be considered as points. Both factors are functions of the position of P in V. The first determines the intensity in the diffraction image B of one single aperture, where the intensity changes relatively slowly from point to point. Into this diffraction image B the factor i introduces irregular fluctuations, by which the intensity changes much more quickly from point to point. We may say that these fluctuations are seen on the background formed by the image B. If really there are radially directed light and dark fibres, these fluctuations must necessarily be caused by the second factor i. Therefore we can confine ourselves to the consideration of the diffraction image of n point-like apertures.

4. Let P be a point of the focal plane and let the vibrations (monochromatic light) reaching that point from the different apertures be represented by:

$$\cos(vt+\varphi_1), \quad \cos(vt+\varphi_2) \quad . \quad . \quad . \quad . \quad \cos(vt+\varphi_n),$$

where $\varphi_1, \ldots \varphi_n$ denote the phases and where for the sake of simplicity the amplitude has been put equal to 1. It will be known that we may write for the resulting intensity

$$i = n + \Sigma\, 2\cos(\varphi_a - \varphi_b) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

or

$$i = n + \Sigma\, i_{ab} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The summation has to be taken over all combinations of two apertures.

Thus it is evident that the intensity can be obtained by superposing many fields viz. by simply adding their intensities.

The first field is uniform: n.

On this there are superposed $\frac{1}{2}n(n-1)$ other fields, each of them belonging to one pair of apertures.

The intensity of one of these pairs is

$$i_{ab} = 2\cos(\varphi_a - \varphi_b) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which value varies between $+2$ and -2.

The negative intensities in each of the $\frac{1}{2}n(n-1)$ "elementary fields" do not cause trouble, as the intensity n of the uniform field is great enough to neutralize even very many i_{ab}'s. The expression (1) namely, being derived from

$$(\Sigma \cos\varphi_a)^2 + (\Sigma \sin\varphi_a)^2$$

can never be negative.

In each elementary field (3) we have a line-system as has been described in § 2. It can be proved that along every line perpendicular to the line connecting the apertures a and b (3) has a constant value. From one line in that direction to the other i_{ab} fluctuates. On the line through the centre O $\varphi_a = \varphi_b$, $i_{ab} = +2$. There are equidistant maximum and minimum lines, $i_{ab} = +2$, $i_{ab} = -2$.

5. It is interesting to remark the following: if homogeneous light is used the maximum and minimum lines cover the *whole* field of view. It is to be expected that a great number of such line-systems crossing each other in all possible directions give a "granular" structure. This would be the granular structure, which according to v. LAUE's theoretical considerations must be observed with homogeneous light.

The question, whether the classic theory can explain the observations of v. LAUE would be answered immediately, if the experiment could be made with really homogeneous light. As to the central spot, this may be done, but because of the small intensities it is already very difficult for the first ring. From our different experiments some will be described:

1st. By the aid of WRATTEN filters the light was first more or less monochromatized. Here as in the following experiments the three different screens were used, that with the apertures, the glass-plate with lycopodium and that with mercury. The fibrous structure at the centre of the central diffraction spot vanished and became granular. At a greater distance from the centre the fibres in the central spot remained. It is interesting to remark that the length of each fibre does not increase proportionally with the radius. The fibres near the periphery are relatively much longer than those nearer the centre of the spot.

2nd. The sodium light-source was used. Now the central spot was beautifully granular even at the periphery. Of the first ring a weak shadow was observed. It is difficult to say whether it contained anything radial. The sodium light was formed by the flame of a BUNSEN burner in which a spoon with sodium was held. Strong light and a high temperature of the flame were avoided, though by means of these a very intense sodium-light can be obtained, as e. g. DU BOIS [1]) described. This was done in order to obtain higher monochromatisy. The light-source being therefore weak the observations were not made with a telescope, but with the naked eye. The glass-plate was fixed close to the eye which instinctively was accomodated to the illuminated aperture.

The observations were made in an absolutely dark room. The flame was placed in a perfectly closed chest of iron with a communication tube to the air. Along a fixed sliding the spoon with sodium could be brought into the flame, so that never any light except through one single fine aperture left the chest.

6. With *natural light* the phenomena are quite different from those observed with homogeneous light. Instead of (2) we then must

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[1]) DU BOIS. Zeitschr. f. Instrumentenkunde, 1892, p. 165.

take a summation of similar expressions for the different wavelengths, so that instead of i_{ab} we may write

$$S i_{ab} .$$

The sign of summation S refers to the different wavelengths. The distribution of intensity is now a line system with a limited number of lines, one light line in the middle and on both sides light and dark ones. At some distance $S i_{ab} = 0$, because there where for one wavelength $\cos(\varphi_a - \varphi_b)$ has a positive value, there will be a wavelength which is hardly different from it (and therefore makes the same impression in our eye) for which this expression has a negative value.

With natural light we thus have the superposition of a uniform field and of *narrow* line-systems crossing each other near the centre.

From this we may conclude: *if* the classic theory can explain the phenomena investigated by v. LAUE, *these* line systems are the fibres observed by him. But whether the theory can furnish the explanation remains for the present more or less dubious.

This is evident from the following considerations. In each elementary line-system a, b the intensity varies between -2 and $+2$, while in the uniform field it has the much higher value n. If there are e.g. 10000 grains, the superposition of one line system on the uniform field will give fluctuations from 10002 to 9998, which it is of course impossible to observe.

One single line-system is thus undetectable. But the lines connecting each pair of apertures and therefore also the line-systems perpendicular to those lines have all possible directions determined by probability. Lines visible on the background n can be formed when accidentally a number of line-systems has so nearly the same direction that at a distance from the centre which is not too great, the maxima $(+2)$ of one system coincide with those of another system. The question whether the classic theory can explain the phenomenon may thus be formulated as follows:

I. Is such an accidental accumulation of different line-systems in a definite direction to be expected often enough, according to the theory of probability?

The theoretical treatment of this question will be left aside here. Only the following may be remarked: In reality many line-systems will fall out. We do not work namely with a light-point, but with a source of certain dimensions (aperture in the screen). Each point of this source gives its own diffraction image and all these images are shifted with respect to each other. In this way the finest line-systems are effaced, and not only this, but they even vanish *totally*, the mean value of $cos\,(\varphi_a - \varphi_b)$ being *zero*. As now the distance between the lines a, b is inversely proportional to the distance a, b every line-system corresponding to apertures or grains at a considerable distance from each other will vanish. It remains however questionable whether this vanishing of a number of line-systems increases the visibility of the others.

7. The phenomena may also be treated in a different way (though of course equivalent with the former). We may namely calculate first the total resulting distribution of intensity i for one wavelength, and then superpose all these distributions for the different wavelengths (Si). Doing this, we must keep in mind that the same intensity which for a wavelength λ occurs at a point P, is found for another λ' at a point P' which lies with O and P on a straight line in such a way that $OP : OP' = \lambda : \lambda'$.

According to the classic theory the distribution of light i is "granular" for a definite λ. If we pass to another wave-length λ', the light and dark spots are shifted in radial direction. This will be called "spectral shift".

It is evident that if we pass from monochromatic light to homogeneous light the grains will in this way be changed into fibres.

Exner [1]) thought that the observed fibres could be sufficiently explained in this way. Von Laue combatted this and drew the attention to one of his photographs of the diffraction figure of a plate covered with lycopodium powder on which the fibres in the first diffraction ring are longer than would correspond to the spectral shift.

(See also the end of § 5).

8. In my view v. Laue has paid here no attention to the fact that by the running into one another of the light spots and also of the dark ones, there may be formed fibres, longer than would correspond to the elongation of each separate spot. Let us consider different lines starting from 0. Because of the accidental distribution of the light and dark spots (in consequence of the accidental distribution of the lycopodium grains) the distances between the dark spots will be somewhat smaller on one radius than on another. At a certain distance from the centre we shall sooner see a somewhat dark line along one radius than along another. In this way the fibres would be formed. The question is now:

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[1]) Sitzungsber. Akad. Wien **76** (1877), p. 522; Ann. d. Phys. **9** (1880), p. 239.

II. This accidental predisposition to run into one another, which according to the laws of probability may be expected along some lines (and which according to the ordinary laws of optics is only a consequence of the irregular distribution of the refracting particles) will it suffice to explain the existence of fibres of the observed length? Evidently the questions I and II come essentially to the same.

Only when these questions had to be answered in the negative we should to have to conclude, that classic optics were not sufficient to explain the observations. We should then be compelled to assume that already with homogeneous light radial fibres would be found. And as was remarked above it is very difficult to decide this in a direct way. The experiments made with sodium light however do not point in that direction. Another possibility would be that except the above mentioned accidental predisposition there existed still *another* along some radii. Both this and the former possibility would compel us to alter the fundaments of the diffraction theory.

As to the running into one another of the fibres formed by the spectral shift, this will evidently occur the sooner the longer the fibres are. Now the fibres formed by the spectral shift have a length proportional to the distance to the centre 0, so that the running into one another will occur more at a certain distance from the centre than in its immediate neighbourhood. With this the fact is in agreement that, as was mentioned in § 5, the length of the observed fibres increases towards the periphery more than would correspond to the distance from the centre.

8. Finally some experiments will be described, which (together with the experiments with the sodium light) support my conception of the phenomenon.

Investigating the diffraction phenomenon of a screen with about fifty apertures we can prove in a very direct way that in fact the diffraction figure is built up of interference lines. To do this experimentally the ocular of the telescope had been fixed on a small car, which could be moved to and fro over a pair of rails. It is evident, that by a displacement of the car the structure in the diffraction image formed in the focal plane of the objective is effaced in a direction perpendicular to that of the motion. Only when an interference fringe has the direction of the motion it remains unchanged.

In fact, when in the indicated way the ocular is quickly moved to and fro, we firstly see how the fibrous structure of the diffraction image is changed into a homogeneous white spot on which secondly interference fringes in the direction of the motion are seen very distinctly.

If the ocular is moved in an arbitrary way, but so that continually it remains sharply focussed on the light-point in the leaden screen, interference fringes are seen rotating on a homogeneous white spot.

This experiment cannot be made with a screen with a very great number of apertures. Probably this is caused by the fact that in this case the number of interference fringes which have the same direction becomes so great. The distance between the parallel fringes is generally very different, by the superposition only the central fringe will therefore remain and a set of parallel interference fringes in the direction of motion will not be seen.

The second experiment that will be described was suggested to me by Prof. H. A. LORENTZ.

The reasoning leading to it was the following: when with purely monochromatic light the distribution of the intensity is granular, the fibrous character of the diffraction image will be due to the spectral shift of the grains from the violet towards the red. But when now this spectral shift, which forms part of the nature of the phenomenon, is the only cause of the formation of the fibres, we may expect that by a second artificial spectral shift the fibres will be no longer radially directed. We have really succeeded in producing fibres in other directions. When before the objective of the telescope, between this lens and the diffracting plate we place a prism[1]), all diffraction images from the violet towards the red will be shifted in a definite direction, the red image over the greatest distance. The light-point at the centre of the circular diffraction figure, the image of the aperture in the leaden screen, is changed into a light-strip AB, red at the end A and blue at the other end B. If we consider the thus formed diffraction figure on the production of AB on the side of B, we shall reach a point P where the original spectral shift belonging to the diffraction image is neutralized by the superposed spectral shift due to the prism. In the neighbourhood of this point the structure must be granular, while the newly formed fibres at some distance from this point must be directed towards it. These phenomena can easily be observed. So this experiment too proves that the fibres are due to a spectral shift.

They are most easily observed when a well-chosen WRATTEN-filter is kept before the eye; with such a plate, through which of the

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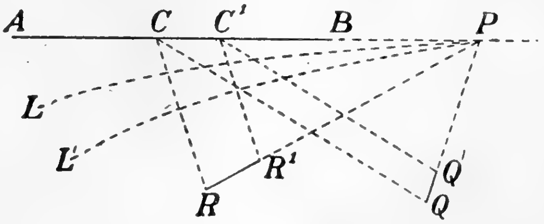
[1]) Also a small prism may be held between the ocular and the eye.

line-spectrum AB only a part in the green of the length $\frac{1}{6}$ AB could pass, a granular structure was observed in the immediate neighbourhood of the point P. This structure was quite of the same nature as that which under ordinary circumstances was seen near the point O. At a greater distance from P small stripes were observed along lines directed towards P; the length of these stripes increased with the distance from P.

The question may be illustrated by a figure. Let CC'' be the part of the linear spectrum transmitted by the filter and let us consider



the two extreme wavelengths for which the image of the light-point lies at C and C''. Let the wavelengths belonging to C and C'' be λ and λ, so that, A being the red end of AB, $\lambda > \lambda'$.

Suppose that, working only with the wavelength λ, we saw a light grain at the point Q. It is evident that, working with the wavelength λ' alone, we should see the corresponding spot at a point Q', which is found by drawing $C'Q'$ parallel to CQ and by determining the length of $C'Q'$ by the equation:

$$CQ : C'Q' = \lambda : \lambda'.$$

For intermediate wavelengths the light-spots fall between Q and Q' viz., as CC'' is relatively short on the straight line QQ'. The production of this line cuts that of AB at the point P, which is determined by the equation

$$CP : C'P' = \lambda : \lambda', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and which has therefore always the same position, *whichever* strip of light QQ' we may consider. When further, RR' is a second strip of light, it can be proved, that the lengths QQ' and RR' have the same ratio as the distances $Q'P$ and $R'P$.

Near P the lines were so short, that they could not be distinguished from "grains". When the wavelength λ gave a light spot just at P, the corresponding light spot of the wavelength λ' would coincide with it, as is evident from (4). The existence of a light-spot at P involves of course that without prism we should observe a radially directed line of the length CC''.

We must observe that the absence of fibres in the immediate neighbourhood of P *proves* that no fibres will be seen with homogeneous light and that those observed with non-homogeneous light are only due to the spectral shift considered in § 7. It is namely evident, that a line at P directed along CP which existed already with monochromatic light, could never be reduced to a point by means of a prism.

Finally we must remark the following. At great distances from P it is no longer true that the fibres are directed along *radii* with P as a centre. In reality the lines along which the fibres are directed have approximately the course indicated in the figure by LL'. To explain this we should have to enter into too many details.

Astronomy. — *"Outlines of a new theory of Jupiter's satellites."* By Prof. W. de Sitter.

(Communicated in the meeting of March 23, 1918).

1. *Fundamental principles of the theory.*

The great difficulty in the theory of the four old satellites of Jupiter arises from the mutual commensurability of the mean motions of the three inner ones. The fourth satellite is not affected by this, and, so far as periodic inequalities are concerned, its theory does not give rise to particular difficulties, and is in many respects similar to the lunar theory, only much simpler, since the ratio of the month and the year, which for our moon is $^1/_{18}$, is for the fourth satellite only about $^1/_{260}$. The secular perturbations of the equations of the centre of the four satellites are however so intimately connected with each other, that it is not possible to keep the fourth satellite apart, but the theory of the *four* satellites must be treated as one whole.

I denote the satellites by the suffixes 1 to 4, and I put:

a_i the semi-axis major,
n_i the mean motion,
w_i the true orbit-longitude,
λ_i the mean longitude,
l_i the mean anomaly,
f_i the true anomaly,
r_i the radius-vector,
e_i the excentricity,
π_i the longitude of the perijove.

If now we put[1])

$$(n_2 - n_3)\, t = \tau\,,$$

then, if an appropriate zero of longitude and time is chosen, we have

$$\begin{aligned} \lambda_1 &= (4 - \varkappa)\, \tau, \\ \lambda_2 &= (2 - \varkappa)\, \tau + 180^\circ, \\ \lambda_3 &= (1 - \varkappa)\, \tau, \end{aligned}$$

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[1]) This τ differs 180° from the angle which was called τ in my previous work. See these Proceedings Feb. and March 1908: "On the Masses and Elements of Jupiter's satellites, and the mass of the system." (Vol. X, pp. 653 and 710).

where $\varkappa$ is a small quantity whose value is approximately

$$\varkappa = \tfrac{1}{69}\,.$$

We will consider $\varkappa$ as a small quantity of the *first order*. The masses of the satellites then are of the *second* order (the largest is about $0{\cdot}8 \times 10^{-4}$).

Generally we put, for all satellites including the fourth

$$\lambda_i = (c_i - \varkappa)\,\tau + \lambda_{i0} \quad \ldots\ldots\ldots \quad (1)$$

We thus have, the value of c_4 being only given approximately,

$$c_1 = 4 \quad , \quad c_2 = 2 \quad , \quad c_3 = 1 \quad , \quad c_4 = 0{\cdot}437\,.$$

If we start with uniform motion in a circle as a first approximation, the inequalities can according to their periods be divided into four sharply separated groups.[1])

I. The inequalities of the first group have periods not exceeding 17 days. They can be subdivided into three sub-groups:

I*a*. The equations of the centre, which are

$$\delta w_i = 2 \Sigma\, \tau_{ij}\, \varepsilon_j \sin(\lambda_i - \varpi_j)\,,$$

$$\delta r_i = -\, a_i \Sigma\, \tau_{ij}\, \varepsilon_j \cos(\lambda_i - \varpi_j)\,,$$

where the sums are to be taken for the values of j from 1 to 4, and where ε_i and ϖ_i are the "own" excentricities and perijoves.

I*b*. The "great" inequalities. These are approximately:

$$\delta w_i = 2\mathrm{e}_i \sin c_i\tau$$

$$\delta r_i = -\, a_i\, \mathrm{e}_i \cos c_i\tau$$

I*c*. Other inequalities of short periods.

II. The inequalities of the second group have periods between 400 and 500 days. Their expressions are

$$\delta w_i = \sum_j \varkappa_{ij} \sin \varphi_j\,,$$

with [2])

$$\varphi_i = \varkappa\tau + \varpi_i\,.$$

III. The libration has a period of about 7 years.

IV. The inequalities of the fourth group have periods of more than 12 years.

The inequalities I*b*, II and III arise out of the mutual commensurability of the mean motions. In a previous communication [3])

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[1]) See also "Elements and Masses", p. 655, where however the libration (III) is left outside the groups and the group IV is numbered III.

[2]) These φ_i differ 180° from the angles so called in "Elements and Masses"; so the coefficients $\varkappa_{ij}$ here have the other sign.

[3]) "On the periodic solutions of a special case of the problem of four bodies", these Proceedings, Feb. 1909, Vol XI, p. 682.

I have pointed out that for the three inner satellites a periodic solution of the second kind (in which the own exentricities ε_i are zero, and the great inequalities Ib appear as excentricities) is a very good approximation to the true motion, in fact much better than the undisturbed Keplerian motion. The mean *anomalies* in the periodic solution are

$$l_i = c_i \tau. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The longitudes of the perijoves are given by

$$\pi_i = \lambda_i - l_i \ ,$$

and consequently their mean value is

$$\pi_i = - \varkappa \tau + \pi_{i_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The perijoves thus have a mean motion common to the three satellites. In the theory here outlined, the equations (1), (2), (3) are taken as a first approximation, or "intermediary orbit", also for the fourth satellite. The solution, considered as a whole, is then no longer periodic, since c_4 is mutually incommensurable with c_1, c_2, c_3. It is however not the periodicity which makes this solution such a good first approximation, but the moving perijove, combined with the circumstance that the "induced" equations of the centre Ib are, for the inner satellites, larger than the own, or "free" ones Ia. For IV the contrary is true. The own excentricity of IV is comparatively large ($^1/_{135}$) and the induced one is entirely negligible. For IV the Keplerian motion with a fixed perijove is indeed a better approximation. In the ordinary theory this approximation is also used for the three other satellites, where it is not appropriate. Here the method which is the best for I, II, III, is forced upon IV. This of course involves some drawbacks, but these are in my opinion not very serious and considerably smaller than those arising in the ordinary theory from the fact that the inequalities Ib appear as "perturbations", and must consequently be treated as quantities of the order of the masses (i.e. by our method of reckoning of the *second* order), while they actually are of the first order (the largest is about $^1/_{107}$).

Briefly the new theory may be stated thus: We start from an intermediary orbit in which the equations (2), (3) are rigorously satisfied. The radius-vector and the true anomaly are then computed from the mean anomaly and the excentricity (which is constant) by the ordinary formulas of Keplerian motion:

$$u_i - e_i \sin u_i = l_i$$

$$tan\ \tfrac{1}{2} f_i = \sqrt{\frac{1+e_i}{1-e_i}}\ tan\ \tfrac{1}{2} u_i \quad . \ . \ . \ . \ . \ . \ . \quad (4)$$

$$r_i = \frac{a_i(1-e_i^2)}{1+e_i \cos f_i}$$

The true orbit-longitude then is

$$w_i = f_i + \pi_i \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (5)$$

This intermediary orbit is, also for the three inner satellites, not the complete periodic orbit, but only contains its leading terms. To get this intermediary orbit we must 1 restrict the perturbative function to a certain part of it (viz: the "secular" and the "critical" parts), and 2 we must take initial values, or constants of integration, which satisfy certain conditions. The complete solution is then derived by adding to the intermediary orbit:

1. "perturbations" which arise from the parts of the perturbative function that were at first neglected;

2. "variations" which are due to the fact that the actual constants of integration do not exactly satisfy the conditions for the intermediary orbit.

Of these the variations are the most important. To get these we must form the variational equations. These lead to a system of equations entirely similar to those which are used in the treatment of secular perturbations by the method of LAGRANGE. The resulting determinant has 5 roots $\beta_1 \ldots \beta_5$, corresponding to the four own perijoves ϖ_i, and the argument of the libration ϖ_5 respectively. The inequalities in longitude and radius-vector are then given by formulas which, if we restrict ourselves to the first order, assume the form

$$\left.\begin{aligned} \delta w_i &= \sum_j W_{ij}\, \varepsilon_j \sin(\lambda_i - \varpi_j) + \sum_j W'_{ij}\, \varepsilon_j \sin \varphi_j, \\ \delta r_i &= \sum_j R_{ij}\, \varepsilon_j \cos(\lambda_i - \varpi_j), \end{aligned}\right\} \quad . \ . \ . \quad (6)$$

where

$$\varphi_i = \beta_i \tau + \varphi_{i_0} = \varkappa \tau + \varpi_i$$

as above, and j assumes the values 1 to 5. These formulas include not only the free equations of the centre I*a*, *but also the inequalities of group* II (arguments φ_1—φ_4) *and the libration* III (argument φ_5).

As to the perturbations: by the introduction of c_i instead of n_i we have realised that *there are no small divisors.* In the ordinary theory small divisors appear in the inequalities I*b*, II and III. Of these I*b* is already included in the intermediary orbit; II and III appear as variations, which are treated by the method of LAGRANGE, together with I*a*, which is also in the ordinary theory treated in the same way. We can say that all small divisors have been concentrated in the equations of condition for the constants of integration of the intermediary orbit. Once these equations have been solved, the small divisors have disappeared, and they cannot reappear in subsequent approximations.

2. *Formation of the differential equations.*

We take an arbitrary system of coordinate axes through the centre of Jupiter, and we put:

f = the Gaussian constant of attraction,
m_0 = the mass of Jupiter,
m_i = the mass of the body with index i, expressed in m_0 as unit,
s_i = the latitude of the body i referred to the plane of Jupiter's equator,
r_i = the distance of the body i from Jupiter,
Δ_{ij} = the distance between the bodies i and j,
V_{ij} = the angle between the radii-vectores r_i and r_j,
$180^\circ - \psi$ = the ascending node of Jupiter's equator on the plane of (xy),
π = the inclination of this equator on the same plane,
J, K = two constants connected with the compression of Jupiter,
b = the equatorial radius of Jupiter,

and further

$$\alpha = \sin \pi \sin \psi,$$

$$\beta = \sin \pi \cos \psi,$$

$$\gamma = \cos \pi.$$

Then the equations of motion are

$$\frac{d^2 x_i}{dt^2} = \frac{\partial \Omega_i}{\partial x_i}, \quad \frac{d^2 y_i}{dt^2} = \frac{\partial \Omega_i}{\partial y_i}, \quad \frac{d^2 z_i}{dt^2} = \frac{\partial \Omega_i}{\partial z_i},$$

where

$$\left.\begin{aligned} \Omega_i = {} & f m_0 (1+m_i) \left\{ \frac{1}{r_i} + \tfrac{1}{3} \frac{J b^2}{r_i^3} (1 - 3 \sin^2 s_i) + \tfrac{1}{10} \frac{K b^4}{r_i^5} (1 - 10 \sin^2 s_i + \tfrac{35}{3} \sin^4 s_i) + \ldots \right\} \\ & + f m_0 \sum_j m_j \left\{ \frac{1}{\Delta_{ij}} - \frac{r_i}{r_j^2} \cos V_{ij} \left[1 + \frac{J b^2}{r_j^2} (1 - 5 \sin^2 s_j) + \frac{K b^4}{r_j^4} (\tfrac{1}{2} - 7 \sin^2 s_j + \tfrac{21}{2} \sin^4 s_j) + \ldots \right] \right. \\ & \qquad \left. - 2 \frac{\alpha x_i + \beta y_i + \gamma z_i}{r_j^2} \sin s_j \left[\frac{J b^2}{r_j^2} + \frac{K b^4}{r_j^4} (1 - \tfrac{7}{3} \sin^2 s_j) + \ldots \right] \right\}. \end{aligned}\right\} \quad (7)$$

The sums are to be taken over the values 1, 2, 3, 4 of j, with the exception of $j = i$, and further over the indices j which refer to the sun, Saturn, etc.

If the plane of (xy) is chosen near the mean position of the equator, then α and β are very small, and γ is very nearly equal to unity. The latitudes s_i of the satellites are very small (the largest does not exceed 0°.7), and also for bodies outside the system of Jupiter s_j is small (e.g. for the sun it never exceeds 3°.1).

Regarding the value (7) of Ω_i we may remark, that the terms multiplied with J and K in the complementary part of the perturbative function (second line of the formula) are here given for the first time. These terms are neglected by LAPLACE, and all subsequent investigators adopt LAPLACE's perturbative function without any criticism. LAPLACE was perfectly right, for these terms are beyond the limit of accuracy which he had set himself; but SOUILLART, who includes other terms of the same, and higher orders, ought also to have included these terms.

If now we put

$$x_i' = \frac{dx_i}{dt}, \quad y_i' = \frac{dy_i}{dt}, \quad z_i' = \frac{dz_i}{dt},$$

$$T_i = \tfrac{1}{2}\left(x_i'^2 + y_i'^2 + z_i'^2\right),$$

$$F_i = T_i - \Omega_i,$$

then the equations become

$$\frac{dx_i}{dt} = \frac{\partial F_i}{\partial x_i'}, \quad \frac{dx_i'}{dt} = -\frac{\partial F_i}{\partial x_i},$$

and similarly for the other two coordinates.

We now introduce the canonical elements of DELANNAY

$$l_i, \quad g_i, \quad \vartheta_i, \quad L_i, \quad G_i, \quad \Theta_i,$$

where

$$L_i = \beta_i \sqrt{a_i}, \quad G_i = L_i \sqrt{1 - e_i^2}, \quad \Theta_i = G_i \cos i_i,$$

I now put[1])

$$S_i' = \Omega_i - \frac{\beta_i^2}{r_i} \quad \dots\dots\dots\dots \quad (8)$$

Then we have

$$F_i = -\frac{\beta_i^4}{2L_i^2} - S_i'$$

and the equations become

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[1]) See: *On Canonical elements*, these Proceedings Sept. 1913, Vol. XVI pages 285 and 287.

$$\frac{dl_i}{dt} = \frac{\beta_i^4}{L_i^3} - \frac{\partial S_i'}{\partial L_i},$$

$$\frac{dg_i}{dt} = -\frac{\partial S_i'}{\partial G_i}, \text{ etc.}$$

It is usual to take $\beta_i^2 = fm_0(1 + m_i)$. By keeping β_i indeterminate we have a parameter at our disposal, which can afterwards be so chosen that the intermediary orbit assumes the desired form.

I now introduce instead of l_i, g_i, ϑ_i, L_i, G_i, Θ_i the canonical set

$$\lambda_i = l_i + g_i + \vartheta_i \quad , \quad l_i \quad , \quad \psi_i = -\vartheta_i \ ,$$

$$G_i \quad , \quad H_i = L_i - G_i \quad , \quad \Psi_i = G_i - \Theta_i$$

Then we have

$$\frac{\partial S_i'}{\partial G_i} = \left(\frac{\partial S_i'}{\partial G_i}\right) - \tfrac{1}{2}\,\frac{\gamma_i}{G_i}\frac{\partial S_i'}{\partial \gamma_i},$$

where

$$\gamma_i = 2 \sin \tfrac{1}{2} i_i = \sqrt{\frac{2\Psi_i}{G_i}}.$$

The second term is of the second degree in γ_i, and consequently very small. If now we put

$$\lambda_i = \lambda_i' + \sigma_i,$$

and if we determine σ_i by

$$\frac{d\sigma_i}{dt} = \tfrac{1}{2}\,\frac{\gamma_i}{G_i}\frac{\partial S_i'}{\partial \gamma_i}, \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (9)$$

then we have

$$\frac{d\lambda_i'}{dt} = \frac{\beta_i^4}{L_i^3} - \left(\frac{\partial S_i'}{\partial G_i}\right),$$

where the parentheses denote that S_i' must *not* be differentiated with respect to G_i so far as it depends on G_i through the inclinations γ_i. This being agreed upon, we can omit the accent of λ'_i and the parentheses of $\left(\frac{\partial S'_i}{\partial G_i}\right)$, if we apply to the value of λ_i so determined the correction

$$\delta\lambda_i = \sigma_i \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (10)$$

where σ_i is determined from (9).

In the theory of the inclinations the approximate commensurability does not give rise to particular difficulties. For this theory the most important point is to choose the plane of $(x\,y)$ so that the inclinations of the satellites and of the equator always remain small. I will not enter upon this problem here, and I will further exclusively consider the four elements

$$\lambda_i \quad , \quad l_i \quad , \quad G_i, \qquad H_i.$$

We can choose a unit of time so that $t = \tau$. This unit is about 9/8 of a day.

Then we have

$$\frac{dl_i}{d\tau} = \frac{\beta_i^4}{L_i^3} - \frac{\partial S_i'}{\partial H_i} \quad , \quad \frac{d\lambda_i}{d\tau} = \frac{\beta_i^4}{L_i^3} - \frac{\partial S_i'}{\partial G_i}.$$

As a general rule I will denote by $[X]$ the non-periodic part of a function X. We must have

$$\left[\frac{dl_i}{d\tau}\right] = c_i \quad , \quad \left[\frac{d\lambda_i}{dt}\right] = c_i - \varkappa.$$

This can be realised in two ways. We can take

$$\left[\frac{\beta_i^4}{L_i^3}\right] = c_i \quad , \quad \left[\frac{\partial S_i'}{\partial H_i}\right] = 0 \quad , \quad \left[\frac{\partial S_i'}{\partial G_i}\right] = \varkappa. \quad . \quad . \quad . \quad (A)$$

or

$$\left[\frac{\beta_i^4}{L_i^3}\right] = c_i - \varkappa \quad , \quad \left[\frac{\partial S_i'}{\partial H_i}\right] = -\varkappa \quad , \quad \left[\frac{\partial S_i'}{\partial G_i}\right] = 0 \ . \qquad (B)$$

Now the perturbative function is given as a development (e.g. by Newcomb's method) in terms of λ_i, l_i, a_i and e_i, and we have

$$\frac{\partial S_i'}{\partial H_i} = \frac{2}{\beta \sqrt{a_i}} a_i \frac{\partial S_i'}{\partial a_i} + \frac{\cos^2 \varphi_i}{\beta \sqrt{a_i}} \frac{1}{e_i} \frac{\partial S_i'}{\partial e_i},$$

$$\frac{\partial S_i'}{\partial G_i} = \frac{2}{\beta \sqrt{a_i}} a_i \frac{\partial S_i'}{\partial a_i} - \frac{\cos \varphi_i \tan \frac{1}{2} \varphi_i}{\beta \sqrt{a_i}} \frac{\partial S_i'}{\partial e_i},$$

where we have put $e_i = \sin \varphi_i$.

In the case (A) we thus find that $a_i \frac{\partial S_i'}{\partial a_i}$ is of the order of $\varkappa$, in the case (B) it is of the order of $\varkappa \,.\, e_i^2$, in both cases $\frac{\partial S_i'}{\partial e_i}$ is of the order of $\varkappa \,.\, e_i$ It thus appears that the method (B) is preferable.

Instead of $H_i = 2\, L_i \sin^2 \frac{1}{2} \varphi_i$ I now introduce

$$\eta_i = 2 \sin \tfrac{1}{2} \varphi_i.$$

We have

$$\frac{da_i}{a_i} = \frac{2}{\beta_i \sqrt{a_i}} (dH_i + dG_i),$$

$$d\eta_i = \frac{1}{\beta_i \sqrt{a_i}} \left(\frac{\cos \varphi}{\eta_i} dH_i - \tfrac{1}{2} \eta_i \, dG_i \right).$$

We find everywhere the denominator $\beta_i \sqrt{a_i}$. We can thus simplify our formulas by putting

$$R_i = \frac{S_i}{\beta_i \sqrt{\mathrm{a}_i}},$$

where a_i is a constant, which, in accordance with (B), is determined by

$$\left[\frac{\beta_i^4}{L_i^3}\right] = \frac{\beta_i}{\mathrm{a}_i^{3/2}} = c_i - \varkappa.$$

Further we put

$$\frac{\beta_i^4}{L_i^3} = (c_i - \varkappa)(1 + \nu_i).$$

Consequently ν_i is purely periodic, and we have

$$\mathrm{a}_i^{3/2} = a_i^{3/2}(1 + \nu_i).$$

Therefore

$$d\nu_i = -\tfrac{3}{2}(1 + \nu_i)\frac{da_i}{a_i}.$$

If then we introduce again

$$\pi_i = \lambda_i - l_i,$$

where of course π_i requires the same correction

$$d\pi_i = \sigma_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

as λ_i, and if we suppose the perturbative function R_i expressed in the variables λ_i, π_i, a_i and η_i, then the equations become

$$\left.\begin{aligned}
\frac{d\lambda_i}{d\tau} &= (c_i - \varkappa)(1 + \nu_i) - 2(1 + \nu_i)^{\frac{1}{3}} a_i \frac{\partial R_i}{\partial a_i} + \tfrac{1}{2}(1 + \nu_i)^{\frac{1}{3}} \eta_i \frac{\partial R_i}{\partial \eta_i},\\
\frac{dl_i}{d\tau} &= (c_i - \varkappa)(1 + \nu_i) - 2(1 + \nu_i)^{\frac{1}{3}} a_i \frac{\partial R_i}{\partial a_i} - (1 + \nu_i)^{\frac{1}{3}} \frac{\cos\varphi_i}{\eta_i} \frac{\partial R_i}{\partial \eta_i},\\
\frac{d\pi_i}{d\tau} &= (1 + \nu_i)^{\frac{1}{3}} \frac{1}{\eta_i} \frac{\partial R_i}{\partial \eta_i},\\
\frac{d\nu_i}{d\tau} &= -3(1 + \nu_i)^{\frac{4}{3}} \frac{\partial R_i}{\partial \lambda_i},\\
\frac{d\eta_i}{d\tau} &= -(1 + \nu_i)^{\frac{1}{3}} \frac{1}{\eta_i} \frac{\partial R_i}{\partial \pi_i} - \tfrac{1}{2}(1 + \nu_i)^{\frac{1}{3}} \eta_i \frac{\partial R_i}{\partial \lambda_i}.
\end{aligned}\right\} \quad . \quad . \quad (12)$$

Of the first three equations we can arbitrarily choose two for use in the computations. The simplest formulas are found if we use λ_i and π_i.

Instead of β_i (a_i) I now introduce the constant μ_i, which is determined by

$$\mu_i = 1 - \frac{fm_0(1 + m_i)}{\mathrm{a}_i^3 (c_i - \varkappa)^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Further we put

$$\varrho_i = \frac{r_i}{\mathrm{a}_i} = (1+v_i)^{-\frac{2}{3}} \frac{r_i}{a_i}. \quad \ldots\ldots \quad (14)$$

Then the terms of R_i which are independent of the latitudes become

$$\left.\begin{aligned} R_i = &-\frac{(c_i-\varkappa)\mu_i}{\varrho_i} + (c_i-\varkappa)(1-\mu_i)\left[\frac{1}{3}\frac{J_i}{\varrho_i^3} + \frac{1}{10}\frac{K_i}{\varrho_i^5}\right] + \\ &+ \frac{(c_i-\varkappa)(1-\mu_i)}{1+m_i}\sum_j m_j \left\{\frac{\mathrm{a}_i}{\Delta_{ij}} - \frac{\mathrm{a}_i^2}{\mathrm{a}_j^2}\frac{\varrho_i}{\varrho_j^2}\cos V_{ij}\left[1+\frac{J_j}{\varrho_j^2}+\frac{1}{2}\frac{K_j}{\varrho_j^4}\right]\right\}, \end{aligned}\right\} \quad . \quad (15)$$

where we have put

$$J_i = \frac{Jb^2}{\mathrm{a}_i^2} \quad , \quad K_i = \frac{Kb^4}{\mathrm{a}_i^4}.$$

3. *The intermediary orbit.*

The perturbative function R_i consists of a series of terms of the form

$$K \cos D,$$

$$D = p\lambda_j - p\lambda_i + ql_i + q'l'_j$$

To get the intermediary orbit we take

$$R_i = [R_i].$$

Hence the argument D must satisfy the condition

$$(p+q')\,c_j + (q-p)\,c_i = 0.$$

The function $[R_i]$ includes the "secular" part of R_i, for which $q = p$, $q' = -p$, and the "critical" part, which becomes non-periodic as a consequence of the commensurability of c_i and c_j.

Since $\frac{\partial R_i}{\partial \lambda_i}$ and $\frac{\partial R_i}{\partial \pi_i}$ contain only sines, we shall have

$$\frac{dv_i}{d\tau} = 0 \quad , \quad \frac{d\eta_i}{d\tau} = 0$$

if

$$\lambda_{j_0} - \lambda_{i_0} = k \times 180^\circ$$

or

$$\pi_{i_0} = k \times 180^\circ, \quad \ldots\ldots \quad (16)$$

k being any integer number.

If we count the time from the epoch of an opposition of II and III, and the longitudes from the longitude of III at that epoch, then we have

$$\pi_{10} = 0 \quad , \quad \pi_{20} = 180^\circ \quad , \quad \pi_{30} = 0.$$

As to the fourth satellite, the condition $\lambda_{40} = 0$ or 180° is generally not satisfied, since there is no relation between the longitudes of the fourth satellite and of the others, as there is in the case of the three inner satellites. It is however easy to choose as origin an opposition of II and III at which the condition is very nearly satisfied.[1]

Then for all satellites η_i and ν_i are constant in the intermediary orbit. We take

$$\nu_i = 0, \quad \text{and consequently} \quad a_i = \mathrm{a}_i \quad ; \quad \eta_i = \bar{\eta}_i \quad . \quad . \quad . \quad (17)$$

Then also $a_i \dfrac{\partial [R_i]}{\partial a_i}$ and $\dfrac{\partial [R_i]}{\partial \eta_i}$ are constants. These must be determined so that

$$\frac{\partial\,[R_i]}{\partial \bar{\eta}_i} = -\varkappa \bar{\eta}_i, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

$$a_i \frac{\partial\,[R_i]}{\partial a_i} = -\tfrac{1}{4} \varkappa \bar{\eta}_i^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Then we have

$$l_i = c_i \tau \quad , \quad \pi_i = -\varkappa\tau + \pi_{i_0} \quad , \quad \lambda_i = (c_i - \varkappa)\,\tau + \pi_{i_0} \quad . \quad (20)$$

The radius-vector r_i and the true orbit-longitude w_i are now determined by (4) and (5), and we thus see that the intermediary orbit is a Keplerian ellipse with the constant semi-axis a_i, the constant excentricity $\mathrm{e}_i = \sin \bar{\varphi}_i$, determined by $2 \sin \frac{1}{2} \bar{\varphi}_i = \bar{\eta}_i$, and the mean anomaly $l_i = c_i \tau$, and these ellipses rotate in their plane with the angular velocity $-\varkappa$, common to all satellites.

The conditions (18) and (19) serve to determine the two parameters μ_i and $\bar{\eta}_i$. For the inner satellites this intermediary orbit is, as has already been pointed out, a very good approximation, better than the fixed Keplerian ellipse. For IV the excentricity as determined from (18) is extremely small, and the intermediary orbit consequently differs very little from a circle described with the uniform velocity $c_i - \varkappa$.

(To be continued next page.)

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[1] Thus e. g. on 1899 June 28, $11^h\ 47^m\ 35^s$ G. M. T. the longitudes counted from the first point of Aries are:

$\lambda_1 = 193°,64 \quad , \quad \lambda_2 = 13°.64 \quad , \quad \lambda_3 = 193.64 \quad , \quad \lambda_4 = 192°.75.$

Astronomy. — *"Outlines of a new theory of Jupiter's satellites".* By Prof. **W. de Sitter**. (Continued).

(Communicated in the meeting of April 26, 1918).

4. *The variational equations.*

The constants of integration of the intermediary orbit satisfy the conditions (18), (19), and the conditions of symmetry (16). The constants of integration of the actual orbit however do not exactly satisfy these conditions. We now put, instead of (17) and (20)

$$\left.\begin{aligned} \lambda_i &= (c_i - \varkappa)\,\tau + \pi_{i0} + \omega_i, \qquad & \nu_i &= \nu_i, \\ \pi_i &= -\varkappa\tau + \pi_{io} + g_i, \qquad & \eta_i &= \overline{\eta_i} + \delta\eta_i \end{aligned}\right\} . \quad (21)$$

Instead of g_i and $\delta\eta_i$ I introduce h_i and k_i by

$$\left.\begin{aligned} \eta_i \cos g_i &= \overline{\eta_i} + h_i \\ \eta_i \sin g_i &= k_i \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \quad (22)$$

The equations then become

$$\frac{dh_i}{d\tau} = -\varkappa k_i - (1+\nu_i)^{\frac{1}{3}} \frac{\partial R_i}{\partial k_i} - \tfrac{1}{2}(1+\nu_i)^{\frac{1}{3}} (\overline{\eta_i} + h_i) \frac{\partial R_i}{\partial \lambda_i},$$

$$\frac{dk_i}{d\tau} = \varkappa(\overline{\eta_i} + h_i) + (1+\nu_i)^{\frac{1}{3}} \frac{\partial R_i}{\partial h_i} - \tfrac{1}{2}(1+\nu_i)^{\frac{1}{3}} k_i \frac{\partial R_i}{\partial \lambda_i},$$

$$\frac{d\omega_i}{d\tau} = (c_i - \varkappa)\,\nu_i - 2(1+\nu_i)^{\frac{1}{3}} a_i \frac{\partial R_i}{\partial a_i} + \tfrac{1}{2}(1+\nu_i)^{\frac{1}{3}} \eta_i \frac{\partial R_i}{\partial \eta_i},$$

$$\frac{d\nu_i}{d\tau} = -3(1+\nu_i)^{\frac{4}{3}} \frac{\partial R_i}{\partial \lambda_i}.$$

We still restrict ourselves to the non-periodic part $[R_i]$ of the perturbative function. Then, if we neglect the squares and products of h_i, k_i, ω_i, ν_i, these equations are of the form

$$\left.\begin{aligned}
\frac{dh_i}{d\tau} &= \sum_j a_{ij}\, k_j + \sum_j b_{ij}\, \omega_j, \\
\frac{dk_i}{d\tau} &= -\sum_j a'_{ij}\, h_j - \sum_j b'_{ij}\, v_j, \\
\frac{dv_i}{d\tau} &= \sum_j d_{ij}\, k_j + \sum_j e_{ij}\, \omega_j, \\
\frac{d\omega_i}{d\tau} &= -\sum d'_{ij}\, h_j - \sum e'_{ij}\, v_j.
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (23)$$

The right-hand members have no constant term. For h_i and v_i these terms are zero in consequence of the conditions of symmetry (16), since they contain only sines. For k_i and ω_i they are zero by the conditions (18) and (19).

The equations (23) are satisfied by

$$\left.\begin{aligned}
h_i &= \sum_q c_{iq}\, \varepsilon_q \cos \varphi_q, & v_i &= \sum_q c'''_{iq}\, \varepsilon_q \cos \varphi_q, \\
k_i &= \sum_q c'_{iq}\, \varepsilon_q \sin \varphi_q, & \omega_i &= \sum_q c''_{iq}\, \varepsilon_q \sin \varphi_q. \\
& & \varphi_q = \beta_q \tau + \varpi_{q0}. &
\end{aligned}\right\} \quad . \quad . \quad . \quad (24)$$

Substituting (24) in (23) we find for c_{iq}, c'_{iq}, c''_{iq}, c'''_{iq} and β_q the conditions

$$\left.\begin{aligned}
c_{iq}\,\beta_q + \sum_j a_{ij}\, c'_{jq} + \sum_j b_{ij}\, c''_{jq} &= 0, \\
c'_{iq}\,\beta_q + \sum_j a'_{ij}\, c_{jq} + \sum_j b'_{ij}\, c'''_{jq} &= 0, \\
c''_{iq}\,\beta_q + \sum_j d'_{ij}\, c_{jq} + \sum_j e'_{ij}\, c'''_{jq} &= 0, \\
c'''_{iq}\,\beta_q + \sum_j d_{ij}\, c'_{jq} + \sum_j e_{ij}\, c''_{jq} &= 0,
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (25)$$

The condition that it shall be possible to determine c_{iq}, c'_{iq} .... from these equations is that their determinant is zero. This gives an equation of the sixteenth degree in β_q. To each root β_q belongs a set c_{iq} .... There are however not 16 different values of β_q. To begin with it is evident that, if we change φ_q to $-\varphi_q$, and consequently β_q to $-\beta_q$, and if at the same time we replace c'_{iq} and c''_{iq} by $-c'_{iq}$ and $-c''_{iq}$, the equations (25) are still satisfied, and (24) is not affected at all. It follows that if β_q is a root, then also $-\beta_q$ is a root.

Further there are *six* roots $\beta = 0$. Each term in the equations (24), i. e. each root β, represents an oscillation of the true motion with respect to the intermediary orbit with the period $2\pi/\beta$. Each of these oscillations corresponds to a small change of the initial values, i. e. a small deviation of the constants of integration from those of the intermediary orbit. The term corresponding to a root $\beta = 0$ is not an oscillation, but a constant correction to one of the elements, which does not affect the character of the motion. Now there are six possible deviations, i. e. six constants of integration by a change in which the intermediary orbit is not essentially altered. These are:

1. A change of the zero of the longitudes and the time. This evidently does not affect the motion at all, and since two constants of integration are involved, it corresponds to two roots $\beta = 0$.

2. A change of $n_2 - n_3 = \frac{d\tau}{dt}$ and of $\varkappa$. The first is evidently only a change in the unit of time. The other does affect the motion of the three inner satellites, but only in so far as the intermediary orbit is replaced by another of entirely the same character.

3. A change of c_4, say to $c_4 + \delta c_4$. We can then call $c + \delta c_4$ again c_4 and nothing essential will be altered.

4. A change of ω_4. In the intermediary orbit we assumed $\omega_4 = 0$. In doing this we neglected a small quantity, and evidently the exact amount of the neglected quantity is of no importance. This corresponds to the fact that all coefficients b_{i4} and e_{i4} are zero, as is found when they are worked out.

It must therefore be possible to transform the equation of the 16^{th} degree in β to an equation of the 5^{th} degree in β^2. This is effected as follows.

By differentiating the second and fourth of (23) we find equations of the form:

$$\left.\begin{aligned} \frac{d^2 k_i}{d\tau^2} + \sum_j A_{ij}\, k_j + \sum_j B_{ij}\, \omega_j = 0, \\ \frac{d^2 \omega_i}{d\tau^2} + \sum_j C_{ij}\, k_j + \sum_j D_{ij}\, \omega_j = 0. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (26)$$

Hence we find for c'_{iq}, c''_{iq} and β_q the conditions

$$\left.\begin{aligned} c'_{iq}\, \beta^2_q - \sum_j A_{ij}\, c'_{jq} - \sum_j B_{ij}\, c''_{jq} = 0, \\ c''_{iq}\, \beta^2_q - \sum_j C_{ij}\, c'_{jq} - \sum_j D_{ij}\, c''_{jq} = 0. \end{aligned}\right\} \quad . \quad . \quad . \quad (27)$$

The determinant of these equations is

$$\Delta = \begin{vmatrix} A_{11}-\beta^2 & A_{12} \cdots\cdots & A_{14} & B_{11} \cdots\cdots & B_{14} \\ A_{21} & A_{22}-\beta^2 \cdots & A_{24} & B_{21} \cdots\cdots & B_{24} \\ \vdots & & \vdots & \vdots & \vdots \\ A_{41} & A_{42} \cdots\cdots & A_{44}-\beta^2 & B_{41} \cdots\cdots & B_{44} \\ C_{11} & C_{12} \cdots\cdots & C_{14} & D_{11}-\beta^2 \cdots & D_{14} \\ \vdots & \vdots & \vdots & \vdots & \vdots \\ C_{41} & C_{42} \cdots\cdots & C_{44} & D_{41} \cdots\cdots & D_{44}-\beta^2 \end{vmatrix} \quad . \quad . \quad (28)$$

Now it can be shown that

$$\left.\begin{array}{c} \overline{\eta}_1\, A_{i1} + \overline{\eta}_2\, A_{i2} + \overline{\eta}_3\, A_{i3} + \overline{\eta}_4\, A_{i4} + B_{i1} + B_{i2} + B_{i3} + B_{i4} = 0, \\ 4\, B_{i1} + 2\, B_{i2} + B_{i3} = 0, \\ B_{i4} = 0, \end{array}\right\} \quad (29)$$

and the same equations remain true if A_{ij} is replaced by C_{ij}, and B_{ij} by D_{ij}. It follows that the equation $\Delta = 0$, which is of the 8th degree in β^2, has three roots $\beta^2 = 0$, and can therefore be reduced to an equation of the fifth degree. To prove (29) it would be necessary to develop the coefficients A_{ij}, B_{ij} ...., which cannot be done here. The proof will be given in a more detailed publication that will soon appear in the Annals of the Observatory at Leiden (Vol. XII, Part I).

There are thus 5 different values of β^2_q. To each of these belongs a set of values of c'_{iq} and c''_{iq}, which are found from (27), and of c_{iq} and c'''_{iq}, which are then found from the first and last of (25).

The first four elements of the diagonal of the determinant Δ are approximately

$$A_{ii} = \varkappa^2.$$

All other elements are at least of the third order. It follows that four of the roots β_q are very nearly equal to $\varkappa$, the fifth being much smaller. If we neglect the masses of the satellites and the compression of the planet, then this fifth root becomes zero, and the four others are rigorously equal to $\varkappa$. The motion $-\varkappa$ of the perijoves in the intermediary orbit is then exactly cancelled by the variations, and since in that case also $\overline{\eta}_i = 0$, and the intermediary orbit is a circle, the varied orbit consists of four Keplerian ellipses with the excentricities ε_i and the fixed perijoves ϖ, as it evidently must be.

If we consider the constants of integration ε_i as quantities of the first order, like $\overline{\eta}_i$[1]), and if we put

$$\varphi_i = \beta_i \tau + \varpi_{i0} = \varkappa \tau + \varpi_i,$$

---

[1]) If follows from (18) that $\varkappa \,.\, \overline{\eta}_i$ is of the second order, and consequently $\overline{\eta}_i$ of the first.

then the effect of the variations on the radius-vector and the longitude is found to be, to the first order

$$\left.\begin{aligned} \delta r_i &= -\tfrac{2}{3} \mathrm{a}_i \sum_q c'''_{iq} \varepsilon_q \cos \varphi_q - \mathrm{a}_i \sum_q \{ \tfrac{1}{2} (c_{iq} + c'_{iq}) \varepsilon_q \cos (\lambda_i - \varpi_q) + \\ &\qquad + \tfrac{1}{2} (c_{iq} - c'_{iq}) \varepsilon_q \cos (c_i \tau + \varphi_q) \}, \\ \delta w_i &= \sum_q c''_{iq} \varepsilon_q \sin \varphi_q + \sum_q \{ (c_{iq} + c'_{iq}) \varepsilon_q \sin (\lambda_i - \varpi_q) + \\ &\qquad + (c_{iq} - c'_{iq}) \varepsilon_q \sin (c_i \tau + \varphi_q) \}. \end{aligned}\right\} . \quad (29)$$

As a first approximation we have $a_{ii} = -\varkappa$, a_{ij} and b_{ij} being of the second order. Also with the same approximation, for $q = 1 \ldots 4$, $\beta_q = \varkappa$, and consequently from (25) $c_{iq} = c'_{iq}$ approximately. The difference $c_{iq} - c'_{iq}$ is thus of a higher order, and the last term of (29) can be omitted. Further also d_{ij} and e_{ij} are at least of the second order, and consequently by the last of (25) c'''_{iq} is of a higher order than c'_{iq} and c''_{iq}. It follows that the first term of δr_i can also be omitted in the first approximation. The equations (29) then have entirely the form (6). At the same time we see the reason why the inequalities II and III are so much smaller in the radius-vector than in the longitude.

5. *The perturbations.*

We must now take into account the part of the perturbative function

$$R - [R_i],$$

which contains terms whose argument D varies with the time, thus $D = E\tau$. We will only give the theory in its broad outlines. For details we refer to the publication in the Leiden Annals. We put for abbreviation

$$h_i = \mathrm{x}_i, \qquad k_i = \mathrm{y}_i, \qquad \nu_i = \mathrm{x}_{i+4}, \quad \omega_i = \mathrm{y}_{i+4}.$$

The differential equations then assume the form

$$\left.\begin{aligned} \frac{d\mathrm{x}_i}{d\tau} &= \sum_E \alpha_{i,E} \sin E\tau + \sum_j \sum_E f_{i,j,E} \sin E\tau\, \mathrm{x}_j + \sum_j \sum_E g_{i,j,E} \cos E\tau\, \mathrm{y}_j, \\ \frac{d\mathrm{y}_i}{d\tau} &= -\sum_E \alpha'_{i,E} \cos E\tau - \sum_j \sum_E f'_{i,j,E} \cos E\tau\, \mathrm{x}_j - \sum_j \sum_E g'_{i,j,E} \sin E\tau\, \mathrm{y}_j, \end{aligned}\right\} , \quad (30)$$

where i and j take the values from 1 to 8. The arguments are of the form

$$D = E\tau = k\tau + k' c_4 \tau,$$

k and k' being any integers, positive, negative or zero. If we take only $k = k' = 0$, the equations (30) are reduced to (23). Thus we have, e.g.

$$f_{i,j,0} = 0, \qquad g_{i,j,0} = a_{i,j}, \qquad g_{i,j+4,0} = b_{ij} \quad \text{etc.}$$

The equations (30) can be satisfied by

$$\left.\begin{aligned} \mathrm{x}_i &= \sum_E A_{i,E} \cos E\tau + \sum_j \sum_E M_{i,j,E}\, \varepsilon_j \cos(\varphi_j + E\tau), \\ \mathrm{y}_i &= \sum_E A'_{i,F} \sin E\tau + \sum_j \sum_E M'_{i,j,E}\, \varepsilon_j \sin(\varphi_j + E\tau), \end{aligned}\right\} \quad . \quad . \quad (31)$$

where

$$\varphi_i = \beta_i \tau + \varpi_{i0}\,.$$

Substituting these in (30) we find again equations of condition for β_q, $M_{i,q,E}$ and $M'_{i,q,E}$. There is an infinite number of these equations. Hence the condition for β_q is an infinite determinant put equal to zero. It is evident however that if

$$\beta = \beta_q$$

is a root, then all numbers of the form

$$\beta' = \pm\, \beta_q \pm k \pm k' c_4 \qquad (k, k' = -\infty \ldots + \infty)$$

are also roots, since changing β to β' does not affect x_i and y_i beyond a change in the notation by which the different coefficients are distinguished.

It is not difficult to get an infinite determinant for β^2 instead of β. If we put

$$\begin{aligned} P_{i,j,E} &= \tfrac{1}{2}\,(M_{i,j,E} + M_{i,j,-E}), \\ P'_{i,j,E} &= \tfrac{1}{2}\,(M_{i,j,E} - M_{i,j,-E}), \\ Q'_{i,j,E} &= \tfrac{1}{2}\,(M'_{i,j,E} + M'_{i,j,-E}), \\ Q_{i,j,E} &= \tfrac{1}{2}\,(M'_{i,j,E} - M'_{i,j,-E}), \end{aligned}$$

Then the equations become

$$\left.\begin{aligned} &\beta P_{i,E} + EP'_{i,E} + \tfrac{1}{2} \sum_j \sum_F \{(g_{i,j,F-E} + g_{i,j,F+E})\, Q'_{j,F} - \\ &\qquad\qquad - (f_{i,j,F-E} + f_{i,j,F+E})\, P'_{j,F}\} = 0, \\ &\beta P'_{i,E} + EP_{i,E} + \tfrac{1}{2} \sum_j \sum_F \{(g_{i,j,F-E} - g_{i,j,F+E})\, Q_{j,F} - \\ &\qquad\qquad - (f_{i,j,F-E} - f_{i,j,F+E})\, P_{j,F}\} = 0, \\ &\beta Q'_{i\,E} + EQ_{i,E} + \tfrac{1}{2} \sum_j \sum_F \{(g'_{i,j,F-E} + g'_{i,j,F+E})\, Q_{j,F} + \\ &\qquad\qquad + (f'_{i,j,F-E} + f'_{i,j,F+E})\, P_{j,F}\} = 0, \\ &\beta Q_{i\,E} + EQ'_{i,E} + \tfrac{1}{2} \sum_j \sum_F \{(g'_{i,j,F-E} - g'_{i,j,F+E})\, Q'_{j,F} + \\ &\qquad\qquad + (f'_{i,j,F-E} - f'_{i,j,F+E})\, P'_{j,F}\} = 0. \end{aligned}\right\} \quad . \quad (32)$$

where we have omitted the index q in β_q, $P_{i,q,E}$, $P'_{i,q,E}$ etc. It is only necessary to consider these equations for positive values of E. The sums however include *all* values of F. We have

$$\begin{aligned} P_{i,F} &= P_{i,-F}, & Q_{i,F} &= -\,Q_{i,-F}, \\ P'_{i,F} &= -\,P'_{i,-F}, & Q'_{i,F} &= Q'_{i,-F}. \end{aligned}$$

Multiplying the second and third of (30) by β, and then substituting in them the values of βP_{iE} and βQ_{iE} derived from the first and last we find equations which contain only P'_{iE} and Q'_{iE}. These have the form

$$\left.\begin{array}{l}(\beta^2-E^2)\, Q'_{i,E} + \sum\limits_{j}\sum\limits_{F} G_{i,j,E,F}\, Q'_{j,F} + \sum\limits_{j}\sum\limits_{F} H_{i,j,E,F}\, P'_{j,F}. \\ (\beta^2-E^2)\, P'_{i,E} + \sum\limits_{j}\sum\limits_{F} G'_{i,j,E,F}\, Q'_{j,F} + \sum\limits_{j}\sum\limits_{F} H'_{i,j,E,F}\, P'_{j,F}, \end{array}\right\} \quad (33)$$

the coefficients G, H, G', H' being all at least of the second order. We have

$$P_{i,0} = c_i, \qquad P_{i+4,0} = c'''_i, \qquad Q'_{i,0} = c'_i, \qquad Q'_{i+4,0} = c''_i$$

$$P'_{i,0} = 0, \qquad Q_{i0} = 0.$$

The infinite determinant resulting from the elimination of P' and Q' from (33) has, for each argument E, 16 rows and columns, corresponding to the 16 unknowns $P'_{i,E}$ and $Q'_{i,E}$. For $E=0$ there are only 8 unknowns, and also the first of (33) becomes an identity for $E=0$, so that there are only 8 columns and rows. The determinant formed by the elements common to these 8 columns and rows may be called the central determinant.

All elements of the determinant outside the diagonal are of the second order[1]). The elements of the diagonal have the form $G + E^2 - \beta^2$, where G is of the second order at least. In the central square we have $E=0$, outside the central square E has a finite value, and therefore $G + E^2$ is of the order zero. The manner in which the determinant is reduced to its central square will be explained by a simple example, in which I take for each argument E only 2 instead of 16 rows and columns, and of the rest of the determinant also only 2 rows and columns are written. This is sufficient to illustrate the principle. We then have the transformation

$$\begin{vmatrix} a_{11}-\beta^2 & a_{12} & a_{13} & a_{14} \\ a_{21} & a_{22}-\beta^2 & a_{23} & a_{24} \\ a_{31} & a_{32} & a_{33}+E^2-\beta^2 & a_{34} \\ a_{41} & a_{42} & a_{43} & a_{44}+E^2-\beta^2 \end{vmatrix} =$$

---

[1]) This is not correct. There are elements outside the diagonal of the orders zero and one. The conclusions reached in the text are however not affected and remain correct. For a more thorough discussion see Leiden Annals XII. 1. (Note added in the English translation).

$$
= \begin{vmatrix} b_{11}-\beta^2 & b_{12} & 0 & 0 \\ b_{21} & b_{22}-\beta^2 & 0 & 0 \\ a_{31} & a_{32} & a'_{33}+E^2-\beta^2 & a'_{34} \\ a_{41} & a_{42} & a'_{43} & a'_{44}+E^2-\beta^2 \end{vmatrix},
$$

where

$$
\begin{aligned}
b_{11} &= a_{11} - x_1 a_{31} - y_1 a_{41}, & b_{12} &= a_{12} - x_1 a_{32} - y_1 a_{42}, \\
b_{21} &= a_{21} - x_2 a_{31} - y_2 a_{41}, & b_{22} &= a_{22} - x_2 a_{32} - y_2 a_{42}, \\
a'_{33} &= a_{33} + x_1 a_{31} + x_2 a_{32}, & a'_{34} &= a_{34} + y_1 a_{31} + y_2 a_{32}, \\
a'_{43} &= a_{43} + x_1 a_{41} + x_2 a_{42}, & a'_{44} &= a_{44} + y_1 a_{41} + y_2 a_{42},
\end{aligned}
$$

and the multipliers x_1, x_2, y_1, y_2 are determined by

$$
\left.\begin{aligned}
a_{13} + x_1 a_{11} + x_2 a_{12} - x_1 (a'_{33} + E^2) - y_1 a'_{43} &= 0 \\
a_{23} + x_1 a_{21} + x_2 a_{22} - x_2 (a'_{33} + E^2) - y_2 a'_{43} &= 0 \\
a_{14} + y_1 a_{11} + y_2 a_{12} - y_1 (a'_{44} + E^2) - x_1 a'_{34} &= 0 \\
a_{24} + y_1 a_{21} + y_2 a_{22} - y_2 (a'_{44} + E^2) - x_2 a'_{34} &= 0
\end{aligned}\right\} \quad . \quad . \quad (34)
$$

The determinant is thus reduced to the product of two determinants. In our case we will in this way "peel off" 16 rows and columns at a time, instead of two. It follows from (34) that x_i and y_i are of the second order at least. The corrections

$$b_{ij} - a_{ij}$$

to be applied to the inner terms are thus of the fourth order. If now we proceed to remove the columns and rows of another argument F, the effect of these corrections on the central determinant will be of the *sixth* order. Consequently, if we agree to neglect quantities of the sixth order in β^2, and therefore, since β itself is of the first order, quantities of the fifth order (i.e. of the order of 10^{-10}) in β, then the rows and columns of each argument can be removed *separately*, independently of all other arguments. The determinant finally is reduced to a product of an infinite number of determinants, of which the central one has 8 columns and rows, and all others 16. Each of these corresponds to one argument $\pm E$. As has been pointed out above, to each root β_q belongs a root $\beta_q + E$ and a root $\beta_q - E$. It is thus evidently only necessary to determine the 8 roots of the corrected central determinant. The corrections which have been applied to the elements of the central square are at least of the fourth order. These 8 roots will therefore differ very little from those of the uncorrected determinant Δ, of which three are zero. For the corrected determinant the relations (29) do not hold, and also the a priori reasoning by which we showed that there must be six roots $\beta = 0$, do not apply here, since $x_i = y_i = 0$ is not a particular solution of the equations (30). The three roots of the corrected determinant corresponding to the three zero roots of $\Delta = 0$ may therefore differ slightly from zero, but they will in any case be extremely small, and for all practical purposes the other five roots are the only important ones.

It remains to determine the coefficients A_{iE} and A'_{iE}. We have the equations

$$\left.\begin{aligned} EA_{i,E} + \tfrac{1}{2} \sum_j \sum_F \{ f_{i,j,F} (A_{j,E-F} - A_{j,E+F}) + \\ + g_{i,j,F} (A'_{j,E-F} + A'_{j,E+F}) \} + \alpha_{i,E} = 0, \\ EA'_{i,E} + \tfrac{1}{2} \sum_j \sum_F \{ f'_{i,j,F} (A_{j,E-F} + A_{j,E+F}) - \\ - g'_{i,j,F} (A'_{E-F} - A'_{E+F}) \} + \alpha'_{i,E} = 0, \end{aligned}\right\} . \tag{36}$$

from which the coefficients can be solved by successive approximation. Nearly always the first approximation

$$A_{i,E} = -\frac{\alpha_{i,E}}{E}, \qquad A'_{i,E} = -\frac{\alpha'_{i,E}}{E}$$

will be sufficient.

Mechanics. — *"Further remarks on the solutions of the field-equations of* EINSTEIN'S *theory of gravitation"*. By Prof. W. DE SITTER.

(Communicated in the meeting of April 26, 1917).

1. EINSTEIN has recently [1]) enounced the postulate, that a solution of the field-equations

$$G_{\mu\nu} - \tfrac{1}{2}\lambda\, g_{\mu\nu} = -\varkappa\, T_{\mu\nu} + \tfrac{1}{2}\varkappa\, g_{\mu\nu}\, T \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in order to be admissible for the actual physical world, must have no discontinuities "at finite distances". In particular the determinant g must for all points at finite distances be different from zero. This postulate is not fulfilled by my solution B, as EINSTEIN very correctly points out, and as is also shown very clearly in my communications. This postulate, however, in the form in which it is enounced by EINSTEIN, is a *philosophical*, or metaphysical, postulate. To make it a *physical* one, the words: "all points *at finite distances*" must be replaced by "all *physically accessible* points". And if the postulate is thus formulated, my solution B does fulfil it. For the discontinuity arises for

$$r = r_1 = \tfrac{1}{2}\pi R.$$

This is at a finite distance in space, but it is physically inaccessible, as I have already pointed out [2]). The time needed by a ray of light, and a fortiori by a moving material point, to travel from any point r, ψ, ϑ to a point r_1, ψ_1, ϑ_1 (ψ_1 and ϑ_1 being arbitrary) is infinite. The singularity at $r = r_1$ can thus never affect any physical experiment, or as I expressed it l.c., the paradoxical phenomena, or rather absence of phenomena, resulting from this singularity, can only happen before the beginning, or after the end of eternity.

2. A similar remark has been made by Prof. FELIX KLEIN, in a letter to the present writer dated 1918 April 19. He writes:

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[1]) *Kritisches zu einer von Herrn* DE SITTER *gegebenen Lösung der Gravitationsgleichungen*, Sitzungsber. Berlin, 7 March 1918, page 270.

[2]) *On Einstein's theory of gravitation and its astronomical consequences, third paper*, Monthly Notices of the R. A. S. Vol. LXXVIII, page 17—18.

On the curvature of space, these Proceedings, Vol XX, p. 229.

"Denken Sie sich die ganze vierdimensionale Welt von Weltlinien durchfurcht. Nun scheint es doch bei allen Ansätzen im EINSTEIN'schen Sinne eine notwendige physikalische Voraussetzung zu sein, dass man diese Linien, so wie sie sich kontinuierlich an einander reihen, mit einem positiven Richtungssinn versehen kann (der von der "Vergangenheit" zur "Zukunft" führt). Dies ist nun im Falle B nicht möglich. Lege ich nämlich einer ersten Linie nach Belieben einen positiven Sinn bei und übertrage diesen unter Beachtung der Kontinuität auf die Nachbarlinien, so komme ich schliesslich, wegen der Zusammenhangsverhältnisse des Elliptischen Raumes, zur Ausgangslinie mit umgekehrtem Sinn zurück. Es entspricht das dem Umstande, dasz die Ebene der elliptischen (wie der projektivischen) Geometrie eine *einseitige* Fläche ist, bei der sich die Indicatrix ↺, die ich um irgend einen Punkt der Ebene herum legen mag, wenn ich sie längs einer durch den Punkt laufenden Geraden verschiebe, bei Rückkehr zum Ausgangspunkte umgekehrt hat: ↻. Meine Bemerkung in Math. Annalen 37, p. 557—58: dass die Uebertragung der Schering'schen Potentialtheorie auf den Fall der elliptischen Ebene unstatthaft ist, ruht genau auf demselben Umstande".

Prof. KLEIN's remark is undoubtedy correct: we return to the starting point with the positive direction reversed, but only if we have travelled *along a straight line*, or at least along a line *which intersects the polar line of the starting point*. This "motion", though mathematically thinkable, is *physically impossible*, for the same reason as above. If we travel along an arbitrary closed curve, which does not intersect the polar line of any of its points, i.e. if we describe a physically possible circuit, then we shall, on returning to the starting point, find the positive direction unaltered.

In my former paper [1]) I pointed out that, in spherical space, the potential $g_{44}-1$ becomes infinite at the antipodal point. I concluded therefrom that, for the representation of the actual physical world, the elliptical space is to be preferred to the spherical. Prof. KLEIN has already made the same remark in his paper of 1890, quoted at the end of his letter. He points out, however, that in elliptical space the sign of the potential would be ambiguous. This would be the case if the above mentioned circuit were possible. Since it is impossible we can choose one of the two possible signs without the danger that any physical phenomena or experiments will ever lead to contradiction or indeterminateness.

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[1]) *On the curvature of space*, these Proceedings, Vol XX, p. 240.

I use this occasion to point out that, as is well known, Prof. KLEIN was the first to call attention to the elliptical space and its relation to and difference from the spherical space, and generally to investigate and explain the different possibilities of non-Euclidean geometry [1]). In fact all geometrical concepts used in the different stages of the development of modern physical theory are contained in KLEIN's general scheme as given in the second of the papers quoted in the footnote.

3. If we start from the assumption, that the gravitational field is of such a nature that it is possible, by introducing a suitable system of coordinates, to bring the line-element into the form

$$ds^2 = -a\,dr^2 - b\,(d\psi^2 + sin^2\,\psi\,d\vartheta^2) + f\,dt^2, \quad . \; . \; . \quad (2)$$

then we can call r the "radius-vector" and t the "time". If now we add the condition that a, b, f must be functions of r only, and not of t, ψ, ϑ, then these conditions may be briefly expressed by saying that the field is *static* and *isotropic*. Then the line-element of three-dimensional space is

$$d\sigma^2 = a\,dr^2 + b\,[d\psi^2 + sin^2\,\psi\,d\vartheta^2] \quad . \; . \; . \; . \; . \quad (3)$$

and consequently we have

$$ds^2 = -d\sigma^2 + f\,dt^2 \, . \; . \; . \; . \; . \; . \; . \; . \; . \quad (2')$$

If now we add the hypothesis that $d\sigma^2$ shall be the line-element of a space of constant curvature, thus

$$\left.\begin{aligned} r &= R\,.\,\chi \\ d\sigma^2 &= R^2\{\,d\chi^2 + sin^2\,\chi\,[d\psi^2 + sin^2\,\psi\,d\vartheta^2]\,\}, \end{aligned}\right\} \quad . \; . \; . \quad (3')$$

then the field-equations (1) reduce to one equation for f, of which the solutions A and B are

$$f = c^2 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (4A)$$

$$f = c^2\,cos^2\,\chi \quad . \; . \; . \; . \; . \; . \; . \; . \quad (4B)$$

If we drop the condition of isotropy, then f may be a function of r, ψ, ϑ. For this case LEVI-CIVITA [2]) has given the general solution of the differential equation for f. He starts from EINSTEIN's original equation, i. e. the equation (1) with $\lambda = 0$. It is however not difficult to extend the proof to the general case. Then the equation (11′) of LEVI CIVITA [l. c. page 530] replaces (11) [p. 526],

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[1]) *Ueber die sogenannte Nicht-Euclidische Geometrie,* Math. Annalen, Band 4 and 6 (1871 and 1872).

Programm zum Eintritt in die philosophische Facultät, Erlangen 1872, reprinted Math. Annalen, Band 43, p. 63.

[2]) *Realta fisica di alcuni spazi normali del Bianchi,* Rendiconti della R. Accad. dei Lincei, Vol. XXVI, p. 519 (May 1917).

and consequently we must use $K + \varkappa p - \lambda$ instead of $K + \varkappa p$. The equation (13) is not affected, but we now have $K^* = K - \frac{1}{2}(3K + \varkappa p - \lambda)$, and again $K^* = K$; therefore instead of (14) we find $3K + \varkappa p - \lambda = 0$, but the general solution of (13) remains the same. This solution, expressed in my notation, is

$$\sqrt{f} = a_0 \cos \chi + a_1 \sin \chi \sin \beta, \quad \ldots \ldots \quad (5)$$

β being the "latitude" referred to a plane of symmetry, whose inclination ε and node ϑ_0 on the plane $\psi = 0$ are given by

$$b_0 = -a_0, \quad \begin{aligned} b_1 &= a_1 R \sin \varepsilon \sin \vartheta_0, \\ b_2 &= -a_1 R \sin \varepsilon \cos \vartheta_0, \\ b_3 &= a_1 R \cos \varepsilon. \end{aligned}$$

$b_0 \ldots b_3$ being the constants of integration introduced by LEVI-CIVITA.

The condition of isotropy now is $a_1 = 0$. If this is introduced (5) is reduced to (4 B), and it thus appears that my solution B is the *general solution* for the case of a static and isotropic gravitational field in the absence of matter.

Meteorology. — "*The Atmospherical Circulation above Australasia according to the Pilotballoon-Observations made at Batavia.*" By W. VAN BEMMELEN. (Communicated by Dr. J. P. VAN DER STOK).

(Communicated in the meeting of April 26, 1918.)

During the years 1909—'17 numerous determinations of direction and velocity of the wind at different altitudes were made by means of pilotballoon-observations, by the personnel of the Observatory of Batavia; in the beginning with a view to investigating the general system of aircurrents above the West of Java, later on aiming at special purposes. Just for this reason the series of observations is in no way homogeneous. Elsewhere I will treat this heterogeneousness in extenso, here however briefly. At present the general results about direction and velocity of the principal aircurrents in the different levels will be given.

In recent years a more or less detailed report on these observations has sometimes been given [1]; for this occasion the whole material has been subjected to a comprehensive calculation, first the observations have come to an end by lack of balloons, secondly as it was considered advisable not to wait any longer with such a comprehensive survey.

A few observations about the endeavours to render the series more homogeneous may be given here. In the first place for levels under 3 km. observations made between 6 a.m. and 9 a.m. have been taken into account only, in order to limit as much as possible the disturbing influence of land- and sea-breezes, which reach about to the 3 km. level and are weak during those morning hours. Moreover to the results for those lowest levels a special correction derived from the results obtained for the diurnal and semidiurnal variation of the wind has been applied [2]).

In the second place of balloon-observations made outside Batavia

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[1] Zittingsverslag van 25 Juni 1910. (These Proc. Vol. XVIII, p. 149).
Observatorium, Batavia; Verhandelingen N⁰. 1.
Nature Vol. 87, 1911; Vol. 90, 1912; Vol. 91, 1914.
Luftfahrt und Wissenschaft, Berlin (J. STICKER) Heft 5, 1913.
Natuurkundig Tijdschrift, Dl. 73, 1913.

[2] Zittingsverslag van 26 Mei 1917. (These Proc. Vol XX, p. 119).

TABLE I. Number of Windobservations

Height	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Total.
0.1 km.	29	21	14	14	16	44	37	61	67	57	41	34	435
1	31	23	31	23	27	52	43	66	79	61	43	42	521
2	29	21	30	24	27	47	41	53	70	57	36	38	473
3	27	22	28	26	39	77	74	108	131	100	56	38	726
4	23	17	25	26	36	66	70	100	121	91	52	34	661
5	23	15	23	26	31	62	61	92	106	82	48	29	598
6	21	13	23	24	29	52	50	87	94	72	40	24	529
7	18	13	21	22	26	38	41	78	77	69	34	22	459
8	13	13	19	21	21	30	30	65	65	54	26	22	379
9	8	11	17	19	20	25	27	50	46	43	20	21	307
10	8	10	15	19	19	23	25	40	36	36	16	16	263
11	8	9	13	17	15	18	23	31	29	27	13	14	217
12	2	7	10	15	15	13	19	22	21	23	9	11	167
13	3	5	7	12	13	12	15	18	17	17	5	9	143
14	3	5	7	11	13	10	13	15	13	9	4	5	108
15	3	3	6	10	11	8	13	12	11	7	3	5	92
16	3	3	5	9	9	8	11	8	9	7	2	5	79
17	3	2	5	9	7	8	10	5	8	4	2	5	68
18	2	2	4	7	5	5	5	5	6	2	2	4	49
19	2	2	4	6	1	5	5	4	5	2	2	4	42
20	1	2	2	6	1	1	2	3	4	2	1	4	29
21	1	1	2	6	1	—	1	3	3	2	1	4	25
22	1	1	2	5	1	—	1	3	3	1	—	3	21
23	1	1	2	5	—	—	1	1	3	—	—	2	16
24	—	1	2	4	—	—	1	1	3	—	—	1	13
25	—	—	1	4	—	—	—	1	3	—	—	1	10
26	—	—	1	1	—	—	—	—	3	—	—	1	6
27	—	—	1	1	—	—	—	—	2	—	—	1	5
28	—	—	1	—	—	—	—	—	1	—	—	—	2
29	—	—	1	—	—	—	—	—	1	—	—	—	2
30	—	—	1	—	—	—	—	—	1	—	—	—	2
31	—	—	—	—	—	—	—	—	1	—	—	—	1

(in Oct. 1916 on the "Duizend Eilanden" and in June/July 1917 at Bandoeng resp. 84 km. to the N. and 119 km. to the S.W. of Batavia) only a small number has been added to the material for the levels above 10 km.

Table I shows the number of observations for each month of the year.

For an earlier publication certain functions of windvalues for five seasons, specially chosen for that purpose, have been compiled by me. These functions gave a synopsis of direction, velocity and frequency and were called relative directional-velocities. Moreover I gave the resultant directions and velocities obtained by simply averaging the rectangular components of the observed windvectors.

On this occasion the average values of the components have been compiled for the different months of the year separately.

To apply this working-system is rendered possible by the constancy exhibited by the direction of aircurrents above Batavia, even when, as is the case for some months, the number of observations is scarce for the higher layers. Above the 10 km. level, however, this number decreases so quickly, that it was indispensable to try getting plausible values of the mean components by the process of graphical averaging.

For this purpose two isopleth-diagrams have been drawn up, one for the eastwest, the other for the northsouth components. From these graphs fresh monthly values have been taken for the different levels and from them new vectors have been computed. These vectors have been united in table II.

They may be considered to give direction and velocity of the main aircurrent for each month and level.

In this table II are framed and typed in different colours 1. easterly winds having a southern component; 2. those having a northern one, 3. westerly winds with components either to the south or to the north. By this the table gives a synopsis of the vertical extensions of the principal aircurrents in the different months throughout the year, which may be described in the following way.

Tradewind. It is a well-known fact that in the southerly winter the tradewind blows over the seas surrounding Java as an easterly wind with southern components. However, the application of the correction for the diurnal and semidiurnal variation mentioned above on the average windvectors under the 3 km. level, has resulted in east-winds with northern components.

This outcome is a consequence of the fact, that by day the suction of air from the sea (N-breeze) is stronger than the flowing off by night (S-breeze). The excess is convected to higher layers.

For this reason there remains in the daily mean a northcomponent, which has a local origin and does not belong to the tradewind.

Here I shall disregard this deviation.

From June till September the upper limit of the trade lies between 3 and 4 km., it then rises and reaches a height of 13 km. in the month of February; afterwards it descends to 10 km. in May and falls down quickly to 3—4 km. in June.

Westmonsoon. It may be said that the Westmonsoon is so to say inserted under the trade from Nov. till April, lifting it up. The upper limit of the monsoon rises to 6—7 km. in February, the velocity attaining in this month a maximum at a height of about 1 km.

In its lower layers the monsoon exhibits northerly components, but at little height (± 1 km.) they change into southern ones. These northern components belong partly to the monsoon itself and are not caused exclusively by the preponderance of the seabreeze, as is proved by the winds observed above the neighbouring seas.

Antitrade-wind. The occurrence of easterly wind with a northern component has to be interpreted as that of a current of antitrade-character, i.e. as the beginning of a flowing off of the equatorial belt, which, proceeding to higher latitudes will be deflected more and more by the rotation of the earth, changing from a north-easterly to a northerly and eventually a northwesterly wind.

Table II shows the antitrade blowing throughout the whole year descending in the southern winter to the 4 km. level, and not being found under a height of 13 km. in the southern summer (February). A maximum velocity is reached in the month of August on the 4 km. level and a secondary one at a height of 15 km. in the month of February.

During the transition of the monsoon to the trade (March-April) the antitrade is weak; on the contrary, when the trade gives way to the Westmonsoon in the month of October, it remains rather strong. Though the table exhibits the fact that in November slight westerly winds blow in the layers between 8 and 11 km., they are no doubt to be interpreted as symptoms of this transition.

Uppertrade-wind. Above the layers of the antitrade air again flows towards the equator. This aircurrent being analogous to the trade, I called it uppertrade. Its lower limit is highest in the month of making use of the results obtained with registering balloons, including those for humidity.[1])

In doing this, for the height and temperature at the base of the stratosphere the following values were taken.

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[1]) Observat. Batavia, Verhandelingen N⁰. 4.

TABLE II.

DIRECTION AND VELOCITY OF THE WIND DERIVED FROM DIAGRAMS OF ISOPLETHES FOR THE AVERAGE E. AND N. COMPONENTS.

	JANUARY	FEBRUARY	MARCH	APRIL	MAY	JUNE	JULY	AUGUST	SEPTEMBER	OCTOBER	NOVEMBER	DECEMBER	
30 K M.													
29													KRAKATAU-WIND
28													
27													
26													
25													
24	15 E	[illegible] E	9 E	[illegible] E	[illegible] E	0 E	0 E	1 S	[illegible] E 18 S	5 E [illegible] S	15 E 4 S	22 E	
23	E [illegible] S	10 E 6 S	[illegible] E 7 S	2 E 27 S	E 90 S	1 W 45 S	2 W 27 S	1 W 27 S		5 E 6 S	9 E 6 S	16 E 3 S	
22	12 E [illegible] S	7 E 8 S	E 18 S	1 W 45 S	2 W 27 S	3 W 18 S	3 W 18 S	2 W	2 W 45 N	5 E	10 E 6 S	16 E 3 S	UPPER-TRADE-WIND
21	[illegible] E 6 S	E 18 S	1 S	2 W 27 S	3 W 18 S	5 W 11 S	4 W 14 S	3 W 10 S	1 W 45 N	3 E 45 N	10 E	12 E 8 S	
20	E [illegible] S	1 E [illegible] S	1 W 45 S	3 W 18 S	4 W 14 S	1.0 W 6 S	5 W 11 S	3 W 10 S	1 W 27 S	4 E 27 N	9 E 16 N	10 E	
19	E	[illegible] E	0 S	2 W 27 S	5 W 11 S	1.0 W 6 S	5 W 11 S	2 W 27 S	0 S	5 E 22 N	10 E 21 N	10 E 14 N	
18	6 E 9 N	4 E 14 N	1 E	0 S	3 W 18 S	1.0 W 6 S	3 W 18 S	1 E 45 S	5 E 11 S	6 E 18 N	9 E 25 N	9 E 20 N	
17	9 E 6 N	7 E 8 N	5 E 7 N	1 E	2 W 90 S	4 W 34 S	2 E 27 S	8 E 3 S	10 E	9 E 12 N	10 E 21 N	7 E 23 N	
16	11 E	11 E	5 E	2 E 27 N	2 E 90 S	2 E 13 S	10 E 6 N	14 E 8 N	13 E 11 N	11 E 10 N	9 E 12 N	8 E 14 N	
15	9.5 E 17 N	12 E 10 N	10 E 6 N	1 E 45 N	3 E 68 N	4 E 27 S	15 E 8 N	20 E 18 N	15 E 23 N	11 E 10 N	9 E 12 N	8 E 17 N	ANTI-TRADE-WIND.
14	9 E 13 N	11 E 5 N	9 E 6 N	4 E 20 N	6 E 45 N	5 E 22 S	17 E 17 N	22 E 14 N	14 E 25 N	9 E 12 N	8 E 14 N	9 E 20 N	
13	8 E 7 N	[illegible] E [illegible] S	7 E	4 E 20 N	4 E 35 N	[illegible] E 22 S	17 E 21 N	18 E 24 N	13 E 27 N	6 E 18 N	6 E 18 N	8 E 27 N	
12	6 E 5 N	[illegible] E 5 S	E 6 S	4 E 7 N	3 E 27 N	5 E 22 N	15 E 16 N	7 E 56 N	12 E 27 N	5 E 24 N	2 E 11 N	6 E 27 N	
11	4.5 E	[illegible] E [illegible] S	E 15 S	E 10 S	3 E 18 N	6 E 22 N	12 E 14 N	13 E 18 N	11 E 18 N	5 E 24 N	2 W 11 N	4 E 21 N	
10	[illegible] E 5 S	[illegible]6 E 16 S	3.2 E 20 S	2.2 E 27 S	3.0 E	5.6 E 12 N	10.1 E 8 N	12.2 E 10 N	10.2 E 11 N	4.8 E 12 N	1.6 W 22 N	4.0 E 7 N	
9	E 1 S	[illegible] E 21 S	3.0 E 2[illegible] S	2.0 E 1[illegible] S	[illegible] E 1[illegible] S	5.5 E 6 N	9.2 E 11 N	10.0 E 3 N	9.0 E 29 N	5.0 E 2 N	1.0 W 6 S	3.0 E 1 S	
8	[illegible] E 20 S	E [illegible] S	E 22 S	1.6 E [illegible] S	[illegible] E 18 S	5.5 E 2 N	8.1 E 9 N	8.5 E 4 N	7.5 E 15 N	5.5 E 2 S	0.6 W 42 S	3.0 E 10 S	
7	1.6 E [illegible] S	[illegible] E [illegible] S	1.6 E 18 S	1.1 E [illegible] S	[illegible] E [illegible] S	5.0 E 7 N	7.2 E 8 N	7.1 E 8 N	6.0 E 40 N	4.8 E 1 S	0.4 S	2.1 E 19 S	TRADE-WIND
6	1.3 W 67 S	0.7 W 82 S		0.5 E 22 S	3.1 E 11 S	4.7 E 13 N	4.4 E 29 N	4.8 E 21 N	4.4 E 16 N	4.2 E 1 S	1.6 E 18 S	1.3 E 42 S	
5	2.3 W 29 S	2.0 W 11 S	1.0 W	0.5 W 64 N	3.0 E 2 N	3.7 E 44 N	3.4 E 5 N	2.6 E 18 N	2.5 E 37 N	3.5 E 2 S	2.5 E 12 S	1.0 S	
4	3.2 W 22 S	3.3 W 33 S	1.6 W	1.0 W 17 N	3.0 E 6 N	2.6 E 16 N	2.3 E 5 N	1.9 E 12 N	1.4 E 21 N	3.4 E 2 S	2.6 E 11 S	1.1 W 7 S	
3	4.5 W 29 S	5.4 W 15 S	2.4 W 19 S	1.0 W 11 S	3.3 E 12 S	2.0 E 11 S	1.8 E	1.2 E 24 S	2.2 E 27 S	4.6 E 22 S	2.6 E 16 S	2.0 W 41 S	
2	4.9 W 23 S	6.3 W 18 S	2.9 W 24 S	1.5 W 48 S	3.8 E 17 S	2.3 E 31 S	2.7 E 42 S	2.7 E 29 S	3.4 E 17 S	4.1 E 15 S	1.8 E 19 S	3.8 W 22 S	WESTMONSOON
1	4.5 W 3 N	6.6 W 15 N	3.0 W 4 S	1.4 E 45 S	2.9 E 4 S	2.6 E 9 N	3.4 E 42 S	4.0 E	4.1 E 3 S	2.9 E 6 N	1.7 W 39 S	3.8 W 6 S	
0.1	2.5 W 37 N	2.6 W 23 N	1.4 W 45 N	0.9 W 6 N	0.6 N	1.3 E 42 N	1.5 E 22 N	1.5 E [illegible] N	1.5 E 18 N	2.5 E 27 N	1.9 W 22 N	1.9 W 26 N	

October, reaching 21—22 km., it descends in the southern summer to 18—19 km. and afterwards soon goes down to the level of 12—13 km. in the month of June.

The velocity of this uppertrade is comparatively small, taking into account the great altitude at which it blows; however in levels exceeding ± 24 km. its main velocity increases and it changes into the:

Krakatoa-wind, as was called by me the equatorial Eastwind which blows at a height of about 30 km. with a velocity of ± 40 m. p. sec.

This current which manifested itself in the days following on the eruption of Krakatoa, was twice found back at Batavia, once in March and once in September.

High Westwinds. Between the layers of the uppertrade in the southern winter is driven like a wedge a layer of high westerly winds. The lower limit of these winds reaches lowest in June, descending to 16—17 km.; the upper limit however is uncertain owing to scarcity of observations; presumably it ascends to a height of ± 24 km. in the same month of June.

The velocity of these high westerly winds may be said to be relatively small for the high altitude of their region, but each time in their season when a balloon entered that region they were met with and they were found to be uninterrupted by westerly winds. For the rest these winds mostly have a southern component.

Explanation of the system of aircurrents.

To give a satisfactory explanation of the system of aircurrents described above is as yet hardly feasible, as the Batavian observations are the only ones in the australasiatic part of the equatorial and subtropical zones. So the attempt to explain them as made here has to be based partly on conjecture.

In order to make an endeavour to investigate the relation between the system of aircurrents on one side and pressure together with temperature on the other, in the first place monthly values of airpressure have been calculated for kilometer levels up to 26 km. making use of the results obtained with registering balloons, including those for humidity. [1]

In doing this, for the height and temperature at the base of the stratosphere the following values were taken.

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[1] Observat. Batavia, Verhandelingen N⁰. 4.

For the stratosphere it was assumed that temperature should increase regularly with altitude, so much so that at the level of 26 km. it would reach the value —57°.2 C,, as was observed on the 4th of December 1913.

Month	Height	Temperature	Month	Height	Temperature
Jan.	17.8 km.	— 89°.1 C.	July	16.0 km.	— 82°.8 C.
Febr.	17.6	— 88.1	Aug.	16.5	— 80.8
March	17.3	— 87.4	Sept.	17.0	— 80.5
April	17.0	— 86.9	Oct.	17.4	— 82.8
May	16.5	— 86.4	Nov.	17.6	— 86.6
June	16.2	— 85.1	Dec.	17.7	— 88.8

Having calculated this table of monthly values of airpressure, I made an attempt to draw isothermal charts of the Archipelago for Jan./Febr. and July/Sept. with the aid of the results of temperature observations made at meteorological stations in the Neth. Indies and the surrounding regions.

Founded on these charts as well as on corresponding isobaric charts drawn by Dr. BRAAK[1]), isobars have been calculated for each km.-level up to a height of 10 km., the required values for the vertical gradient of airpressure being taken from table III. Moreover it has been taken into account that in the same season the atmosphere has a contrary character (dry and wet) north and south of the equator.

The uncertainty, unavoidably attached to the isothermal lines designed, and to the values of the vertical gradient, was evidently much too great not to render the results wholly unsatisfactory. On the surface of the earth isotherms and isobars run somewhat parallel to each other, but their horizontal gradients have contrary signs. Consequently, when rising in the free atmosphere, the influence of increase of airpressure in any direction at the bottom being positive, it is counterbalanced partly by the negative influence of temperature.

The result, being a difference, is extremely susceptible to small variations in the bottomvalues of isotherms and isobars, so that it is the uncertainty attached to them, which is greatly increased and spoils the results.

Westmonsoon. In the westmonsoon the direction of the wind at

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[1]) Observations at Secondary Stations in N.-I. Vol. I.

TABLE III. Mean yearly airpressure (in mm.) in the free atmosphere above Batavia together with monthly deviations.
(Correction for latitude not applied).

Height	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N	D.	Year.
0.0 km.	0.0	0.1	—0.1	—0.4	—0.4	—0.1	0.2	0.4	0.5	0.2	—0.1	—0.3	759.5 1)
0.5	—0.1	0.1	0.0	—0.5	—0.2	0.1	0.1	0.3	0.4	0.2	—0.1	—0.2	718.3
1	—0.2	0.0	0.0	—0.5	0.0	0.1	0.0	0.3	0.4	0.3	0.1	—0.2	678.3
1.5	—0.2	0.0	0.1	—0.5	0.1	0.1	—0.2	0.2	0.2	0.2	0.2	0.4	640.2
2	—0.2	0.1	0.1	—0.4	0.2	0.1	—0.2	0.1	0.1	0.2	0.3	—0.4	603.8
2.5	—0.4	0.0	0.1	—0.4	0.4	0.1	—0.3	0.0	0.0	0.2	0.2	—0.5	569.2
3	—0.4	0.1	0.1	—0.3	0.6	0.2	—0.3	0.0	—0.1	0.3	0.4	—0.7	536.2
3.5	—0.4	0.2	0.1	—0.2	0.7	0.2	—0.4	—0.1	—0.2	0.3	0.5	—0.7	504.8
4	—0.5	0.0	0.1	—0.1	0.8	0.3	—0.4	—0.2	—0.2	0.3	0.4	—0.7	475.0
4.5	—0.5	0.1	0.1	—0.1	0.9	0.2	—0.5	—0.3	0.3	0.3	0.4	—0.8	446.7
5	—0.4	0.2	0.1	0.1	1.1	0.3	—0.6	—0.3	—0.3	0.4	0.6	—0.9	419.7
6	—0.6	0.2	0.2	0.2	1.3	0.3	—0.7	—0.5	—0.5	0.5	0.7	—1.0	369.9
7	0.6	0.2	0.2	0.4	1.6	0.2	—1.0	—0.6	—0.5	0.5	1.0	—1.0	325.1
8	—0.8	0.1	0.2	0.5	2.0	0.1	—1.2	—0.7	—0.6	0.5	1.1	—1.2	284.9
9	—1.0	0.0	0.2	0.6	2.4	0.0	—1.5	—1.0	—0.6	0.5	1.3	—1.3	248.8
10	—1.0	0.2	0.4	0.8	2.6	—0.1	—1.7	—1.2	—0.5	0.6	1.5	—1.4	216.3
11	—1.0	0.2	0.5	0.9	3.0	—0.1	—2.0	—1.6	—0.6	0.7	1.6	—1.6	187.2
12	—1.0	0.2	0.5	1.0	2.9	—0.1	—2.0	—1.9	—0.7	0.9	1.7	—1.4	161.1
13	—0.9	0.2	0.8	1.0	2.7	—0.2	—2.0	—2.0	—0.6	1.2	1.9	—1.4	137.8
14	—0.9	0.0	0.8	0.8	2.3	—0.3	—1.9	—2.0	—0.7	1.3	1.8	—1.7	117.3
15	—0.7	0.0	1.0	0.8	1.9	—0.3	—1.7	—1.8	—0.4	1.5	1.9	—1.8	99.1
16	—0.7	—0.1	0.8	0.4	1.3	—0.5	—1.7	—1.5	—0.1	1.5	1.8	—1.8	83.4
17	—0.6	—0.1	0.7	0.2	0.9	—0.5	—1.3	—1.1	0.2	1.5	1.6	—1.8	69.8
18	—0.8	—0.3	0.3	0.1	0.8	—0.3	—0.9	—0.6	0.4	1.3	1.2	—1.8	58.4
19	—0.9	—0.5	0.2	0.1	0.8	—0.2	—0.6	—0.3	0.6	1.3	1.0	—1.7	48.9
20	—1.3	—0.8	0.2	0.1	0.7	0.0	—0.2	0.0	0.7	1.2	0.8	—1.5	41.0
21	—1.3	—0.8	0.1	0.1	0.6	0.1	—0.2	0.1	0.6	1.0	0.6	—1.4	34.6
22	—1.1	—0.7	0.1	0.1	0.5	0.2	0.0	0.2	0.6	0.9	0.5	—1.2	29.2
23	—1.0	—0.6	0.1	0.1	0.5	0.2	0.1	0.2	0.5	0.9	0.4	—1.0	24.7
24	—0.8	—0.6	0.1	0.1	0.4	0.2	0.1	0.2	0.6	0.7	0.4	—0.9	21.0
25	—0.7	—0.5	0.1	0.1	0.4	0.1	0.1	0.2	0.5	0.6	0.3	—0.8	17.9
26	—0.6	—0.4	0.1	0.0	0.3	0.1	0.1	0.2	0.4	0.5	0.2	—0.7	15.3

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1) Reduced to sealevel. Index corr. of the Standardbarometer (+ 0.05 mm.) applied.

the bottom of the atmosphere is in accordance with isobars and rotational influence. It is shown by BRAAK's isobaric chart how the NE-trade, delineated on it after VAN DER STOK's results, deviates to the right in the northern hemisphere, has no deviation at all at the equator (being directed perpendicularly to the isobar), and deviates to the left in the southern hemisphere, having turned eventually to a northwesterly direction.

Above Batavia however, already at a height of ± 2 km., the wind deviates to the SW, blowing pretty parallel to the isobars, as I could judge from the isobaric chart projected for that level. There the aircurrent does not proceed from the northern hemisphere, but from the part of the ocean which lies to the SW of Java.

The motion of the air parallel to the isobar points to a stationary character of the current, which evidently cannot develop in the bottomlayers, owing to disturbances caused by friction (cf. HANN's Lehrbuch der Met. 1915, p. 437).

The pressure-gradients which maintain the westmonsoon must change signs at a certain height, because the influence of the temperature-gradient on them grows in excess of that of air-pressure-gradients, the two gradients having contrary signs.

Quite in accordance with this we see the Westmonsoon disappearing at a height of a few kilometers, and we meet easterly winds above that level.

The isobaric charts for the different levels not being reliable, I give here only the pressure-differences up to 9 km. between

Pressure-difference for Jan.—Febr.

Height	Batavia-Port Darwin	Batavia-Koepang
0 km.	2.8 mm.	1.9 mm.
1	1.8	1.5
2	1.0	1.2
3	0.4	0.9
4	—0.2	0.7
5	—0.5	0.6
6	—0.8	0.3
7	—1.1	0.2
8	—1.3	0.0
9	—1.5	—0.1

Batavia and Port Darwin (N. Australia) and between Batavia and Koepang. They have been calculated applying for P. Darwin and Koepang the same vertical gradients as were found for Batavia.

The heights at which the differences given above change sign, viz. 3—4 km. and 8 km., do not agree, however their average 5—6 km. coincides with the observed vertical extension of the westmonsoon.

Eventually the direction of the monsoon deviates with increasing height in the same spiral manner which elsewhere has been observed in aircurrents, moving to the right in the northern and to the left in the southern hemisphere. Accordingly, at the bottom the wind is WNW turning through WSW and S to SE gradually proceeding upwards.

Trade and Antitrade-wind. Disregarding the northerly components in the bottomlayer, which originate from local disturbance, the lower easterly winds have a southern component throughout the year; accordingly the aircurrent is called by me tradewind, as flowing towards the equator.

Its ESE-direction tallies with BRAAK's isobars as drawn for the months of July/Sept. Computing for these three months the pressure-differences between Batavia and Koepang in the same manner as was done for Jan./Febr. the following values were found:

Pressure-difference for July/Sept.

Height	Batavia-Koepang
0 km.	1.4 mm.
3	0.1
4	—0.2
5	—0.5

According to these figures the pressure difference disappears between 3—4 km. and higher up changes sign; nevertheless the ESE-wind not disappearing as does the Westmonsoon at its upper limit, turns to the E with decreasing force and going on turning, it gains force and reaches an ENE-direction.

In behaving so it renders it impossible to bring it in direct compliance with any isobaric chart to be derived from given data for surface-isobars and isotherms. For the trade which blows above the layers of the westmonsoon in the southern summer, it is utterly impossible owing to its high level to calculate a gradient with any degree of certainty; it may be considered remarkable that also in these levels the direction spirally turns from ESE through E to ENE, when we proceed upwards.

The usual explanation of trade and antitrade still remains acceptable viz. in the southern hemisphere the air which flows towards the equator gives birth to an ESE-current owing to the rotation. This air ascends and has to flow off. By rotational influence it deviates more and more to the left, turning from ESE through E towards ENE. In latitudes higher than that of Batavia its direction eventually becomes N and NW.

It should be observed, that a slight rising of the air suffices to cause a rapid deflux, because the horizontal plane through which rising takes place, exceeds so much in extension the vertical one through which the air flows off. For instance rising over 10 degrees of latitude (more than 1000 km.) and deflux through a layer 10 km. thick, give a ratio of a quick deflux with velocity of meters against a slow rising by centimeters.

The nature of the phenomenon of convection makes it easy to understand the existence of a level with maximum deflux; its altitude above Batavia is rather high viz. 14 km. in Aug. and 15 km. in February.

That the flow is less in the southern summer is evidently caused by the fact that the thermic equator has a more southern position in this season and accordingly that part of the zone of deflux, which lies to the north of Batavia is smaller than the one in the other season. In accordance with this also the deviation of the direction to the left is slighter.

Important information about this flow gives the drift of Ci-clouds, because these clouds float in the layers of the antitrade; at Batavia their average height is 13 km. in the southern summer and in winter 11 km.

The following mean directions of drift have been observed.

	Batavia	Pontianak	Manila
Latitude	6° S	0°	15° N
Dec./Febr.	E 1° N	E 5° S	E 35°—84° S
July/Sept.	E 25 N	E 27 N	E 6°—27° N
Oct./Nov.	E 22 N	E 27 S	E 24°—43° S

In Dec./Febr. the thermic equator is situated near to the latitude of Batavia; in accordance with it the deviation from E-drift is nearly zero. To the north of Batavia, at Pontianak the deflux already shows a northward component, and at Manila deviation to the right is great.

On the contrary in July/Sept. deflux is strong at Batavia and Pontianak, but at Manila the drift, though showing less deviation, still has a southward component, which cannot be explained without more data.

Very evident is in Oct./Nov. the contrast between the southward deflux at Batavia and the northward one at Manila.

Attention may be drawn to the fact, that above Batavia the level of strongest deflux (14—15 km.) does not much exceed that of the layer where the yearly range of pressure-variation shows a maximum; by which it is rendered probable that actually the greatest horizontal pressure-gradients occur at that level.

As further this flow should be considered to be for the greater part a consequence of equatorial rising of the air, it will evidently stop or nearly stop at the level, where this rising ends.

Now the altitude to which convection rises, is the height of the troposphere or of the base of the stratosphere, so that the upper limit of the antitrade will not differ much from the base of the stratosphere. The monthly values found for both, given below, indeed show this connection.

	Height of			
	Upper limit of the Antitrade	Difference with 17.5 km.	Bases of the Stratosphere	Difference with 17.0 km.
Jan.	18—19 Km.	1 Km.	17.8 Km.	0.8 Km.
Febr.	18—19	1	17.6	0.6
Mrt.	18—19	1	17.3	0.3
Apr.	18	0.5	17.0	0.0
May	15–16	—2	16.5	—0.5
June	12—13	—5	16.2	—0.8
Juli	16—17	—1	16.0	—1.0
Aug.	16—17	—1	16.5	—0.8
Sept.	17—18	0	17.0	0.0
Oct.	21–22	4	17.4	0.4
Nov.	20—21	3	17.6	0.6
Dec.	19—20	2	17.7	0.7

In the southern summer rising is strongest, extending to higher levels; consequently the deflux by antitrade will reach greater height too. The alleged figures for the oscillation in height fits in closely with this conception, only the range for the antitrade is much larger. No doubt the relation is not so simple as presumed above.

Uppertrade-wind. Above the antitrade reappears a flowing of air towards the equator, thus an uppertrade prevails there.

Without doubt it is a consequence of the fact that the air in the equatorial belt cools down over a higher column than in the subtropics, the airpressure sinking therefore to a lower value, by which a gradient towards the equator takes origin.

I have compared Batavian airpressures for heights above 10 km. with those PEPPLER [1]) found for the N.-Atlantic and Europe and obtained the following differences.

Height in km.	10	11	12	13	14	15	19	20	25
30°N—6°S	—3.8	—3.6	—3.3	—2.3	—1.6	0.4	3.9	4.2	2.7
45°N—30°N	—8.9	—7.9	—5.9	—5.6	—3.9	—2.7	—0.2	0.3	0.6

According to these figures for the tropics and the subtropics the reversal of pressure-difference, on which the transition from afflux to deflux has to depend, occurs between 15 and 19 km.

PEPPLER himself, comparing his pressures for 30° N (really 22½°—37½°) with those for 2½°S—22½°N, finds this reversal to occur at a height of no less than 25 km., but his figures for the tropical belt are uncertain. However they bear on the same meridianal zone as those for the subtropics, whereas Batavia is situated over a quadrant more easterly.

Consequently for Batavia the relation with the pressure-gradients cannot yet be given with certainty; however, it should be brought forward that no doubt the sinking of the uppertrade to low levels in June is closely connected with the quick falling of airpressure in that month after the maximum in May.

Where, however, does the air go which flows in?

Deflux by an upper-antitrade is not probable. On the contrary in higher layers the uppertrade turns into a strong westward current, the Krakatoa-wind. Moreover, in the stratosphere, which is the home of the uppertrade, we lack the heating by the sun, which causes the strong convection in the troposphere. It must, however, be

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[1]) Beiträge z. Phys. d. fr. Atmosf. Bd. IV.

assumed to be possible, that over large areas slow rising or sinking of air takes place, which is quite compatible with the stratified structure of the stratosphere.

Sinking is likely to occur, because the air flowing in from higher latitudes comes in from colder regions and will thus be cooled strongly and become denser.

It might also be possible that the vertical component of rotational deviating-force, which is downward for eastern winds and reaches a maximum ($2\,\omega\,v$) at the equator, favours sinking of air.

Krakatõa-wind. It is my opinion that this wind in the first place should be regarded as a stationary aircurrent. Arguments in favour of this assumption are, first the observation made in 1883 that the Krakatoa ashes were carried seven times round the globe with a pretty constant velocity of ± 40 m.p. sec., secondly that at Batavia twice (once in March and once in September) at the same height of about 30 km. an Eastwind of equal velocity was observed.

The current being stationary it will run parallel to the isobars, hence the latter will possess a meridionally directed gradient. The amount of this gradient may be calculated with the aid of the formula 1)

$$\Delta\ B = 0157 \frac{b}{760}\,\frac{T_0}{T}\, v \sin\varphi \text{ }^{1}).$$

Assuming in latitude 6° $\frac{b}{760}$ to be $\frac{1}{100}$, $T_0 = 223°$ and $v = 40$, the formula gives $\Delta B = 0.005$ mm.

The same calculation may be made for the trade in the bottom-layers.

In this case for v may be taken the mean velocity $v = 10$ m. p. s. observed at the lighthouse "de Bril" (situated in midsea S. of Celebes latit. —6°). The formula gives $\Delta B = 0.17$ mm., fairly agreeing with the amount 0.20 borrowed from BRAAK's isobaric chart.

However in this case the application of the formula is restricted as ΔB should become zero at the equator, which does not happen. Indeed we see there the trade not paralleling the isobars, but to be directed perpendicularly to them.

Regarding the Krakatoa-wind presumably the gradient disappears and changes sign at the equator and the following calculation shows how the alleged pressure-differences corroborate with this conjecture.

For the pressure difference at 25 km. height between latitudes 6° and 30° has been found 2.7 mm., or a gradient 0.11. If this

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1) HANN, Lehrb. d. Met. 1915, p. 488.

value is reduced proportionally to the air pressure at 30 km. $\left(b=\frac{760}{100}\right)$, their results $\frac{0.11}{2.2}=0.05$.

Notwithstanding the uncertainty attached to this value it is fairly large compared with the amount 0.005 found above for the gradient of the Krakatoa-wind and this makes it probable that the gradient decreases to zero when approaching the equator.

High Westwinds. Table III shows clearly that the high Westwinds predominate in the southern winter blowing the stronger the larger the northern declination of the sun is. Their average lower limit comes down lowest (16—17 km) in June.

From the separate observations themselves it follows that also at less heights W-winds occasionally were met with; conspicuously more often in March and April than in July—Sept.

This is in relation with the force of the antitrade, which has a maximum in August and a minimum in April. Therefore it makes the impression that the antitrade squeezes away the W-winds.

Berson, the discoverer of these winds, observed them in Africa in the months of August and September, but, contrary to Batavia experience, also met with them in lower levels. Now it is remarkable he did not observe at all the predomination of a strong antitrade up to an altitude of ± 16 km. as is the case for Batavia.

Gold [1]) has drawn the attention to Overbeck's [2]) theory of general circulation in which the western winds of the temperate zones approach the equator in great altitudes. Overbeck did not take into consideration the influence of the oblique position of the earth's axis, but it is evident, that through it W-winds will approach nearer to the equator in the winter-hemisphere.

Observations at Batavia indeed agree with this, the winds preponderating strongly in the southern winter.

In his theory Overbeck kept separate the currents generated only by the distribution of temperature, from those caused by rotational influence. The latter, however, also depend on the distribution of temperature. Now, it was just for the higher atmospherical layers, that he assumed a wrong distribution; not until many years later was the true one discovered by balloon-soundings.

Consequently the agreement of Overbeck's theoretical results with the observations cannot be unconditionally accepted as an explanation of the high equatorial Westwinds.

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[1]) Quarterly Journal 1910.

[2]) Sitz. Ber. Preuss. Ak. Berlin. 1888.

As it is the reappearance of Eastwinds above these Westwinds, which was experienced in Africa as well as in Java, does not fit at all with the theory.

The yearly inequality of high Westwinds above Java renders it probable that they may be an extension towards the equator of the main Westvortex of the southern hemisphere, on which general circulation is intense; especially in wintertime the vortex will be powerful.

For the northern hemisphere HANN, computing wintergradients between the latitudes 10° and 50°, found for a height of 9 km. 2.04 mm. and for 15 km. 2.90 mm. (reduced to an airpressure of 760 mm.).

Accordingly it is doubtless that in the levels of the high W-winds (17—24 km.) gradients on the southern hemisphere are strong, maintaining a mighty W-vortex.

From the vanishing of the high W-winds above the level of ± 24 km. and the appearance of the Krakatoa-wind higher up might be drawn the inference, that also the W-vortex in those altitudes will dwindle away. Presumably above the vortex southerly winds, will be blowing which nearer to the equator will turn into the stationary Krakatoa-wind.

Such winds with a component towards the equator have already been met with at very great heights in the northern hemisphere by some balloonflight-observations.

The high W-winds above Batavia also carry the air towards the equator as southern components prevail strongly. The same might perhaps be assumed to be the case above Africa according to BERSON's observations.

Consequently above the antitrade, deflux of air towards the equator takes place as well in the uppertrade as in the high westwinds, this in agreement with the alleged reversal of the pressure-gradients in the airlayers between 15 and 19 km.

Royal Magn. and Met. Observatory Batavia. Dec. 1917.

Anatomy. — *"On the Relation between the Quantities of the Brain, the Neurone and its Parts, and the Size of the Body"* [1]. By Prof. EUG. DUBOIS. (Communicated by Prof. H. ZWAARDEMAKER).

(Communicated in the meeting of March 23, 1918).

In an earlier communication [2] it was shown how the quantity of the brain in all the classes of vertebrates increases regularly with the size of the body. From species to species, and also from the one sex to the other the quantity of the brain increases proportional to the power 0.56 of the body weight; from individual to individual of identical species, however, the quantity of the brain increases proportional to only half so great a power, probably 0.22, as computed from the data at disposal.

It is not inconsistent with this regular proportionality that the composition of the brain greatly varies in the different classes of vertebrates, and is unequal, to some extent, even in different genera — this proportionality concerning only animals of equal morphological and physiological organisation, which differ only in the size of their bodies. For them we may, indeed, expect that in this most complicated organ the most intimate morphological and functional conformity should obtain.

We see, however, that the regular *enlargement* of the brain as a whole is accompanied by a disproportion in the *composition*, even in those animals which are equal morphologically and functionally, such as were compared in each case. Most striking is this quantitative alteration of the composition of the brain between those parts commonly regarded as more active, in opposition to other parts considered to be more passive, *i.e.* between the substantia grisea, which contains by far the most cell-bodies, and the substantia alba, which consists principally of myelinated nerve fibers. Besides, the cell-bodies are more densely placed in the substantia grisea of small

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[1] As the densities of the (airless) body and these constituents are pretty well equal, all about the same small amount above 1 (H. VIERORDT. Anatomische, physiologische und physische Daten und Tabellen, 3 Aufl. 1906, p. 55—60), "volume" and "weight", are sometimes indifferently used for each other in this communication.

[2] These Proceedings. Vol. XVI. (1913), p. 647—668.

animals than in that of large animals, very strikingly so in the brain of the mouse as compared with that of the elephant[1]).

Thus the total volume of the cell-bodies in large species and individuals increased less than that of the nerve fibers and of the medullary sheaths, the last being commonly regarded as an accessory isolating envelope of the solely essential, although passive, cylinder axis. This interpretation, has, however, not been confirmed; the results of DONALDSON and HOKE[2]) among others show that the average volume of the medullary sheath, in all classes of vertebrates, is equal to that of the enclosed cylinder axis, a fact distinctly pointing to a less insignificant part played by the medullary sheath than is commonly ascribed to it. This, however, does not account for the seemingly inexplicable combination of a regular increase of the volume of the brain with the disproportionate increase of the more passive fibrous parts and the constituents regarded simply as enveloping and supporting interstitium in the brain with regard to the cell-bodies contained in it.

It has been tried to explain this disproportional increase of the substantia alba, with increasing size of the body, by the *lengthening* to which the nerve fibers are subjected in proportion to the longitudinal dimension of the brain, while at the same time their *number* increases in proportion to its volume, the nerve fibers being assumed not to increase in diameter nor the cell-bodies, from which they arise, in volume.[3])

According to this explanation we are obliged to accept that with the supposed disproportion of the parts of the neurone, enforced on it independently of its character by external circumstances, functional disproportion too is enforced on it, even if we should continue to hold the opinion of the passive part played by the nerve fiber.

In the spinal cord something analogous is to be observed as in the brain; for in the cord too the substantia alba increases much more rapidly than the substantia grisea.[4]).

A "disproportion" of the same kind exists necessarily between the volume of the peripheral nerve fibers, which must lengthen

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[1]) H. OBERSTEINER, Die Kleinhirnrinde von Elephas und Balaenoptera. Arbeiten aus dem Neurologischen Institute der Wiener Universität, 1913, p. 153.

[2]) H. H. DONALDSON and G. W. HOKE, On the Areas of the Axis Cylinder and Medullary Sheath as seen in Cross Sections of the Spinal Nerves of Vertebrates. Journal of Comparative Neurology and Psychology. Vol. XV. (1905), p. 1—16.

[3]) ERNST DE VRIES, Das Corpus striatum der Säugetiere Anatomischer Anzeiger. Bd. 39. Jena 1910, p. 387—388.

[4]) This has been very clearly demonstrated by A. J. HOVY: On the relation between the Quantity of White and Grey Substance in the Central Nervous System, These Proceedings. Vol. XVI, (1913), p. 311—318.

about proportionately to the length of the animal, and the cell-bodies in the central nerve apparatus, from which they start.

Then we may ask: is it indeed a disproportion with which we are occupied here? Will not the lengthening of the nerve fiber necessarily be accompanied by an increase of the volume of the cell-body from which it arises, at least in a certain degree? The starting point of the explanation indicated above and of other similar explanations, given of the striking disproportion in the composition of the brains, although admittedly equivalent, of two aminals different in the size of their bodies, but equal in their organisation, would then be wrong, and perhaps another explanation is to be found, not leading to contradiction or even to absurdity.

So the starting-point in question, the supposed constancy of the volume of the cell-body and the diameter of the nerve fiber certainly calls for a closer enquiry. In the conception of the nervous system as composed of specific cells, the neurones, it is of principal significance to know what is the relation between the volume of these cells, which are so particularly specialized, and between the volume of their parts, and the body weight.

For the longest and very peculiar process of the neurone, the neurite or nerve fiber, it is unvoidable to assume that, when it reaches the periphery of the body, its volume increases with the length of the body, this implying an increase of the volume of the neurone. Concerning the constancy or variability of the volume of its central part, the cell-body or "ganglion cell", in the spinal cord or the brain, there still exists (unnecessarily) difference of opinion. Many neurologists still (erroneously) consider the variations in the dimensions as entirely accidental, and so slight that they are negligible in our conceptions of the structure of the central nervous system.

There is, however, evidence that differences in the size of homologous nerve cell-bodies constantly occur, in a certain relation to the size of the body; so that they have a real significance.

These differences are most striking where we compare homologous cells of animals, such as the elephant and the mouse, which differ greatly in the size of the body. The largest central parts, "cell-bodies", in the columnae anteriores of the intumescentia cervicalis (6th cervical segment) of the Indian elephant have a diameter above 3 times and a volume more than 29 times that of the homologous cell-bodies in the mouse. Apparently this is exceedingly little when compared with ratios like that of the linear dimensions of the body (60 : 1) and that of the body weight (180.000 : 1). Nevertheless a regular proportion may exist, causing in the albino rat, the body weigth of which is only $12^1/_2$ times, and the length nearly $2^1/_2$ times that of the albino mouse, the diameters of homologous nerve cells to be proportional to 5 and 4.

In order to give a preliminary idea of the order of magnitude of these differences in the size of the nerve cells, may serve that in general, with closely related species, a doubling of the length measures of the animal is accompanied with an increase of the diameter of the cell-bodies in the ratio of only 5 : 6. Given, moreover, the very irregular form of many cell-bodies, it is easy to understand that most neurologists did not ascribe any signification to those differences in size.

However, chiefly IRVING HARDESTY [1], GIUSEPPE LEVI [2], H. OBERSTEINER [3] have conclusively proved that the size of the cell-bodies in general increases with the bodily size of the species of animals. Taken as such, the data supplied by them, do not, however, prove the existence of a distinct, regular proportionality. On the contrary, although the large species in most cases are very distant from the small species, there are many exceptions, the series being in part very irregular. For certain kinds of cell-bodies this is owing to their shapes, which do not admit of accurate measurements, but assuredly to a much greater extent to the circumstance that mostly cells from different parts of the spinal cord are compared, though the size of the cell-bodies varies greatly for the same animal according to the place in this central apparatus [4], further to the circumstance that there can exist specific inequality in the size of corresponding cells according to the species of animal, and last not least to this that the examined specimens by no means always represented

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[1] IRVING HARDESTY, Observations on the Medulla Spinalis of the Elephant with some Comparative Studies of the Intumescentia Cervicalis and the Neurones of the Columna Anterior. Journal of Comparative Neurology. Vol. 12. Philadelphia 1902, p. 125—182.

[2] *a.* GIUSEPPE LEVI, Studî sulla grandezza delle cellule. I. Ricerche comparative sulla grandezza delle cellule dei mammiferi. Archivio Italiano di Anatomia e di Embriologia. Vol. V. Firenze 1906, p. 291—358. For all classes of Vertebrates: *b.* G. LEVI I Gangli Cerebrospinali. Supplemento al Vol. 7 dell' Archivio Italiano di Anatomia e di Embriologia. Firenze 1908.

[3] H. OBERSTEINER, Bemerkungen zur Bedeutung der wechselnden Grösse von Nervenzellen. (Del Volume Jubilare in onore L. BIANCHI. Catania 1913), 8 pp.

[4] We may remind here of the fact that for man, the dog, the cat, the rabbit, and the rat, all adult, E. CAVAZZANI found on an average smaller mean diameters of the cell-bodies in the ganglia spinalia between the thoracic vertebrae than between the cervical and lumbar vertebrae. (Sur les ganglions spinaux. Archives italiennes de Biologie. T. 28 (1897), p. 52—53).

the type of their species (body weights and other data to form an opinion about this were mostly wanting).

Where the aim was merely to show that in general the size of the nerve cells increases with that of the animal, the results at disposal were sufficient. But the usefulness of the available data for an enquiry into a possible regular relation gets very limited in this way.

The spinal ganglion cells are the most suitable for measurements, for they are fairly round, and have few if any dendritic processes [1]). But at corresponding places in the spinal cord, also the cells from the columnae anteriores can be compared with sufficient accuracy, as is particularly the case with HARDESTY's measurements. On the other hand the pyramidal cells of the cortex of the cerebrum and also the Purkinje cells of the cerebellum can only be compared for closely related animals, in which case alone we can be sure, also in view of the partly undefined line of demarcation of these cells, that we compare really homologous cells, at least corresponding dimensions. G. LEVI [2]) demonstrated that only of the largest cells of each kind the dimensions clearly increase with the size of the body. For the small cells — in this way I think LEVI was justified in accounting for this fact — the cell-body possesses less defined limits that admit of measurement; a great deal of its substance (for large animals disproportionally more than for small ones) evades observation, and what is measured as such is only part of the functionating cell centre.

In the same way as for the quantity of brain, an exponent of relation r can be calculated between the volume of the nerve cell-body and that of the animal by comparison of a large species and an if possible closely related small one, but with very large differences of bodily size, for so far as spinal cells and cells of spinal ganglia are concerned, also of animals of more distant relationship. Then r is found from the formula $\left(\frac{S}{s}\right) = \frac{G}{g}$, in which S and s and G and g represent the body weights and the volumes of the nerve cell-bodies of the large and the small animal.

In the following table of the most accurate available data the letters placed between parentheses, behind the values in every column, denote where the data are derived from. [3])

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[1]) Compare: G. LEVI, loc. cit. (*a*), p. 342—343.

[2]) Loc. cit (*a*), p. 330, 333.

[3]) (Ha): HARDESTY, l. c. p. 159—162 and 171—173; (DH): DONALDSON and HOKE l. c. p. 9—11; (L): LEVI, l. c. *a*. p. 331—332; (Add): W. H. F. ADDISON, The Development of the Purkinje Cells and of the Cortical Layers in the Cerebellum of the

	Body weight (Grammes)	Nerve cell-body Mean diameter (Micra)	Nerve fiber Mean diameter my: with medullary sheath ax: only axone (Micra)	Nerve fiber Length (Millimeter)
1. Homo sapiens	72000 (Ha)	58 (Ha)	8.4 ax (Ha)	800 (Ha)
2. Elephas indicus	3600000 (Ha)	84.4 (Ha)	11.76 ax (Ha)	2128 (Ha)
3. Equus caballus	562500 (Ha)	61.9 (Ha)	—	—
4. Lepus cuniculus dom.	2000	39.2 (Ha)	13.3 my (DH)	175 (D)
5. Mus norvegicus albinus	250	34.7 (Ha)	—	—
5*a*. Mus norvegicus albinus	250	16.5 Pu (Add)	—	—
6. Mus musculus albinus	20 (Ha)	27.4 (Ha)	4.2 ax (Ha)	35 (Ha)
6*a*. Mus musculus	20 (D)	13.0 Pu (O)	—	—
7. Felis domestica	3300 (D)	53.5 (Ha)	16 my (DH)	280 (D)
7*a*. Felis domestica	3300 (D)	69 Be (Br)	—	—
8. Felis leo	119500 (D)	96.5 Be (Br)	—	—
9. Canis familiaris	23000 (L)	80.8 Spi (L)	—	—
10. Canis familiaris	3750 (L)	67.5 Spi (L)	—	—

All the values refer to the adult state; the dimensions in the second column to the largest nerve cells from the columna anterior of the intumescentia cervicalis, except those in the lines 5*a* and 6*a*, which denote the cross-diameters of Purkinje cells; in the lines 7*a* and 8: the mean maximum diameters of the cortical giant pyramid cells of BETZ, and the lines 9 and 10, which refer to cells of the ganglia spinalia IV and V. All the nerve fibers are in direct or indirect connection with the intumescentia cervicalis. 1)

Albino Rat. Journal of Comparative Neurology. Vol. 21 (1911), p. 469; (O): OBERSTEINER, Die Kleinhirnrinde, l. c. p. 153; (Br): K. BRODMANN, Vergleichende Lokalisationslehre der Grosshirnrinde in ihren Prinzipien dargestellt auf Grund des Zellenbaues. Leipzig 1909, p. 83 and W. BEVAN LEWIS, Researches on the Comparative Structure of the Cortex Cerebri. Philos. Transact. Roy. Soc. London, Vol. 171 (1880), Part. 1, p. 53; (D): DUBOIS (l. c. and later determinations).

1) For *Mus norvegicus albinus* (still called then *Mus rattus albus* by HARDESTY and by DONALDSON and HOKE) the mean body weight of the adult specimen was put at 250 grammes according to several records, for Lepus cuniculus domesticus at 2000 grammes.

In the cited paper by HARDESTY an error has crept into the calculation of the volume of the cell-body of the mouse. This amounts to 10771, instead of 7974 cub. micra. The elephant has only a little more than 29 times the volume of the cell-body of the mouse, and man $9^1/_2$ times this volume, instead of 39 and 13 times.

The species compared for the calculation [1] gave the following values for r:

1.	1. and 6.	Homo sapiens and Mus musculus albinus	0.2747
2.	2. „ 6.	Elephas indicus and Mus musculus albinus	0.2788
3.	5. „ 6.	Mus norvegicus albinus and Mus musculus albinus	0.2805
4.	5a. „ 6a.	Mus norvegicus albinus and Mus musculus (Pu)	0.2832
5.	8. „ 7a.	Felis leo and Felis domestica [2] (Be)	0.2804
6.	9. „ 10.	Canis familiaris and Canis familiaris (Spi)	0.2975
		Mean	0.2825

On the whole G. LEVI's data and those of H. OBERSTEINER are not suitable for these calculations, because they either refer to cell-bodies from different parts of the spinal cord or from ganglia spinalia of different places, or rest in general on measurements that could not yield accurate results. [3] G. LEVI does furnish particularly accurate data as means of measurements of in each case 100 cells from the 4th and from the 5th ganglion spinale, both of a dog of a body weight of 23 kg. and one of 3.75 kg., self-evidently of different race and probably also of different sexes (they may be considered of different species at any rate as regards ratios of brain quantity). For the Purkinje cells of the common mouse OBERSTEINER gives the mean cross dimension, just as ADDISON for the albino rat.

The mean value of r found is now pretty accurately half the exponent of relation 0,56 found for the brain. In other words:

The brain volume increases proportionally to the square of the increasing volume of homologous nerve cell-bodies.

The cell-body, the cell-centre of the neurone is now only part, even a small part of the entire cell, for the processes, and especially the neurite or nerve fiber, constitute by far most of the substance of the neurone. DONALDSON computed for certain neurones of the spinal cord of man that the cylinder axis alone has a 187 times

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[1] Between the horse and the mouse the exponent of relation is found to be 0,2387, on the other hand between the cat and the mouse 0,3931. The significance of the deviations may be discussed in a subsequent communication.

[2] Comparison of Felis domestica with the only about four times heavier Felis pardalis, in which the mean diameters of the largest homologous giant cells of BETZ are 60 and 69 micra (B. LEWIS, l. c. p. 53 and 58), leads to a less sharp definition of the exponent. Starting from the stated ratio of the body weights 4 : 1, we find 0,3024, on the other hand 0,2830 for a ratio of weight of 4.4 : 1.

[3] Compare OBERSTEINER, Bemerkungen zur Bedeutung der wechselnden Grösse von Nervenzellen, p. 3.

greater volume than the cell-body in which it arises[1]; this becomes 374 times, when the medullary sheath, which if not genetically, at any rate physiologically belongs to the nerve fiber, is included[2]).

Now the question rises whether there also exists a regular relation between the volume of the cell-body and the entire neurone.

The length of the nerve fiber which connects a cell-body, *e. g.* in the spinal cord, with the periphery of the body, must naturally increase with the size of the animal species, indeed about proportional to the length of the body. That also the caliber of the nerve fiber is to some extent dependent on the size of the body, *cœteris paribus*, was repeatedly demonstrated, perhaps most convincingly by HARDESTY[3]) by means of a number of measurements in the same sections of the spinal cord in which the cell-bodies were measured, for the elephant, for man, and for the mouse. The area of the section of the nerve fiber appears *cœteris paribus* to grow about equally with that of the cell-body.

The subjoined series rests on calculations of the exponent of relation for the volume of the cell-body in proportion to that of the entire neurone, *the medullary sheath included.*[4]) The latter on the ground of what DONALDSON and HOKE have shown and in view of the comparison with the quantity of the brain.

Everywhere the largest neurone, with the longest neurite, of the hand is concerned.

1.	Homo sapiens	0.2707
2.	Elephas indicus	0.2513
3.	Lepus cuniculus domesticus	0.3104
4.	Mus norvegicus albinus [5])	0.2665
5.	Mus musculus albinus	0.2681
6.	Felis domestica	0.2991
	Mean	0.2777

It then appears that:

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[1]) H. H DONALDSON, The Growth of the Brain. London 1895, p. 154.

[2]) According to the result of DONALDSON and HOKE's research, l. c.

[3]) L. c. p. 171–172, Compare also: G. LEVI, l. c. p. 338, who measured the thickest nerve fibers of the sciatic. For animals that do not differ much in size, the change is by no means regular (for reasons that will be discussed later on).

[4]) The cell-body was considered as a sphere with the indicated diameter; in the calculation of the volume of the entire neurone collateral branches and the greater portion of the dendritic processes were neglected.

[5]) The figure placed after this rat rests on a calculation from data of

The volume of the neurone increases proportionally to the square of the increasing volume of its central part, the cell-body.

Thus we come to the conception that the *whole of the brain-volume*, inclusive of all that we are used to consider as intermediate and supporting substance (interstitium) of the effective elements, particularly also the neuroglia, forms just as well an essential entity as the neurone with the medullary sheath: the brain is *entirely* built up of neurones or of such elements, either cellular or non-cellular, that physiologically belong to them, though known by different names, as those connecting the spinal cord with the periphery of the body.

The explanation of the fact that with increasing size of the body the quantity of the substantia alba, which chiefly consists of nerve fibers, increases to a disproportionally greater degree, in the spinal cord and the brain, than the substantia grisea, which contains by far the most cell-bodies, is an unsought result of the found proportionality. This explanation is not in conflict with the imperative nature of the neurone as cell, as other explanations were.

On *individual enlargement* of the body within a species, the quantity of the brain increases only in the ratio of $S^{0.22}$ or not much more, probably because the *number* of cell-elements does not increase here in ratio of $S^{0.33}$ or somewhat less, i. e. of the length dimension of the body, as it does for the increase of the size of the body from species to species. It is self-evident that thus the mutual ratio of the substantia alba and the substantia grisea in the central nerve apparatus does not become different from that with bodily *enlargement of species*, because every neurone behaves exactly the same in the one case as in the other.

The explanation of the factor $S^{0.28}$, according to which the neurone centre, the cell-body, increases in proportion to the body weight S, is now to be found in the very particular behaviour of the size of the eye. Large animals have relatively small eyes in comparison to their body weight; here exists a relation of a similar nature as with regard to the quantity of the brain, which is likewise relatively small for animals of high body weight.

Shinkishi Hatai, Number and Size of the Spinal Ganglion Cells and Dorsal Root Fibers in the White Rat at Different Ages. Journal of Comp. Neurology. Vol. 12 (1902), p. 121. Of twenty of the largest cells in the sixth ganglion spinale and twenty of the thickest nerve fibers in it of a white rat of a body weight of 167 grammes, for which maturity had been reached as regards size of the cells, the mean diameters were resp. 52.7 and 13.9 micra. I found 78 mm. for the length of the longest nerve fiber of the rat of this body weight.

It can be derived from numerous determinations of the size of the eye of higher and lower vertebrates by LAPICQUE and LAUGIER [1]) that the exponent of relation for the area of the retina must in general lie at 0.28.

Hence we are induced to look for the cause of the existing enlargement of the nerve cell centre in the enlargement of the receptive area of the organ of sight.

I purpose to treat this relation and the significance of the eye-area, which in my opinion, is to be found in the ratio of the motive force and the moved mass for large and for small animals, in a subsequent communication.

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1) L. LAPICQUE, La grandeur relative de l'oeil et l'appréciation du poids encéphalique. Comptes rendus de l'Académie des Sciences. Paris 1908. (2). Tome 147, p. 209—212. — Relation du poids encéphalique à la surface rétinienne dans quelques ordres de Mammifères. Ibid. 1910. (2). Tome 151, p. 1393—1396. Also: L. LAPICQUE et H. LAUGIER, Relation entre la grandeur des yeux et le poids de l'encéphale chez les Vertébrés inférieurs. Comptes rendus de la Société de Biologie. Paris 1908. Tome 64, p. 1108—1110; G. WATERLOT, Déterminations de poids encéphaliques et de grandeurs oculaires chez quelques Vertébrés du Dahomey. L. LAPICQUE, Remarques sur la série de pesées encéphaliques recueillies au Dahomey par M. WATERLOT. Bulletin du Muséum d'Histoire naturelle. Paris 1912, p. 491—497.

According to the first determinations the exponent was found somewhat lower; in the above meant sense it became 0.25.

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